Epidemiology, or, The remote cause of epidemic diseases in the animal and in the vegetable creation ... Part 1 / by John Parkin.

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EPIDEMIOLOGY;

OR, THE

REMOTE CAUSE OF EPIDEMIC DISEASES

IN

THE ANIMAL AND IN THE VEGETABLE CREATION.

BY

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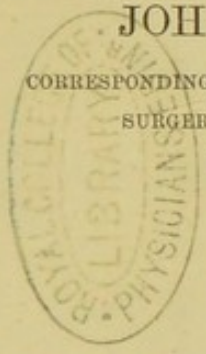

Ne quæras quis, sed quid.

PART I.

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PART I.

PART II.

Will shortly be issued, and will embrace the following subjects:—

INTRODUCTION.

Diseases—that is to say, ordinary diseases, those not arising from individual or constitutional causes—are usually divided into two classes, epidemic and endemic. The latter comprise those maladies, that are to be met with constantly in certain countries or localities; as, for instance, ague in the alluvial districts of temperate climates, and remittent or continued fever, in those of inter-tropical regions. Epidemics, on the other hand, are those peculiar affections which, springing up suddenly in some particular spot, spread over a certain portion of the habitable globe, and then disappear altogether. After a certain interval, of longer or shorter duration, they re-appear, prevail for a given period, and then subside, but only to repeat the same series of phenomena again and again, sometimes for centuries. It is these peculiar maladies, commonly termed pestilences, the cause of which is now about to be considered. In addition to the epidemics of the human race, those of the brute and of the vegetable creation will also be discussed—subjects that were previously treated of separately.

Such is the object of the present work—an inquiry into the remote or *primary*, *not* the immediate or proximate, cause of epidemic diseases, in the animal and in the vege-

table creation. This is not only an obscure and a difficult subject, but it was, until lately, an almost abandoned one. This may be ascribed to two circumstances—the presumed impossibility of solving the problem, and its entire neglect by medical writers.

Dr. Hecker, impressed with the same feeling, has made a powerful address to his professional brethren in Germany; and as his ideas coincide with my own, and are so very apposite, I cannot do better than quote them on the present occasion. After a few prefatory remarks, on the duties of physicians generally, Dr. Hecker adds:—"Let us consider only the doctrine (or pathology) of diseases, which has been cultivated since the commencement of scientific study. It has grown up amid the illumination of knowledge and the gloom of ignorance; it has been nurtured by the storms of centuries; its monuments of ancient and modern times cannot be numbered; and it speaks clearly to the initiated in the language of all civilized nations. Yet, hitherto, it has given an account only of *individual* diseases, so far as the human mind can discern their nature. In this it has succeeded admirably, and its success becomes every year greater and more extensive.

"But, if we extend our inquiries to the diseases of nations, and of the whole human race, *science is mute*, as if it were not her province to take cognizance of them. She shows us only an immeasurable and unexplored country, which many consider to be only a barren desert, simply because no one, to whose voice they are wont to listen, has given any information respecting it. . . . . Hence it is, that the doctrine of epidemics, surrounded by the other flourishing branches of medicine, remains alone unfruitful —we might almost say, stunted in its growth; for, to the weighty opinions of Hippocrates, to the doctrines of

Fracastoro, which contain the experience of the much-tried middle ages, and, lastly, to the observations of Sydenham, only trifling and isolated facts have been added. Beyond these facts there exist, even up to the present time, only assumptions, which might long since have been reduced to their original nothingness, had that serious spirit of inquiry prevailed which comprehends space and penetrates ages.

. . . . . "No epidemic ever prevailed, during which the need of more accurate information was not felt, and during which the desire of the learned was not loudly expressed to become acquainted with the secret springs of such stupendous engines of destruction. . . . . But we are constrained to acknowledge, that, among our medical predecessors, these incentives were scarcely ever sufficiently powerful to induce them to leave us circumstantial and scientific accounts of contemporary epidemics, which, nevertheless, have, even in historical times, afflicted, in almost numberless visitations, the whole human race. Still less did it occur to them to take a more exalted stand, whence they could comprehend, at one view, these stupendous phenomena of organic collective life, wherein the whole spirit of humanity powerfully and wonderfully moves, so as to regard them as one whole, in which higher laws of nature, uniting together the great diversity of individual parts, might be anticipated or discovered. Here a wide and almost unfathomable chasm occurs in the science of medicine, which, in this age of mature judgment and multifarious learning, cannot be overlooked. . . . . Up to the present moment, it might almost seem, that the most essential preliminaries are wanting for the accomplishment of such an undertaking. . . . . None of the Universities of Germany, whose business it ought to be to provide, in this respect, for the prosperity of the inheritance com-

mitted to their charge, can boast a Professor's Chair for the History of Medicine; nay, in many, it is so entirely unknown, that it is not even regarded as an object of secondary importance; so that it is to be apprehended, that the fame of German erudition may, at least in medicine, gradually vanish, and our medical knowledge become as practical indeed, but, at the same time, *as assuming, as mechanical, and as defective,* as that of France and England." *

Whether this sweeping condemnation of English writers be merited or not, will be better understood hereafter. At present, it is sufficient to remark that, independently of Dr. Hecker, whose researches have been confined to the epidemics of the middle ages, the only general history of epidemic diseases that existed, up to the present time, was written by a non-professional person, Noah Webster, an American, and the well-known author of the English Dictionary. And yet, the subject is almost exclusively a medical one, while there are few on which the attention and the pens of medical men could have been better employed. Independently of the scientific interest attaching to the study of pestilential diseases, it has a general interest not less important: in fact, the history of epidemics is the history of the human race. The fate of a campaign, as, also, that of a nation, has sometimes depended on the prevalence and fatality of some plague, which, like the host of Sennacherib, by the destroying angel, has swept away a whole army, and almost an entire population. There can be, therefore, to quote the language of one writer, no subject connected with the temporal interests and concerns of mankind in which, in the abstract, they are more fearfully engaged than in the

* Added to Dr. Hecker's work, "The Epidemics of the Middle Ages."

study of those diseases which occasionally ravage extensive districts, attacking with indiscriminate fury all persons susceptible of their influence, or exposed to the causes which ensure their propagation. War, however destructive in its sphere, famine, however pinching and disastrous, presses heavily but on the denizens of comparatively small surfaces of the earth. To the one, human conventions may put a period—human industry and the natural and speedy revolution of the seasons afford a certain remedy to the other. But pestilence, commencing in one quarter of the globe, soon spreads over the remainder; while human power has been hitherto unable to arrest its progress, and human skill insufficient to prevent the dire effects which have so invariably followed in its train.*

Springing up spontaneously, it attacks, without discrimination, all persons within its reach—the rich and the poor; the strong and the weak; man in the pride of his strength, and woman in the freshness of her beauty; helpless infancy and tottering old age; the minister of religion and the votary of pleasure; the kind Samaritan, with him who passes by on the other side; and, although last, not least, the professor of that art which, if it were perfect, would prolong the life of man, not only to three score years and ten, but to ten times three score—the primitive age! Having selected its tithe of victims, it marches on to enact the same scenes, and to commit the like devastation, in other districts. When, apparently exhausted by these conflicts, it has at length slumbered for a time, and men have congratulated themselves on the disappearance of the destroyer, it suddenly reappears; and, seeming to have acquired fresh strength by its

* History of the Epidemic Cholera.—*Lancet*, 1832.

apparent defeat, commences its attacks with redoubled fury. Knowing no distinction of country, and being unopposed in its progress by any barrier of art, or the natural boundaries of kingdoms, it numbers among its victims the Asiatic and the European; the negro and the white man; the inhabitant of the old and the settler in the new world. Not confining the sphere of its operations to the land, it traverses, with equal ease, the boundless and the trackless ocean; visiting alike the far distant sea-girt isle, and the solitary wandering barque.

Sad and melancholy as these descriptions are, fortunate would it be, if disease and death were the only evils with which the human race had to contend at such periods. But, as if the above catalogue were not enough, man—weak, ignorant, presumptuous man—has added to the amount by woes and sorrows of his own forging. The affrighted multitude, seeing their fellow-creatures cut off by the agency of a power equally inexplicable and extraordinary, and wishing to account for it by their own imperfect knowledge, have referred the cause to the machinations of their fellow-mortals. Hence the persecution of the Jews at one period, and the massacre of particular persons, or whole bodies of men, at another—individuals who, from accidental circumstances, were at the time the most obnoxious to popular suspicion, prejudice, and hatred. "The tendencies of the mind," remarks Dr. Hecker, "the turn of thought, of whole ages, have frequently depended on prevailing diseases; for nothing exercises a more potent influence over man, either in disposing him to calmness and submission, or in kindling in him the wildest passions, than the proximity of inevitable and universal danger. Often have infatuation and fanaticism, hatred and revenge, engendered by an over-

whelming fear of death, spread fire and flames throughout the world." *

Thus, in the black death of the fourteenth century, the Jews were accused by the people of poisoning the wells; and so general was this belief that, in many places, criminal proceedings were instituted against them by the local authorities, who sanctioned alike their conviction and execution. Independently of other towns—for the same tragical events were witnessed over the greater part of Germany—at Strasburg alone, 2,000 Jews were burnt alive; while at Mayence, 18,000 are said to have been put to a cruel death.† "No reasoning," says the Rev. Dr. Ireland, speaking of the plague of Milan, in 1630, "could persuade the people that they were not suffering from the malignant agency of poisoners; and so strong was the ascendancy of this notion, that not only common friends, but members of the same family, *and even husbands and wives,* grew to be suspicious of each other." ‡ Although, fortunately, we have not to record such acts in the nineteenth century, still, we are not entirely free from the effects of similar ignorance and prejudice. Thus, the physicians in Hungary, the agents of Government in France, and the monks in Spain, were accused of poisoning the wells, and, in many instances, murdered by the infuriated and infatuated populace. Worse scenes occurred in Italy in 1867. In a letter dated Florence, Sept. 15, the writer remarks: "Popular tumults, attended by the most atrocious acts of bloodshed, continue to take place in those parts of the southern provinces visited by cholera. The populace, in every case, appear to have been excited

* Preface to "Der englische Schweiss; or, the Sweating Sickness."

† Hecker, on the Black Death of the Fourteenth Century.

‡ On the Plague of Athens.

by their belief, that the cholera was propagated amongst them by the authorities or other persons, who either poisoned their wells, or had some means of infecting the air with the deadly epidemic. Particulars have just reached the city of a terrible scene that occurred within the past week at Ardore, a town in Calabria. Upon the first appearance of cholera at that place, the populace assembled in arms before the druggist's shop, loudly declaring their intention of burning it to the ground. There were no military at Ardore, except a few men belonging to a company stationed at Gerace. The officer in charge, Signor Garzoni, on hearing the tumult, immediately hastened to the spot, and endeavoured to dissuade the rioters from their purpose; but the populace had lost all respect for the authorities, who besides had no means of enforcing the law. The earnest appeals of Garzoni only served, in fact, to increase the fury of the mob, who finally rushed forward, crushing the unhappy officer to death under their feet. The druggist's shop was immediately afterwards set on fire, and the whole family within ruthlessly butchered. But the thirst for blood was not yet appeased, for twenty other persons also fell victims to the ferocity of the populace. The body of the officer Garzoni, adds the narrator of the scene, was thrown to the pigs." * The same melancholy results were observed in the New World in 1837. "In Central America," writes the editor of the "Boston Medical Journal," "the destruction of life has been melancholy in the extreme (1837), and whole districts are represented to have been depopulated. An impression that the rivers were poisoned seems to be universally diffused, which has led to several barbarous outbreaks against the Government, hardly less fearful than the cholera itself."

* Correspondent of the "Daily News."

Such are the consequences resulting from the ignorance and credulity of the unlearned. Painful it is to reflect, that the evils arising from, what it will be my endeavour to show are, the false views of the learned and scientific, have also been injurious and hostile to the best interests of humanity. I allude to the promulgation of the doctrine, that *contagion* is the sole and only cause of the extension, or propagation, of these maladies. From this doctrine have arisen those sanitary regulations which are productive of so much injury, and such irreparable losses, both to individuals and to communities at large. But these evils are trifling, when compared with the separation of the sick from their home and their kindred, and their isolation from all who might administer to their wants, with that sympathy which friends alone can feel or express—for the committal of their persons to hirelings, who, for the sake of gain, are induced to undertake such offices, is, to say the least, an inhuman and unwise measure. To find what the results of the prevalence of such a doctrine are, we have only to turn to the writers of the fourteenth century, one of whom, Boccacio, in detailing the horrors of the black plague of that period, states, "When the evil had become universal, the hearts of all the inhabitants (speaking of Florence) were closed to feelings of pity and humanity. They fled from the sick, and all that belonged to them, hoping by these means to save themselves; others carried their precautions still further, and thought that the surest way to escape death was by flight. They therefore left the city, women as well as men, abandoning their dwellings and their relations, and retiring into the country; but of these also many were carried off, most of them alone, and deserted by all the world—they themselves having set the example. Thus it was that one citizen fled from another; a neighbour from his

neighbours; a relation from his relations; and, in the end, so completely had terror extinguished every kindlier feeling, that the brother forsook the brother, the sister the sister, the wife her husband; and, at last, even the parent his own offspring, and abandoned them, unvisited and unsoothed, to their fate." Once admitted as true, such opinions could not fail, as Dr. Rochoux has observed, to become the fruitful source of the most lamentable and irrational consequences. Hence, not content with establishing *cordons*, Lazarets, quarantines, purifications, &c., villages infected with the plague have been burnt, with the acclamations of the crowd, who thought to secure their own safety by these barbarous executions.* Digne, in France, was only saved from this fate in consequence of the authorities ascertaining, at the last moment, that several other towns were also infected; and they naturally hesitated to apply the match to so many.† But enough of such scenes! It needs not, alas! the pen of fiction, or the sober but more heartrending reality of truth, to paint the horror of such woes, in order to inflame the imagination, or to raise the sympathies of any one in the present day, when both have been so powerfully, and so recently, excited by the recurrence of catastrophes, less in degree, it is true, but similar in kind to those now detailed; for although our own country has been mercifully spared during the visitation of the late epidemic, other countries have suffered severely, from both the ravages of the disease and the moral and social evils that this doctrine brings in its train.

This was the case in Jamaica, during the prevalence of cholera in 1850-51, as the following recital will show. Although I left England a few days only after the news

* Arrêts Notables du Parlement de Toulouse, liv. 3, tit. 7.

† Noticia ecclesiæ Diniensis.

arrived of the outbreak of the disease in that island, the epidemic had ceased at Kingston, Spanish Town, and that part of the island before my arrival. Finding that the disease had broken out at Lucea, on the north side of the island, I started for this place immediately. On my arrival, after a journey of three days—and that was an unusually quick one—I learnt that the disease had been prevailing between two or three weeks, although the news had only reached Kingston a few days before. Scarcely had I entered the hotel, when I was waited upon by a deputation, who designated themselves district visitors; they had come with a request, that I would go round with them and visit some of their patients. On my asking where the resident medical men were, I was told, that one was ill in bed, that the military surgeon had shut himself up in the fort, and that the other two had retired to their country houses, and only came into the town for an hour or so in the day! If the doctors showed this fear of the disease, it cannot be a matter of surprise that the common people should do the same, as was actually the case. On entering the room of almost the first patient that I visited—after taking formal possession of the town, at the request of the Board of Health—I beheld, to my surprise, *a coffin* standing up in one corner. On my asking, if the patient was an undertaker or a carpenter, they replied, "Oh! dear no; but we sent for the coffin, as soon as the patient was attacked, to be ready when wanted." "In that case" (I replied) "you do not require me; for when the undertaker is summoned, the services of the doctor are useless;" and I turned round and walked out of the house. On the friends following me, and entreating me to return, I did so, on the condition that the coffin was first removed, which was done. Fortunately, it was not required, as the man recovered. This practice, as I soon found, was universal,

although, after the above scene, the people took good care not to allow me to be cognisant of the fact: the coffin was concealed, either in the house or in the adjoining premises. Direful as this practice was, there were others still worse. Directly that life was extinct, or *supposed to be*, the body was placed in the coffin, nailed down, and consigned to the dead-cart, to be carried to the common field appropriated to cholera cases. If the cart was not arrested in its course, a body might be in the grave, and covered over with earth, fifteen or twenty minutes after the last expiration. This, perhaps, would not be a matter of much importance if life were really extinct. But supposing it were not? What then? A death, the most horrible that can be imagined. That there were such, cannot be doubted: although every effort was made by me to prevent such a catastrophe.

It was stated, on more than one occasion, that noises were heard in the coffins, but the drunken drivers of the dead-cart refused to stop. No feeling of pity could be expected from men in a constant state of intoxication; for it was only under the stimulus of drink, that any one could be found to perform these duties. Never can I forget the second night passed by me in Lucea: my room, the first night, not being in the front, or facing the street. Having been engaged visiting patients from nine in the morning until midnight, I had gone to bed so tired, that it was almost too great an exertion to undress. Scarcely had I closed my eyes, when the dead-cart came rolling by, which I should not, perhaps, have heard, but for the yelling, the shouting, and the blasphemy of the attendants. No sooner had one passed than another came, so that, being unable to obtain a wink of sleep, I rose at four o'clock and commenced my rounds.

Similar scenes were enacted at Barbadoes, the first

island visited on the second outbreak of cholera, in 1854. The Commissioners of Health state, in their Report, that persons could not be found even to carry food and medicine to the sick, so great was the fear of contagion; while it was found to be almost impossible to obtain nurses, or attendants, at any cost! When the living were neglected and left to perish for the want of assistance, it is not to be supposed that the duties to the dead—sacred though they be—were better performed. In the generality of cases, the corpse would be left alone in the house, the relatives having fled, either before or immediately after the death. In other instances, as narrated by one writer—Mordichien—children would be found with the corpse, the parent, or parents, having deserted both the living and the dead.

Again, the haste to bury the dead was so great, that two and three bodies would be placed in the same box, while portions of the body might be seen hanging out as the cart passed through the streets. The appearance of these vehicles, also, as I was informed by an eye-witness—for the epidemic had subsided before my arrival in this island—more nearly resembled knackers' carts than anything else. The scenes at the interments were still worse. When the cemetery became full, a piece of ground of about four acres, called Peazas, at the western extremity of Bridgetown, was purchased, and trenches dug for the reception of the dead. Into these the bodies were thrown, as if they were dead dogs rather than human beings. This was not all. Mr. Drumm, a respectable chemist at Bridgetown, describing a visit that he paid to this spot, and the abuse, quarrelling, and blasphemy of the carriers of the dead, adds—"It was a fearful, appalling sight, with about 160 coffins lying round, to see and hear such unrestrained and unchristian language." No wonder, then, that the writer before quoted should exclaim: "Peazas!

the horror of the living, the dread of the dying; it comes curdling, even now, to my memory; for I have seen the dying in greater dread of being carried there, than they appeared to be of eternity and judgment." * Fortunate would it be if this were all; but the worst remains to be told.

It appears that the grave-diggers had 1*s.* 6*d.* for the interment of each body; and one of them, on a particular occasion, heard a noise in the coffin when about to throw it into the trench. "He then rested the coffin on the side of the grave, and reasoned with himself, addressing his supposed auditor within the coffin. 'If I open the coffin,' said he, 'it is night, and I cannot get any assistance for you, so that you will only live a short time, and I shall lose my shilling and a-half.' Cupidity at length prevailed, and, according to his dying confession, he buried alive the unfortunate victim of cholera and mammon." † That this was not a solitary instance will be apparent by the following touching narrative, while it throws a clear light on the cause of these premature interments:—A woman at Bridgetown, as narrated by the Rev. J. Butcher, believing that her daughter was dead, sent for a coffin, and the dead-cart at the same time. The man who brought it was unable to take the body at the moment, but promised to return shortly. "During his absence, as the mother stood looking sadly on her child, she fancied she saw a movement; first in one hand then in the other, and going close to her with a light, she discerned that her eyes were open. Uttering a scream, the poor woman immediately applied stimulants, and gradually she had the happiness of seeing her daughter

* Mordichien.

† Barbadoes "Globe," June 19th, 1854.

recover. The girl rehearses now the agony she endured when, unable either to speak or move, but perfectly conscious, she heard her mother and the undertaker speak about putting her in the coffin, and was sensible that it was laid close to her on the bed."

The wonder is that I was not myself a victim to this inhuman practice. On the termination of the epidemic in a country district, of which I had charge in Jamaica, I was attacked with a low kind of fever, the result of fatigue and exposure to the night air, having been engaged generally from six in the morning until midnight in visiting patients over an extended area, and in a malarious district.* There were no symptoms of cholera, nothing like it, and I was then residing in the house of one of the most intelligent men on the island—a planter and an Englishman. And yet listen to what follows. Feeling one evening faint and giddy—the effect, I believe, of an over dose of quinine administered to me by a medical friend—and the attendant having left the room, I rose to open the door and obtain a little more air.† To my surprise, I saw, lying on the floor of the saloon, three men whose faces were quite familiar to me—too much so; they were the carriers of the dead. Knowing their object, I said to them, you may go at once, as your services will not be required: I do not intend to die yet! They slunk away, with downcast faces, like wild beasts deprived of

* No medical man in Jamaica will venture out, in country districts, after sunset, unless in cases of great emergency, for fear of contracting fever. Nothing is so much dreaded in warm climates as the *night air!*

† It is not generally known that large doses of quinine—medicinal doses—are sometimes fatal. This result is common enough in Italy; and it is universally ascribed to the improper administration of this drug. Such cases are popularly termed *accidentes.*

their prey. Had I really fainted—a most probable circumstance—my body would have been wrapped in a cloth—they did not wait for coffins in the country districts—placed in the grave, and covered over in the space of five or ten minutes, long before a person could recover from an ordinary swoon. That my grave had been previously dug admits of no doubt; for this was the universal custom in Jamaica, in the country districts. Often have I been obliged, on my *first* visit to a patient, to step over the grave that had been already dug *in front* of the cottage. The custom was, as soon as the breath was out of the body—sometimes, I believe, before—to put a rope round the neck and drag the patient from his bed direct into the grave; as such, the nearer this was to the door the easier was the task.

In the settlements, of which so many are scattered over the island, this rule could not always be observed. Coffins were then made and used. But this, so far from being a benefit, was an injury in general to the unhappy patient; for the coffins were almost invariably made on the spot, and within sight and hearing of the sick, as the following example will show:—Being requested by one of my assistants to visit two patients, whom he had previously seen, we were surprised, on approaching the settlement, to observe the glare of several torches, for it was then dark. On nearing the house, the cause was at once apparent. Four or five men were engaged making two coffins by the light of the torches stuck in the ground, and immediately *in front* of the cottage! Riding into the midst of them the workers fled in dismay, for they were unaware of our approach; we then extinguished the torches, and had the coffins removed. As the cottages, in the interior of Jamaica, are generally made with bamboos, the interstices between which are not filled up,

everything that passes outside can be seen within. What, then, must be the feelings of a patient who, while lying in bed, not only hears the sound of the hammer, but who also sees every nail driven into his own coffin? And what must be the effect of such a scene in a disease, in which fear plays so important a part? My readers can answer the question themselves; they can readily imagine what the effect would be, if they were placed in the same circumstances.*

Such are the results that attend the promulgation of, and the belief in, the doctrine of contagion—results that are worse, a thousand times worse, than the evils that the practice emanating from this doctrine is intended to avert. Were I as strong a contagionist as I am an anti-contagionist, I should blush to avow the fact, on account of the moral and the social evils that this doctrine brings in its train. "This system" (of quarantine), remarks Sir F. Maitland, "cuts up by the root all those feelings of domestic life, which are peculiarly endeared to the mind of man, in a moment of sickness and distress; rends asunder all the usual bonds of society, and places the unfortunate patient in a situation of the most desolate isolation, at the moment when the only remaining comfort of life exists in the kindness of natural friends and connexions. . . It cannot be astonishing to any man who has seen it, that even *the last dregs of hope* must expire, before any society can submit patiently to a system of discipline which can be

* Referring to the influence of terror during the prevalence of the sweating sickness in Germany, Dr. Hecker remarks:—"It often happened at this time that, amidst a circle of friends, if the sweating sickness was only brought to mind by *a single word*, first one and then another was seized with a tormenting anguish; their blood curdled, and, certain of their destruction, they quietly slunk away home, and there actually became a prey to death."—Der englische Schweiss, p. 240.

stated, at best, as only an inferior evil to plague itself." * Whether diseases be contagious or not, it is better to stand, shoulder to shoulder as soldiers do, and meet death like men, not like cowards and poltroons. It is, besides, politic; if we forsake our fellows to-day, they will, of course, forsake us to-morrow, and we may perish by a sort of retributive justice.

These evils are not confined to individuals: communities suffer in the same way, and from the same cause. When a disease is confined to a particular city, as frequently happens, all communication with the surrounding country is, of course, interrupted. Provisions, therefore, are liable to run short, and, when this is the case, the ravages of the epidemic are invariably increased, the want of food being a most powerful predisposing cause of disease. No less powerful will be the mental depression produced with persons thus shut up in a town, and doomed, as it were, to certain death.

Gafrandi, who has given a history of the plague which prevailed at Digne, in Provence, in 1629,† states, that the ravages of the disease were such that, in the space of a few months, only 1,500 inhabitants were left out of 10,000. He attributed this great mortality to the strict quarantine in which the inhabitants were placed, so that no one could pass the boundary line and retire into the country. Again; on the outbreak of the plague at Marseilles, in 1720, an Act of the Parliament of Aix, confirmed by the Council of State, prohibited, under pain of death, the inhabitants of Marseilles and of the suburbs from leaving the town. Hence, as we are told, a great amount of misery, and a mortality *truly frightful.*

* Despatch addressed to Lord Bathurst, in 1819, on the plague at Malta.

† Notitia eccles. Diniensis.

Something of the same kind would have occurred at Lucea, Jamaica, but for an accidental circumstance. A soup kitchen had been established, to which the greater part of the population resorted; for the shops were closed, all business suspended, and nothing but biscuits and salt fish to be obtained, while even these were becoming scarce. About a week after my arrival, I was told that no meat, or live stock, could be had, as the people on the adjoining estates refused to hold any intercourse with the purveyors, or to allow them to come even on the grounds. Discussing the matter with some members of the Board of Health, one of them remarked; "It is unfortunate we cannot use *turtle*." On my asking him what he meant, he replied; "Oh! there is plenty of turtle in the adjoining bay, but it would not, of course, be right to use them, as fish has been prohibited as injurious!" "Prohibited, or not prohibited," I said, "send for the turtle at once, convert them into soup, and hold me responsible for the result." This was done, and the product so much relished that the people became clamorous for it afterwards. Up to that time, I had been living on salt fish and biscuits, the former half rancid, and the latter not particularly good; but turtle then became my standing dish, or rather dishes, for it was converted into a variety. During the rest of my stay in Lucea, I lived, to use a common phrase, like an alderman; so much so, that when I afterwards removed into a country district, I often looked back with longing eyes to the flesh (*i.e.*, fish) pots of Egypt, or, of Lucea. And what was the result of all this city feasting on this prohibited food? Not an increase, but a decrease of the epidemic, which subsided gradually from this time. The truth is, there is a great deal of nonsense written and spoken about different articles of diet, during the prevalence of cholera, so that people are actually frightened out

of their wits, and know not what to eat and what to avoid. But this is a digression. It is time now to return to the subject that more immediately concerns us.

Having pointed out the evils that flow from a belief in the doctrine of contagion, let us now inquire whether any injurious results are found to arise from the opposite system—the absence of precautionary measures.

At Kingstown, St. Vincent, which I visited during the second outbreak of cholera in the West Indies, in 1854, no precautionary measures were adopted after the disease made its appearance. Like others, they had tried to keep out the disease by isolating themselves, and refusing all communication with other islands; so much so, that I was unable to land there at that time, although sent expressly by the Governor of Barbadoes; and although, as we had heard, the epidemic was then actually prevailing at the other extremity of the island. Strange to say, it was precisely in that part of St. Vincent where there is no town, no harbour, and no direct communication with other islands, that the first cases occurred. The disease then spread from north to south, until it reached the capital, Kingstown. All precautionary measures were then abandoned. Not only did the sick receive every care and attention, but the body, after death, was kept a sufficient time to prevent the risk of premature interment. Instead of the dead-cart, the funeral was conducted in the ordinary way. The friends followed the body to the grave, which was in the churchyard, in *the middle of the town*, and the funeral service was regularly performed by the rector, the Rev. H. Laborde, or his curate. In one word, the inhabitants of Kingstown were treated as Christians, instead of being buried, as in Jamaica, Barbadoes, &c., like dogs or carrion crows. And did any ill results follow from this praiseworthy conduct? Were these worthy

professors of Christianity carried off by the plague, from the presumed exhalations given out by the numerous bodies over which they performed the last offices of humanity? Did the friends and the relatives suffer more for the kindness, the sympathy, and the respect, which they paid to the living and the dead? None of these results followed. Not only was the mortality in Kingstown less than in any other town of its size in the West Indies, but the worthy rector and his curate both escaped even an attack.

A similar example was afforded in the island of Trinidad, after the authorities discovered that the strictest quarantine and isolation had failed to keep out the disease. In Port of Spain, the capital, the ratio of mortality was 12 per cent.; in Kingstown, 11; but in Bridge Town, Barbadoes, where 8,000 were cut off, the ratio was 20 per cent. of the population.

The same beneficial results followed the abandonment of all restrictions, in some instances that have been recorded, during the prevalence of the plague in Europe. The authorities at Ferrara, in 1630, struck with the ravages of the disease in some neighbouring towns, subjected to the strictest quarantine—for the houses of individuals, in which the plague appeared, were surrounded by *a cordon*, the same as the town—caused every person, as soon as attacked, to be taken out of the house, as well as all the other inmates, and to be removed to another locality. The house was then purified and ventilated, and nó one allowed to enter for a month. The good effects of this system were so apparent, that it was considered to be of public utility to record the results in the public annals of Ferrara.* A similar plan was afterwards successfully

* Muratori: Governo della peste e delle maniere de guarda sena. Modena. 1714.

adopted at Rome, in 1657, by Cardinal Gastaldi, who was charged with the sanitary superintendence of the city. It was remarked, previously to the adoption of this system, that when the disease broke out in a house, nearly all the inmates were attacked, whereas, among those who were sent away, not more than five per cent. fell sick.

If such are the reasons that can be assigned, why we should not act on the belief that diseases are propagated by contagion, what shall we say, if it can be shown that the doctrine itself is a false one? That such is the fact, it will be my object to demonstrate in the next chapter.

CHAPTER I.

THE DOCTRINE OF CONTAGION.

In the first ages of the world—periods when intercourse and communication between different and distant nations were either very limited, or else almost entirely unknown—people could have had no other theory, in order to account for the production of epidemic diseases, than that which they entertained with respect to all other, or endemic, diseases. As it would be impossible to ascribe the production of endemic diseases to contagion—inasmuch as they only appear at particular periods of the year, and are confined to certain situations of limited extent—and, as the inhabitants of one town must have been ignorant, when an epidemic sprung up, that the same disease had previously visited other localities, or that it had subsequently spread to distant regions, contagion, or infection, could not then, we may presume, have entered into their minds. Hence the aphorism of Hippocrates—*aer est omnium rex morborumque causa,* applied equally, at that period, to epidemic and endemic diseases. When, however, commercial traffic and social intercourse became more extended and frequent, and the occurrences which took place in one country or town were known to the inhabitants of other regions, a different opinion began to be entertained. Observing that epidemic diseases are characterized by their universality and extent, and by bringing under their influence, not a few individuals in some single locality, but a large portion of the human race, certain theorists attempted to show, that these diseases

were produced by human, not by natural, agencies. This was more particularly the case during the prevalence of the Black Death—a disease that sprung up in the East, and then spread over the whole of Asia and Europe during the fourteenth century, and when, as it would seem, the doctrine of contagion was first generally promulgated.

That certain local diseases were considered to be contagious ages before this, we have proof from Holy Writ. Lepers, for instance, were enjoined, by the law of Moses, to separate themselves from their companions; and the rules for their treatment are alike precise, complete, and severe. But there are no instances on record of the application of the doctrine to epidemic diseases, or of the isolation of the sick during their prevalence. And yet, the history of the several plagues that occurred, during the sojourn of the Israelites in Egypt, has been given. The Arabian physicians, also, who had such frequent opportunities of observing and investigating the plague, regarded it simply as an epidemic, due to the same causes as those which gave origin to other, or endemic, diseases. It was not until the middle of the sixteenth century, that the doctrine of contagion or infection was established, having been promulgated and formulated by the celebrated Verona doctor, Fracastor. This doctrine, unlike the majority of other doctrines and theories, was not only eagerly adopted at the time, but it has continued, with slight modifications, to the present day. The reason is clear. It offered a very easy solution of a most difficult problem, for as there is said to be a cat in every house, to whom all untoward accidents are usually ascribed, so, also, there must be one or more persons in every situation, on whom the sin of the propagation of epidemic diseases can be readily laid. But, as it would be wrong to allow either the cat or human beings to be unjustly accused, it will be desirable

to ascertain on what foundation the doctrine of contagion is based.

According to the Fracastorians, the cause of all pestilential diseases is a specific virus, which is given out by a sort of exhalation from the bodies of the sick, infecting, to a certain but limited extent, the surrounding air. They also conclude, that the direct contact of persons and things with a diseased body is sufficient to propagate the disease. Hence, there are two methods, according to these theorists, of spreading infectious diseases, by mediate and immediate contact. As to the nature or quality of the virus, no explanation was offered, and yet this is an important consideration. If diseases can be propagated by the inspiration of an infected atmosphere surrounding the bodies of the sick, the poison must be a gaseous or volatile substance, or, at all events, a substance of a specific gravity less than that of atmospheric air. If, however, these diseases are produced by direct contact with a diseased body, the poison ought to be in a fluid or solid state; otherwise it would not, we may presume, be retained either by animate or inanimate substances; more especially if the statements made, on this part of the subject, bear the least semblance of truth. Thus we are told, that the virus of contagion attaches itself to certain bodies, called *contumacious*, which not only allow of its transport to any distance, but which are susceptible of preserving it intact for thirty or more years. Are we then to assume, that the virus is given out from the bodies of the sick under two different forms, that of a gaseous or volatile, and that of a fluid or solid substance? This is possible, but it is not probable.

If unable to determine the exact nature or quality of the agent productive of epidemic and pestilential diseases, we shall be still more puzzled to account for the manner

in which it produces its effects. If these diseases be propagated from individual to individual, and by mediate contact, there must be numerous instances in which only a small portion of poison can be imbibed—the contact of the healthy with the sick being sometimes only slight, temporary, and accidental. So, also, if a piece of rope, a morsel of straw, or a flower, sent by a lover to his mistress in a letter, be sufficient to produce, and have in fact produced, as we are gravely informed, an attack of plague; an homœopathic dose only of the poison could have been inhaled or absorbed by the individual who handled these articles. We must therefore infer, either that the *virus* of contagion is of so subtle and powerful a nature that, like prussic acid, it is capable of producing an attack of plague, or other contagious disease, in the smallest possible quantity; or, else, that the poison, after its introduction into the human body, is capable of reproducing itself. The latter is the only conclusion which can be drawn, if the doctrine be true, from a consideration of all the facts bearing on this important question.

In the first place, if a minimum quantity of the poison can produce an attack of plague or other disease, those who are in constant attendance on the sick, and who, consequently, must have imbibed a larger quantity of the deleterious matter, ought not only to be attacked in larger proportion than other classes, but also with greater intensity. This, however, is not the case; the contrary is, rather, the fact. In the next place, those who have been accidentally and temporarily exposed to some supposed source of infection, and who could only have received a minimum dose of poison, have had the disease in as severe a form as others. Hence it has been inferred, that the germs of contagion, when introduced into the blood, reproduce themselves; more especially as some of

those, exposed to some presumed source of infection, have not been attacked until many days—six, seven, eight, or more days—after. The process itself was compared to that of fermentation—the blood being the *must*, and the septies virus the *ferment*. In this way a small quantity of leaven, or poison, would be sufficient to leaven or render poisonous the whole mass—a doctrine that received a considerable impulse after the discovery by Schwann, in 1836, of the yeast plant. If the ordinary process of fermentation, it has been argued, be the result of the presence of a living organism, an explanation is at once afforded of the multiplication of the virus within the system—the great characteristic of these vegetable organisms being their rapid development and propagation in an appropriate pabulum.

This theory, so simple and so fascinating, has found numerous advocates, not only formerly but in the present day—a circumstance not to be wondered at when we remember, that it is the oldest of all theories—as old as the time of Hippocrates. The latter, however, although concluding that diseases were produced by a sort of fermentation, thought that the poison was derived from the air not from the bodies of the sick. Even chemists have entered into the arena, in order to do battle, on this vexed question, with those to whom the subject more particularly belongs. Thus Baron Liebig, who, since the death of Davy and Faraday, may be regarded as the modern apostle of chemistry, thus expresses himself:—"A substance, in the act of decomposition, added to a mixed fluid in which its constituents are contained, can reproduce itself in that fluid." And Sir Thomas Watson, alluding to the opinion of Liebig, remarks:—"In order, therefore, that a specific animal poison should effect its own reproduction in the blood, and excite that commotion in the system, which

results from the formation and expulsion of the new virus, it is requisite that a certain ingredient (analogous to the gluten in the brewer's wort) should be present in the blood, and this ingredient must have a definite relation to the given poison." And Sir Thomas adds, in another place:—"Thus the virus of smallpox (which virus is formed out of the blood) causes such a change within the blood as gives rise to the reproduction of the poison from certain constituents of that fluid; and whilst the process is going on, the natural working of the animal economy is disturbed: the person is ill. The transformation is not arrested until the whole of that ingredient in the blood, which is susceptible of the decomposition, has undergone the metamorphosis." * Mr. Simon, who draws the same conclusion, remarks:—"By vaccination, therefore, all the substance, susceptible of change by the virus, becomes so altered, that an additional quantity can produce no further change, so the person becomes insensible to its influence."

Several objections can be raised to these conclusions. If all the ingredients in the blood, susceptible of change, undergo decomposition, on the introduction of a certain portion of the specific poison into this fluid, how does it happen, we may ask, that the effects resulting from vaccination and inoculation are so slight, when compared with the natural disease? In the instances referred to, we have ocular demonstration that a poisonous, or morbid, matter is introduced into the blood; and yet only a single vesicle or a few pustules will be produced, while, in the natural disease, the body will be covered from head to foot with a pustular eruption! How, then, is this difference to be explained? It cannot be explained by a reference to this fermentation theory. Were this doctrine true, all

* Principles and Practice of Physic, vol. ii., p. 790.

contagious diseases ought to be invariably fatal: the fermentative process, when once set up in the system, would go on until all the *must* or ingredient in the blood, susceptible of change, had been converted into *ferment*. The fluid in the blood vessels would then be no longer blood, but a new compound; and, as no other compound can perform the functions of the vital fluid, death would be the result. We have proof, in fact, independently of induction, that this fermentative theory is a false one; blood has been drawn in all diseases and in all stages of every disease, but no one ever saw that fluid in a state approaching even to that of fermentation. We may therefore conclude that this fermentation theory is an erroneous one.

There are, however, two species, or groups, of fungi, the yeast, or ferment plants, and the putrefactive fungi, which are developed during the process of putrefaction. It is to the latter species that certain writers have referred the production of cholera and other diseases. That fungi have been found in the stools of cholera patients is undoubted; a result that might have been inferred *à priori*, these plants being rapidly produced in substances that have undergone only a partial decomposition. The mucus and other organic substances present in the evacuations will necessarily, when exposed to the air, undergo decomposition, and the putrefactive fungi be immediately developed —an effect that appears to take place in an incredibly short space of time. The spores of fungi are so light that they float in the air; and as they appear to be universally diffused in this fluid, they will be deposited on everything, ready to be developed in their appropriate medium. Dr. Beale remarks:—"That fungi are not essential, and that they result from the development of germs derived from without, coming in contact with the

discharges already in a state of incipient decomposition, seems proved by the fact, that specimens of the secretions in which they usually abound, and in which all the essential morbid characters are remarkably distinct, may be obtained, which are altogether free from bactaria and fungi; while, if these very specimens be kept for some hours, at the temperature of the body, bactaria make their appearance." * As, also, some of the matter thrown off from the intestinal mucous surface must be in a state of partial decay, even before it passes out of the body, there is no reason why fungi should not be developed within, as well as without the body, under such circumstances, if the spores be present. That such is the case has been generally inferred. It can hardly be otherwise, if the spores exist constantly in the atmosphere, as we are certain is the case; for the production of these plants, at all times and under all circumstances, cannot be explained in any other way. If, therefore, spores be constantly present in the atmosphere, the probability is, that they would enter the system with the air inspired. According to the preceding author, " these vegetable germs (bactaria) have been found in the alimentary canal, and in the interior of the epithelial cells of the mucous membrane of the intestines, in cholera; as, also, in other and slighter affections." They have also been found in the blood vessels, both in this disease and in the cattle plague. Hence Dr. Beale infers, that these spores exist in the blood and tissues at all times. But their presence there will be perfectly innocuous, unless they meet with an appropriate pabulum—decomposing matter. As this can only occur under particular circumstances, in states of

* Microscopical Researches on the Cattle Plague. By Lionel S. Beale, M.B.F.R.S. Appendix to Third Report of the Cattle Plague Commissioners.

disease, not of health, the fungi must be regarded, not as causes, but as effects of some other and antecedent cause.

The same arguments will apply to other diseases, which have been referred to the same cause, and with apparently more reason, inasmuch as fungi exist in these to a much greater extent than in the epidemic cholera. This is the case with the disease among cattle termed "the blood," and also with the "malignant pustule" in man and animals, which have been referred by M. Davaine to the presence of vegetable organisms—bactaria or *vibriones.** As, however, the spores of these fungi exist in the air at all times and under all circumstances, and as these diseases are only developed at particular times, and with particular individuals, it shows that the presence of these vibriones in the system is an effect, not a cause, of the morbid state of the blood that then exists. These conclusions have been confirmed by some experiments of MM. Leplat and Jaillard. They found that rabbits, inoculated with the blood of a cow that had died of anthrax, or carbuncle, were attacked with the same symptoms, and died. Nevertheless, it was impossible to discover, after death, the slightest trace of bactaria, or other genera of fungi, in the blood, or in the secretions of these animals. The converse of this experiment was then made. A clot of blood was washed and separated from the supernatant liquid, and the rabbits were inoculated with some of the latter fluid, swarming with bactaria. Not the least symptom of disease was produced. They therefore concluded—1st. That the presence of bactaria in the blood or in the secretions is merely an accidental circumstance (*épiphénomène*), and cannot be considered as a cause.

* Comptes Rendus, August, 1864.

2ndly. That the blood of the spleen (in the disease called "the blood," *sang de rate*) is the more infectious, when it contains less bactaria." *

That singular and destructive disease, which has prevailed of late years among the silkworms in France, and termed *Pébrine*, has also been referred by Pasteur to the presence of those organisms termed *Psorospermiæ*.† Notwithstanding the labours and the researches of this distinguished chemist, who spent some years in investigating the cause of this alarming disease, which threatened the total destruction of the silkworm in France; and notwithstanding that the conclusion of Pasteur has been accepted by all scientific men, we may yet be allowed to question its validity. If this disease be due to the presence and the multiplication of these parasites in the bodies of the silkworms, how is it that *pébrine* was never observed before 1840? This disease was then as new among this species of the animal creation as the epidemic cholera was with man, in 1832. As these organisms must have been co-eval with the silkworm, we have a right to conclude, that the same result would have occurred long before, if it were due to the operation of such a cause. That these parasites existed in large numbers has been clearly shown by the microscopical researches of Pasteur; but, then, we must regard them simply as effects, having been developed by a morbid change or decomposition of the tissues, produced by another and antecedent cause.‡ Previously to this, another disease, termed *muscadine*, and

* Comptes Rendus, 1865, p. 298.

† Sur les Maladies des vers à soie.—Comptes Rendus, 1865, p. 506.

‡ The bodies of the silkworms were covered with black or gangrenous spots, and hence the term *pébrine*.

supposed by Bassi to be caused by a vegetable parasite, had also prevailed among the silkworms in France; but it was neither so general nor so fatal as the former.

That fungi do not and cannot produce disease in the animal creation, we may be assured from the fact that many species form articles of diet with man; while as Dr. Beale has remarked, "some animals devour fungi in every mouthful of food they take, yet these animals live and thrive on these terrible agents."

Instead of vegetable organisms, epidemic diseases have been referred, since the time of Linnæus, to animal organisms. Remarking that the blight in vegetables was always accompanied by swarms of insects, and regarding the latter as the cause of the former, this celebrated botanist inferred, that epidemic diseases in man were also the effect of the same insectile agency. This opinion has been advocated by other writers, and particularly by Sir Hy. Holland, in an Essay, published in 1839.* He remarks:—"The question is, what weight we may attach to the opinion that certain diseases, and especially some of an epidemic and contagious kind, are derived from minute forms of animal life existing in the atmosphere under particular circumstances, and capable, by application to the lining membranes, or other parts, of acting as a virus on the human frame." This theory, like the vegetable one, was advocated for the same reason as the latter, viz., that it offered an explanation of the problem, the presumed multiplication of the virus of contagion within the body. That might be, but, then, this is merely the enunciation of one hypothesis in order to prove a previous one, for this multiplication of the virus within the body is an entirely gratuitous assumption.† Granting, however, that

* On the Hypothesis of Insect-Life as a cause of Disease.

† As Aristotle remarks: *Posito uno absurdo multa sequuntur.*

it be the fact, we should then have to prove the presence of these insectile agents in the system. But this proof has never been afforded in any epidemic or contagious disease. On the contrary, Sir Hy. Holland states, that these destroyers of the human race are "minute, beyond the powers of all sense."* Having thus an invisible enemy to cope with, it will be better to leave him until he manifests himself to us poor mortals in some tangible shape. We have enough to do in combating those that are visible!

Instead of insects, more recent theorists, as Pacini, Beale, etc., have referred the production and propagation of contagious diseases to the very lowest organisms, or, rather, to organic particles. This new doctrine has received the name of "The Germ-Theory of Disease." As Dr. Beale has written a work expressly to advocate this theory, we cannot do better than turn to the arguments contained therein, in order to ascertain its truth or its fallacy.

These particles, corpuscles, or disease germs, have been discovered in the blood by the aid of a high magnifying power. "A disease germ," according to Dr. Beale, "consists of soft matter in a living state, and behaves in a manner peculiar to matter which *is alive*." It is less than the 1-100,000th of an inch in diameter, and is often so very clear and structureless, as to be scarcely distinguishable from the fluid in which it is suspended. It is further inferred, that these germs have been detached from already existing living matter; that is to say, from the germinal or living matter of the blood. The conversion of this matter into disease germs is thus explained. "Disease germs are liable to be suspended in the air we breathe, or they may be disseminated through the water we drink, or

* Medical Notes and Reflections, 2nd Edition, p. 583.

hidden in the food we eat." It is principally through their suspension in the atmosphere that these germs are supposed to find their way into the system, either by means of the inspired air, or through the skin, the mucous surface of the conjunctiva, etc. "In all cases in which disease germs produce their characteristic effects, they reach the blood. Until they have entered this fluid there is no possibility of their exerting any deleterious effects upon the system."* When present, the increase and multiplication of these germs within the body may, Dr. Beale infers, be explained in this way.

"1. That the contagious material, passing into certain portions of the living germinal matter of the organism, might excite in these new actions, and communicate to them the same properties which the original particle possessed. Or 2. The contagious particle or particles having gained access to the fluids of the uninfected organism, may absorb nutrient matter, grow and multiply, giving rise to a progeny exactly like themselves in every particular."† Although Dr. Beale adds: "It is open to discussion which of these two views is supported by the greatest number of facts," he inclines to the latter hypothesis. It is remarked in another place:—"Upon the whole, then, I venture to conclude that *the millions* of contagious particles produced in the organism in eminently contagious diseases, are all the direct *descendants* of the very few, or perhaps even *single particle* first introduced. ..... Concerning the conditions under which these germs are produced, and of the manner in which the rapidly multiplying matter acquires its new and *marvellous* specific powers, we have much to learn; but with vegetable

* Disease Germs: their real nature.

† Loc. cit., p. 151.

organisms the germs have nothing to do."* With the last conclusion we are in accord, but here our agreement ceases, as will be presently seen.

Dr. Beale remarks: "These poisons (the disease germs) not only seriously derange the healthy functions, but having entered the body they multiply many million-fold. They are *living*, and increase as living particles alone increase. They grow, they feed upon the nutrient juices of the organism and upon the tissues, and, in some cases, flourish at their expense and destroy them. The poison, when it enters, may be so infinitesimal in quantity that it can neither be measured nor weighed, *nor*, under ordinary circumstances, *seen;* but having gained access to the blood and tissues, it increases to such an extent that, in many cases, sufficient is produced *in one subject* to infect hundreds of persons—the population of a town, or even a whole country."† Not only is all this pure hypothesis, but the conclusions are the most gratuitous, the most unwarranted that were ever made by a scientific observer. That the particles or corpuscles exist in the blood is undoubted, but not the slightest proof has been afforded that these particles are the germs of disease. There is nothing, in fact, to distinguish them from the other particles in the blood, either by their colour, their form, or other peculiarity. This is allowed by Dr. Beale himself. He ob-

* By Pacini's estimate 1,000 millions of his particles would not occupy more space than a cubic millimetre, and there are 1,000,000 cubic millimetres in a litre of water. Allowing that the dejections of a cholera patient amount to eight litres, there would then be, in this quantity of fluid, 41,000 million corpuscles, if the dejections be filled with them. This number would be sufficient to destroy, not one but 1,000 persons, provided only that they be morbid particles, and that the cholera is due to their presence in the system.

† Monthly Microscopical Journal, Oct., 1870, p. 205.

serves: "It will, probably, strike many as very remarkable, that the highest magnifying powers hitherto placed at our disposal, serve but to convince us that a minute particle of the bioplasm of the most malignant tumour, or the most rapidly growing pus corpuscle, resembles, in every particular that we can ascertain by observation or experiment, a minute particle of healthy living bioplasm, from the blood or from the tissue; and it is proved beyond a doubt, by the same means of inquiry, that the living particles of bioplasm in vaccine lymph cannot be distinguished from those present in normal lymph or chyle. ..... I think we shall find ourselves compelled, by the necessities of the case, to refer the properties of these different substances to what must be termed a difference in *vital power*."—(P. 162.) This, to say the least, is a very extraordinary confession. We are first told that these germs are the product of other germs; and then, when we expect to have it announced that the infectious matter, like the pus of a smallpox pustule, has been discovered or can be discerned, we are informed that they owe their properties to a difference of *vital power!* If this be the conclusion to which Dr. Beale has been driven by *the necessities of the case*, he must have been sadly in want of an argument to substantiate his theory.

One of the arguments made use of by these theorists is, that these particles, or disease germs, possess movement, and, consequently, that they must be living units. Now without waiting to inquire whether non-living matter be not sometimes possessed of a similar property, it is sufficient to remark, that the same vital movements may be observed in the white, colourless blood corpuscles, and also in the bioplasm of the tissues, the cornea, the cartilage of bones, etc. These movements take place in every kind of bioplasm or healthy germinal matter. "When

blood is examined under a power of 700 diameters or upwards, here and there colourless, slightly granular, and apparently spherical bodies will be seen amongst multitudes of the well-known blood-corpuscles. These are white, or colourless blood-corpuscles! They consist of *living* bioplasm or germinal matter, and exhibit movements like those referred to in the amæba (the lowest and simplest forms known), and in the mucus-corpuscle. The movements continue for *many hours* after the blood has been withdrawn from the body."—(P. 19.) This property, therefore, is insufficient to distinguish these particles from other particles, or to entitle them to be called separate entities.

Another circumstance mentioned in connexion with these disease-germs is their increase, or multiplication, by a sort of exudation from their surface, or covering. But this property is also common to all the other particles of the blood, or, in other words, to the germinal matter. Hear what Dr. Beale himself says on the subject:—"The only material in the organisms of living beings capable of growth and multiplication is that which I have recently named *bioplasm*, hitherto known as germinal, or living matter." (P. 27.) The growth and multiplication of these bioplasts, as Dr. Beale calls them, or particles and corpuscles, is best seen when the blood, from any cause, becomes arrested in the minute capillaries. They then escape through the coats of these vessels, and form, in great part, the exudation and swelling that then occurs. Finding, therefore, that these disease-germs differ in no respect, excepting in size,* from the other particles in the blood, the probability is—and I throw out the suggestion with all humility and deference, not being a professed

* Dr. Beale remarks:—"I consider some of the most minute particles present to be alone the active agents."

microscopist — that they are not morbid, but merely healthy particles of the vital fluid, or the blood. What has been said by one writer, respecting the cattle plague, will probably hold good with respect to all other diseases to which this theory has been applied. "Dr. Beale has examined portions of infected blood, textures and mucous discharges, with the highest magnifying powers that exist, viz., one-fifth of an inch focal adjustment. This magnifies 2,800 diameters; or, to express the magnifying power by some examples, an inch would appear to extend over 111 yards, and a child three feet tall would look as high as Mont Blanc. With such power, particles of even 1-100,000th of an inch in diameter, having any distinct character, would not be passed over. But he has found no definitely formed substance that can certainly be said to be the cause of the cattle plague. He finds a great increase of granular matter, but no new appearance decidedly characteristic of the disease." *

Like the former theorists, the advocates of this doctrine dwell much on the circumstance that it offers a ready explanation of the presumed increase of the virus within the body. But the explanation thus offered in support of the doctrine, the same as that advanced for the fermentation theory, is fatal to it. If these organisms can and do reproduce themselves, when present in the blood, in the tissues, and in the secretions, there can be nothing to prevent their self-multiplication, *ad infinitum*, short of their destruction or that of the patient. As it would be folly to suppose that these germs could be destroyed by any means with which we are at present acquainted, if it be true that a cubical inch of water, and, consequently, of blood, can contain millions of particles, they would go on increasing as long as any pabulum—the nutrient portion

* Third Report of the Cattle Plague Commissioners, p. vi.

of the blood—remained. When that were destroyed the patient must die, and these diseases be invariably fatal. But this is not the ordinary result. Fatal as pestilential diseases sometimes are, the half, and even two-thirds, will frequently recover. No such result as this would, or could, be witnessed if these lilliputian cannibals were present in the body.

That the theory is an erroneous one may be shown in another way. If a patient who has been attacked with, say, plague or cholera, in one locality, removes immediately to another, in which the causes productive of disease are not in operation, he will almost invariably get rid of the disease, even without the aid of medicine. M. Penay, principal surgeon of the 3rd cavalry regiment, stationed at Neguillé (Lower Egypt) during the epidemic of 1841, states that they had twelve plague patients dangerously ill when they received orders to leave for Zagazig. "They were placed on board boats, the healthy and the sick together; and, as soon as the patients began to leave the *epidemic focus*, they experienced an immediate amelioration of their symptoms; and when they arrived at Zagazig, after a voyage of ten days, nearly all were convalescent, one only having died." * Such a result as this would not, and could not, be observed if the individual carried with him—in his blood or in his body—those living organisms to which the disease is ascribed by these theorists. They would live, thrive, and multiply the same in one locality as in another; and the result would be death, not recovery.

Another argument which may be employed is no less cogent. Some diseases of this class are so rapid in their course, that there is no time for the development or the propagation of the germs, or the virus, in the manner now

* Rapport au Conseil de santé du Caire.

indicated. The influenza, which followed in the wake of the cholera, in 1832, traversed the Continent of Europe, from Russia to England, in the short space of a week; while the disease spread over the whole of London within forty-eight hours, all the members of a family, or a household, being attacked almost simultaneously. In this instance, as is evident, there was actually no time for the transit of living germs from one country to another, unless they had been sent by electric telegraph, which, however, did not then exist. There was still less time for the development and multiplication of these germs in the bodies of those attacked, or for their propagation from individual to individual.

Precisely the same results have been observed, occasionally, in other diseases, and particularly in the epidemic cholera. At Bassorah, from 10,000 to 18,000 perished in eleven days, which would give about one attack or one death every minute during this period. Now, although there might have been time for these entities to pass from one body to another, if they travelled at railroad speed, there could have been no time for their subsequent propagation in each of their victims. At Muscat, scarcely ten minutes elapsed, in innumerable instances, from the commencement of the attack, before life was extinct. At Punderpoor the attack was so sudden, that people were seen tumbling over each other in the public streets, as if struck by lightning. In the Black Death, also, to quote the expression of Hecker, "many were struck, as if by lightning, and died on the spot." To talk of disease being produced and propagated by living entities, or by contagion, in cases like this, is an absurdity.

Being thus unable to ascertain how the virus of contagion is produced, or what its nature and properties are, we must adopt some other method in order to ascertain if

the doctrine of contagion be true. There is only one method to be pursued under such circumstances : this is, to discard theories altogether—throw them to the winds, together with the germs of contagion—and trust only to facts and to experience. We shall then find ourselves, not in the unknown and wide realms of fancy, but on *terra firma*,—on which we may rest with something like security. The only thing to be guarded against is to ascertain that the so-called facts be facts, not invented or perverted tales.

If we turn to the works of the contagionists, more especially to those of the middle ages—an epoch of darkness, ignorance, and superstition—we shall find the most extraordinary accounts of the propagation of plague and other diseases. To repeat these now, or to attempt to refute them, would be merely a waste of time. We may judge of the value of them by some of those that have been published in the present day. At one of the Meetings of the Academy of Sciences in Paris, in 1832, M. Moreau de Jonnes attributed, *on the authority of diplomatic documents* that had been sent to him, the outbreak of cholera in the town of Oranbourg expressly to the arrival of a caravan from Kirguis. Baron Humbolt, who happened to be present, stated that, on the contrary, the disease appeared with great intensity in Oranbourg *three months previously* to the outbreak, at which time he was himself there! But for this accidental circumstance, this tale, like so many others, would have been handed down to posterity as a proof, "strong as holy writ," of the contagious nature of the epidemic cholera. Talk of the Arabian Nights' Tales, they are nothing compared to the tales and the romances of the contagionists! Again, Dr. Gaëtani-Bey stated, that there was not a single case of plague in Cairo in 1835 in any of the establishments,

and the barracks in particular, placed in strict quarantine. But M. Aubert-Roche stated in his Report that he received into the hospital of Ras-el-Tin, of which he was the physician, at this very time, and from these very barracks, 300 plague patients! With such fabulous tales as these, it is not surprising that the doctrine of contagion should have prevailed so long, more especially as, being generally believed in, no one took the trouble to ascertain its truth.

It was not until the last visitation of the plague in France, in 1720, that a reaction took place, and that the truth of the doctrine of contagion was impugned. Chicayneau, Veruy, and Deidier—who has been styled the chief of the anti-contagionists—ridiculed the notion, and attempted to show its fallacy.

Still the general opinion, both of the profession and the public, continued in favour of contagion; and the French surgeons, who accompanied the army to Egypt at the end of the last century, all concluded, with the exception of Assalini, that the plague was propagated by contact with the sick. Thus matters remained until 1835, when a number of physicians from France, Germany, and Italy had occasion to observe the visitation of plague in Egypt in that and subsequent years. Concluding, with the generality of persons, that the disease was strictly contagious, they changed their opinion after having come in contact with the plague, as did MM. Brayer and Cholet, who studied the disease previously at Constantinople in 1819, 1826, and 1834.

Such being the case, it will not only be interesting but important, in the highest degree, to study the facts which led to a change of opinion in the minds of these philanthropic observers. The first conclusion at which they arrived was, that contact with the sick did not produce an attack. Numerous instances have been adduced by

these writers in proof of this conclusion: one or two examples will suffice on the present occasion.

Dr. Ibrahim, a physician at Cairo, states that he was called to see the wife of Hassan Pacha in 1841, whom he found labouring under all the true and severe symptoms of plague. She died at the end of thirty-one days. This lady had in her service twelve white and twelve black slaves, four eunuchs, and four pages. These were in constant communication with the patient, and with the rest of the household of the palace—a hundred persons in all—and yet not one was attacked.*

We are also informed by Dr. Delong, another physician at Cairo, that a girl five years of age, in the house of Saad-Pinto in the Jews' quarter, was attacked with plague in 1841:—"She was nursed constantly by her mother; she was surrounded by her brothers, her sisters, and her cousins; and she was in contact with all the household. The young patient died, but all the family continued healthy." Another fact is related by the same writer:—"Two young Turks belonging to the Cadi, the chief Judge in Cairo, were attacked with plague about the same time, and were placed in the same room. The disease was of a severe form. All the numerous persons attached to the Palace went to visit them: the visitors took the patients' hands, consoled them, nursed them, and touched them without taking any precaution. The two patients died, one shortly before the other. No other case was observed in all the vast enclosure of the Palace of Justice." †

To the question, "Is the plague propagated by con-

* Rapport adressé au Conseil de santé du Caire. 1841. Document 17.

† Rapport adressé au Conseil de santé du Caire. 1841. Document 19.

tagion?" M. le Dr. Clot-Bey answers:—"I repeat, it has been demonstrated to me by a profound study of the history of this disease, and especially by the numerous facts that I have myself observed, or which have been made known to me by all the medical men who have had to combat the plague in Egypt lately, that the plague is *never* propagated by contagion." And to another question, "Is contact with an infected person necessary for the production of plague?" the reply of this experienced physician is:—"The *contact* with an infected person not only is *not necessary* for the production of plague, but it is of itself absolutely *innocuous*, and is only productive of danger because it brings the individual near the focus of the disease," * or of the cause productive of it.

Mr. Laidlaw, surgeon to the European hospital at Alexandria, in answer to the question from the same authority, remarks:—"I have under my eyes numerous examples of persons in health who have been in direct contact with plague patients, and who, nevertheless, have not been attacked: when the disease enters a family all do not fall victims; the persons who attend the sick continue healthy and well; those who render the last duties to the dead are not affected. I have seen a daughter, in spite of the supposed danger, throw herself on the body of her mother and embrace it until it had been carried away. I have seen a father raise his dying son, covered with plague blotches, in his arms, and retain him there until he had died. I have often, very often, been witness of the indifference with which the Turks and the Arabs place themselves in contact with the sick, without, in consequence, being attacked with the disease."

* Reponse aux questions posées par le Ministère Anglais en 1839.

Another proof of the non-communicability of plague is derived from the prevalence of what are termed sporadic (single, isolated) cases of plague. The last writer states, "that sporadic (or single) cases of plague are continually occurring in Egypt and in Turkey, but they do not frighten the inhabitants. Experience has shown that they are not productive of any danger; they break out in the centre of a town or a village, and pursue their course, without any other attack being observed, in spite of the number of persons who are placed directly or indirectly in contact with the disease. The natives have given the name of *kassif* (mild) to this form of plague, not because the symptoms are less severe, but because the disease does not become general. From the month of June, 1835, to the end of December, 1838, 649 sporadic cases of plague were observed in Alexandria; but the disease was not transmitted to any of the persons who surrounded these patients and who attended upon them. The same fact had been observed before, and commented on by Pugnet, at the commencement of the present century. If, also, we turn to the Bills of Mortality in London, a similar result will be apparent. Although the plague only prevailed epidemically in certain years, there are very few in which deaths from this disease are not recorded, although the number was generally small, sometimes only three or four. So, again, although the plague has never returned in an epidemic form since 1665, there were 1,998 deaths in the following year, 35 in 1667, and 14 in 1668. After this the number never exceeded five, the last mention made of the plague being in 1679.* The non-propagation of the

* It is often asserted that the cessation of the plague in London was the effect of the great fire that then occurred. This is an error. The plague ceased in February, 1666, but the fire did not occur until September. That the fire had nothing to do with the cessation of

disease in the latter years, could not be referred, as Mead truly remarks, to the want of subjects, as only about a fourth part of the population had been cut off in 1665, while the places of these had been supplied by recruits from the country.

One more example will suffice for this part of the subject. There is a medical school in Egypt at Abouzabel, situated about four leagues from Cairo. When the plague broke out in the latter town, the patients in the hospital and the hospital staff were sent out of the town and placed under tents pitched in the adjoining desert. They were attended by the professors and pupils of this school. "Five physicians," remarks Dr. Duvigneau, "of whom I was one, were attached to the hospital; each day we made two or three visits to the plague patients (sent there from Cairo), and autopsies were made soon after death. We never took any other precaution than that of washing the hands with vinegar and water or soap; we did not change our clothes before entering the hospital, and we approached and touched the patients the same as at other times. Well, in spite of all these means of propagation, not one of us experienced the least accident; not a single patient in the hospital (suffering from other diseases and attended by the same medical men), was attacked with plague; and yet the number of those who were

the plague, may be concluded from the fact that this disease disappeared at the same time in all other parts of England, although twenty or more towns were attacked the same year. It also disappeared about the same time in Paris and other places on the Continent. Added to this, there had been two great fires in London previously, in 962 and 1087; but they did not then cause a cessation of the plague. In the latter year, "St. Paul's Minster and the Bishop's Palace, together with many *other* monasteries, and the greater and handsomer part of the whole city of London, were burned down."—Anglo-Saxon Chronicle.

treated, during the months of April, May, and June (and attacked with plague), amounted to a thousand."* These results are not new; similar facts have been recorded from the first appearance of the plague in Europe. Procopius states that, in the plague of 543, no physician or attendant caught the disease. Evagrius makes the same remark, and adds, that some, in despair for the loss of friends, threw themselves in the way of infection, and yet escaped; while others contracted the disease in the open market, and without communication with the sick. Lord Verulam remarks: "The plague is not easily received by those that continually attend the sick, as physicians; nor again by old people, and such as are of a dry cold complexion. On the other hand, the plague soonest seizes those that come out of fresh air, and those that are fasting, and children." Lastly; it has been stated by Dr. Fischer, that "several physicians of Cairo and Alexandria, who never went near a patient, but kept at a respectful distance, and who went out *enveloped in wax cloth*, mounted on horses whose bridles were made of cords from date trees, and the saddles covered with straw, etc., were carried off by the plague."

In addition to the above, it may be remarked, that isolation—complete isolation—is not a preventive of plague. Of this fact numerous examples could be adduced, not only lately, but formerly.

In the *Recueil des pièces historiques sur la peste de Marseilles* of 1720, published in 1820, we find the following:—"That which appears the most strange is, that the plague attacked all those who shut themselves up in their houses the most closely, and who were the most careful not to receive anything, excepting with the greatest precautions.

* Reponse aux questions posées par le Ministère Anglais.

The plague insinuated itself there, no one knows how." A similar result was observed at Toulon. "The plague," said Antrechau, "seems to have been introduced into the Hotel de Ville, merely to inform us that no barrier is able to arrest it. Neither our care to prevent all communication, nor our barriers, could preserve from its attacks those whom at first it appeared to spare. It found victims in dwellings, the entrance to which they thought was inaccessible, such great precautions having been taken to shut it (the plague) out." Similar results have been remarked in Egypt. Dr. Dulany states, that the young wife of a writer named *Sgnerous*, chief clerk in the office of the Governor of Cairo, alarmed at the appearance of the plague, put herself into quarantine, together with a few domestics. "She had no communication with her husband, who remained at his post, nor yet with any of the household. She contracted the plague nevertheless. Following my exhortations and example," continues the narrator, "they approached the patient; they touched and they nursed her, the most tender care being bestowed on her by her husband. She died at the end of five days. The disease was not transmitted to a single person, not even to an infant which the patient suckled, and which remained near her until the last moment."* Again, "On the first appearance of the plague at Cairo," remarks Dr. Euzieres, "M. Antoum, wood merchant, shut himself up with his wife, four children, and several domestics. All the windows and other openings of his apartment were carefully closed, and the *cats* had been driven away! M. Antoum had performed a strict quarantine for about a month, when, on the 20th April, 1841, he was attacked with plague. I was called to see him on the 23rd. I found him in bed with one of his children of tender age;

* Loc. cit.

his wife was lying by his side on a mattress; the other three infants, as also the servants, were near the bed of the patient." He died on the 24th, eighteen hours after M. Euzieres' first visit. "Not a person in the house," adds the writer, "experienced the least symptom of the disease."* Not only is contact with living and diseased or infected bodies thus shown to be innocuous, but contact with the same bodies after death is not attended with more danger. M. Seisson states that he was witness to the cessation of the plague at Kauka at the very moment when the cemetery, being overcharged with the bodies of the victims, gave out putrid exhalations in their greatest intensity.† Clot-Bey, also, from having witnessed similar results, considers that putrid exhalations, or those from dead bodies, do not increase the ravages of the plague. This conclusion is confirmed by another fact. When the plague ceased in Moscow, the houses of all those who had died were fumigated, and were then taken possession of by their original occupants. Soon after it was discovered that the inhabitants, fearing to be sent into quarantine, had not made all the deaths known; they had concealed the bodies in their houses! These were found *in thousands*. They were interred, and not a single case of plague was observed afterwards.‡ We also know that "the carriers of the bodies of those destroyed by the plague of London were peculiarly exempt from the attacks of the disease; the grave and pit diggers enjoyed a similar immunity. Butchers were also comparatively free from the disease; in short, all who were exposed

* Rapport adressé au Conseil de Santé du Caire sur la Peste de 1841.

† De la Peste, p. 208.

‡ Schurrer: Faits récueillés pour servir à l'histoire des maladies épidémiques, p. 63.

to *putrid* exhalations seemed to find in them *a mighty antidote* against the inodorous miasms which desolated the city."*

With these remarks we may now pass on to a consideration of the next question—viz., Can the plague be produced or propagated by means of the secretions, excretions, or other morbid matters of plague patients?

"During the five months that the epidemic of 1835 lasted," remarks Clot-Bey, "MM. Gaëtani, Lacheze, Bulard, and I, at Cairo; MM. Duvigneau, Seisson, Perron, and Fischer, at Abouz-Abel; and MM. Rigaud and Aubert, at Alexandria, have attended plague patients in the hospitals and in private houses. Not one of us has employed any prophylactic or precaution. We were placed in immediate contact with plague patients, and at all periods of the disease. We received on our clothes, and on our hands, the vomited matter, the blood, and the pus, from *thousands* of buboes that we opened. More than a hundred autopsies were made at Cairo, and we passed entire hours in seeking, in the bodies of those who had died, those pathological alterations which had been so little attended to before. Dr. Rigaud is the only one among us who fell a victim to the reigning epidemic. But," adds the writer, "by a remarkable singularity, many physicians, who had scrupulously avoided the contact of patients and of suspected objects, were attacked with the disease and died. Of this number were Drs. Manucci (senior), Leopold, and Lardoni."

Not only was the mere contact of diseased secretions and morbid productions unattended with danger, but the introduction of these substances into the blood was not followed by any fatal result. In 1803, M. Valli, an Italian physician, inoculated twenty-four persons with a

* "Lancet," July 11, 1831.

mixture of variolous and plague matter, in order to test the value of some fanciful speculations of his own—viz., that the presence of the variolous poison would prevent the operation of that productive of the plague. Dr. Sola, also, a Spanish physician, inoculated fourteen deserters condemned to death, in 1818, at Tangiers, with a mixture of plague matter, or pus, and oil, from an idea that the latter would prove an antidote to the former. Be this as it may, neither in this instance, nor yet in the former, were those inoculated attacked with plague. That the variolous matter and the oil exerted little influence in warding off an attack of plague, may be inferred from the following experiments. They were made in 1835, at the Hospital Esbékiè, in Cairo, in the presence of Gaëtani Bey, Clot-Bey, Dr. Lacheze, and Dr. Bulard:—

Three men, condemned to death, were voluntarily subjected to the following proofs, a free pardon having been first granted them. A lancet, charged with *the blood* of an infected patient, was introduced under the skin on the inside of the arm, on the 18th of April. One of these was attacked with plague on the 21st April, but in a mild form, as he was convalescent on the 26th. The other two experienced no ill effect. A fourth, who had experienced a slight attack of plague on the 15th April, was inoculated on the 5th May, in the groin and under the armpit, with the serosity taken from the carbuncle of a plague patient, and eight days after with the blood of another patient; but no ill result followed either operation. We thus find that one man out of the four was attacked with plague; but the wonder is that they were not all attacked, having been brought from the gaol, where the disease did not prevail severely, into the very focus of the epidemic. The escape of the other three shows very clearly, that the blood of an infected patient cannot produce the plague when

introduced into the system of another and a healthy person. With the view of confirming the truth of this conclusion, Clot-Bey inoculated himself with *the blood* of a plague patient in six places—three in the left forearm, and three in the right groin. No effect followed!

In addition to the preceding, a fifth criminal was inoculated, on the 20th April, with the serosity taken from a carbuncle, but this not producing any effect, he was again inoculated, on the 30th, both in the groin and in the armpit, with some pus taken from a bubo that had just been opened. No attack of plague, or other ill effect followed the operation.* Dr. Clot-Bey also inoculated himself on the inside of the left arm with some pus taken from the bubo of a patient. Slight *malaise*, and other symptoms, having no analogy with plague, were alone experienced.

From the above facts we shall be justified in concluding that the blood, the secretions, and the morbid productions of plague-patients, do not, and cannot, produce an attack of the disease, when introduced into the body of a healthy person. There are, as might be expected, other examples in which attacks of plague have followed inoculation with pus and other matters; but then the operation was performed within the pestilential area. This was the case with Mr. White, an English surgeon, who, according to the Report of Sir James M'Gregor, inoculated himself with pus taken from the bubo of a plague-patient, during the stay of the British forces in Egypt. He died a few days after, a result that might have occurred precisely the same had he not been inoculated. Had the experiment been made beyond the morbific boundary—for these

* Communication faite à la Commission de l'Académie de Médécine, par M. le Dr. Lachèze. Document No. 25.

diseases invariably have a well-defined boundary—a different conclusion would, of course, have to be drawn. But there is no example of such an experiment having been made.

As already mentioned, not only are animate bodies considered to be the vehicle for the propagation of the plague, by the absorption and extrication of the virus of contagion, but inanimate objects, also, are said to possess the same property. This conclusion, which is an important one, will require almost as attentive a consideration as the preceding question. The articles that have been considered as the most liable to become infected are the clothes and the bedding. The former would be quite as dangerous as the latter, in consequence of the practice which exists in Egypt and Turkey of not changing the clothes at night. More than this, patients not only wear their ordinary clothes during their illness, but the majority also die in them. Notwithstanding, the clothes of plague patients in Turkey and in Egypt are not destroyed.

We are informed by Dr. Brayer, that the Jews of Constantinople have magazines for the clothes worn by the Mussulmen and the *rayas*. "If the plague rages, the market is crammed with clothes. It was there that the effects of 150,000 victims to the epidemic of 1812 were collected. Do not suppose," he remarks, "that they trouble themselves to disinfect them: no one dreams of such a thing. A part of these clothes passed quickly into the hands of the inhabitants of Constantinople: another part was sent into the principal towns of Turkey. What remained unsold was heaped up in the magazines—small, dirty, obscure, without windows, and where the air could not circulate. These were re-sold the following year. Notwithstanding, although the cases of plague were

sufficiently numerous at the commencement of the month, they ceased entirely at the end of December."* It is worthy of remark, that the Jews, who deal in, and live among, these infected clothes, suffer less from the plague, according to this writer, than the Greeks, who are contagionists, and take every precaution to prevent the disease. The observations made in Egypt, and the facts collected, during the recent visitations there, are still more conclusive. After the epidemic of plague at Cairo in 1835, all the furniture and the clothes, of those that had died, were sold in the bazaars; the latter were worn afterwards without being disinfected. The effects of more than 50,000 plague patients, who had died in this capital, did not communicate the disease to a single person.†

Clot-Bey also states, that the hospital of Esbequié, at Cairo, received more than 3,000 plague patients in 1835. When the epidemic ceased, the hospital was restored to its former destination, and received again those who were suffering from ordinary disease; and this, too, while convalescents from the plague were still there. "They were placed in *the very beds* in which plague patients had died. The sheets only were changed. They gave them woollen coverlids, which had not been disinfected, which had not even been aired, since they were used by plague patients. Well, more than 500 of these coverlids, yet *impregnated and saturated*, as we may say, with the emanations of the infected, and a multitude of other objects, which had been placed at their disposal, did not give the disease to a single person." "The fact is," remarks Dr. Seisson, Professor of Pathology in the Medical School,

* Neuf Années à Constantinople, t. 2, p. 354.

† Rapport de l'Académie de Médécine, p. 104.

at Cairo, "the plague ceases here at the end of June, at the moment when Cairo contains a multitude of objects of every kind, which had belonged to the victims of the epidemic: and when a great part of the population is clothed in the garments of the dead; for as the Arab does not take off his clothes, when he goes to bed, the greater number of the patients passed their illness and died in the same clothes. In the hospital, also, the clothes of patients, who have died of plague, are placed indiscriminately with those taken from other bodies."*

We are also indebted to Clot-Bey for the following particulars, which tend to prove, that the plague cannot be propagated from country to country by means of infected articles of clothing. "The Egyptian army, which then occupied the country, suffered severely from the plague which raged in the Morea in 1826, 1827, and 1828. In September of the last year, the troops returned to Egypt, and the clothes of all the soldiers who had died, both of the plague and of other diseases, were taken to Alexandria, deposited in the magazine of a barrack, and eventually sold. Nevertheless, not a single case of plague was observed that year in Egypt."†

Similar results had been observed and recorded previously in Europe. "One thing," said Francis Poona, "which was observed and which is worthy of admiration, is, that among so many *employés*, and so many men vulgarly called *purificators*, who handle every moment these same clothes on which plague patients had lain and died, *not one was infected by it*—a result contrary to so many opinions, and so many conclusions drawn by the mind of man, and which induces us to say, with Hippocrates, that there is,

* Réponse aux sept questions posées par le Ministère Anglais.

† De la Peste.

in diseases, a something, we know not what, of Divine."* Another and a more recent example has been recorded. In 1713, there was a visitation of the plague, in Holstein, shortly before the Swedish troops entered the province. "No precaution," as we are informed by Schurrer, "was used to preserve them from the disease, yet no one was attacked, although they *wore the clothes and used the beds of the plague patients.* †

With these facts before us—not imaginary tales, but well authenticated facts—it seems to be impossible to draw any other conclusion than this: that the clothes worn by plague patients do not contain the germs of the disease. They cannot, therefore, give the disease to other and healthy persons, even when worn by them. And yet tales are told of the plague having broken out after boxes and trunks, coming from an infected locality, have been opened. But all these instances have occurred within the epidemic radius, and may therefore be referred to other causes—to that which produces the disease in all other instances. There is no case on record of the plague having broken out after such an occurrence beyond the epidemic area. As the French Commission, in their Report, remark:—"Facts in great number prove, that the clothes and other articles which have been used by plague patients have not communicated the disease to those persons who have worn them, although not previously purified. The facts, which would appear to give the opposite result, cannot be of any value unless confirmed by new observations made beyond the epidemic focus—far from the centres of miasmatic infection, and far from the countries where the plague is endemic." ‡

If the clothes and other articles, worn by patients, and

* Histoire de la grand contagion de Verone, en 1630, p. 103.

† Loc. cit., p. 61. ‡ Loc. cit., p. 112.

saturated with the morbid secretions, do not give the plague, it is improbable that articles of merchandise, handled by the healthy only, can produce a different result. Numerous instances are recorded, nevertheless, of the presumed transmission of the plague by merchandise; but, then, the majority of these cases, if not all, occurred during epidemic periods and within the morbific boundaries of the disease. Hence, some particular occurrence which was observed at the time, but which would otherwise have escaped notice, has been set down as the cause of the outbreak. Thus Hodges states, that the plague of 1665, in England, was introduced by the importation of some bales of cotton from Holland. But the plague prevailed in London, and sporadically, the previous year: while this accidental circumstance will not account for all the other visitations in England—22 in number. Then, again, if a few bales of cotton could spread the plague around, how was it that the 35 persons who died in 1667, and the 14 in 1668, as also those in subsequent years, did not produce the same result? The fact is, that when a particular phenomenon arises, the cause of which is unknown, men are apt to ascribe it to the first accidental and co-incident circumstance that strikes their imagination. Thus, it was stated and firmly believed, that the last plague at Marseilles, in 1720, was imported by a vessel which had arrived at the time from Syria. But it was proved, subsequently, by a Commission of medical men, sent there by the French Government, that several persons had been attacked with the true symptoms of plague, before the arrival of this vessel.* Other and similar accounts, if investigated, would, there

* In a letter published by Dr. Deidier, Professor of the Faculty of Medicine in 1721, and sent to Marseilles by order of the King, there are the certificates of Drs. Robert and Rimbaud. They state,

can be little doubt, prove to be as fabulous as this. "It is commonly said," remarks Assalini, "that on opening a letter, or a bale of cotton, containing the germs of plague, men have been thrown down and killed by the pestilential vapour. I could never meet," he adds with much *naïveté*, "with an actual witness of the fact, in spite of the researches that I have made in the Lazarets of Marseilles, of Toulon, of Genoa, of Livourne, and of Malta, and in the Levant; all agree that they have heard of such things, but that they never saw them. Among these was the Captain of the Lazaret at Marseilles, who said to me, he had seen *millions of bales* of cotton, silk, and wool, skins, feathers, and other articles, coming from places in which the plague existed, opened *without* his ever having observed any accident to arise therefrom." It could hardly have been otherwise, for no ill result has followed, during the present century at least, from the immense quantity of merchandise that has been imported into Europe from Egypt and other places; not only in the intervals, but, also, during the prevalence of the plague. To show this, one example will suffice.

During the severe visitation of plague in Egypt, in 1835, cotton continued to be exported during the whole period of its prevalence, and, even, at the very height of the epidemic. According to Dr. Laidlaw, in a Memoir addressed to the English Consul-General at Cairo, there

that the ship of Capt. Chaseau did not arrive at Marseilles until the 25th of May: whereas Madl. Angier died of a disease with the characteristic symptoms of plague, on the 20th of April: and Madl. Coursand on the 4th of May; while a female, named Rose, was also attacked on the 20th. Added to this, the ship was placed in quarantine, and none of the passengers, or goods, were admitted into the town until the 14th of June, when the disease had become general.—Journal des Savans, pour l'Anné 1722, p. 611.

were exported, that year, from Cairo:—To England, 31,709 bales of cotton; to France, 33,812; to Trieste, 32,262, and to Holland, 150. But no instance is known of plague having appeared in any of the ports to which these ships were consigned; although no steps were taken to disinfect the cotton. Of the 16 English vessels, charged with cotton, 8 had plague patients on board; but this circumstance made no difference in the result. It may also be added, that none of the labourers employed at the Lazaret at Marseilles to unload the vessels, have contracted the plague since 1720—the year of the last visitation there.

If such be the results, and if we also conclude, as we are bound to do, from a consideration of the facts previously advanced, that the plague is not contagious, and cannot be propagated from individual to individual, of what use, we may ask, are Lazarets? and why is quarantine kept up in the present day? The only reason, the only excuse, that can be given, is the belief that plague has been actually imported from those countries in which it now prevails into France and Italy. As such, it is further argued, if a disease has been before imported, it may be again, and into other countries as well. It is desirable, therefore, to ascertain on what foundation this belief rests. For this purpose, we cannot do better than turn to the valuable and voluminous Report of the French Commission, in which all the facts bearing on this important question have been collected.* All the documents that had been received from the Sanitary Board at Marseilles, and other Lazarets—500 in number—were forwarded by the Minister of Agriculture and Commerce

* Rapport à l'Académie Royale de Médécine, sur la Peste et les Quarantines: fait au nom d'une Commission par M. le Dr. Prus. Paris. 1846.

to the Commission. From an analysis of these documents, it appears that 25 ships, with cases of plague on board, or else coming from suspected places, have entered the Lazarets of France and Italy since 1720—the date of the last visitation of the plague at Marseilles. Of these, 15 were received in the ports of Italy, and 10 in the port of Marseilles. We will confine our investigation to the latter, as the evidence respecting the others is not sufficiently clear to enable us to arrive at satisfactory conclusions on the subject. To give even a brief history of each of these cases, would occupy too much time and space: as such, the following Table has been drawn up—by an inspection of which the reader will become acquainted with the principal facts connected with the outbreak of plague on board of these ships, and thus be better enabled to follow the observations about to be made.

TABLE 1.—Containing the date of arrival, with the number of attacks of plague, in the following ships, and the number of employés attacked in the Lazaret.

No.	Date of Arrival.	Names of Ships.	No. of cases before, or at the time of arrival.	Do. after.	Attacks among Employés.	Date of attack of Employés.
1	June 19, 1741 .	L'Etoile du Nord . .	2	5	2	July 3.
2	May 8, 1760 . .	La Sainte Famille.	3	7	—	—
3	April 30, 1784 .	The Assomption . .	—	—	5	May 23.
4	Jan. 22, 1785 .	The Marianne	2	—	—	—
5	May 23, 1786 .	The Providence . . .	—	4	2	June 20.
6	June 12, 1796 .	Le Malonet	—	1	—	—
7	Aug. 7	The Eulalia	3	—	—	—
8	May 1, 1819 . .	La Continuation. . .	2	—	1	May 14.
9	June 30	L'Hereuse Sabine .	4	1	—	—
10	July 9, 1837 . .	The Leonidas . . . . (Steam Packet)	2	2	—	—

If we analyse the preceding examples, we shall find that, in seven of the ships, there had been, or were, cases of actual or presumed plague on board at the time of the arrival of the ship. In two, the attacks did not commence until after the ship's arrival, and, in another, the cases were confined to the *employés*.

Again, in six of the above instances—Nos. 2, 4, 6, 7, 9, and 10—the disease did not extend to any of the *employés* or sanitary guards, while the number of attacks, in each ship, was exceedingly small. The largest number was ten, and the smallest one. Although told, that one patient can infect a thousand, and even a whole town, we here observe the disease limited to a solitary individual: the remainder of the crew, continuing perfectly healthy. But the most remarkable instance is the tenth—that of the *Leonidas*—on board of which there were two genuine cases of plague: and although there were sixty-five persons on board—eighteen passengers and forty-seven officers and sailors—not a single other case was observed. If, therefore, plague be contagious, it must be a very extraordinary kind of contagion: for if there be one place, more than another, where an infectious disease could be propagated, it would be on the confined and *heated* deck of a steam packet. With these remarks, we may now pass to an examination of the four remaining cases, in each of which some of the *employés* at the Lazaret were attacked, as well as the crews of the ships.

Taking the first of these examples—that of the *Etoile du Nord*—we may conclude, that the two first patients contracted the disease at Algiers, the port from which the ship had sailed. Leaving the other five cases, whence, we may ask, did the two *employés* derive their attacks? Had it been shown, that the plague is contagious, we should at once have inferred that they had contracted the disease

from the two patients on board; but the previous facts and arguments forbid our drawing such an inference. If, however, the disease was not derived from contact with infected persons, or things, it must have arisen spontaneously in the Lazaret. That such was actually the case, will be rendered more than probable by the following circumstances.

Although the plague had subsided in the north of France at the end of the seventeenth century—no visitation having been observed in Paris after 1668—it lingered in the southern provinces of France long after this. In 1720—only twenty years before the arrival of the above ship—there was a severe visitation of the plague at Marseilles: while all attempts to prove its importation failed altogether. The cause, therefore, productive of plague, whatever that may be, had not entirely subsided, in that part of France, at the above date. In addition to this, a malignant fever, which put on all the characteristic symptoms of the plague, broke out at Rochefort in May, 1741—*a month before the arrival of the Etoile du Nord.* The attack, as we are informed by Chirac, commenced with chills, and other symptoms common to fever. To these succeeded syncope, unaccountable prostration, smallness of the pulse, epistaxis, swelling of the parotid glands, and axillary *buboes,* but few inguinal ones. *Carbuncles* on the head and hands were not uncommon, while none of those who had them, it is stated, recovered.* This outbreak was ascribed by Chirac to two causes: in the first place, to a famine that prevailed there, and, in the next, to the exhalations given out during the drying of the marshes, that had been flooded by an inundation of the Charente. But these exhalations could not have been

* Traité des fièvres malignes et pest. Paris, 1742. Pp. 53 and 135.

the cause of the outbreak; for they did not arise until the end of June, whereas the fever, or plague, commenced in the month of May. Be that as it may, this attempt to trace the visitation to local causes, shows very clearly, that no reason existed for ascribing it to importation.

In Case 5—the *Providence*—we have another example of the *employés*—the surgeon of the Lazaret and one of the Guards—being attacked with plague, as well as some of the crew—none having been attacked previously. But the latter were not taken ill until a week after the arrival of the ship at Marseilles, and sixteen days after she had left Bona, where the plague was prevailing—the reason why the ship was placed in quarantine. Such being the case, we can hardly suppose that these men brought the seeds of the disease with them, or that it could have remained latent in the system for so long a time. The latent period of plague, according to the best authorities, is not more than eight days. Dr. Grassi, surgeon to the Lazaret at Alexandria, states, that of the multitude of persons who, during the visitation in 1835, fled to Upper Egypt, where the plague does not exist, a certain number were attacked by the disease, but none after an interval of *eight* days! It has also been before stated that the plague did not prevail at Abouzabel, during the same epidemic, although this town was only four leagues from Cairo, and there was daily communication. Several individuals, however, who had been to Cairo, and imbibed the seeds of the disease there, were subsequently attacked; but no one was seized after an interval of *six* days, according to the evidence of Drs. Duvigneau, Perrou, Fischer, and Seisson. If such be the general rule, and these are not solitary examples, we must infer that the patients belonging to this ship contracted the disease on the spot. In that case, we can readily understand why the surgeon and

guard of the Lazaret were attacked, without concluding that they contracted the disease from the crew of the ship.

That the disease arose spontaneously, in this instance, the same as in the former, we may presume from another circumstance. This is, that a disease, presenting all the characteristic symptoms of plague — viz., carbuncle, swelling of the inguinal and axillary glands, &c.—was observed in France, in the Department of the Meuse, as late as 1788—two years after the outbreak in the Lazaret of Marseilles. It commenced, according to Dr. Vimat, with the sheep feeding in the marshes near to Marsal, and then extended to man—a common occurrence. We have thus proof, that the epidemic influence lingered in that part of France, long after it had disappeared from other parts of Europe.* There can be no reason, therefore, why it should not have prevailed, and have sprung up spontaneously, in the Lazaret of Marseilles, as well as in the Department of the Meuse ; for it must have sprung up there spontaneously.

That the plague could, and did actually, arise spontaneously in the Lazaret of Marseilles, would appear certain from the facts presented to our notice in the third case—the *Assomption*. This ship arrived at Marseilles in 1784—two years before the arrival of the *Providence*. Although the *Assomption* had come from an infected port, there were, and had been, no cases of plague on board. Nevertheless, between two and three weeks after the arrival of the ship, one of the quarantine guards was attacked with fever, and died on the 26th May. On the

* The following are some of the last visitations of the plague in Europe. Prussia, Hungary, and Poland, 1702-9 ; Germany, Livonia, and Sweden, 1710 ; Vienna, 1712-13 and -14; Hungary, 1756 ; Holland and Denmark, 1764; and Russia, 1771.

28th, another guard was attacked, and died on the 30th—the disease, in these two instances, being designated by the medical attendants as malignant fever—not plague. On the 9th June, another guard was attacked and died; the case, in this instance, being considered, by the same authorities, a suspicious one. On the 13th June, one of the quarantine surgeons and another guard were taken ill; the symptoms, with these patients, presenting the true and characteristic ones of the plague.

Here, then, there is an undoubted example of a spontaneous outbreak of plague, unless we were to infer that the germs of the disease had been brought by the ship or the crew. As, however, none of the sanitary guards were attacked until nearly a month after the arrival of the ship, and as the crew remained perfectly healthy, such a conclusion would not only be a gratuitous, but an illogical, one. None of the passengers, 153 in number, were attacked until the day the ship sailed from Marseilles for Tangiers, on the 24th May. On that day one died, and several more during the voyage.* After the arrival of the ship at Tangiers, eight of the crew were attacked with plague and died. Whether the three passengers, who died subsequently to the departure of the ship, were attacked with plague or not is immaterial; we are bound to conclude, whatever the disease may have been, that it was contracted in the Lazaret of Marseilles. Not so with those of the crew subsequently attacked at Tangiers; these must have derived the seeds of the disease during the stay of the ship at the latter place, where the plague was then prevailing.

* As the passengers were all Moors, who are fatalists, and do not seek either medical or other aid, the first intimation that the captain had of the illness of these men, was by seeing their bodies thrown overboard. It is impossible therefore to say, of what disease they died.

In the eighth case, *La Continuation*, one of the Lazaret guards had a decided attack of plague and died. This looks the more suspicious, as there was a plague patient on board when the ship arrived, while the plague had not appeared in France, excepting in the Lazaret, for thirty-three years before. Still, as the man was then convalescent, and as an interval of thirteen days had elapsed after the arrival of the ship, it seems difficult to imagine that the infection could have been derived from this source. When also we find that, in six of the other instances, the disease had not been communicated to any of the sanitary corps, although there were decided cases of plague on board; and when it also appears that, in the remaining three, the disease evidently arose spontaneously, we can hardly fail to infer, that the outbreak in this last instance, as well as in the former three, was spontaneous.

Such a result as this will not appear singular to those who have paid much attention to the subject of endemic diseases; and who are aware, that there are certain spots on the face of the globe which may be designated pestiferous. These spots not only retain their unhealthiness for many years, but even for centuries, and after the surrounding districts have become comparatively healthy. Witness the Pontine Marshes, which have been pestiferous from time immemorial, in spite of all the attempts that have been made to render them salubrious. As is well known, a stranger cannot traverse these districts even now, at a certain time of the year, without being attacked subsequently with fever. The results in other localities are still more marked. For instance, at Antigua, during the occupation of that island by European troops, "it was no uncommon thing," as we are informed by Dr. Ferguson, "for a seasoned soldier, who had descended to take guard at the dock-yard, surrounded by marshes, to

be attacked with furious delirium *while on post*, and to expire in twenty-four hours with all the horrors of black vomit. But no cases of fever were observed among the officers or men whose duties confined them to the heights above."* There is a similar spot on one side of the town of Port of Spain, Trinidad: so pestiferous, that a military post, established during the war on some heights—called the La Ventille Hill—immediately above the marsh, had to be abandoned: every man who slept there, even for a single night, being attacked with fever.

It is not those on the land, who are alone liable to be attacked in such situations: sailors on board ship suffer equally, provided only that the ship lies near the shore or in a confined harbour. For instance, Captain Cooke, in his first voyage round the world, anchored at Batavia, on the 3d October, 1779, the whole crew, with the exception of a native of Otaheite, being in the most perfect health. In nine days they buried seven men, and on the 3rd December they left Batavia, with forty men on the sick list. The ship proceeded to Prince's Island, in the Straits of Sunda, where they buried twenty-three more in the course of six weeks: the majority of those that survived having also been attacked.† It is necessary, in such cases, that the ship should lie within a certain distance of the shore—the distance varying according to the locality and direction of the prevalent wind—otherwise no ill effect is produced. Sir Gilbert Blane states, "when the ships watered at Rochefort (Jamaica), they found that, if they anchored close to the shore, so as to smell the land breeze, the health of the men was affected; but upon removing five cables' length, no inconvenience was perceived."‡ Again: When Commodore Long's squadron lay off the mouth of

* Medico-Chirurgical Transactions, vol. 8.

† Voyage by Hawksworth. ‡ Diseases of Seamen, p. 206.

the Tiber, in 1744, some of the men of one or two ships lying closest to the shore were attacked with fever; but the crews of the others, lying farther out at sea, "had not a man sick."* At this time, the Austrian Army on shore suffered so much from sickness, that they were obliged to break up their encampment and leave.

It is different if the crew are engaged on shore, and particularly if they sleep on shore—the risks being then multiplied. The following narrative will show this. "A Danish ship," Dr. Clark states, "anchored at Long Island, near the Straits of Sunda, and sent twelve of her people on shore to obtain water, where they only remained two nights. *Every one* of them was seized with fever, o f which *none recovered,*" but the rest of the ship's crew remained exempt.† Instead of an intertropical island, let us take another example drawn from a locality, not far from the Port of Marseilles. In 1750, the *Prince* ship-of-war, according to Dr. Lind, anchored in the Bay of Aristane—Sardinia—and sent twenty-seven of her crew on shore to fill the water casks and perform other duty. Of this number, twelve were taken on board *delirious,* and seven died. Of those that remained on board, *not one* was attacked.

We thus find, that there are certain pestiferous spots on the earth's surface, in which human beings, and especially strangers, are unable to reside, even for a few days, with impunity—fevers and other diseases being the invariable result. It is no less certain, that these fevers, although sometimes of the most severe and malignant type, are not contagious: that they are not, and cannot be, propagated from individual to individual. If it were pos-

* Essay on the Health of Seamen, p. 64. (Lind.)

† On the Diseases which Prevail in Long Voyages to Hot Climates.

sible to spread disease, by means of infection, it is on the lower and sleeping deck of a man-of-war, that this result would be attained the most easily and the most certainly. But no such result is ever observed with these malarious diseases. Bearing these facts in mind, we may draw some important deductions, with respect to the subject now under consideration. Let us suppose that, instead of Marseilles, the Bay of Aristane, Sardinia, had been the quarantine station; and that, instead of the Prince, a vessel had arrived from Egypt, or other port, where the plague was prevailing. She might have had sickness on board previously, or she might not; but it is certain, whether she had or had not, that some of her crew if they remained on shore for a few days, or if the ship were anchored close in shore—near enough to smell the land breeze—would have been attacked with fever, and that too of a malignant type. If the fever assimilated at all to plague, which all malignant fevers do, it would have been concluded at once, that they had brought the disease with them; and yet, how erroneous would have been the conclusion. Then, again, if some gendarmes had been sent from the neighbouring town to look after the ship, or to keep the crew within the restricted bounds, some of them, we may presume, would also have been attacked. Although, being natives, they would not be so susceptible as strangers; they would yet be liable to attacks, inasmuch as they would have come from a comparatively healthy to an unhealthy, or pestiferous, locality. In all malarious districts, the town is invariably more healthy than the country, or the suburbs; so much so, that M. Michel has laid it down as a "law," that population decreases the insalubrity of a town like Rome, situated in the midst of a pestiferous plain. Such being the case, it is more than probable, that some of the guards would

have been attacked; and, if so, it would have been said, that they had caught the infection from the crew, more especially if the latter had been the first attacked—an almost certain result. And yet, this conclusion would have been no less erroneous than the former. And this is the way in which false deductions are drawn from imperfect and misunderstood premises.

Now we have only to change the scene from Sardinia to Marseilles, in order to comprehend the facts narrated in the previous histories; and the manner in which the disease broke out in those instances—provided only that the quarantine ground there is an unhealthy spot. That such is the fact, there can be little doubt. M. Dubois, a Member of the Commission on the Plague, went to Marseilles to visit the Lazaret—situated on the islands of Pomègue de Ratonneau and of Chateau d'If—and he describes his visit there in these terms: "The purity and transparency of the waters of the Mediterranean are well known, but once entered into the port, it is absolutely necessary to hold one's nose at each stroke of the oar, so great are the putrid exhalations which arise. This neglect, in so rich and active a town, is incomprehensible. The aqueduct of Roquefavour alone cost three millions; and yet, nothing has been done to disinfect the port. Marseilles desires to have pure water at any rate; but does not take any trouble, in order to respire a pure air."

We can thus understand how it was, that the sanitary guards and others, who were confined, not for days only but for weeks, in an unhealthy locality like the Lazaret of Marseilles, should be attacked with fever and even plague, although it did not prevail elsewhere. We can also understand, why even the men on board those ships that had come from an infected port, should have been

attacked in the Lazaret, without inferring, that they had imbibed the poison previously, or in another locality. In the one instance, they had been confined to the morbific boundaries, for a longer or shorter time; in the other, they had not been restricted to one spot, or had not come within the focus of the disease.

It may be said, and, no doubt, will be, that plague never arises spontaneously: that it is a specific disease, generated in the bodies of men, and propagated from individual to individual by means of contagion. These inferences cannot be maintained now, with any degree of plausibility, for a single moment. As has been already stated, sporadic cases of plague are met with constantly in Egypt, in years in which the disease does not prevail epidemically. These single and isolated cases,—to the number of ten, twenty, thirty or more,—must arise spontaneously, as there is no other way in which to account for their production. The following case is still more noteworthy. Mr. Laidlaw states, that he visited, in the Port of Alexandria, a sailor, recently arrived from England, who had been attacked with plague, although the disease did not exist in the town, or elsewhere. As such, he could not have contracted the disease from any one. Nor, on the otber hand, did he infect the rest of the crew, although he slept in the midst of his companions for several nights. In other instances, the disease will spring up spontaneously in a town without extending beyond. Baron Larrey states, as he learnt from the inhabitants of Jaffa, that the plague appeared there every year; and had done so for thirty years previously; although the disease did not prevail elsewhere, or in any other town. If, therefore, plague can arise spontaneously in one locality, it can do so in another, provided only that the causes productive of the disease be in operation in that locality.

Although the plague had not prevailed epidemically, in that part of France, for thirty or forty years, when the last cases referred to occurred, we have yet seen, that certain localities continue pestiferous for indefinite periods. If this be the case with other diseases, there can be no reason why a similar result should not be observed with plague; more especially in localities, in which this disease prevailed previously epidemically.

There is another reason why we may infer, that the cases of plague, or, at least, the cases that assimilated themselves to plague, arose spontaneously in the Lazaret of Marseilles. This is that plague is merely an aggravated form of ordinary fever. Sydenham, Lieuland, and other writers have compared plague to a malignant fever. It is not, in fact, the only disease, in which swelling of the glands, carbuncle and petechial spots, are observed. Hence it is, that the plague has almost invariably been ushered in by malignant, or severe, forms of fever; but without the characteristic symptoms: while the latter have almost as generally taken the place of the former, on the termination of the epidemic. "It is observable," says Dr. Heberden, "that, at its first breaking out, the disease is never known to be plague. It has generally been preceded by a severe putrid fever. This was the case in London, in 1665; at Marseilles, in 1720; in Holland, in 1764, and at Moscow, in 1771." In the last city, the fever prevailed for three years, before the plague broke out. A severe or malignant form of fever also prevailed at Algiers in 1816-17. This was regarded as the *avant courier* of the plague, which, it had been noted, was usually preceded by some such form of disease. The prediction was realized the following year, by an outbreak of plague. Sydenham has remarked, that he did not

know, if the disease which broke out in May, and which afterwards merged into the plague, was the plague or not. More than this, since the plague has ceased to prevail epidemically in Europe, it has been replaced by another, and a common, form of fever, viz., typhus; which is merely a mild form of plague—the one disease frequently running into the other. Dr. Pruner states, that the plague of 1835, was preceded by malignant fevers of a peculiar character: as, also, by a visitation of smallpox. It was followed by the cholera and typhus, well marked cases of plague occurring at the same time as the latter.* When typhus appears in a severe form, as is sometimes the case, it is scarcely possible to distinguish it from plague.

This is not all. In particular instances, and in certain localities, the plague would appear to be merely a severe form of intermittent fever. This was rendered most evident, during a campaign of the Russians in Turkey. On arriving at Bucharest, in April, 1828, the Russian troops were attacked with a fever, which was accompanied by vomiting, great debility, and, with some patients, by buboes and carbuncles—being followed, generally, by death, at the end of three or four days.

The medical men of the town stated, that such a disease was common there, and that it usually subsided at the commencement of the hot weather. This was found to be the case; but the disappearance of the disease was only temporary; it re-appeared in August, and became more general than before. At this period, however, the plague was preceded, in a great many places, by a general prevalence of remittent and intermittent fever. And it is added, "these diseases were productive of greater ravages,

* Answer to questions proposed by the English Consul-General in Egypt.

during the whole course of the campaign, than the plague itself." * Precisely the same results were observed the following year, except that the disease was more general and more fatal—especially at Varna, where its ravages were terrible. In July, the army crossed the Balkan and occupied Ardos, when intermittent fevers began to prevail. These continued until August and September—the hospitals at Ardos and Adrianople, all this time, being crowded with cases of intermittent fever and dysentery. "It was not until the 19th October, after peace had been signed with the Turks, that the first case of fever, accompanied by a bubo and a carbuncle, made its appearance. These cases soon began to multiply, and to present the aspect of a well marked epidemic of plague: it committed, consequently, frightful ravages at Adrianople,"—where the principal part of the army was stationed. †

"But," observes the writer, in another place, "it is not only while considering these epidemics in their entirety, and, as a general fact, which induces us to infer, that the plague, during this war, was only the last development of the endemic intermittent fever: the study of particular cases of these diseases appeared, sometimes, to confirm, entirely, this opinion. Thus, Dr. Milovanof, who treated the plague at Achial, tells us: 'The soldiers and officers, who had intermittent fever, suffered also with buboes and carbuncles: from the first establishment of the hospital, we have observed tumours and swollen glands at the neck. In the month of September, the disease was met with, principally, in the relapses from intermittent fever, while it took the form of a tertian ague.' At Adrianople, also, the first soldier, who was attacked with plague, had been

* Extrait des Notices de M. le Dr. Seidlitz: par M. le Dr. Bussemaker, d'Amsterdam.

† *Idem.*

in the hospital for two months suffering from intermittent fever." Dr. Rink, who was stationed in this town all the time of the epidemic, remarks: "The mildest form of plague resembles intermittent fever so much, that it was almost impossible to distinguish the disease, before the appearance of buboes." And he adds: "It is to be remarked, that this form of plague was more common at the commencement and towards the end of the epidemic than at its height." Hence,—and also, from the fact, that, whenever the Russians have invaded Turkey, the plague has made its appearance on the borders of the Black Sea, —Dr. Seidlitz has enunciated the opinion, that "the plague, on these occasions, is only the severest form of the endemic fevers of the country." *

Dr. Begin † and Dr. Boudin ‡ have also inferred that plague belongs to the family of marsh, or malarious, fevers. If so, we can understand, why intermittent fevers were so common in Europe during the prevalence of the plague, of which we had abundant proof, even in London. In 1661-2-3 and 4, intermittents raged like a plague, to use the common expression, and ushered in the true plague of 1665 and 66. These fevers were also accompanied by a severe continued fever. They then disappeared, for some years, for reasons, as Sydenham remarks, with which we are at present unacquainted. After a short interval, these fevers returned, and continued to prevail epidemically until the middle of the eighteenth century, when a severe visitation was experienced. They then gradually subsided in London, and had almost entirely

* Med. pract. Abhandlung. 1835. Also, Memoires sur les Sciences Médicales, publié par l'Académie Imperiale de S. Petersbourg. 1844.

† Dictionnaire des Sciences Médicales. Art. Maries.

‡ Geographie Médicale, p. 43. Paris. 1843.

disappeared at the commencement of this century. With these facts before us, there can be no difficulty in accounting for the spontaneous outbreak of plague in the Lazaret of Marseilles; or, in any locality in which intermittent and remittent fevers are endemic, and in which the plague formerly prevailed in an epidemic form. This is not the only conclusion we can draw on the subject. If plague be only a severe form of intermittent or remittent fever, and if it be produced by the same cause—as the preceding facts would lead us to conclude—we must also infer, that plague, irrespective of all previous arguments and conclusions, is non-contagious. Not only intermittent and remittent fevers, but those severe forms of continued fever, met with in tropical climates, are universally allowed not to be contagious: the facts presented during their prevalence forbid such an inference. The examples, before given, of the attacks of fever with sailors, who landed in pestiferous localities; and the exemption of their messmates among whom they were placed, in the confined deck of a man-of-war, are sufficient to prove the truth of this conclusion. If, therefore, a disease, presenting the same pathognomonic effects, with some slight variations, and which, apparently, is produced by the same cause, be non-infectious, we cannot err much in concluding that plague also is incapable of propagation from individual to individual. This conclusion is in accordance with all the facts previously advanced, and with which we are acquainted. Of what use then are lazarets, quarantine, and all the obstructions and annoyances to commercial traffic and human intercourse? They can be of no earthly use, or benefit whatever: while they are productive of direct injury, by producing the very result they are intended to avoid—that is, outbreaks of disease.

The truth of these conclusions has been demonstrated, in a satisfactory manner, by the researches of Dr. Rossi, who instituted a series of inquiries respecting the practical results of the system. His object was to ascertain, if Lazarets and Quarantine prevented the appearance of the plague, in those countries in which they had been established; and the result—the unexpected result—of his researches shows, that the outbreaks of plague were actually more frequent after than before the establishment of Lazarets, at least in Europe. The case is different with Turkey and Egypt, Lazarets having only been recently established in these countries. These facts will be evident by a reference to the following Table, in which the number of outbreaks of plague in each country, before and after the establishment of Lazarets, has been inserted.

By a study of this Table—Table 2—some remarkable facts may be elicited. Thus, in France, there had been, from the commencement of the Christian era to the establishment of Lazarets, 29 visitations of plague; being 1 every 50 years: while there were, subsequently, or from that date to the cessation of the plague, 27 visitations. This gives an average of 1 visitation every 9 years—a remarkable difference. The same result is apparent in all the other countries in which Lazarets were established, with the exception of England and Holland, in which no visitations occurred subsequently. The reason is, that the plague had ceased in England 55 years before the institution of Lazarets; and in Holland, 31 years previously. That the formation of the Lazarets had nothing to do with the cessation of the disease in England is certain, from the fact that the plague subsided, at the same time, in the north of France and other countries in the north of Europe. It may also be stated, that

TABLE 2.—Number of Visitations of Plague in the following Countries, during each century of the Christian era; with the number of attacks in each, before and after the establishment of Lazarets.

COUNTRIES.	CENTURIES.																			Total.	Before the Formation of Lazarets.		After the Formation of Lazarets.		Date of Institution.
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.		No. of Attacks.	Average. Years.	No. of Attacks.	Average. Years.	
France	..	..	..	..	..	10	..	..	2	1	4	..	2	5	5	14	11	1	..	55	28	1 in 52	27	1 in 9	1476
Germany	..	..	..	..	1	1	1	..	1	3	4	1	..	3	6	12	19	4	1	57	21	1 in 71	36	1 in 8	1490
Dalmatia	..	..	..	..	..	1	..	..	..	1	1	..	2	3	11	9	7	5	1	41	17	1 in 86	24	1 in 14	1466
Spain	..	..	..	..	1	1	..	..	1	1	..	..	..	3	4	5	3	3	1	23	11	1 in 135	12	1 in 25	1494
Italy	2	3	1	..	2	6	3	2	2	4	5	4	7	9	19	11	11	1	2	94	51	1 in 27	43	1 in 9	1403
Turkey (in Europe)..	..	..	2	..	..	1	1	1	4	3	1	..	..	..	1	6	4	7	6	37	35	..	2	..	1835
Egypt.....	..	..	1	..	..	..	1	..	..	..	..	..	1	2	..	1	2	19	8	35	31	..	4	..	1825
Syria	1	1	..	..	1	..	1	1	1	..	1	..	1	..	..	..	..	4	2	14	12	..	2	..	1840
England	..	..	..	..	..	..	1	..	1	..	2	1	1	1	4	5	6	..	..	22	..	..	..	..	1720
Holland.....	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1795

although there had been no visitation of plague for 55 years before the formation of Lazarets, there were no less than 6 in the previous 65 years.

It may however be asserted, that the plague had been introduced into the preceding countries—those in which the plague appeared after the establishment of Lazarets—by land, instead of by sea. But this supposition does not apply to Venice, which cannot be approached by land. Hence, Dr. Rossi was careful to ascertain the exact number of visitations of plague that this city has experienced. There were, in the 465 years before the establishment of Lazarets and Quarantine, 4 visitations of plague in Venice; viz., in A.D. 938, 1006, 1347, and 1405: this gives an average of 1 in 116 years. In the 227 subsequent years, there were 16 irruptions of plague, viz., in 1411—13, 38, 47, 56, 64, 68, 85, and 96; also in 1500—23, 27, 56, and 1630. This is an average of 1 visitation in every 14 years! These statistical results confirm the deductions previously drawn, as to the uselessness of Lazarets, quarantine, and other measures, invented by foolish and ignorant men, to prevent the operation of natural laws. Vain effort: they may as well attempt to arrest the sun in his course, or the moon in her orbit, as try to prevent the irruption of epidemic diseases. It is also to be remembered, that Lazarets can only be useful on the supposition, that the plague is always imported by ships. But pestilences do not always commence on the sea-coast. That in the time of Thucydides began in Ethiopia, on the borders of Upper Egypt. The plague of A.D. 252 also began there, as did the one in 1736. So, again, the plague of 1348, which ravaged France, broke out at Avignon, not at Marseilles or other Ports: as was the case, also, in 1482. But the most remarkable fact was the breaking out of the plague in

Poland in the visitation of 1702 : before any other place or country was infected!

These are not the only facts that are brought out by a study of the preceding Table. It is commonly supposed, and many arguments and conclusions are based on the supposition, that Egypt is the home, the birth-place of the plague. Nothing can be more erroneous than such a conclusion. Up to the end of the 12th century, only two visitations of plague in Egypt have been recorded ; and only 6 more to the end of the 17th century—8 in all. During this period, there had been 22 visitations in England, and 55 in France. After this, the reverse is the fact, for, in the 18th century, when the plague had ceased entirely in England and other countries in Europe, there were no less than 19 outbreaks in Egypt. This shows that, so far from plague having been imported from Egypt into Europe, it might be said, with more truth, that it had been imported into Egypt. As will be remembered, the crusades to the East took place in the 12th and 13th centuries: the very period when the plague raged to the greatest extent and the most frequently in Europe. And yet, during these two centuries, there was only *one* visitation of plague in Egypt; one in Syria, and not one in either European or Asiatic Turkey or Palestine. Again, the commercial relations of Europe with the East, by the Mediterranean, were the greatest at this epoch, and up to the end of the 16th century : after this, they were diverted into another channel, by the discovery of the Cape of Good Hope. Now it was precisely at this period, when commercial intercourse and traffic with the Mediterranean had almost ceased, that the plague began to prevail in Egypt more frequently, and to a greater extent; thus showing, that this disease pursues its own course, irrespective of commercial traffic and human intercourse.

There is another circumstance that may be mentioned, which shows very clearly, that the plague cannot be propagated by either animate or inanimate objects. This is, that although there are neither Lazarets, nor quarantine, the plague never spreads beyond certain well-defined boundaries to the south and east of Egypt. Thus, it is unknown in Upper Egypt beyond Assuan, or the first Cataract; in Nubia, Abyssinia, Sennar, and all the countries to the south of this Line. "Here then," exclaims Dr. Aubert, "is a country in which each year 70 or 80,000 persons, coming from localities in which the plague reigns, are assembled, without spreading the disease; and although they carry merchandise and other articles with them. So, also, if patients infected with plague, are carried to Philoe, beyond the morbific boundary, they die or recover, without ever giving the disease to the inhabitants."* Its limits eastward are equally well defined. According to Dr. Duchèze, the plague never passes beyond a line drawn from the Caspian Sea to the mouth of the Persian Gulf. Hence it has never appeared at Teheran and Ispahan, although it carried off at Bagdad, in 1831, half the population. Even here, remarks the above writer, it spared certain quarters of the town, notwithstanding the constant communications with the infected localities. Dr. Duchèze also states, that *the intervals* of the return of the disease *are longer*, the farther you proceed towards the south-east, from Trebizond to Aleppo. These are remarkable facts, and entirely opposed to all the deductions drawn from the doctrine of contagion. Were plague contagious, in the slightest degree, it would be, we should conclude, precisely in southern latitudes—in warm climates, and among half-civilised nations—that the disease would have spread with the greatest ease, and with the

* De la Peste, p. 101.

greatest rapidity. And yet, it is precisely in such situations, where no barriers are erected, and where no *cordon sanitaire* has been formed, that nature draws an invisible line, and exclaims, "Thus far shalt thou go, and no farther." When, however, man erects a barrier, and establishes a *cordon sanitaire*, she laughs, to very scorn, his puny efforts to arrest her progress. Thus, the epidemic cholera, during its first visitation in Europe, marched directly across a *triple line* of bayonets, formed with 50,000 Prussian troops—a *cordon* so rigidly kept, that even a cat, it was said, could not have passed across. The same result is observed, sometimes, with respect to elevation. On the mountain of Alemtaghe, five leagues from Constantinople, there is a village, which has hitherto remained exempt from the plague; although it has always served as a place of refuge to the inhabitants of the infected city. This is the more singular, as another village, half way up the mountain, does not enjoy the same immunity. This singular fact, the non-propagation of the plague by infected persons, beyond the epidemic area, has been observed and recorded long ago. Evagrius, speaking of the plague of the sixth century, remarks: "But what above all appeared singular and surprising was, that the inhabitants of infected towns, removing to places where the disease had not appeared, or did not prevail, were the only persons who fell victims to the plague, in the cities which *were not affected.*"*

Although it may be superfluous to do so, there is yet another argument that can be employed, in order to show, that contagion has nothing to do with the propagation of the plague. This is, that the disease invariably subsides in Egypt, at a particular and certain period. When the plague prevails epidemically in that country, it com-

* Hist. Eccles. lib. 4, ca. 29.

mences in the month of November, and ceases its ravages the end of June. This fact was mentioned, long since, by Prosper Alpinus, and has been confirmed by nearly all writers since. Hence, as Dr. Lachéze remarks: "When the 25th June arrives, the ordinary period for the cessation of plague in Egypt, all precautionary measures are abandoned; and yet, no ill results are known to follow. This must not be ascribed to the fact, that all the susceptible people have been attacked; for strangers, who then arrive in considerable numbers, also remain exempt. The negroes of Sennar are brought into the market to replace the black slaves that have died; and are generally clothed with the garments worn by the plague patients. In 1835, out of 600 negroes brought to Cairo, in this way, fourteen only were carried off."* This result cannot be ascribed to season, or temperature, for although the plague thus appears at the commencement of winter, and disappears at the beginning of summer in Egypt, the reverse is the case at Constantinople. Here, the plague usually commences in July—from the 1st to the 20th—and ceases on the approach of winter; so that, as one writer has remarked, while 16 degs. of Reaumur are sufficient for its production in Egypt, 25 or 30 degs. are required in Constantinople. This *invariable* cessation of the disease at a particular epoch, is fatal to the doctrine of contagion, more especially as, the fact being well known, the means previously adopted to prevent the spread of the disease are then laid aside. "What," adds Dr. Perron, the Director of the Medical School at Cairo, "is the contagion of a disease, which ceases *officially* at a particular season; at a particular period of the season, at such and such a temperature of the air; and which varies, in its in-

* Séance de l'Académie Royale de Médécine, du 28 Décembre, 1844.

tensity of action, with the variations of the atmosphere—of humidity, of cold, and of the sudden transitions of the day and of the night. If this be called contagion, then we have nothing more to do with that which is called the contagion of men and things."

With such an amount of evidence, direct and indirect, we may now conclude this part of the subject, by drawing the following inferences. 1st. That the plague is not propagated by contagion, either mediate or immediate: in other words, by the contact, direct or indirect, of the healthy with the sick. 2ndly. That the clothes, bedding, etc., of plague patients, even when saturated with the secretions and excretions, do not give the disease to those who may handle, or wear, them afterwards. And 3rdly. That articles of merchandise do not imbibe the germs of plague, and cannot, consequently, convey the disease to other and distant countries.

If the plague be not contagious, we might also infer *à priori*, that no other disease would be. But this is not the general opinion: a number of other diseases—the majority, in fact, of epidemic diseases—are also considered to be contagious. One of these is yellow fever. After the discussion that has taken place, respecting contagion in general, it would be superfluous to enter into any detail of the alleged instances of the propagation of this disease by infection. It is sufficient now to remark, that all the circumstances and arguments which have been advanced, in support of the contagiousness of yellow fever, would appear to be negatived by the following facts.

1st. Yellow fever is confined to certain latitudes, and well-defined boundaries, beyond which it never passes. It is unknown beyond 45° N. latitude, and 23° S. latitude, and between 20° E. longitude, and 120° W. longitude. The chief seat of its ravages is be-

tween latitudes 10° and 30° N.—in the West India Islands, on the continent of America, and on the West Coast of Africa. It is unknown in the interior, in the southern, and eastern parts, of Africa, as also in the whole of Asia. It has prevailed on several occasions in the South of Europe; at Cadiz, Seville, Gibraltar, Malaga, and along the eastern coast of Spain, as far as Barcelona. It reached Leghorn, on the one side, and Portugal on the other; but it has never extended beyond these boundaries in Europe. From the Equator to the 26° or 28° N. the yellow fever may, and does, prevail all the year round; but from this line to the 43° N. only during the heats of summer. In the former latitudes, the inhabitants are rarely attacked, only strangers; but in the latter, or northern latitudes, the inhabitants are as subject to its attacks as strangers. How a disease, which is confined to a particular portion of the earth's surface, can be regarded as contagious, must be inexplicable to ordinary minds; more especially when no means are taken to prevent its spread. Yellow fever prevails, it is true, principally in large commercial towns, and seaports, but it is not confined to such situations. It spreads into the interior of America, as it did also to the small towns and villages near to the coast of Spain; although it did not extend beyond, in the latter situation. But there is no reason why it should not have done so, if the disease be contagious.

2dly. Although the torrid zone would appear to be its source, yellow fever is not generated exclusively there, nor does it always spread from these to other and higher latitudes. Instead of commencing in the south, it occasionally pursues the opposite course. This was the case in 1798, when the epidemic commenced at Boston, in lat. 45°, in June, and travelled southwards, reaching New

York, in lat. 41°, in July. Its course, through this town, was also the same, having commenced at the northern extremity, and then proceeding in a southerly direction.

3dly. Removal from the focus of the disease, or the locality in which it prevails, is always sufficient to arrest its progress. This is so well known, that men-of-war, on the West India Station, depart immediately, when yellow fever breaks out on board, and make for other and more northern latitudes. But the disease has never been carried to the North American colonies, nor is the infection spread to the remainder of the crew, excepting with those who have imbibed the seeds of the disease before their departure. That the subsequent attacks ought to be referred to this cause, we may learn from another circumstance. It sometimes happens, that a part of the crew only is exposed to the causes productive of yellow fever. When this is the case, the disease is never found to spread to the other men, although placed in immediate contact with them. Some examples of this kind, with other forms of fever, have been already given: but the following is more to the point. In 1782, "the *Assistance* (man-of-war) wooded and watered at St. Thomas—a noted place for yellow fever—and, with a view to expedition, a tent was erected on shore, in which the people, employed on these services, lodged during the night. On the middle passage, *every man,* who had slept on shore, was attacked with fever and *died:* while the rest of the ship's company remained perfectly healthy." *

This immunity of high northern latitudes—those beyond the 45th degree—and the cessation of the plague, when a ship arrives there, or near, cannot be referred to temperature or cold,—the heat being sometimes as great in high northern latitudes, as in more southern ones.

* Trotter: Medicina Nautica, vol. i., p. 456.

Besides, although the epidemic spreads as far north as 43°, or 45°, it does not pass beyond the 23rd degree, in the southern hemisphere, although the heat, in that latitude, is nearly tropical. Then, again, it is precisely in a northern direction that yellow fever spreads, when it becomes epidemic, passing from the West India Islands to the coast of America, and travelling from the south to the north of this continent.

Added to this, we observe precisely similar results in the same locality or latitude. One instance has been already given: that of soldiers on guard at the Dockyard, Antigua, being attacked and dying of yellow fever, while their comrades in the barracks above, to which they were removed, remained entirely exempt. The barracks here, it is true, are at some elevation, and, consequently, enjoy a cooler atmosphere. But, then, the same result is observed at Trinidad, where the circumstances are reversed. There, the military post was on the La Ventille Hill; while the barracks are on a plain—on the same level as the marsh. Nevertheless, men attacked with fever on the hill, and taken to the barracks below, did not give the disease to others, in this situation, more than at Antigua. It has also been stated, that strangers are constantly attacked with yellow fever within the torrid zone, although the residents and natives remain exempt. Cold, therefore, cannot be the cause of the non-propagation of this disease in high northern latitudes. What the cause may be of the limitation of range of yellow fever, is immaterial at the present moment: it is sufficient now to know, that this disease cannot be propagated, either within or without its natural boundaries, by means of contagion. The preceding facts are more than sufficient to prove the truth of this conclusion. Notwithstanding, one more proof may be added. This is that the attendants on the sick, in yellow

fever, the same as in other diseases, are less prone to attacks than other classes. In the following Table, the proportion of attacks and deaths among the white troops, stationed at Newcastle, Jamaica, during a visitation of yellow fever, has been given; those who attended on the sick being separated from the others:—

TABLE III.

PROPORTION OF ATTACKS AND OF DEATHS, FROM YELLOW FEVER, AMONG THE FOLLOWING CLASSES.*

	Strength.	Attacked per cent.	Died per cent.	Proportion of deaths to attacks.
Men who attended fever cases	156	5.1	1.9	21
Men who did *not* attend on the sick	523	17.0	7.3	41

Not only was the ratio of attacks and of deaths, among the attendants, very much less than among other classes: but, what is still more singular, the disease was less fatal. Among the attendants, 21 per cent. of those attacked died: but 41 per cent. of the others were carried off—exactly double. And yet, both classes were exposed to the same local or general causes; while the attendants were exposed, in addition, to the emanations from the sick. So far, then, from these presumed sources of disease being injurious; we might ask, and with apparently more reason, whether they be not actually beneficial and preservative? We may leave the contagionists to answer the question: and to show cause against the hypothesis, if

* The facts were taken from one of those valuable documents, the Army Medical Reports—Reports which deal in facts rather than theories—but the date, or the year, was accidentally torn off the extract.

they be dissatisfied with it. There are certain facts, which tend to show, that the ships are sometimes infective, or pestiferous, although the crews are not; this is an entirely different question, and will be discussed hereafter.

If yellow fever be not spread by means of contagion, it must be ascribed to the operation of some general, but, at present, unknown cause. We may also infer, without much risk of error, that it springs up spontaneously in those countries in which it is indigenous, uninfluenced by individual causes, or by man: more especially as it prevails in uninhabited, the same as in populous districts. That it did so formerly, in America, would appear certain. Noah Webster states, that "the same pestilential disease (yellow fever) which has lately afflicted our cities, appeared among the aborigines of this country (America) *before* it was settled by the English; before the West Indies were settled by the English or French; and before a single vessel from the Islands had ever reached our shores."* As the disease, therefore, must have sprung up spontaneously in these instances, there can be no reason why it should not do the same in all others. As yellow fever does not attack the natives in those latitudes, its existence in the West India Islands could not have been ascertained, until after their settlement by Europeans. The Indians, or, at least, some of them, lived beyond the boundary in which the disease is endemic. As such, they would be as liable to be attacked as the Americans, whenever the disease became epidemic, and spread to other latitudes. With these remarks, we will pass on to another part of the subject, or, rather, to the consideration of another disease—that of small-pox.

Of all diseases, this is the one that is considered to be the most contagious—the model contagious disease, in

* On Epidemic Diseases, vol. ii., p. 183.

fact. To doubt it would be a medical heresy; one that will place an individual, not in the Inquisition, for that, fortunately, has been abolished; but, under the scalpels of his professional brethren. Still, in spite of this punishment, severe as it may seem, I must avow myself to be a heretic, in this instance, having no faith in the doctrine of the origin or propagation of small-pox by contagion, under ordinary circumstances. That it can be so propagated is more than probable: all I maintain is, that small-pox arises spontaneously from some unknown cause, and that it is not propagated, in the generality of cases, by contagion. A variety of reasons induce me to draw this conclusion.

We may remark, in the first place, that small-pox is an epidemic, appearing only at particular periods: it must therefore, like all other epidemics, have had an origin. No mention has been made of this disease before the commencement of the sixth century. Alexander Trallianus, who published his work at this period, and who described all the diseases then prevalent, makes no mention of it. Rhazes was the first who wrote on the disease in Europe, at the commencement of the tenth century; but he states that it had been noticed by Arabian writers previously. Montfalcon observes that Ahron, a physician of Alexandria, first mentioned the symptoms, the different varieties, and the treatment of small-pox, at the commencement of the seventh century. According to an Arabian MS. in the library at Leyden, referred to by Dr. Mead, the malady appeared, for the first time, in Arabia, in 572, the year of the birth of Mahomed. But Marius, Bishop of Avenches, states that it prevailed in France, in 570, (two years previously) and again in 580—the wife of Gontran, King of Burgundy, having died of the distemper

in the latter year.* We have no accurate account of its appearance in England before the ninth century, while it appears to have been unknown in Norway, Lapland, &c., as late as the end of the thirteenth century. According to the Census of Ireland, it appeared there in 760.

Now this was precisely the period when the plague, or black death, made its appearance in Europe; while both diseases have continued to prevail from that time to the present day, and, apparently, under precisely the same circumstances. We may, therefore, infer that the cause of the production of the two diseases is, in all probability, the same. Sydenham has made the remark, that when there are a number of cases of small-pox in the autumn, the fever, then prevalent, partakes of the inflammatory character of the former. The only difference is, that, in the one case, there is an eruption; in the other not. The abundant perspirations and the increase of the salivary secretion, which were met with in each disease, prove, he thought, the identity of the two.† Be this as it may, there must have been a cause for the production of small-pox in the first instance; or, in the first case, irrespective of contagion. Granting that small-pox was first generated in the body of some unhappy mortal, and then propagated from individual to individual; how are we to account for its production at subsequent visitations? By the same means? That would seem to be hardly probable, hardly possible; when we find, that this epidemic has continued to return, at certain intervals, during the last twelve centuries, in thousands of instances, in every variety of climate, and among all the different races of mankind. It would be contrary to all analogy to suppose that the same

* Historia Francorum Scriptorum, &c., vol. 2.

† Chap. 2, on Epidemic Diseases.

poison could be accidentally produced under so great a variety of circumstances, and among all these various races—some living on a purely vegetable diet; others, on an almost exclusively animal one. Nor can we imagine, that the germs of the disease have remained latent, either in animate or inanimate objects, during the intervals of the outbreaks—the period being too long, in most cases. In Mexico, according to Humboldt, the disease only returns every seventeen or eighteen years. Sometimes the interval has been much longer. In Iceland, there was no visitation of small-pox from 1672 to 1707, or from 1707 to 1785—seventy-eight years. Besides, this disease prevails, not only in warm climates, but, as we have just seen, in cold ones; not only in Iceland, but in the Arctic regions, among the Esquimaux—situations in which the germs, or virus, of contagion, could not exist, in a free state, out of the body, for a single day, or hour. And yet the disease, in these latitudes, is not less virulent than in warmer climates. In Iceland, in 1707, 18,000 are said to have perished, out of a population of only 50,000. If, therefore, the disease were not imported—and that would be all but impossible in such localities, not only shut out, to a great extent, from intercourse with other regions; but separated from them by a distance too great to allow of the transport of a patient from one situation to the other—we must infer, that it arose spontaneously. This granted, we may also conclude, that it arises spontaneously in all other instances; not only for the reasons before mentioned, but, also, because it would be unphilosophical to suppose, that there are two different causes in operation, productive of precisely the same result. As such, it only remains to consider, whether small-pox, after it has arisen, is capable of being propagated from individual to individual, or from the sick to the healthy.

If there be one disease, more than another, which would render an attempt to prove its non-contagiousness difficult or useless, it is precisely small-pox. Unlike the majority of other diseases, there is here actual proof, ocular demonstration, of the presence of a contagious matter, by the formation of pustules on the surface of the body. And yet, it is, perhaps, this very circumstance that renders the disease non-infectious. Assuming, and this would appear to be the only inference that can be drawn on the subject, that small-pox is produced by the operation of a specific poison, and its introduction into the system; we may also infer, that the pustules, on the surface of the body, are also due to the presence of the poison in the minute capillaries of the skin. There would seem to be no other way in which to account for their production; unless we suppose, that the pustular fluid is formed in the blood, and is thence expelled by the secerning vessels on the surface of the body. But, were this the case, the blood would not only become so changed, but also so infectious, that death only could be the result of an attack of small-pox; and this, too, before the appearance of any pustular eruption. Hence, if small-pox be the effect of a specific poison, introduced into the system from without, we may also infer, that the morbid agent, or the greater part of it at least, is contained in the capillaries of the skin, or in the pustules, during attacks of this disease.

If these conclusions be sound, what is there to fear? The agent productive of the disease, or the greater part, is locked up—hermetically sealed—in the capillaries of the skin, from which it will be unable to escape unless the pustule bursts, which only occurs in the more severe cases. That the matter, which then escapes from the pustules, is infectious, it would be superfluous to assert: but, then, it is necessary that this matter should be

applied to an abraded surface, or be introduced into the blood, by means of an incision, in order to produce any specific effect. Added to this, it is only at a particular period, the first stage, or so, that the pustular fluid would seem to possess infective properties—precisely the period before the pustules break. Afterwards, the matter evidently undergoes some change, as it is actually absorbed into the blood, when the pustule does not break—it being by the absorption of the fluid part that the pustule dries up. As, also, it is precisely in cases of recovery, that this effect is observed, it shows that the matter contained in the pustules, in the last stage of the disease, cannot be possessed of any injurious or infective properties.

Whether these conclusions be sound, or unsound—and they are entirely hypothetical—we know from practical experience, that the matter discharged from the pustules, in small-pox, is quite innocuous under ordinary circumstances. It is constantly applied to the skin and the hands of nurses, attendants and doctors, without producing any injurious effect. To be productive of any morbid effect, it must be applied to an abraded surface, or be introduced into the system by means of a puncture. As such, the only way in which small-pox could be propagated from individual to individual, excepting in particular cases, would be either by means of the expired air of the sick, or else by means of the exhalations given off from the surface of their bodies. But is it propagated in either of these ways? This is a question that it is necessary to answer.

According to the deductions previously drawn, there can be no more danger from the expired air of a small-pox patient than from the pustular matter on the surface of his body. If the poison, or the greater portion of it, as has been inferred, is contained in the capillaries of the

skin, or in the pustular vesicles on the surface of the body, there can be little danger of any being given out from the lungs; or, at least, not in a sufficiently concentrated state to produce any injurious results. The expired air of a small-pox patient, it is true, is always more or less offensive; and so it is, in other fevers, in those in which it is morally impossible to conclude, that they are infectious—such as the continued fevers of intertropical climates. No conclusion, therefore, can be drawn from this circumstance alone: we must seek, in other facts, an answer to the question. The same argument will apply to the exhalations from the surface of the body, more especially if the morbid agent be not volatile, as certain facts would seem to show. M. Chauveau concludes, from his experiments, that "the vaccine and variolous *serum* is not infective; and that the activity of the matter resides in the solid granulations."* This is precisely the case with the poison termed malaria—a non-contagious agent—which is not absorbable by water. There will be little chance, therefore, of the poison of small-pox escaping from the containing vessels, or from the pustules, unless they burst.

If the expired air of a small-pox patient or the exhalations from the surface of his body were infectious, we should expect to find, that the attendants in hospitals, and in private houses, as also the doctors, would be more predisposed than others to attacks of the disease. But the contrary is the case.† Then, again, if infectious

* Théorie de la Contagion Miasmatique ou Médiate. Comptes Rendus, Feb. 10, 1868.

† It may be said, and in fact, has been said, that the exemption of these persons is to be referred to vaccination. If this be the case, how are we to account for the attacks of small-pox with other classes that have been vaccinated? In the late visitation, *two-thirds* of the patients admitted into the small-pox hospitals had been previously vaccinated.

matter were given out from the lungs of such patients, the mortality in hospitals ought to be very much greater than in private practice. In fact, there would be little chance for a person, placed in such an atmosphere, filled with the exhalations from the lungs of twenty or thirty other patients. The result would be as certain, and as fatal, as that of inhaling the pestiferous air, given out from the marshes below the La Ventille Hill at Trinidad. But does such a result occur? Certainly not, for the mortality in hospitals is not greater than in private practice, if we make allowance for the difference in the condition of these patients, when compared with those in their own homes. The former are generally severe cases, while they are, as frequently, sent in after the disease has existed for some time. Taking these circumstances into consideration, it will be found, that the rate of mortality, with the patients thus crowded together, is not greater than with isolated cases. We may therefore conclude, that no pestilential matter is given out by the breath, or from the bodies of small-pox patients; and, consequently, that the disease is not propagated in this way. Another circumstance which tends to prove, that contagion has nothing to do with the propagation of small-pox, is the existence of sporadic cases. If we look through the old bills of mortality, it will be apparent, that there is no year, in which such cases do not occur. How is it, then, that the disease is not propagated at those periods the same as at others, or when it prevails epidemically? It cannot be for the want of subjects, or the want of susceptibility; inasmuch as the very persons, who then escaped, are precisely those who are subsequently attacked. The only way to account for the phenomenon is, by supposing that there is some general cause productive of small-pox; and that this cause is in operation to a great extent at one period, and to a small

extent only at others. As Dr. Prus, the able Secretary to the Plague Commission has remarked: "When *certain conditions of the atmosphere* are absent, a first case of small-pox is seldom followed by a second. We observe this daily, in the hospitals of Paris, although the small-pox patient be placed in the midst of other and ordinary patients." Its diffusion must be ascribed to a general and external, not to an individual and internal cause.* Although there are several other epidemic diseases, which are set down as contagious—as scarlet fever, measles, etc. —it is not necessary to discuss the validity of this conclusion now. If small-pox be not contagious, we may rest assured that these affections are not. Besides, the arguments that apply to one disease, more especially to plague, will be equally applicable to the others. Then, again, there are some diseases reputed to be contagious, for which there cannot be the shadow even of a pretence. One of these is relapsing fever. Referring to this disease, Mr. Simon remarks: "The chief points to be borne in view are the following."

1. "The greatest personal predisposition to relapsing fever is given by states of poverty and privation: so much

* The following announcement appeared recently in the newspapers. "John C. Hartnell, a newspaper reporter at Ryde, was on Tuesday (Feb. 13, 1872) fined by the magistrates 2*s.* 6*d.* and costs, for selling a local newspaper, which had been *exposed to infection* from a person in his house, suffering from small-pox, without having previously *disinfected it!*" This must surely be a joke of "Punch," and be intended to ridicule the folly and the whims of modern contagionists. A newspaper imbibe the germs of small-pox! Wicked Mr. Punch! If a newspaper is to be disinfected, it will be necessary to put the doctors, nurses, etc., into a copper, like the Australian meat cans—as it has not yet been ascertained what substance is really disinfectant—in order to preserve them and others from infection!

so, that the disease is often known by the name of famine fever."

2. "Relapsing fever is, in *a very high degree*, communicable from sick to healthy."*

How a disease, which is confined to one particular class—those who have not sufficient nourishment—can be called contagious, is certainly past my comprehension. Contagion, we may be assured, would be no respecter of persons. More than this, it is precisely the classes which this fever spares, that ought, we should presume, to be attacked. If the matter of contagion be an animal poison, as most writers assert, and if it live and thrive on animal matter, and animal matter alone, it would be more disposed to attack those who fare sumptuously every day, and whose veins are full of turtle and venison. This is the medium in which an animal poison would find its best and most congenial food, and in which it would delight to dwell: not in the poor, watery, non-animalised blood of the destitute poor. And yet, we have not heard of relapsing fever having entered the Mansion House, Guildhall, or the Exchange; although the distance between the East-end and the City is so short. We may therefore conclude, that this fever is not contagious, for the virus of contagion—if there be such an entity—instead of fixing itself on the half-famished body of Lazarus, lying at the gate, would pass direct into the rich man's abode, where alone it could find its appropriate pabulum.

Equally inexplicable is the revival, in the present day, in England, of the old idea, that the epidemic cholera is contagious: although this question was settled—and satisfactorily settled—50 years ago by the unanimous voice of

* Twelfth Report of the Medical Officer of the Privy Council, 1870, p. 8.

the profession in India—men who had an opportunity of observing the disease in its severest form and on the most extended scale. But the discussion of this question will require a separate consideration—it being more important than any other in the present day.

CHAPTER II.

MODERN THEORIES, ANALYSIS OF.

Having already considered the germ theory—which may be designated the modern theory of contagion—we may pass on to that which sprung up subsequently, and in which the doctrine of contagion is altogether ignored. Whether it was, that this doctrine appeared to be unsatisfactory, or whether it arose from the fact, that no special or dominant epidemic had been observed in England for a century and a-half—from the cessation of the plague in 1665—is immaterial: the advocates of this new theory laid another and a different foundation for their superstructure. The first notice we have of this theory, is contained in Dr. Southwood Smith's Work on Fever, published in 1820. Referring to the origin of fever, he observes:—"The immediate, or the exciting, cause of fever is a poison formed by the corruption, or the decomposition, of organic matter." Drs. Arnott and Kay, at a later period, express themselves in nearly similar terms. The former observes:—"In many situations on earth, where there is going on the putrefaction or decomposition of animal and vegetable substances, and, often, in proportion to the amount of this, there arises into the air an exhalation now called Malaria, which produces the state now called Fever."* Had these writers stopped here, we might have concluded that they were referring to that

* Report on the Sanitary Condition of the Labouring Population in Scotland.

which is the general and recognised theory, in order to account for the production of endemic fever, both in temperate and tropical climates. But the further development of their opinions, and of those who followed in the same track, soon showed, that there were some very important differences between the old theory and the modern one. In the first place, endemic diseases alone were previously supposed to be the product of malaria, but epidemics are also referred to the same cause by the last-named theorists. "The same conditions," as we are informed by Mr. Grainger, "which either favour or control the spread of fever, promote or oppose the ravages of cholera." And again:—"With respect to the more specific, or eruptive fevers, as scarlatina and measles, they only flourish, as to the rule, amidst the filthy and neglected dwellings of the poor, where they acquire a malignity which gives them almost a new character."*

In the next place, it was before concluded, that malaria was generated in the soil; and that it only existed in certain localities, such as marshes, and low alluvial tracts. But these theorists inferred, that malaria, or the agent productive of epidemic and endemic diseases, was generated on the surface, and in all situations—in towns as well as in marshy and alluvial districts. These opinions and conclusions were subsequently adopted by Mr. Simon, and advocated with his usual warmth and energy, in those memorable Reports addressed by this gentleman to the Common Council, during the time he held the office of City Officer of Health. That the emanations arising from the decomposition and putrefaction of organic matter, is not a cause of any ordinary disease, either endemic or epidemic, it has been my object to show in a previous

* Cholera Report by the Board of Health. 1848-9.

publication.* Although entirely opposed to the dominant theory of the day, as, also, to that which had existed previously, in order to account for the production of malaria, my arguments and conclusions have never yet been impugned or refuted by any competent authority. More than this, these opinions have been since adopted and advocated by some of my professional brethren; those whose authority is greater than my own, and whose conclusions, consequently, will have more weight with others.

In the address delivered by Dr. Christison, as President of the Social Science Congress, in 1863, he thus expressed himself:—"In the Metropolis, indeed, it is at present a prevailing opinion, much in favour, also, as I understand in this Association, that the sources of foul air are likewise the sources of typhus. But such a rule will not apply to Edinburgh. Foul air will not account for the origin of the scattered (sporadic) cases of typhus in non-epidemic times. As little will air, merely foul, account for either the rise or the fall of epidemics. Every physician of experience, in this city, has repeatedly seen, in a family, a solitary case of well-marked typhus, which no skill could trace to foul air in any shape, or from any source."† At the Meeting of the British Association, the following year, 1864, there was a special debate on the physiological aspect of the sewage question. At this Meeting still stronger opinions were expressed. Professor Hughes Bennett, who opened the debate, placed before the members the following, among other, propositions:—1. *That atmospheric air, strongly impregnated with odours, of various kinds, is not necessarily injurious to health.* 2. *That atmospheric air, without smell, is often most dangerous.* 3. *That smells, as smells, are neither*

* Causation and Prevention of Disease. 1859.

† "Lancet," Oct. 24, 1863.

*injurious to health, nor are they a nuisance to those who live amongst them.**

As the examples brought forward by Dr. Bennett, in proof of these propositions, were not particularly striking or happy, and as abundant evidence has been adduced, in the work before alluded to, it is unnecessary to reproduce them on the present occasion. It is only necessary to remark, that several members, then present, concurred in the opinions enunciated by Dr. Bennett. Among others, Dr. Livingstone, with his great African experience, although entering a protest against being thought "an advocate for stinks," stated:—"He believed it was most important to know, that stinks were not the cause of fever in Africa. That was the conclusion to which his and Dr. Kirk's experience led them." Although so much proof has been already advanced, it may not be superfluous to add one more example, the facts connected with which had not been made known when my last work was published. As all persons, who have perused the Reports of Mr. Simon, must be aware, it is not the emanations arising from decomposing matter on the surface of the earth, that are alone productive of disease: those which arise from the surface of the water are also considered to be equally injurious. "From city and suburb," remarks this writer, "from an area of 100 square miles, covered by a quarter of a million of houses, with their unprecedented throng of metropolitan life, there pours into that single channel every conceivable excrement, outscouring, garbage, and refuse, from man and beast, street and slum, shamble and factory, market and hospital." And it is then added:—"From the polluted bosom of the river steam up, incessantly, though unseen, the vapours of a retributive poison: densest and most destructive, no

* "Medical Times and Gazette," Oct. 4, 1864, p. 366.

doubt, along the sodden banks and stinking sewers of lowest level; but spreading over miles of land—sometimes rolled high by wind, sometimes blended low with mist, and baneful even to their margin, that curls over distant fields." * Such is the theory, or, rather, the poetry, of the subject: now for the prose.

In 1858 and 59, we had, as will be remembered, what was somewhat inaptly termed "The Thames Pestilence," for the child was actually christened before it was born; while, as it turned out, the infant never saw the light. It was an abortion, or, rather, a false conception. In consequence of the increased temperature—from 9 to 14 degrees above the average—and the subsequent drought, the level of the water in the Thames became much lower than usual; and a large surface of mud was exposed. The organic matter constantly present in this mud underwent decomposition, at a more rapid rate and to a much greater extent than usual—aided, as it was, by a larger influx of salt water. Although a large quantity of salt prevents putrefaction, a small quantity favours it. Hence marshes situated near the coast, and which are occasionally overflowed by the sea, are invariably more offensive than others. Dr. Letheby, in his Report, dated June, 1859, remarks:—"The amount of ammoniacal vapour and fœtid gases, evolved from the water, has been enormous—as much as fifteen cubic inches of the latter, and a grain and a half of the former, have been obtained from a gallon of the water, by simply heating it; and although the fœtid gases do not contain a trace of sulphuretted hydrogen, they contain a *putrid organic vapour* which is, in the highest degree, offensive." It cannot be a matter of surprise, then, that the alarm in the public mind became extreme, and that the greatest evils were prognosticated!

* Fifth Report, pp. 23 and 24, 1853.

"Can any one," exclaimed Mr. Austin, "read the statement in this day's papers, without feeling a kind of creeping horror at the steady, but *sure and certain death*, which is encircling thousands in the metropolis, and drawing nearer and nearer to its victims daily!"* Some persons, it is asserted, were so alarmed that they actually refused to cross the river, or pass over the bridges, and were compelled, in consequence, to change their residence for a time. And what was the actual result? Did the plague return, or did the cholera make its appearance? Did fever, bowel complaints, &c., prevail to a greater extent than usual; and was the rate of mortality in London raised to the highest degree? Assuredly not: none of these results were observed. On the contrary, the sickness and the deaths among those classes, the most exposed to these emanations, were actually less than the average. This will be evident by a reference to the following communications, addressed to the late Dr. Miller, the then Professor of Chemistry at King's College:—

"Dear Sir,—In reply to your inquiries as to the influence of the emanations of the Thames at Greenwich upon the health of the patients in the Dreadnought Hospital Ship, I have to state that, so far as I know from my own observations, or can learn from the report of the resident medical officers, no injurious effects have been perceived to arise from that source. The nauseous smell has, of course, been very unpleasant to all the residents in the ship, but no ill effect has arisen to either the sick or healthy.

"I am, &c.,

(Signed) "George Busk."

"My dear Sir,—Our experience of the effects of the Thames air on the patients on board of the Dreadnought this year quite confirms the conclusions published by me last year. I may add that Professor

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* "Building News," July 29th, 1859.

Busk, Mr. Tudor, and Dr. Stephen Ward, my colleagues, are of the same opinion, namely, that no injurious influence, certainly no evidence of epidemic disease, can be traced to this source. I hope the time is coming when the evidence of direct medical observation, confirmed as it now is by experience, will weigh with the public in allaying the apprehensions that have been excited, that the Thames is the cause of the diarrhœa in London, and is likely to induce a pestilence.

"I am, &c.,

(Signed) "ROBERT BARNES." *

Dr. M'William, also, in his Report, for 1858, remarks: "The state of the river had, at this time, excited considerable anxiety; and it was seriously apprehended that the metropolis was on the verge of a pestilential outbreak. My duties, as Medical Inspector, led me to watch closely for any unfavourable influence, which the condition of the river might exert upon the health of the water-guard and the waterside officers under my charge. The former, in number of upwards of 800, may be said to live on the river, or in the docks, in ships, or in barges and open boats; and the latter, numbering upwards of 500, are employed during the day in the docks, or at the various wharves of the bonded warehouses on each bank of the river." And he then adds: "It is nowhere sustained by evidence, that the stench from the river, or docks, however noisome, was in any way productive of disease. As respects bowel complaints, in which I include diarrhœa, choleraic diarrhœa, dysentery, &c., the types of those forms of disease which, in this country, noxious exhalations are commonly supposed to originate, we find the additions, during the four hot months of the past year, from this class of complaints, 26.3 (per cent.) *below the average* of the corresponding period of the three previous

* Report to the Metropolitan Board of Works, 30th June, 1859.

years, and 73 less than those of 1857." * The following Table, formed from the statistical data in Dr. M'William's Report, will illustrate these facts more clearly.

TABLE 4.—Rate of Sickness with the Officers of H.M.'s Customs, for the Months of June, July, August, and September, in the following years.

YEARS.	Bowel Complaints.	
	Total Cases.	Daily Rate per cent.
1855-6 and 7 ..... (Average rate.)	402	85.3
1858	319	59
Difference	83	26.3
1857	.....	132
1858	.....	59
Difference	.....	73

Although every effort was made to arrest the putrefaction, these means were entirely unsuccessful: the emanations, in 1859, being nearly as great as in the preceding year. Still, the same immunity was observed: the rate of mortality of all the diseases referred to this source, being below the average. The result is not singular. We might have arrived at this conclusion, *à priori*, and from the simple fact, that the elements productive of disease are *inodorous*, not odorous. Thus, the town of Point au Petre, at Guadaloupe, is situated among the most offensive marshes in the world, yet it is, as we are informed by Dr. Ferguson, far from being unhealthy. "The same," he

* On the Health of the Water Guard, and Waterside Officers of Her Majesty's Customs. 1859.

adds, "may be said of Port Louis—the first outpost—where the waters are so stagnant and putrid, that it is even more offensive than Point au Petre. But at Fort Fleur d'Epée—the farthest outpost at the extremity of the marshes—where *no smell exists*, there cannot be conceived a more deadly quarter; and all white troops, during the occupation of that island by the English, considered their being sent there equivalent to a sentence of death." Take another example. The town at Green Island, Jamaica, is situated close to an extensive swamp, and it was prophesied that, if the epidemic cholera broke out there, the inhabitants would die off like rotten sheep. Some cases having occurred, I was requested by the authorities to proceed there, and took up my abode in a house on the margin of the marsh, the stench from which was so powerful, that I was obliged to close the window of my bedroom, with the thermometer between 80° and 90°. To the surprise of everybody, there were only thirty-three attacks and five deaths, the latter occurring previously to my arrival. Of the remaining twenty-eight attended by me, *all* recovered, so that I obtained the most favourable result, as regards the treatment of the disease in Jamaica, in this pestiferous spot. But, what is still more singular, there was no fever in the town of Green Island, although it was prevailing to a considerable extent, and epidemically, on the neighbouring hilly districts, and in the healthy parish of Hanover. It was also in these very districts, on dry, calcareous hills, where the decomposition of organic matter went on to the smallest possible extent, that the ravages of the epidemic cholera were the greatest —fifty, sixty, and seventy per cent. of the population in some of the small villages and hamlets having been cut off.

There is another property of malaria, a knowledge of which would also have enabled us to predict the result, in

the preceding instances. This is, that malaria is *never* given off from the surface of water, under any condition whatever.* In fact, the most pestiferous spot in the world may be rendered perfectly innocuous, by covering the surface with water, be it only an inch deep—a practice that was adopted in ancient times, by the successors of Hippocrates, if not by Hippocrates himself, and which has been repeated, with success, on several occasions since.

With such an amount of evidence, in proof of the conclusions previously drawn by me, it would have been unnecessary to have adduced any fresh arguments, or facts, on the present occasion, but for two circumstances. In the first place, the object of that work was the elucidation of the cause of endemics, rather than epidemics; and, in the next, a particular disease, which, in consequence of its prevalence and other circumstances, has unfortunately acquired much notoriety of late, is now almost universally referred to the same cause. It is to the emanations arising from the decomposition of a particular kind of matter, that the origin of typhoid fever is now ascribed, by English writers. "The typhoid fever, or typhia, as it may be termed, to distinguish it from typhus, Dr. Southwood Smith, Dr. Murchison and others have shown is a kind of *night soil fever*."† And Dr. Murchison remarks: "So far as we know, it is necessary, for the production of the poison of enteric fever, that the matter undergoing fermentation be either in a confined space, as in a drain or sewer, or that it be in a state of stagnation."‡

That such are the opinions generally entertained in the profession, has been, unfortunately, demonstrated lately by

* See *Law* 3, Causation and Prevention of Disease, p. 78.

† Registrar-General's Report, for 1859. (The 22nd.)

‡ On the Continued Fevers of Great Britain.

the lamented illness of his Royal Highness, the Prince of Wales—the cause of the attack having been ascribed to the emanations given out from the drains of Londesborough Lodge. It has been stated by the Medical Commissioners, sent down by some of the medical journals to investigate the matter, that the gases, which naturally exist in drains and sewers, were driven back by the flow of the tide; and, for the want of ventilators, escaped into the Lodge, and even into the Prince's bed-room. That such an effect does frequently occur is undoubted; it is, in fact, an inevitable result, when sewers discharge their contents into the sea. But, then, this result will be common to all houses similarly situated; and, as we may presume, to the majority of houses in Scarborough. If no provision were made for the escape of the pent-up gases at Londesborough Lodge, it is not probable that there would be in many other houses in the town. As such, if the attack of his Royal Highness is to be ascribed entirely to the escape of sewer gases into Londesborough Lodge, how is it, we may ask, that there were no other cases of typhoid fever in Scarborough, either then, previously, or subsequently? If such an effect could be produced by such a cause, Scarborough and other towns, on the sea-coast, would never be free from typhoid fever.

It has been assumed, without, as it appears to me, any actual proof, that gaseous matter did escape into the closet adjoining the Prince's bed-room. As these gases invariably betray their presence by their effect on the olfactory organs, we can hardly imagine that his Royal Highness would have been placed in that room, had there been any effluvia perceptible to others. Allowing, for the sake of argument, that there was such an escape; and that it only occurred at the very time of the Prince's visit; how then are we to account for the illness of another individual

—the butler, who also had an attack of typhoid fever? He did not sleep in the same chamber, nor did he even sleep at Londesborough Lodge, as appears from the following account. Mr. Frome, butler to Mr. Sykes, M.P., went to Londesborough Lodge to take the place of the Earl's butler, who was ill. He remained there from Tuesday until the following Sunday, sleeping all the time at the Royal Hotel. It was the 5th of November when Frome returned home to Brantingham Thorpe: on the 19th he felt indisposed, but continued his ordinary pursuits until the 28th, when he took to his bed. After this, but not before, the characteristic symptoms of typhoid fever made their appearance. There was no fever at Brantingham Thorpe, nor had there been any cases in the Royal Hotel. Whence, then, did this patient contract the fever? It will, of course, be said, that the attack arose from the emanations given out in other parts of the house, from the same drains. Granted; but, then, we may inquire, in return, how it was that these emanations did not produce any ill result on the remaining twenty-seven persons, who then inhabited Londesborough Lodge, more especially as they all slept in the house? Was the butler the only susceptible person among the twenty-eight; or, did the gases find an entrance into the wine-cellar, having a predilection, like a great many people, for that particular locality? We will leave the advocates of this theory to answer these questions. Without waiting for their reply, we may be allowed to infer, that the butler did not contract the disease at Londesborough Lodge, but at Brantingham Thorpe. The interval—between two and three weeks—was too long for the incubation of the disease, on the supposition that the germs had been imbibed at Scarborough. If so, we shall have proof, that there was some general cause in operation productive of this disease, in

that part of England, irrespective of locality, house, or drains. Leaving this part of the subject—the cause of fever at Londesborough Lodge, respecting which only negative proofs can be adduced—it will be more profitable and instructive if we endeavour to ascertain, whether the emanations arising from fecal matter do, or can, produce fever, or any similar disease. To ascertain this, two methods may be adopted. One is, to examine the chemical properties of the gaseous products of this matter: and the other, to inquire what the actual result is, when the human frame is exposed to these emanations to the greatest extent, and in their greatest intensity. We will discuss the latter proposition first.

At Florence, in consequence of the poverty of the soil in the surrounding districts, the night-soil becomes a valuable manure, and it is, therefore, carefully preserved in cesspools, or reservoirs, *beneath the houses*. But, as the water-closets are not provided with traps, and as these conveniences are to be found on all the floors, the effluvium escapes, not only into the houses, but into nearly all the rooms—even bed-rooms. In fact, I was obliged to change my apartment while residing there from this very cause.* But however disagreeable the effluvia were

* The late Dr. James Johnson, speaking of this town, says:—"The city of Florence, then, like too many of its neighbours, is a city of filth, where *not a single wave of air* is unimpregnated with the most disgusting, if not pestiferous effluvia that imagination can conceive." And he asks:—"Why is this infernal box of Pandora, compared with which assafœtida is incense, gradually collected in the cellar, and annually disgorged by carts, instead of being daily carried subterraneanly into the Arno?"† The answer is, that although sewers exist in every street, the Florentines are too wise to allow such valuable matter to be cast into the sea: although they would be still wiser, not to allow the emanations from the cesspools to enter the houses, which could be easily effected.

† Change of air.

to the olfactory organs, and although experiencing certain anomalous symptoms, referrible to the head, I had no attack of fever; while I was told that strangers never suffer from this disease in Florence. And yet neither they nor I could probably have traversed the Campagna of Rome, in the hot season, without experiencing an attack after, although such emanations do not exist there; the night-soil of Rome, like that of London, being discharged into the river, instead of being applied to agricultural purposes. But, what is still more remarkable, Florence is entirely free from the endemic of Italy—intermittent fever; while it is not only the healthiest city in Italy, but as much so, perhaps, as any in Europe. On my departure from Florence, there was only one case of fever in the hospital: on my arrival in Rome, there were between 200 and 300 in the hospital appropriated to such patients—*el Spirito Santo.* It has also been very lightly visited by epidemic cholera.* The chief complaints in this town are those referrible to atmospherical vicissitudes, dependent on its position, being somewhat elevated, and surrounded by mountains: so that, while suffering an unusual amount of heat in the summer, it is equally cold in the winter.

Precisely similar results have followed the adoption of the same system at Milan—the excreta of the inhabitants being retained in cesspools, and then distributed on the lands surrounding the city. In the Report of the Deputation sent by Government—consisting of Dr. Southwood Smith, and Messrs. Way and Austin—it is stated:

* During the severe visitation of cholera in Italy, in 1867, there were only *two cases* in Florence, although not one of the forty-nine provinces of Italy had been spared. As many as 63,376 cases and 32,074 deaths had occurred from January to July, while the epidemic was still raging in the south.†

† Correspondent of the "Daily News."

"We could obtain *no evidence* of the production of disease, by the existing mode of dealing with the solid portion of the excrementitious matter of the city.....that it is a source of disease, *is denied* on all hands." Dr. Vaga, physician of the "Ospitale Maggiore," in reply to the questions of the Deputation, as to the effect of the distribution of the matter on the land, stated, that "he has never observed fever, or any other special disease to be produced by it." And he added: "Intermittent fever is not *endemic* in the city of Milan."* Dr. Bianchi, physician of the suburb, called Borgodegli Ortolani—the district in which the people live, who collect the night soil, before being distributed to the fields—gave confirmative evidence. He denied that there were any peculiar maladies in this district: or that epidemic diseases—such as typhus, *typhoid*, scarlet, or miliary fever, small-pox, and cholera,—were in any way prevalent there. On the contrary, he stated, that the general mortality of the district is one per cent. less than in Milan: and he then added: "Decidedly, the cesspool matter is unpleasant to the nose, but quite *uninjurious* for the health." Nor does the city of Milan itself suffer by these emanations, the rate of mortality, in 1859, being 1 in 33—30 per 1,000: while in Mantua, Pavia, Lodi, and other towns, where the same matter is not collected, or employed for agricultural purposes, the rate was 1 in 28, or 36 per 1,000. In Rome, it is 1 in 25—40 per 1,000, although sewers exist in this city; while, in consequence of the greater number of the houses being on a higher level than the river, the drainage is so good that the emanations complained of at Florence do not exist, or, at least, not to any extent. Independently of the above, the supply of water in Rome is so abundant that the accumulation of

* Report of the Commission on the Distribution of the Sewage of Towns, 1858, page 46.

matter in the drains is scarcely possible. There is, perhaps, no city in the world so plentifully supplied with water as Rome: it is, in fact, a city of fountains, for they are to be met with in every street, while they play continuously day and night. Notwithstanding these advantages, and the absence of excrementitious matter, Rome is now, as it always has been, a hot-bed of fever, and especially of intermittent fever.*

It was unnecessary to go thus far in search of evidence: the same results, and the same facts, have been observed at home. In London, the contents of the water-closets were not allowed to pass into the drains and sewers, until the commencement of the present century. Up to this time, cesspools were alone employed. There are thus two epochs—and it is necessary to bear this in mind always—in the sanitary history of London—the cesspool epoch and the sewer epoch. As such, it will be instructive to ascertain, what the result has been under each of these two systems. And first, as regards the cesspool epoch.

As all persons, conversant with the history of this country, must be aware, London was little better than a pest-house, until the end of the 17th century,—the plague prevailing to a fearful extent, from time to time. As an account of these plagues, from the earliest period, will be given hereafter, it is only necessary now to remark, that 21,000 persons died in the visitation of 1592-3. In that of 1603 there were 36,561: and 45,433 in 1625. Another visitation occurred in 1636, but the ravages of the disease were not so great this year, 10,400 deaths only having been recorded. The plague did not prevail again, in an epidemic form, until 1665. In this visitation, which was the last in London, the victims, as set

* There was, according to Baronius, an ancient saying: "Rome subdues men by the sword, and kills them with fever."

down in the bills of mortality, were 68,596. But this was much below the actual number. Lord Clarendon states that many, who could compute very well, considered there were, in truth, double that number. No account was taken of those buried in the country, and in private cemeteries: and Tillison states, that the Quakers alone buried 1,000 in one week. Nearly a third of the population, it is supposed, perished during this one visitation. This would be equivalent to 700,000 or 800,000 with the present population of London. In one week, the deaths amounted to 8,297, equivalent to about 70,000 at the present time. It was not only at these periods, when the plague raged, that the mortality was so great, but in the intervals also and from ordinary diseases. This will be best shown by comparing the deaths with the births. Short states that "for the first 40 years, in which the bills of mortality were published, the total number of births and deaths in the City of London were as follows *:—

Died of the plague	116,087
Do. of common diseases . . .	355,326
Total	471,413
Born	332,168
Difference . . .	149,245

The healthiest time," he adds, "the City's records can show, is from 1611 to 1625—14 years—which scarce had any plague; when the former last plagues had swept off multitudes of its *crazy* inhabitants." Yet, in these years, 1,000 more died annually than were born. But, in 20 specified years, between 1592 and 1665, in which the

* A Comparative History of the Increase and Decrease of Mankind in England, p. 4.

plague prevailed, there was a *yearly waste*, according to Short's calculation, of 10,400 people. Although the plague ceased entirely at this period, fevers did not: these continued to prevail, and to a fearful extent, until the middle of the next century. Independently of continued and remittent fever, intermittents were then as common in London as they now are in Rome, or in the plains of Valencia: while they occasionally prevailed epidemically, and then raged like a plague. From the middle of the 18th century, the mortality from fever gradually lessened; while at the end, ague had almost entirely disappeared.

Another disease, the endemic of India, viz., dysentery, was also very prevalent and very fatal. "In the 17th century, the number of deaths under the titles of *bloody flux* and *griping in the guts* appear never to have been less than 1,000; and, some years, to have exceeded 4,000; and for 25 years together, from 1667 to 1692, they every year amounted to above 2,000." * But, from the commencement of the 18th century, this disease gradually subsided: so that the deaths from dysentery, which, in 1700, were upwards of 1,000, only amounted, in 1799, to 13! These facts will be better remarked in the following Table. (See next page.)

We observe the same remarkable results with respect to the mortality from children. Between the years 1728-38, when the ages were first set down in the bills of mortality for London, the burials of children amounted, on the average, to upwards of 10,000. In the next decade, the number was 9,000; in the following 7,800; and between 1790 and 1800 the number was little more than 6,000 annually.† The same writer adds, that the burials

* Heberden, On the Increase and Decrease of Different Diseases, p. 34.

† Heberden, loc. cit., p. 39.

TABLE 5.—Total mortality from fever and bowel complaints at the following decennial periods:—

Years.	Fever.	Dysentery, &c.	Do. Annual Average.
1675 to 1685	26,455	31,255*	3,125
1685 to 1695	34,665	23,464*	2,346
1700 to 1710	30,467	10,038	1,003
1710 to 1720	31,543	6,924	692
1720 to 1730	39,525	6,514	651
1730 to 1740	34,841	3,707	370
1740 to 1750	43,221	1,067	196
1750 to 1760	28,234	1,104	110
1760 to 1770	34,502	750	75
1770 to 1780	25,112	678	67
1780 to 1790	24,047	419	41
1790 to 1800	17,169	195	19

at the commencement of the above period, were two-thirds more than the number of christenings, whereas, at the end, *i.e.*, from 1790-9, the excess was on the side of the christenings. It has also been stated, that the mortality among the children of the Foundling Hospital—under the age of 12 months, and who are all put out to nurse—diminished during the last 30 years of the 18th century, in the proportion of 12 to 7.‡

It is thus apparent, that the diseases of London decreased during the last half of the 18th century, exactly in proportion to the increase of the cesspool matter, and

* The figures, for these years, have been taken from a Table by Major John Graunt, who took some pains to investigate the diseases of that period.† The others have been taken from the ordinary Bills of Mortality.

† Observations on the Bills of Mortality.

‡ Account of the Foundling Hospital. 1799.

the emanations thence arising,—the natural result of the great increase of population at this period. We now arrive, however, at another and a different epoch—that of house drains and water-closets.

As previously remarked, the contents of the water-closets were not discharged into the sewers until the commencement of the present century: before then, the act was a penal one. The change, however, was very gradual, thousands of houses having retained the cesspools, until within the last 20 or 30 years. It was stated by Mr. Rawlinson, C.E., in a paper read before the Society of Arts, in 1862, that the greater portion of the 1,500 miles of sewers, which then existed, had been made since 1824; and he added:—"Probably not less than 100,000 cesspools have been abolished, within the last (or previous) 20 years." The change of system produced no change in the prevalence and fatality of diseases during the first 30 years of the present century: the subsidence in the death rate, already noted, continuing during the whole of this period. This is clearly shown in the annexed Table.

TABLE 6.—Total mortality from fever and bowel complaints at the following decennial periods :—

Years.	Fever.	Dysentery, &c.	Ditto, Annual Average.
1800 to 1810	17,777	232	23
1810 to 1820	10,637	276	27
1820 to 1830	9,884	276*	27

* Of this number, 131 were set down as dysentery, and the remainder—145—as diarrhœa. The first time, in which deaths from diarrhœa are noted, is in 1819, when two deaths from this disease were recorded.

As will have been remarked, there was a slight increase in the fatality from bowel complaints, when compared with the decade, 1790—1800; but the diminution of mortality from fever was as great as during the last half of the previous century. This was the termination of what might have been a sanitary millennium, had it not been suddenly terminated by the advent of a new pestilential epoch. Instead of a diminution, there has been an increase of all diseases, from that time to the present, while a new disease—the epidemic cholera—has made its appearance. This will be evident by a glance at the following Table, in which the mortality from fever and bowel complaints has been given: that from cholera has not been inserted, not only because this disease was unknown previously, but, also, because its prevalence and its fatality are familiar to all.

TABLE 7.—Deaths in London from the undermentioned diseases, at the following epochs:—

Years.	Diarrhœa and Dysentery.	Fever.	Scarlet Ditto.	Total Fever.‡
1840 to 1850*	17,382†	20,335	19,099	39,434
1850 to 1860	26,954†	23,286	24,204	47,490
1860 to 1870	30,403†	26,283	29,644	55,927

* As the new mode of registration commenced in 1838, and the old bills of mortality were discontinued; and as the returns are consequently somewhat irregular at this period, the deaths from 1830-40 have not been inserted.

† The cases of dysentery, for these particular epochs, were, respectively, 1,933, 1,744, and 1,018.

‡ The numbers in the previous tables, included all the different forms of fever: no distinction being made in the old bills of mortality, after the cessation of the plague.

By comparing this table with the previous ones, the change that has taken place will be apparent and striking. It is right to remark, however, that the old bills of mortality do not give the total deaths in London, but the numbers that were buried, according to the rites of the Church of England. Still, for the purpose of comparison with previous periods, they are equally valuable. But allowance must be made when the mortality under the new system of registration is compared with the old: the returns of the Registrar-General including all the deaths, or all the burials, of all the sects. Irrespective of former periods, the great increase in all the diseases belonging to the class termed *zymotic*, during the last thirty or forty years, is sufficiently apparent by the figures contained in the last table alone. It will be still more so, if we compare the first and the last years of that period, as now shown:—

Years.	Diarrhœa.	Fever.	Scarlet Ditto.	Total Fever.
1840	522	1,300	1,954	3,254
1870	3,814	2,018	6,040	8,058

Had we been able to have drawn the same comparison with the year 1830, before the present pestilential epoch commenced, the difference would have been still more striking. Not only diarrhœa, but fever, has invariably followed in the train of cholera. The great increase in the mortality from scarlet fever is very remarkable. In 1841, the ratio was only 32 in every 100,000 inhabitants; in 1863, it had increased to 174, and in 1870 to 200. Not that this great increase in the prevalence of disease is peculiar to London: the same melancholy result has

been observed in all other parts of England and of Europe. The only singularity is, that London should have continued so unhealthy, when there are so many circumstances that ought to make it the most healthy town in England. What these circumstances are have been pointed out by me on a previous occasion. It is only necessary now to add, that there are two, which will probably render London more healthy than before, although not so healthy as during the first thirty years of the present century. One is, the embankment of the Thames, a measure so strongly recommended in my last publication. The other is, the great and rapid increase of the population. It has been remarked, in the previous chapter, that M. Michel has laid it down as "a law," that population decreases the insalubrity of an unhealthy town like Rome.* The truth of this conclusion has been conclusively verified not only in Italy, but in America. In Rome, it is a fact of common notoriety, that the healthiest spots are the most populous, and the open, cleared spaces, the most unhealthy. Many of the villas of the rich are nearly uninhabitable from this cause—as the Villa Borghese, for instance, standing in its own park-like grounds—while it is invariably found that the pulling down of houses, and making a clear, open space, will convert a previously healthy district into an unhealthy one. That these results are to be ascribed to some cause connected with population, and not to any other or accidental circumstance, many facts clearly demonstrate. For instance, several of the monasteries and convents are situated in the more elevated and unhealthy parts of Rome; yet the inmates, although the environs are all but uninhabitable, reside there with comparative impunity. If, however, they

* Récherches Médico-Topographiques sur Rome.

become untenanted from any cause, it is then found that the place is uninhabitable, or dangerous to the servants and others left in charge. The same fact is observed with the villas of the rich, if they become abandoned from any cause by the family. So, again, the decrease of population in a district, without any other change in its physical conditions, is invariably found to render it more unhealthy than before; while, on the other hand, the increase of population *alone*, in the same or other districts, is attended with a proportionate decrease in the prevalence of fever. If, therefore, London should hereafter become more healthy than during the last forty years, we shall know what the probable cause is. At all events, it ought not to be referred to the establishment of house drains. The facts previously adduced preclude such an idea.

Had we confined our observations to the first thirty years of the present century, we might have inferred, that the diminution of disease, then observed, had been caused by the abolition of cesspools, and the substitution of drains and sewers. But the previous, and the subsequent, history of London negative such a conclusion. Nevertheless, this is precisely what is done by certain writers. They draw a fearful sketch of the state of London in the seventeenth century, when the plague prevailed, and then compare it with the actual, or recent, condition of this city; passing over, altogether, the intermediate period, and the diminution of disease that then took place. See, they exclaim, what sanitary reform has effected! although the beneficial change commenced a century before sanitary reform was inaugurated, or even thought of; and although it is precisely since the commencement of this, so-called, sanitary reform, that the prevalence and fatality of all

diseases have increased so greatly.* Independently of London, there are other, and, perhaps, more striking examples of the uselessness of drainage—house drainage—for the prevention of disease. For instance, drains and sewers were made, in accordance with the ideas, and under the immediate direction, of the late Board of Health, at Croydon and Sandgate; the cesspools being abolished, and a fresh supply of *pure* water being laid on. And what was the result? A severe outbreak of fever at Croydon, and of cholera at Sandgate, although neither of these towns had before suffered such a visitation, having been, up to this period, two of the healthiest towns in England. Precisely the same result occurred in Golden-square district, the fearful outbreak of cholera there, in 1854—the most severe yet experienced in this country—having followed the formation of a new sewer.† At Launceston, also, a severe visitation of scarlatina was experienced after the application of the Public Health Act of 1848; while the aggregate of the mortality was greater afterwards than before. There was, also, a visitation of epidemic fever at Shipley, after the formation of house drains, and the completion of other *sanitary improvements!*

It will, of course, be said, that the construction of the sewers was imperfect: that the supply of water was insufficient; and, as such, that the matter discharged into them was retained, giving rise to emanations, the same as from cesspools. That such a result occurs, to a certain

* Mr. Rawlinson, in the paper before referred to, stated, that the annual mortality of 44 in the 1,000 has been reduced to 27, and of 30 to 20 and even 15 per 1,000—a result *entirely due to sanitary arrangements!*

† For the particulars of these outbreaks, as, also, for the cause of their production, which neither the Government Commissioners, nor the Board of Health and their inspectors, could discover or imagine, see "Causation and Prevention of Disease."

extent, is undoubted, but it is absurd and farcical to suppose, that the emanations from the matter contained in a sewer, or drain, can produce disease, when those from a cesspool do not. These emanations must be trifling, infinitesimal, compared with those given out from a cesspool, not emptied for years and years. That the organic matter contained in the sewers is not productive of disease, we have proof from the interesting fact that all diseases gradually subsided in London for the first thirty years after the formation of house-drains—although these drains were very imperfectly made. On the other hand, it has been precisely since the construction of the 1,500 miles of new drains—constructed, too, on the best and most approved principle, according to Mr. Rawlinson —that all diseases have been on the increase. Then, again, it was at the first formation of the drains, in the towns before alluded to, that the outbreaks of fever and of cholera occurred—at that period when there could not possibly have been any accumulation of organic or other matter. At Croydon, the Government Commissioners were unable to ascertain that there had been any emanations experienced from the drains by the inhabitants, excepting those from—a brewery! Besides, if there be pestilential emanations in the drains of one town there will be in those of another, and of all others, constructed on the same principle. And yet, whenever a town, blessed with these modern inventions—these *cloacæ minimæ* —shows a diminished rate of mortality, it is ascribed at once to their formation. Like many other persons, these theorists blow hot and cold with the same breath. Suppose, however, that other theorists, observing these results, were to ascribe the healthiness of these towns, not to the absence, but to the presence, of these emanations! Would they not be as logical, as other theorists are in drawing

the opposite conclusion? It would seem so, if we are to judge from a practice that formerly existed in England. Dr. Guy remarks: "A fact mentioned to me by Mr. Stevens, who did me the favour of accompanying me in my inspection of the scavenger's premises, is too curious to be omitted. He stated, that he perfectly well recollects, thirty years ago, when he was a lad, seeing as many as twelve patients, directed by the faculty of that day, to walk round the shoots, where the night-soil is collected, on his father's premises; and he appealed, for confirmation of this statement, to his brother, who said that he had seen *scores* of patients, industriously *inhaling* this very curious dose of physic."* Without waiting to inquire what the result of this mode of treating diseases was, we have proof, from the facts previously advanced, that the emanations from night-soil cannot be the cause of ordinary diseases. This conclusion will be confirmed by an examination of the gases evolved from this particular matter.

The principal, and the most important gases given off from the matter in question are sulphuretted hydrogen and the hydro-sulphuret of ammonia. The former gas is always present, to a greater or less extent, and more particularly in recent collections; but it is the latter which is principally met with, when the matter has accumulated for a long period. At other times, the gaseous matter, then given off, is a compound, according to Orfila, of ninety-four parts of nitrogen, two parts of oxygen, and four of carbonic acid; but when ammonia is present, the latter gas unites with it to form the sesqui-carbonate of ammonia. There are thus three states in which these receptacles will be found; while the effects that are pro-

* On the Health of Nightmen, Scavengers, and Dustmen. Journal of the Statistical Society. 1848. Vol. II.

duced, by the exhalations given off, must necessarily vary in each.

In the first, we shall have sulphuretted hydrogen as the chief agent, combined with a greater or less amount of atmospheric air, as well as a portion of the other gases named. Although this gas, when inspired in certain quantities, is prejudicial to life, it does not produce any symptoms analagous to fever, or other ordinary disease. On the contrary, taken into the system, in lesser quantity, both by the lungs and by the stomach, as is the case with visitors to the sulphuretted baths of Aix and other places, a beneficial result is produced, in a great many diseases. More than this, one practitioner (Mr. Atkinson) administered this very agent as a remedy in cholera; and, as he states, with more success than with the ordinary remedies. Sulphur, also, has been administered with a like good result, and particularly in the first stages of the disease, although a considerable part of this substance is converted into sulphuretted hydrogen in its passage through the body. And yet Mr. Grainger stated, that the presence of this gas in the air is the cause of ordinary diarrhœa *among the poor:* as if this class of persons were not subject to any other cause productive of derangement of the bowels—such as improper diet and poor living—and as if the rich never laboured under the same complaint from the opposite cause—over-eating and excess in drinking. Under other circumstances, instead of the preceding, the hydrosulphuret of ammonia will be the principal substance extricated. This gas, which is not a product of ordinary putrefaction, is, like the former, deleterious, but it produces a more powerful effect on the human frame. If, however, we turn to medical records, and study the symptoms produced with individuals who have become asphyxiated, by the inhalation of this gas after descending into

cesspools, and who have recovered afterwards; we shall find that no symptoms similar to those of cholera or fever are produced. The principal effect would appear to be on the nervous system, as indicated by the muscular contractions and convulsions that supervene. When these have disappeared, the recovery of the patient is usually very rapid, so much so that individuals, who were in a state of asphyxia for some hours, have sometimes left the Hôtel Dieu at Paris *the next day*.* The only symptoms experienced afterwards are those of great prostration. Had it been possible to produce fever, cholera, or any other ordinary disease by such a cause, some symptoms would assuredly have manifested themselves in these cases; particularly as such patients had imbibed the largest possible quantity of poison short of producing death itself. We may therefore conclude, that this gaseous compound, like sulphuretted hydrogen, is neither an exciting nor a predisposing cause of ordinary diseases.

With respect to the gaseous mixture, which is found in the last condition described, as it contains no noxious substance, it can only act injuriously, when in a state of concentration, by preventing the ingress of oxygen into the lungs: it cannot exert any influence in the production of the effects now under consideration.

We can thus understand, why it is, that persons constantly exposed to these emanations do not suffer more than others from ordinary diseases. Mr. Thackrah states, "the nightmen of London are generally healthy, notwithstanding their disgusting occupation. Of eighteen examined by my assistant, only two had even slight disorder. *Appetite*, they declare, *is increased by the effluvium*. Their only complaint is defect of food, from lowness of

* Traité de Toxicologie, p. 212.

wages."* Sir Anthony Carlisle also states, that "out of between forty and fifty men, employed in the sewers, only three had fever."† According to the statistics of Dr. Guy, the proportion of attacks of fever, among the classes examined by him, was as follows:—

Nightmen	....	....	....	3 per cent.
Scavengers	....	...	...	8 ,,
Brickmakers	....	....	....	21½ ,,
Bricklayers' labourers	....		....	35½ ,,

Mr. Rose, night-soil contractor, in answer to some observations that were made on the subject, replied: "It is worthy of remark, that the occupation of night-soil men is so healthy, that about fifty, who go out brickmaking or to harvest work in summer, are so much reduced in health, that it takes some months, on their return, to recover their usual looks." Nor does the occupation of nightmen appear to shorten life. The greatest age of any man at work, among the ninety-eight scavengers examined by Dr. Guy, was sixty-six; the oldest brickmaker sixty-eight, and the oldest bricklayers' labourer, sixty-four. "Men were reported to me by name," adds Dr. Guy, "who had passed their seventieth year, and were still at work in the laystalls (where the night-soil is collected); and one Richard Tyrrell, of the parish of Shadwell, was stated to have reached the age of ninety-seven, after having worked all his lifetime at the trade. Another man, familiarly known as Old Wood, was said to have completed a century."‡ Nor does employment in the sewers appear to be productive of greater injury. According to a return, made by Mr. Bazalgette to the Metropolitan Board of Works, December, 1871, it appears that, among 234 persons employed in, or about the sewers, there have only been, during the previous fif-

* The Effects of Arts, Trades, &c., on Health and Longevity.

† Practical Observations on the Preservation of Health. ‡ Loc. cit.

teen years, two cases of typhoid fever, four cases of typhus fever (and of these, one case originated at home), and eight cases of intermittent fever. The last attacks occurred among the men employed at Crossness; the disease being derived, as we may presume, not from the sewers but from the surrounding marshes. Of the above number, five are inspectors and ten are in the engineers' office, and are only occasionally employed in the sewers; but the remainder are all employed in and about the sewers, in cleansing, flushing, etc.

With such an amount of evidence before us, we are bound to conclude, that enteric, or typhoid, fever, is not, as has been generally inferred, "a kind of night-soil fever." This is the conclusion at which Dr. Christison has also arrived. Referring to his experience in Edinburgh, he remarks: "Our street drains in the old town have been much improved, during the very period that enteric fevers have been increasing. ..... Further, this fever does not, by any means, generally break out where the streets are ill-drained, water-closets wanting, and habits filthy. In countless places of that sort, in Edinburgh, it is unknown."* Dr. Bennett, also, at the Meeting before mentioned, remarked, "That typhoid fever, which was previously unknown, had recently appeared in Edinburgh, and, strange to say, it appeared *to have followed* the improvements."† This is precisely the result that has been observed in London, and in many other places. On the other hand, the retention of cesspools has not been found to be productive of any injury. Mr. Blair stated, at the same Meeting, that in Manchester, where the old system of cesspools had been adhered to, they were as free from fever as in any other town of its size in England. If,

* "Lancet," Oct. 24, 1863.

† "Medical Times and Gazette," Oct. 4, 1864.

therefore, typhoid fever arises, where drains exist, its production ought not to be referred to the emanations arising from fecal matter, but to some other and different, or unknown, cause. With this conclusion, we may now pass on to another part of the subject.

It is not the emanations from putrescent matter, that are alone injurious; the presence of such matter in the water, is considered to be equally potent in the production of disease. It is stated, in the Report of the Commissioners appointed to inquire into the sanitary state of the army in India: "We next come to consider the probable effect of this condition of the water-supply, upon the health of the troops; and, in so doing, we shall have to be guided, to a large extent, by the experience obtained in sanitary inquiries *at home*." Why the Commissioners depended so much on this home experience, it would puzzle another person to divine: unless they thought, that medical knowledge is restricted to the narrow circle comprised within the sound of Bow Bells. We might have inferred, considering the subject of their inquiry, that the evidence from India would have been principally, if not solely, depended on; not only because medical practitioners there witness disease in its severest form, and to the greatest extent; but also because they, and they alone, have, or can have, any experience in the diseases of such a climate. More than this, it is precisely there, that the effect of impure water on the health of man can be best ascertained, as will be more particularly shown hereafter. Still, as we have been referred to this source, we will endeavour to ascertain, what this home experience amounts to, and what it is really worth!

Dr. Letheby, in his examination before the Commission, stated, that impure water both produces and predisposes to, disease. "So that," it was asked, "next to impure

air, you would rank it as one of the most powerful causes of disease?" "I am disposed to think," was the reply, "it is before impure air, as being one of the most powerful causes of disease." . . . . (Question 4,567.) "This evidence," adds the Reporter, "gives the experience up to the present time. The diseases to which bad water peculiarly predisposes are mainly epidemic diseases, affecting the intestinal canal, such as cholera, diarrhœa, and dysentery. To the use of impure water have also been traced fevers of the intermittent type. The evidence on these points is *conclusive*, and extends not only to well water but also to impure river water. During the cholera epidemic of 1848-9, several instances occurred in which nearly every person using water from particular wells was affected with diarrhœa or cholera, and of the cholera cases nearly all died."

The principal instance referred to here, is that of an outbreak of cholera in some houses at Clapham, the water of which had become contaminated by the overflow of a sewer, after a thunderstorm. Had these been the only attacks, we might reasonably infer, that the cause was the contamination of the water. When, however, we remember that the cholera was prevailing epidemically in England at the time, and that 10,000 or 12,000 persons died in London, with whom no such agency could have been brought into play, the probability is, that the above accident was merely a coincidence, and *not* a cause. We are confirmed in this conclusion by several facts. Mr. Atkinson, in a letter to the Editor of the "Lancet," states: "That the deaths at Albion-terrace did not arise from any contamination of the water, for the following reasons." "1st. Because in some houses, where deaths occurred, the water remained unaffected." "2ndly. Because in some houses, where the water was used, without intermission, the inhabitants

escaped." "3rdly. Because, when the water became turbid, in many cases it was not used, but procured (*i.e.*, another supply) from other sources." "4thly. Because the attacks did not coincide with the discoloration of the water, the thunderstorm which flooded the sewer having occurred a week previously; and the water, at least in some instances, had returned to its original purity."* Having decided this case, we may now pass on to the next charge against the water.

"During the epidemic of 1854, of 73 persons who died of cholera in *a certain street* in the metropolis (how mysterious in the case of a criminal indictment; why not say Broad-street, Golden-square?) 61 had drunk water from a pump polluted by sewage." As thus stated, one would imagine that no other attacks, and no other deaths, had occurred, excepting these 73; and that the visitation was entirely local. Instead of this, the cholera was not only epidemic at the moment in London, but it spread, at the same time, over the whole of that and the adjoining sub-district. There being a pump in the street, it was natural that this water should be used, by some of the inhabitants, for drinking, in preference to that supplied by the water companies. The latter, as may be supposed, was used exclusively for culinary purposes, and, in the majority of the houses, for drinking also. In consequence of the severity of the outbreak—the most severe that has been experienced in England—a Committee was appointed by the Vestry, to inquire into the causes of the outbreak: and their attention was particularly directed to the state of the water, in this well, by the late Dr. Snow. By their labours, a great deal of valuable information has been obtained; and this was made public by a special Report, published in the following year. To this was added a

* "Lancet," Sept. 22nd, 1849.

Report from Dr. Snow; and another from the Rev. H. Whitehead, the indefatigable and zealous Curate of St. Luke's district Church.

With respect to the public water supply, the Committee state:—"The western half of the 'cholera area' in this parish is supplied with water by the Grand Junction Company; whilst the eastern part, corresponding very nearly with the Berwick-street registration sub-district, is supplied by the New River Company. It is obvious therefore, that the public water supply from these two Companies, either had some equal and simultaneous share in favouring the cholera outbreak, which seems very unlikely, when we consider the suddenness and limited extent of that outbreak, or, what is more certain, had no share at all." * For these reasons, as also from the fact that Dr. Snow ascribed the outbreak, in this district, principally to the contamination of the well-water, Mr. Whitehead undertook to collect the facts respecting its use. The following is the result of his inquiries and labours.

Out of a population of 896 in Broad-street—to which street this gentleman's inquiries were restricted—particulars were ascertained respecting 497; but none respecting the other 399, the majority of whom had removed to another locality. Of 137 persons who drank the well-water, 80 were attacked, and 57 escaped: while, of those attacked, 45 died and 35 recovered. On the other hand, of 309 persons who did not take the well-water, 30 were attacked and 299 had no attack: but then of the 30 attacked, 23 died, and only 7 recovered. In addition to the above, there were 51 who gave uncertain and contradictory accounts: and of these, 30 were attacked, 23

* Report of the Cholera Outbreak in the Parish of St. James's, Westminster. 1854. P. 69.

died, 7 recovered, and 21 had no attack. As more convenient for illustration, these results, together with the per-centage under each head, have been arranged in a tabular form as under.

TABLE 8.—Result of the employment of different waters, among 497 of 896 persons resident in Broad-street:—

Residents.	Who drank the well-water.		Who did not drink it.		Uncertain and contradictory.	
		Per cent.		Per cent.		Per cent.
Total number ...	137	—	309	—	51	—
Attacked	80	58.49	30	9.70	30	58.82
Died	45	56.22*	23	77.66*	22	73.33*
Recovered.....	35	43.71*	7	22.33*	8	26.66*
Had no attack ...	57	41.60	279	90.29	21	41.17

It is thus apparent from these statistics, if correct, that there was a much larger proportion attacked of those that drank the well-water, than of those that did not; in the ratio of 58.49 to 6.47 per cent. But, then, we have no account of the 399 that left the neighbourhood after the outbreak. Supposing that half of these were drinkers of the well-water, the proportion of attacks between the two classes might have been nearly equal, for none of these persons were attacked, at least, in Broad-street. If any of them died elsewhere, their deaths could not be ascribed to the drinking of the well-water. In this way, the calculations and deductions drawn from the first class might have been entirely set aside and reversed. That these

* This is the per-centage of deaths and recoveries to attacks, not to population.

statistics, although true as far as the information went, were faulty in some respects, and valueless, as far as the deductions went, would appear probable from the result obtained with the third class—those respecting whom Mr. Whitehead could obtain no information, that they ever drank the well-water. Now it is a remarkable circumstance, that the ratio of attacks, with this class, is precisely the same as with those that drank the well-water. How, then, are we to explain this anomaly? We shall be obliged to infer, either that the third class drank the well-water, or else that this water exerted no influence in the production or spread of the disease. That the latter inference is the correct one, a variety of facts tend to show.

In the first place, the disease, as previously stated, extended over that and the adjoining sub-districts; there having been, altogether, 700 deaths in the three sub-districts into which the parish of St. James's is divided. Of these, 90 occurred in Broad-street; and the half, or 45, drank, either regularly or occasionally, the well-water. Deducting these, and the 22 doubtful cases, there will remain 633; the majority of whom would not, or could not, have used this water on account of the distance. Allowing that one-third of these drank the well-water—and that is a very broad margin—there would then be 211 deaths among those that drank the well-water, and 422 with those that did not—exactly double. In the next place, if this water were so contaminated as to produce attacks of cholera, and render these attacks more malignant, the ratio of deaths to attacks ought to have been greater with those who drank the water, than with those who did not. But the contrary is the fact. With those that drank the well-water, the ratio of mortality, as is evident from the Table, is 56.22 per cent; while with

those that did not drink it, the ratio is 77.66. It is nearly as high with the doubtful cases.

Lastly, if the well-water were so injurious, that the drinking of a pint, or half a pint, once only, could bring on an attack of cholera, as we are told, the partaking of it, in larger quantity, and during the attack, ought invariably to have produced a fatal termination. But here, again, the result is in direct opposition to the prediction. According to Mr. Whitehead, "One house, in which a very remarkable recovery from collapse took place, contained at that time but two permanent residents, *i. e.* two only who slept there throughout the pestilence. They were the two servants of a gentleman, who was absent during the greater part of the time. One of these servants was seized badly September 1st, 8 A.M. She soon became completely collapsed, but ultimately rallied and passed safely through a most dangerous fever, being carefully nursed throughout by her fellow servant. I visited her daily, and can myself testify to the fact, which, so far from keeping in the back ground, I am anxious to state plainly and unequivocally, that she drank the pump water *incessantly and abundantly* during her illness. I could mention many other instances to the same effect. One lad, who recovered from a serious attack, drank ten quarts of it on Sunday, September 3rd —whilst a girl, whose recovery from collapse seemed little less than miraculous, drank seventeen quarts of it the same day, September 3rd." And the writer then adds:— "I take this opportunity of recording, that the pump water was very generally, indeed almost universally, administered, during the period of illness, both to those who recovered and to those who died. In another house, in which no one died, there were, to a population of twenty-six, three recoveries from collapse, two being in the same family. This family, seven in number, used

the pump water every day at dinner, three pints to the seven persons, of whom five were not attacked. The two lads attacked were seized, one August 31st, 2 P.M., the other September 6th, 8 A.M. The third case in this house was of a foreigner, seized September 1st, 10 A.M., who also was a drinker of the pump water. I made inquiries of the other residents in the house, and found only one other person who drank this water either at that or any other time. He drank it with impunity." * On the other hand, abstinence from the well-water did not prevent a fatal termination. Independently of the deaths that took place, among those that did not drink the well-water, in the district, or in their own homes; 150 of the inhabitants died in the different hospitals, in which, of course, this water was not used. Although the handle of the pump was taken off on the 7th September, there were 114 deaths in the parish subsequently to this. These deaths, therefore, could not be ascribed to the use of the water.

It is no less clear, that the first attacks, in this district, were entirely unconnected with the use of the well-water. The first fatal case in St. James's parish occurred on July 26th, in St. James's Market, Jermyn-street. The next two fatal attacks were in the west and centre of the district, viz., in South-street and Silver-street, on the 3rd and 5th August. "The fourth fatal seizure in St. James's was on the 7th, in the south, in Great Windmill-street; the fifth and sixth, both on the 11th, were in the west, viz., in King-street and Marlborough-row. On the following day, three persons were fatally attacked, two in the south and south-east of the parish, viz., in Piccadilly and Great Windmill-street, and one in the very centre of the district to be presently rendered so memorable, viz.,

* Loc. cit., pp. 136-7.

in Broad-street, at No. 31."* It thus appears that of the first ten fatal cases in this district only one resided in Broad-street: while the majority of the others were so far removed that the idea of their drinking the well-water cannot be entertained for a moment. With respect to the last case—the first in Broad-street—Mr. Whitehead tells us that he could learn nothing respecting the use of the water. "The next (in Broad-street, August 28th) was the case of an infant, whose mother emphatically denies that it ever tasted of the pump water, assigning as a reason a decided objection to this water on the part of her husband, who was himself fatally seized with cholera on the 8th of September, being almost the last person who was attacked, either fatally or not, in this street. He of course was no drinker of the pump water. And I may here add, that a like positive denial upon this point is given, by persons competent to decide, both in the other case marked September 8th and in that of September 9th. The third fatal attack in Broad-street, that of August 30th, was the case of a lad who went to Bayswater on Saturday, August 26th, returning Monday, the 28th. The family with whom he resided in Broad-street are positive in their assertion that he never drank of the pump water. The precise hour of his attack was 5 A.M. At noon, the same day, he was sent back to Bayswater. It is worthy of notice that his mother and sister (at Bayswater) were also seized the following evening, and died before the end of the week."†

When, therefore, we find, that so large a proportion of the population died, who did not use the well-water: that, of those who did, the per-centage of deaths was greater than with others; that the drinking of this water during

* Loc. cit., p. 22. † Mr. Whitehead's Report, pp. 126-7.

the attacks, even in very large quantities, did not prevent recovery, and that the first attacks were unconnected with the use of it; we are bound to conclude that the well-water had no influence in the production, propagation, or fatality of the disease. Independently of the conclusion drawn by the Committee, that "no proof existed of the percolation of extraneous matter into the well" at the time of the outbreak, this was so sudden, and subsided so rapidly, that it is only by the operation of some cause more temporary than the contamination of water, that we shall be enabled to account for the rapid rise and decline of the disease—the majority of the deaths, in this street, having occurred on two days. This will be apparent by a glance at the following Table.

TABLE 9.—Showing the Date of Attack of all the Fatal Cases in Broad-street.

Date of Attack.	No. of Fatal Attacks.
August 12th	1
,, 28th	1
,, 30th	1
,, 31st	6
September 1st	26
,, 2nd	24
,, 3rd	9
,, 4th	8
,, 5th	6
,, 6th	5
,, 7th	0
,, 8th	2
,, 9th	1
	90

It was more like a blast of wind, or tornado, travelling with the rapidity of the simoon of the desert, than any other agent. It is also evident, that the epidemic had subsided in Broad-street, before the handle of the pump

was taken off (on the 7th September): although the water continued to be used both by the sick and the healthy, up to this period. We may therefore infer, from the preceding facts, that the conclusion drawn by the Sanitary Commissioners is an erroneous one: and that the well-water, in Broad-street, had no influence in the production and propagation of the cholera in 1854.

The charge of the Commission extends, as previously stated, not only to well-water, but to impure river water. It is added: "During the same epidemic the population of Southwark and Lambeth suffered severely from cholera, which was attributed to the state of the water-supply. The affected district was supplied by two Companies, one of which gave a comparatively pure water, and the other a very impure one, containing sewage matter from the Thames. The deaths from cholera in the houses supplied by the purer water were in the ratio of 37 in 10,000, while the ratio among those supplied with the impure water was 130 in 10,000." Such being the statement, the question arises, where did the Commission obtain this important and somewhat astounding information? From two sources; from the Registrar-General's Report, and from the Report of the then Medical Officer to the Board of Health—Mr. Simon. As the account is nearly the same in the two Reports; and as the data are more complete in Mr. Simon's, we will confine ourselves to an examination of the figures contained in the latter.*

The districts, on the south side of the river, are supplied by three Water Companies—the Southwark and Vauxhall, the Lambeth, and the Kent. The former, it is stated, supplied 39,726 houses; the Lambeth, 24,854, and the

* Report on the last two Cholera Epidemics, as affected by the Consumption of Impure Water. By the Medical Officer of the Board of Health. 1856.

Kent, 14,594, including Greenwich and Lewisham. In the houses supplied by the Southwark Company, there were, according to the Report, 3,476 deaths out of a population of 268,171—being a ratio of 130 to every 10,000 inhabitants. But the deaths, in the houses supplied by the Lambeth Company, were only 611, in a population of 166,906—being a ratio of only 37 to every 10,000. In order to account for the great variation in the ratio of mortality, in these instances, it is stated, that the Lambeth Company, in 1854, drew its supply of water from the river at Thames Ditton; while the Southwark and Vauxhall drew its supply from the Thames at Battersea. The latter, according to Mr. Simon, "was not only brackish with the influence of each tide, but contaminated with the outscourings of the metropolis, swarming with infusorial life, and containing unmistakeable molecules of excrement."

Such being the state of the water in the river, at the point indicated, it is not surprising, that the great mortality, stated to have occurred in the houses supplied by this water, should have been referred to its contamination —assuming, of course, that the statistics before given be correct. But that is the question. It has been remarked, that statistics will prove anything. Of course they will, if misapplied, or if they be erroneous; but not otherwise. Now there is one fact—and facts are stubborn *chields*, that cannot be smothered either by statistics, or theories, however plausible—which tends to show, if it does not prove, that both the statistics and the preceding conclusions are entirely erroneous. This is, that the ratio of mortality in the districts supplied by the Chelsea Water Company, which drew its supply, at that time, from *precisely the same point in the river* as the Vauxhall and Southwark Company, instead of being 130 per 10,000 inhabitants, was only 54. This is shown in the following Table.

TABLE 10.—Ratio of mortality in the following sub-districts, supplied by the Chelsea Water Company in 1854.*

Sub-Districts.	Deaths to 10,000 inhabitants.
Chelsea, North-East	43
Belgrave	49
Chelsea, North-West	38
Ditto South	76
St. Margaret's, Westminster ...	54
St. John, Ditto ...	63
Mean	54

How, then, are we to account for the difference in these instances? Are the inhabitants, on the north side of the river, less susceptible to the influence of impure water than those on the south? Or do they bear a charmed life? We cannot believe it. Nor can it be ascribed to the purification of the Chelsea water, after it was drawn from the river, for the process of filtration with this Company and the Southwark Company was precisely the same, viz., through sand and gravel.† In fact, Dr. Thomson, in his Report to the Board of Health, in 1854, states, that the Chelsea water was the most impure of any: the degree of impurity being 60 grains per gallon for the Chelsea water, and 40 for the Southwark. Then, again, although we are told, that the Lambeth Company supplied "a comparatively pure water," there was scarcely any difference in

* Extracted from the Registrar-General's Report, for this year.

† See Report of Committee to the President of the General Board of Health, on the Metropolis Water Supply, 1856.

the quantity of organic and inorganic matter contained in the water of the three Companies. When analysed in 1856, the Lambeth water contained nearly twice as much organic matter as the Chelsea Company. This will be evident by a reference to the following Table:—

TABLE 11.—Analysis of the water of the following Companies, in 1856, by Professor Hoffman and Mr. Blyth.

Companies.	Per Gallon.	
	Organic.	Inorganic.
Lambeth, filtered ...	1.26	17.85
Ditto unfiltered ...	2.91*	26.18
Chelsea, filtered... ...	1.20	22.19
Ditto unfiltered ...	1.64	23.03
1851.†		
Southwark, filtered ...	1.51	19.52

There are other facts which go to prove, that the purity, or impurity, of the water exerted no influence in the propagation or prevention of cholera on the south side of the river, in 1854. If the greater or lesser ratio of mortality in particular houses or districts, is to be referred to the greater or lesser purity of the water, something like a uniform rate ought to have been observed. But that is not the case. The ratio of mortality, according to Mr. Simon's table, varied with the Southwark Company from 10, per 10,000, in the Wandsworth sub-district, to 246,

* This is the mean of the two samples, on the north and south side of the river.

† As this Company began to draw a part of its supply in 1856, and at the time of the analysis, from Hampton, the analysis of 1851, by Professor Graham, has been given.

in the Borough-road sub-district; while, with the Lambeth Company, the ratio varied from 16 in the Lambeth Church sub-district to 211 in St. Saviour's sub-district. There is, in fact, the greatest variation in the ratio of mortality in the different sub-districts—thirty-one in number—supplied by the two Companies. Independently of the deaths in houses supplied by these two Companies, there were, in the same districts, 1,436 deaths in houses that derived their supply of water from other sources—from wells or from the Kent Company—as pure a water as any. The number of these houses amounted to 7,764. Calculating the population, in these houses, by the average in all the districts—the plan adopted by Mr. Simon—we shall have a population of 55,900. This gives a ratio of mortality of 257 per 10,000—123 higher than that of the Southwark Company. So much for the influence of impure water in the production of the epidemic cholera; and the use of pure water in its prevention!

If these facts be of any value, we must infer, that there are gross errors somewhere, either in the facts or in the statistics adduced by Mr. Simon, on this occasion. Where the error lies, it would be somewhat difficult to say, as a personal investigation would be useless now; while my absence from England, in 1854, and subsequent years, prevented my attention being directed to the subject previously. All we can do, at present, is, to examine the facts and the figures presented to our notice in the Reports of the Registrar-General, and of Mr. Simon, and then endeavour to ascertain the probable sources of error on this occasion.

In so doing, the first circumstance, that demands inquiry, is, how were the deaths in the different houses supplied by the two companies, ascertained? On this point, not a word is said in Mr. Simon's Report; it is

stated, in the Registrar-General's Report, that the registrars, on the south side of London, were instructed to inquire, in all cases of death by cholera, whether the house, in which the patient was attacked, was supplied by the Southwark, the Lambeth, or the Kent Companies; or with water from pumps, wells, ditches, or other sources. The inquiry was attended *with considerable difficulty*." * But this order was not given until the 26th August, and after the epidemic had existed seven weeks on that side of the river. Previously to this, the inquiry was conducted by Dr. Snow; a somewhat Herculean undertaking for an individual, among a population amounting, at that time, to 511,000. Independently of the personal trouble and fatigue, and the utter impossibility of visiting every house where a death took place, there were other impediments, not less striking, and which have been pointed out by Dr. Snow himself. He observes, in a pamphlet published at the time, "There were very few instances, in which I could at once get the information I required. Even when the water-rates are paid by the residents, they can seldom remember the name of the Water Company till they have looked for the receipt. In the case of working people, who pay weekly rents, the rates are invariably paid by the landlord, or his agent, who often lives at a distance, and the residents *know nothing* about the matter. It would, indeed, have been almost impossible for me to complete the inquiry, if I had not found that I could distinguish the water of the two Companies, with perfect certainty, by a *chemical test*." † This consisted in testing the water for chloride of sodium, or common salt. Granting that this test was a valid one, and that the water of the Southwark Company contained more chloride of sodium than

* Report, 1853-4, p. 94.

† On the Mode of Communication of Cholera, 1855.

that of the Lambeth Company, it could only have been employed in a comparatively few cases; while the very employment of the test shows what the difficulties of the inquiry were. If, therefore, Mr. Simon depended on the statistics of Dr. Snow, for the first seven weeks of the outbreak, they would not be worth a great deal. Not that those derived from the local registrars would be worth much more, and precisely for the reasons just given. Added to this, it is not always that the tenant goes himself for the certificate of death: he sends a nurse, or a friend, who, in all probability, would be as ignorant of the Company that supplied the house with water as the child unborn. And yet, such is the perversity of human nature, and the unwillingness of people to acknowledge their ignorance, the chances are, they would name one at hap-hazard—that with which they happened to be the best acquainted, or which was the first mentioned by the registrar. Here, then, we have one probable, almost certain, source of error.

Another subject of inquiry, not less important, is the manner in which the ratio of mortality, with the population supplied by the two Companies, has been ascertained. No information has been afforded on this part of the subject, any more than on the other. By a careful examination of Mr. Simon's Tables, the manner in which this was effected becomes apparent. In No. 3 Table, the population, the number of houses in each sub-district, and the *average* number of inhabitants in each house, have been given. As the two first items could be obtained from the Registrar-General's Returns, we may conclude that these are correct. Then we have the number of houses, with the population, supplied by each of the two Companies, together with the number of deaths among these two classes. The following example of one,

out of the thirty-one sub-districts supplied by the two Companies, will show this more clearly:—

TABLE 12.—Showing the population and houses supplied by the following Companies, with the deaths in the undermentioned district*:—

Sub-District.	Population.	No. of Houses.	No. in each.	Lambeth Co.		Vauxhall Co.		Deaths.		
				Houses.	Population.	Houses.	Population.	Lambeth Co.	Vauxhall Co.	Other Sources.
Christchurch..	16,470	1,887	8·5	1,557	13,234	343	2,919	84	30	13

It is not stated how the number of houses supplied by the two Companies was ascertained. It was not from the Companies, as they are themselves ignorant of the number in the sub-districts. They, in fact, know nothing of the arbitrary division of the locality into districts, which has been done for the purpose of registration, with which they, of course, have nothing to do. They only know the total number of houses supplied by their mains, although they might, perhaps, be able to specify the number, in a particular street, by a reference to their books. We may therefore presume, that the number of houses was ascertained in the same way as the deaths—by personal inquiry. If so, the calculation would be liable to the same doubts and the same errors, as when applied to the mortality.

Having settled this point, the next question is, how was the population of the two classes obtained? In the same way, or by some other method? Not by the former, but,

* Extracted from Mr. Simon's Table.

as it would appear, by a more simple method. We have seen, that the average number of inmates in each house, in the whole district, has been given ; obtained by dividing the population by the number of houses. Such a calculation would have been useless, unless for the purpose of being applied, as in this way. If we multiply the number of houses, supplied by the two Companies, by the average number of inmates in the whole district, we shall have the population in each of the two classes, as will be evident by a reference to the last table. As the calculation holds good for all the sub-districts, this, no doubt, was the method adopted. But it is a very erroneous method; one that may disturb the whole of the calculations. Not only do the inmates vary in different houses, but they vary in different districts, according to the poverty or the wealth of the inhabitants. Mr. Simon makes the average population, per house, for the thirty-one sub-districts, between seven and eight, or 7·2. But the Cholera Committee of St. James's state, that in the "cholera area"—or the sub-districts of Golden-square and Berwick-street —the ratio is between seventeen and eighteen persons to each house. And yet the ground floors of more than *half* the houses were occupied as shops. In Broad-street, rather a respectable one, "the average number of persons in a house is about eighteen, and to each floor five and a-half. But the greatest differences prevail, for even in Broad-street, there are instances of thirty persons living in one house: in one of the smaller houses, fifty-four persons were crowded into one dwelling." * Supposing, then, that the Southwark Company supplied the poorer districts to a greater extent than the Lambeth Company, not only would the number of inmates in these houses be greater, but the per-centage of deaths would necessarily

* Report, p. 53.

be decreased in an equal ratio. It might so happen, that the population, in the houses supplied by the Southwark Company was three or four times greater than in the houses supplied by the Lambeth Company; in which case, the proportion of deaths, in these instances, would have been nearly equal. This must have been another source of error.

Then, again, there are four sub-districts, exclusively supplied by the Southwark Company—viz., Rotherhithe, St. Mary Magdalen (Bermondsey), St. Olave and St. John (Southwark); in which 727 deaths are reported. All these deaths ought to have been excluded from the general calculation, not only because these districts are low, unhealthy ones—being scarcely above high water mark—but also because, it is a law of the epidemic cholera to attack one district in preference to another—the suburb instead of the city, or *vice versâ*. This law has been constantly observed at each visitation of the epidemic cholera, and was well marked in that of 1854. In the parish of St. James's, the general ratio of mortality was 148 per 10,000 of the inhabitants; but in the sub-district of Golden-square, it amounted to 217; in that of Berwick-street, to 212; while in the sub-district of St. James's-square, it was only sixteen. In addition to the preceding, there are seven other sub-districts, which, although not exclusively, are principally supplied by the Southwark Company. These, also, for the reasons just mentioned, ought also to have been excluded from the calculation, thus leaving twenty sub-districts only for comparison. Even then, we shall require a great deal of information, not to be found either in the Registrar-General's Report, or in that of Mr. Simon. It is not sufficient to know the number of houses that are supplied by a particular Company, and the number of deaths that

occurred in them: there are other circumstances of equal, if not greater importance, that ought to be taken into consideration.

It is necessary to know, in the first place, not the average, but the actual, number of persons in each house and in each sub-district, supplied by the two Companies. Unless this were done, the calculation of the ratio of mortality would be merely guess work. In the next place, we ought to be certain that the persons attacked had drank the water; for the number of water drinkers is comparatively small. What they do take is only in their tea or coffee, after it has been boiled, and all noxious ingredients expelled; or, else, in the form of infusion, the water of which comes from "Burton-on-Trent," or from one of the large London breweries. A large proportion of the working classes rarely, if ever, take a drop of water in their own houses. They leave home early in the morning, return late at night, and take their meals in other localities. Hence the necessity for special information on this point. Then, again, in order to ascertain if the greater ratio of mortality—supposing this to have been the fact—is to be abscribed to the impurity of the water, it will be necessary to ascertain if the proportion of deaths to attacks was greater with those who drank this water, than with others. If it were not, we should be certain that the water had nothing to do with the production of the disease, or its increased mortality. This information was afforded us, as we have seen, by the Rev. H. Whitehead, as far as Broad-street is concerned; and it enabled us, at once, to show that the well-water there had no influence whatever, in the spread or fatality of the disease, in that district. There are other and minor points which ought also to be taken into consideration in such an inquiry. One is, the localisation of

the deaths. If the water were the principal agent in the production of the disease, the deaths would be, or ought to be, pretty uniformly spread over the entire surface: but, if they were concentrated in particular streets, or parts of the streets, we should have to look to local causes for their production. In the parish of St. James's nothing could be more irregular than the distribution of deaths in different streets. In Hopkin's-street the ratio of mortality was 18.5 per cent.—1,850 per 10,000. In Regent-street it was only 0.1, and in Angel-court 0.5 per cent.—1 and 5 per 10,000. So, also, as regards houses. In New-street, 8 houses out of 11 had fatal cases; and in St. Ann's-court, 21 out of 32 houses. But in Rupert-street and King-street 2 out of 63; and in Great Windmill-street, 4 out of 51 houses only were fatally visited. In Regent-street there was only 1 fatal case in 127 houses.* In the absence of all information on these important points, and taking into consideration the probability that the statistics in Mr. Simon's tables are, for the reasons previously given, entirely erroneous, we shall be justified in rejecting all conclusions drawn from such imperfect premises. No valid deductions can be drawn from an inquiry, conducted in such a loose, uncertain, and incomplete manner as this. Remembering, also, that the rate of mortality in the districts supplied with the condemned water, on the *north side* of the river, was nearly two-thirds less than that in the districts to which it was distributed on the south side, we may fairly conclude, that the water of the Southwark Company exerted no influence in the production or fatality of the epidemic cholera in 1854.

We might have arrived at this conclusion without any analysis of the evidence that has been brought forward on the present occasion—by the production of other and

* Table in Appendix of Report.

opposing facts. It has been before stated, that all diseases gradually declined in London from the middle of the last century up to the advent of the epidemic cholera in 1832. During the last 30 years of this period, the contents of the water-closets were discharged into the river: and yet, during all this time, cholera was unknown, while bowel complaints were so rare that, in one year, only two deaths were registered under this head. The water, it is true, was not contaminated to the same extent then as now, from the lesser amount of population: but it was contaminated to a sufficient extent to have caused some morbid effect, if such a result could be produced by such a cause.

Let us take another example, derived not from the former, but during the present period, when the contamination of the water has arrived at its height. As is well known, the ships that leave the port of London are supplied with Thames water, taken, in general, from the docks, where the contamination is even greater than between bridges. In the ships sailing to the East, this water, after entering the tropics, undergoes decomposition: not so much now, since the introduction of iron tanks, but yet to a sufficient extent as to render the water more impure than when taken from the river. When stored in butts, the decomposition is so great, that it is usual to allow the water to stand twelve or twenty-four hours after the cask is broached, to allow the gaseous products of putrefaction to pass off, and for the solid matter to subside to the bottom. But this, from stress of weather and other circumstances, cannot always be done; in which case people must take it as they find it—pure or impure, sweet or stinking. On one occasion, in a ship in which I was, we had been, during a gale of wind off the Cape, without water for many hours, and were all

dying with thirst. As soon, therefore, as the cask was broached, we eagerly swallowed a large draught, in spite of its nasty appearance—black flocculi floating about—and still more nasty smell. So offensive was it, in fact, that we were actually obliged to hold our noses while drinking the horrid draught, which the addition of brandy failed to disguise. But neither I nor any one else on board—about 170 souls in all—suffered any subsequent inconvenience, or felt any ill-effect from our nauseous dose. It is, in truth, a remarkable fact, and one that demonstrates the innocuousness of Thames water, that the crews of ships, in the voyage from the port of London to the East Indies, *invariably* remain perfectly healthy during the whole time. Epidemic and endemic diseases are unknown: the only affections met with are those chronic or constitutional ones, that people take on board with them. There is no instance on record, of cholera having broken out on board ships during the voyage from London to the East, by the Cape of Good Hope. As soon, however, as the ship has anchored in the Straits of Sunda, or in the river Hooghly, or a few days after, cholera, fever, and dysentery make their appearance: the water remaining the same. What a farce, then, to talk of Thames water producing attacks of cholera or rendering the attack more severe and more fatal when it arises.*

Cholera is not the only disease, supposed to be produced by the use of impure water: typhoid fever is also referred to the same cause by Mr. Simon and other writers. Three instances have been adduced by the former

* There are plenty of examples of cholera breaking out during the voyage from England to America; but, then, the majority of these ships sailed from Liverpool, not London, and took their supply of water from the former place. The cause of the difference, in these instances, will be better understood hereafter!

writer, in proof of this conclusion, viz., the visitations at Guildford, Winterton, and Terling, in 1867. As the subject is too important a one to be passed over, we may pause for a short time, in order to ascertain what are the facts on which this conclusion reposes. And, first, as regards Guildford.

This town is placed on the side of a hill of chalk, the houses, in the lower part, being built on the London Tertiaries, overlying the chalk; and those in the upper part immediately on the chalk. As the natural drainage is so good, and as cesspools are alone used, drains and sewers are not required. "The cesspools," remarks Dr. Buchanan, "being sunk in the chalk, keep themselves dry, and are so inoffensive that they are not emptied for many years together."

Typhoid fever broke out, in this town, on August 1st, 1867; the number of cases, during the first four weeks of this month, being ten. The numbers increased rapidly in September, so that 140 cases had come under treatment by the 10th; and 264 by the end of September, in a population amounting to 9,000. The outbreak has been thus accounted for by Dr. Buchanan, who was sent down by the Privy Council to make a special Report on the subject: "The public, in this town, is supplied with water from two wells, sunk about twenty feet in the chalk, in the lower part of the town: an old well, from which water is raised by the power of an adjacent water-mill; and a new well from which, from April to July inclusive (of 1867), water was distributed to the higher parts of the town by engine-power. This engine had broken down on the 1st August, and the high-service mains were supplied, from this date, with water from the old well by means of the water-wheel, with *the exception of one day*—August 17th—when some water from the new well, that had been

stored in the reservoir, was distributed in lieu of the water from the old well—the water-wheel requiring some repairs. It was distributed on no other day, and to no other houses," than those supplied by the high-service mains.* As the number of cases happened to be greater in the houses supplied by the high-service mains than in others, Dr. Buchanan jumped to the conclusion, that the water was the offending agent; and as one kind of water had been exclusively distributed to these houses on one day, on the 17th August, it was further inferred that this water—the store-water in the reservoir—must have been the means of propagating the disease in the upper part of the town. But how, it will be asked? Dr. Buchanan states, "there is no chance of impurities having entered the water, while it was stored in the new reservoir." As, however, when an individual is accused, evidence, that would not otherwise be thought of, crops up unexpectedly to support the accusation: so, also, in this instance, it was subsequently discovered, that the water in the new well had become contaminated with sewage. It is stated, in a letter from Mr. Taylor, a medical practitioner at Guildford, to the Privy Council, that the men, in the engine-house, observing an exudation on the walls, which they thought had the smell of sewage, dug down, and found that it was caused by the leakage from a sewer running near, and saturating the ground around with the filth from the sewer. As the new well was only about ten feet from the sewer, it was at once concluded, that the water, in this well, had become contaminated from this source. This is not only possible, but probable: while, as the state of the sewer, as described by Mr. Taylor, must have existed for a long time, the water in this well would

* Dr. Buchanan's Report, p. 37, in Tenth Report of the Medical Officer of the Privy Council. 1867.

necessarily have been contaminated from the first employment of it, the beginning of April. How did it happen, then, that the fever never made its appearance in the houses supplied with this water until four months after its first use, and until nearly a month after its use had been discontinued, with the exception of a *single day?* As to the idea that all the cases, subsequently observed in the upper part of the town, are to be ascribed to the water distributed on that single day, the supposition is the most preposterous that ever entered into the mind of a scientific investigator. There were no attacks, be it remembered, in the houses supplied with this water, until ten days after it was distributed. Are we then to conclude, that the morbid matter productive of the disease lay dormant in the system all this time? Granting that such were the case, and that the first attacks were to be ascribed to this cause, it would be a stretch of the imagination to suppose, that all the attacks which occurred subsequently,—amounting to 200 before the end of September, and 200 more by the end of December—could have been produced by this cause, for the disease was not confined to the houses supplied with the water from the new well. The first cases that occurred were in the lower part of the town, to which the water in the old well was distributed. There were other cases, in this part of the town, subsequently, making altogether thirty, that drank the water of the old well: while there were fifty-seven, that used neither of the wells, having derived their supply from other sources. How then are we to account for the production of the disease in these instances? Were there three causes in operation, in Guildford, productive of precisely the same effect? That is not according to the Newtonian axiom, nor is it in accordance with the ordinary laws of nature. We cannot err much, then, in

concluding, that the state of the water had nothing to do with the outbreak of fever in Guildford: and that the real cause remains, at present, a mystery. With these remarks, we may now pass on to the next case referred to —that of Winterton.

This town, which is situated in Lincolnshire, and has a population of about 1,800, had suffered from typhoid fever, to a slight extent, for several years, but more severely in 1865-6, and again in 1867. In the former years, 100 were attacked, and seventeen died: what the numbers were in the latter year, has not been stated. When Dr. Thorne, who was sent down by the Privy Council, in April, 1867, arrived, there had been six deaths. From this gentleman's Report, we glean the following particulars.* The town is on high, sloping ground, and being built on the *Oolitic* formation, which is very porous, the natural drainage is, of course, very good. In addition to this, "a very efficient drainage had been laid through the town, a few years (four) previously. Into this drain, a running stream has been diverted; and thus a continual flow of water is kept up, throughout its whole length." The sewer empties itself into a brook, which flows into the river Ancholine, just before this stream joins the Humber. With these advantages we might have inferred, that Winterton would have been less liable than the majority of towns to the incursions of fever. But alas! for human predictions, and human forethought. Typhoid fever selected Winterton in preference to other towns, less favourably situated. "The cause of all this terrible sickness and mortality," as we are told by Mr. Simon, "was, *of course*, evident to their Lordships' Inspector, at *a glance!* It was the merest question of filth!"† As the cause is thus easily ascertained, we may

* Mr. Simon's Report, 1867, p. 28. † Loc. cit., p. 10.

as well take a glance ourselves at the sources whence this filth was derived. It was from the soil surrounding the cottages, according to Dr. Thorne.

After observing, that absolute poverty is hardly known there, and that the houses are well built, the writer states, that, with the exception of about six cottages, where water-closets have been constructed, all the cottages are provided with privies, built of brick. At the back of the privy, is an aperture to allow of its being cleared out, but as this is generally open, "the result is, that the contents of at least half the privies in the town run out into the garden, soak into the earth, and penetrate, in many instances, into the well, besides producing the most offensive odour." In addition to this, there are uncovered ashpits to many of the houses, while the tenants, in others, throw the slops and even the dejections of the patients into the garden, or into the ashpits.* To the emanations arising from these matters, and to the contaminations of the water in the wells, Dr. Thorne and Mr. Simon ascribe the outbreak of fever in this town. If, however, the arguments and conclusions previously drawn be of any value, we must infer that the emanations arising from these sources had no influence in the production of the fever at Winterton. With this conclusion, we will proceed to consider the third and last case—the outbreak at Terling.

* The Rev. Mr. Moule, for his patent night-stools, makes use of earth, in order to neutralise the effluvia arising from the excreta of man. This it does, even in small quantities, most effectually. Ashes produce the same effect. We also find, when sewage water is thrown on the land, that the effluvia arising from it is destroyed in the course of a few minutes. Some persons, therefore, would have inferred, that the tenants in these houses adopted the best course in order to destroy all the noxious effluvia arising from these matters. But doctors sometimes differ!

"In that village," remarks Mr. Simon, "of only 900 inhabitants, and, for the most part, within a period of two months, fully 300 persons were attacked with typhoid fever. That is to say, the one *preventable* disease killed, in that short space of time, a larger proportion of the population than all the causes of death put together ought to have killed there in two years." As this disease is so easily prevented, it must be equally easy to ascertain the cause of its production. This we will now proceed to point out. It may be as well to premise, that Terling is a village in Essex, composed entirely of labourers' cottages, scattered, in groups of two or three, over an area of several miles. The houses are built on gravel and sand, overlying a bed of London clay: and as the ground slopes gradually to the banks of a rivulet—the Ter—which empties itself into the Blackwater river, the natural drainage is good. The vicarage and the residence of Lord Raleigh—Terling Place—are the only other, or respectable, houses in the neighbourhood.*

The disease commenced on the 4th Dec., 1867, and on the 13th of the following month 208 persons had been attacked, and fresh cases were still occurring. A large proportion of these were attacked on three particular days—the 15th, 16th, and 17th December—when there were twenty-two, nineteen, and twelve cases respectively. The conditions of this outbreak were, according to Mr. Simon, "local filth. Round what pretends to be the house accommodation of the tillers of the soil in Terling, every possible source of pollution of air and water was accumulated: the peculiarly porous soil, which underlay all this filth, was of course continuously absorbing it: the water supply of the population was derived from wells, most of them sunk in the excrement-sodden sponge of

* Dr. Thorne's Report, inserted in Mr. Simon's for 1867, p. 41.

earth."* That such was the condition of these villages, we might have inferred without consulting these official Reports: for this is precisely the condition of the majority of villages in England. The only difference is, that both Winterton and Terling were more favourably situated than many other villages, while they had the advantage of being well drained. Without waiting to inquire whether "the excrement-sodden sponge of earth" at Terling be a fact, or merely an assumption, it is sufficient now to remark, that the *simultaneous* contamination of so many wells—there being one to every two or three cottages†—from such a source is highly improbable. Besides, if these causes be productive of typhoid fever, how is it, we may inquire, that only a certain number of villages have been, as yet, attacked with this disease? Many of these villages are in a worse sanitary state, than either Winterton, or Terling; but they have hitherto been spared from the ravages of typhoid fever. More than this, the conditions now pointed out must have existed in Winterton, Terling, and hundreds of other villages, in England, for centuries: but typhoid fever was unknown before the present generation. Then, again, if the causes we have been considering can produce typhoid fever, how is it that this disease was unknown, until lately, in India, although all these causes exist there, to quote the language of the Sanitary Commissioners, "in a state of absolute perfection," and have existed from time immemorial? Were typhoid fever an effect of the ingestion of impure water, this disease would assuredly have been produced by the use of the contaminated water in the "nullahs," or ponds, of India: on the borders of which the natives perform their ablutions, and all other *natural offices*, although it is the only

* Loc. cit., p. 11.

† See Map added to Dr. Thorne's Report.

water they have for drinking. But this disease is as new in India as it is in England.

Typhoid fever was first distinguished in France by Bretonneau, in 1812: but it was not recognised in England until 1827. Although described by Bright and others, no cases were observed in Scotland until 1847: now it is constantly met with. In 1860, there were 41 cases in the Edinburgh Infirmary, 35 in 1861, 79 in 1862, and 67 in 1863: during which period the cases of all other varieties of fever did not exceed 150.* Has Edinburgh, then, become a *nidus* of filth, since 1841; and has the water there become contaminated, the same as in certain towns of England? That must be the case, if Mr. Simon's doctrine be true: but it is not a conclusion likely to be adopted by our brethren, on the other side of the border. Dr. Christison, at the meeting just mentioned, while referring to the prevalence of typhoid fever in Edinburgh, remarked: "Our street drains, in the old town, have been much improved during the very period that enteric, or typhoid, fevers have been increasing .... Further, this disease does not, by any means, generally break out where the streets are ill-drained, water-closets wanting, and habits filthy." And the speaker then added: "It may be worth while mentioning, in reference to an independent question likely to be discussed in this section, that enteric fever *is not known* in, or near, the 'foul meadows' of Craigentenny."† Dr. Bennet, also, at the Meeting, the following year, stated that, formerly, foreign physicians came over to Edinburgh to study typhus; while Edinburgh went to the Continent to study

* Address of Dr. Christison, at the Meeting of the Social Science Association, 1863.

† It is on these meadows, that the sewage of Edinburgh is distributed.

typhoid fever. But now, he added, "The latter has recently appeared in Edinburgh, and, strange to say, it appeared *to have followed the improvements*." With all these facts before us, we may conclude, without any risk of error, that the contamination of the water in the wells at Winterton and Terling had nothing to do with the outbreak of typhoid fever, in these villages. As Professor Griesinger has remarked, while referring to these fevers: "Their causes are specific, and do not reside merely in those external and injurious influences which exist everywhere: that is a fact which we must accept independently of the general reasons already given; for their geographical distribution has something which is peculiar to them: the development of their causes is, besides, limited to certain climates, to certain localities, to special and particular conditions in which men live. At certain periods, they become very frequent, at others very rare, without our being able to discover any change in the action of those injurious, external influences to which we can attribute the increase or diminution of these diseases."* That the purity or impurity of the water has nothing to do with the prevalence or fatality of epidemic and ordinary diseases, we may learn, not only from the instance of Rome, before referred to, but also from the statistics of two English towns—Glasgow and Manchester. These two towns are supplied with a purer water than any other town in Great Britain: that from Loch Katrine, distributed to Glasgow, being almost as pure as distilled water. Notwithstanding, the rate of mortality in this town has been as high since as before its introduction: while, as it so happens, this rate is nearly the highest of all the towns in either Scotland, or England. The new supply was laid on in January, 1860.

* Infections—Krankheiten, etc. In Virchow's Handbuch. Band 2. Abtheil 2. 1857.

During the ten subsequent years—1860-9—the average annual death-rate was 31 per 1,000 inhabitants: in the four previous years—1856-9—the rate was 29.50—one and a-half per 1,000 less. I am not aware when the fresh supply of water was laid on at Manchester; but it is not many years since. Still, no diminution has been observed in the rate of mortality in this town, any more than in Glasgow. During ten years—1851-60—the average death-rate was 31 per 1,000: while it was the same during the last three years 1868-70. In London, the average rate of mortality, during the above periods, was 24 per 1,000: so that, of those that used the pure water of Loch Katrine 7 per 1,000 more died than of those who are compelled to drink the filthy water of the Thames. So much for theories, when unsupported by facts and experience.

Besides, is it not somewhat farcical to make all this fuss about a few grains of organic matter in a gallon of water; three or four grains being the greatest amount that has yet been discovered in the most impure water? Any one might suppose, when celebrated chemists are called upon to analyse these waters, and to make special reports on the subject, that, instead of organic matter, some powerful poison would be detected. For what is organic matter? Are not our bodies composed, principally, of organic matter? Does not our food consist almost entirely of organic matter; and do we not live and have our being by means of the organic matter that we take into our system? We shall of course be told, that this matter is in a state of decomposition, and that the gaseous products of putrefaction are present at the same time. That may be, but it is not a necessary consequence of the presence of organic matter in the water. This matter, it should be remembered, is speedily converted, by the action of

oxygen, into other and innocuous substances — into ammonia and nitric acid. Supposing, however, that it be, or a portion of it, in a state of decomposition? What then? Do we not all, and these theorists, doubtless, among the number, sit down and take, without any compunction, not a few grains only, but ten or twelve ounces, of half-putrid matter, in another form—in that of game or venison? But we do not find, that attacks of either cholera, or typhoid fever, follow the ingestion of this *poisonous* matter! The inhabitants of northern, or the Arctic, regions never take their food, unless it be in a half putrid state; like the gourmands of Europe, they would not relish it unless it were, what is popularly termed, *high!* And yet, fevers are absolutely unknown among them. Mr. Forster says; "There is not, perhaps, on the face of the earth, a human creature who lives on coarser fare, or, to a civilized people, more disgusting, than a Kalmuck Tartar. Raw *putrid* fish, or the flesh of carrion, horses, oxen, and camels, is the ordinary food of the Kalmucks; and they are more active, and less susceptible of the inclemency of the weather, than any race of men I have ever seen." * It is, in truth, a wise provision of nature, that man and carnivorous animals should be able to partake of half-putrid food with impunity; and we know the reason. The gastric juice is a powerful antiseptic. Hence, dogs can live constantly upon the most putrid substances. It is not, it is true, to the pollution of water by ordinary organic matter alone, that Mr. Simon refers the production of typhoid fever; but to fecalised, excrement-tainted water more particularly. This is not a particularly pleasant subject to discuss, but the blame—if blame there be—rests with those who have broached it, not with those who discuss it. No one, of course, would par-

* Journey from Bengal to England, p. 258.

take of such water unless obliged; but when we look at the subject, in a scientific point of view, we must exercise, not our imagination, but our judgment, and trust, not to our feelings, but to facts.

It is somewhat singular, that Mr. Simon, who is an anatomist and a physiologist, should attach so much importance to the presence of a few grains of fecal matter in drinking-water; when he remembers, that this same matter exists, constantly, in the intestines of every human being—not in small, but in large quantities; in ounces, instead of in grains. More than this, the products of its decomposition exist there to a greater or less extent, and, sometimes, to a considerable extent. This is not all: these products are absorbed into the blood, and pass out by the lungs, as we know by the odour of the breath of certain persons; for this tainted breath, *with those in health*, can be derived from no other source. According to Mr. Simon's doctrine, then, the normal condition of man should be that of disease, not of health. The contrary being the fact, we may conclude, that this matter, however disagreeable to our olfactory organs, is not the cause of any ordinary disease, when diffused in the water partaken of by man. The causes of disease must be sought for, not in ordinary and natural processes, but in extraordinary and unnatural ones; for diseases—at least epidemic diseases—only prevail at particular times and under particular circumstances. What these causes are, will be considered hereafter: it is sufficient now to remark, that typhoid fever would appear to have been produced, not by filth in the soil, but by the removal of it; in other words by drainage. M. Boudin remarks: "The drying (or drainage) of the land, or its conversion into a lake, while producing the disappearance or diminution of paludal diseases, appears to predispose the organism to a

new pathological condition, according to the locality, and in which phthisis and typhoid fever play a prominent part."* This conclusion is confirmed by another circumstance. Dr. Thorne states—and it is the only important information contained in his Report—that, "Ague was, up to thirty years ago, very prevalent in the district, but since the neighbourhood has been properly *drained*, no cases have occurred." Their place has been supplied by typhoid fever! † We have thus a clue to the outbreak of fever at Winterton, if not at Terling and other places.

That the drainage of the land, although productive of benefit in some cases, is injurious in others, admits of no doubt. Although vast sums have been spent in draining the Pontine Marshes, these efforts have only rendered the country more uninhabitable than before. Large and populous cities formerly occupied the plains, that are now a desert. The drainage of the marsh called "La Chartreuse," near to Bordeaux, was followed, as we are told by Macculloch, by a succession of bad fevers, which lasted many years. In 1803, the fever was so general that 12,000 persons were attacked in Bordeaux, and 3,000 died.‡

We have thus proof, that the drainage of the land is sometimes productive of disease: but whether the prevalence of typhoid fever in England, of late years, is to be referred to this cause or not, is immaterial at the present moment. Abundant evidence has been advanced to

* Étude de Géologie Médicale, 1848.

† In New England, America, typhoid fever is called the autumnal, or fall fever; arising, like ague and other malarious diseases, at a particular season of the year.—Bartlett on Fever.

‡ For an explanation of the variation, in these instances—the beneficial effects of drainage at one time, and its injurious operation at another—see "Causation and Prevention of Disease," p. 146, *et seq.*

show, that this disease, like cholera, is not caused by the use of impure water, and, consequently, that the theory we have been discussing is a false and illogical one. And yet, it is precisely in accordance with this theory, that Mr. Simon has had the temerity to propose that legislative enactments should be carried out, rendering water companies and others responsible for outbreaks of cholera and of fever. "It seems to me," adds Mr. Simon, "that the time has now come when, not only as regards commercial water companies, but also as regards local sanitary authorities, certain sorts of *malfeasance* should involve an obvious and unquestionable liability to pay pecuniary damages to persons whom the malfeasance has injured." * And it is then added: "It seems to me, that the distribution of fouled water, by the Guildford Board, is as proper a case of judge and jury, on action for damages by any of the 500 people who had typhoid fever in the town, as any case in which a railway collision brings some score of passengers into harm: and the fact, as I believe it to be, that these water purveyors *gave typhoid fever* to their customers, would be brought home to their consciences, and be suggested as a warning to other water purveyors." Of this there can be no doubt, and the warning would probably be so effectual, that there would very soon be no water purveyors at all: the public might then have to obtain and fetch their own water, where they could find it. What company would run the risk of having to remunerate the friends of the 20,000 or 30,000 victims of typhoid fever, or cholera, during an irruption of either of these diseases in London, for they are to be rendered responsible for the production of the one disease, as well as the other? Whether a clause has been inserted

* Tenth Report of the Medical Officer of the Privy Council, 1867, p. 13.

in the new Sanitary Act, about to be presented to Parliament, we know not; but it appears to me, that sanitary laws should be based, not on theories, but on facts; not on the crude opinions of an individual, or, individuals, but, on the well ascertained conclusions of the whole medical profession—in this country and in others. If this be not done, we shall be going back to the false accusations and the unjust persecutions of the middle ages: when the supposed witches were drowned, and the Jews were executed, for effects produced by the operation of natural laws, which neither they nor their persecutors could have prevented or controlled, for a single moment. We had an example of this, a few years since. In 1848, an outbreak of cholera occurred in an asylum for pauper children at Tooting; the proprietor of which was prosecuted at the instigation of the then Board of Health, as the author of the mischief. He was tried at the Old Bailey, like a criminal for murder or manslaughter, and although the case broke down, by the cross-examination of the medical witnesses for the prosecution—thanks to the information furnished by me to the prisoner's counsel—the trial, and the worry of mind, caused the death of this unhappy victim to medical ignorance and medical presumption. Had the friends brought an action against the Government, as the authors of his death and the ruin of his family—for the establishment was broken up and the children dispersed—they would have been perfectly justified in so doing. Having visited the asylum at the time of the outbreak, I can affirm, that there was nothing in the state of the yard or of the house to account for the disease, even had the theory, on which the prosecution was founded, been as true as it is false.*

* This outbreak was merely the *avant courier* of the visitation in 1849—a phenomenon constantly observed at all epidemic periods.

If prosecutions are to be carried out, in accordance with the prevalent theory of the day—theories as fleeting as the fashions—there will be no end of indictments. Besides, there is generally more than one theory in vogue at a time; while they almost invariably differ from each other. Even the same writer will sometimes have two or three different methods of accounting for the production of diseases. This is the case with Mr. Simon. In his Report to the Common Council, in 1854, we find it stated, that, "for the permanent avoidance of epidemic diseases, *cleanliness is the sole safeguard*," *e.g.*, the removal of decomposing matter on the surface. Now we are told, that cholera and typhoid fever are due to the presence of organic and fecal matter in the water. Then, again, Mr. Simon has adopted another water theory—that of the late Dr. Snow, who ascribes cholera and other diseases to the contamination of the water from the dejections of patients —a doctrine that will be presently considered. This is not all. Mr. Simon has also become of late years—*horribile dictu*—a contagionist: not only as regards cholera and other diseases, but, also, with respect to typhoid fever. It is stated, in the "Lancet" (Jan. 18th, 1862): "Mr. Simon's latest opinions, on the much discussed and important question of the etiology of typhoid fever, have *undergone changes*. In the last printed Report to the Privy Council (1861) Mr. Simon thus expresses himself: 'Since I last reported generally on the subject of typhoid fever (Report, 1858), an addition has been made to the literature of that disease, by the publication of a series of Papers by Dr. William Budd, of Bristol.'" * Mr. Simon then adds: "The facts which Dr. Budd adduces, from his own experience, and from that of other observers, are, in my opinion, sufficient to prove, that

* See "Lancet," July, 1859, and March, 1860.

the contagion of typhoid fever is importable by persons who have it." To the question, put by the Chairman of the Vaccination Committee, "Do you, or do you not, consider that sanitary improvements, both as regards dwellings, and as regards greater cleanliness of the person, might in themselves very much diminish the prevalence of small-pox?" Mr. Simon replied: "I have no evidence that it can do so." * As typhoid fever is also considered, by Mr. Simon, to be a contagious disease, we might reasonably inquire, why this disease should be propagated or produced by impure water any more than small-pox? Until this question be satisfactorily answered, a Grand Jury would certainly throw out any bill of indictment, brought against the Water Companies, for the murder of Her Majesty's subjects! Here then we have three different theories, in order to account for the production and propagation of diseases; and, what is still more singular, the last theory is in opposition to the first; for Dr. Budd repudiates the idea of sewage, or fecal matter, being the cause of typhoid fever. And yet Mr. Simon adopts Dr. Budd's theory, as well as his own; while it is only by the latter, that the Water Companies, Boards of Guardians, &c., could be indicted. According to the other two theories, the medical men and the nurses would be the responsible persons; and those who ought to be prosecuted, if anybody were. In this way, there would be no end of prosecutions, so that a Sanitary Attorney-General would probably be required: as the time of such a functionary might be fully occupied in these State trials: unless the mob, which is possible, took the law into their own hands, and exterminated these modern witches! If, however, medical men, who are now responsible for the health and the lives of their patients, are also to be made responsible

* Report of the Vaccination Committee, p. 165.

for the attacks of disease—and they will not be spared any more than the Water Companies, if suspicion attaches to them—few will be found at their post in the hour of danger: they will flee away, not from fear of the enemy, but for fear of their pockets, or their necks. This attempt to render individuals or bodies of men liable for the production of disease, either by direct or indirect means, would appear to be a very problematical experiment. The proverb says: it is dangerous to play with edged tools: it is no less so, to employ accusations, which sometimes recoil upon ourselves. Suppose, for instance, that the people, at the next visitation of cholera, or other disease, should turn round and accuse the Government of negligence, for not having prevented the outbreak, in accordance with the promises held out to them, so many years since. Some forty years ago, Dr. Southwood Smith, the founder of what is termed Sanitary Reform, thus wrote: "I would express my conviction, derived from much observation and some experience, that the most distressing of the evils of which we have been speaking (the prevalence of diseases) are capable of being *almost wholly prevented* in future." * And Mr. Simon, at a later period, remarks: "Its untimely extinction (life) has depended on the direct operation of local and *preventable causes*."† As the Editor of the "Standard," in an article on the cholera outbreak of 1866, has observed: "They," the Sanitarians, "have formed public opinion to a certain standard, in regard to health and sanitary arrangements. But they must not be surprised, if they are themselves judged by this standard. They have meted out this measure to others, and now it will be meted back to them in return. . . . . London was promised health and longevity, if she would give herself up into the hands of

* First Report of the General Board of Health.

† Fifth Annual Report. 1853.

the sanitary men. She has done so—she has had to pay for it, and will have to pay for many years to come. But still she is sick,—not so sick as she might be, but sicker than she likes." The answer to this will, of course, be, that the carrying out of these measures has depended more on the different local authorities than on the Government, or its medical officers—a fair and reasonable answer. But, then, the mob, when excited, is not in the habit of reasoning or arguing: with them it is the word and the blow. Added to this, even scientific men are beginning to ask, how it is that no beneficial results have hitherto followed the adoption of sanitary reform. Dr. Christison, while referring to the prevalence of typhoid fever, remarked: "If we are to believe, what some have advanced on the subject, there is no case, which may not be traced to *foul air;* derived mainly from one of these sources (faulty drainage and faulty provision of water-closets). Were this a well-established principle in social science, the extinction of so deadly a fever should be no very difficult matter."* Nevertheless, this preventable disease continues to prevail; and, what is more, it has continued to increase, up to the present time.

Instead of holding out fallacious hopes—hopes that never will be, never can be, realized—and instead of attempting to throw the blame on others, it would seem to be better and sounder policy, frankly to avow the fact,—that these general pestilences are due to the operation of natural causes; and that they are regulated by natural laws, over which man has no control. If we fail to convince others, we shall, at all events, have the satisfaction of knowing that we have done our duty, by boldly proclaiming the truth: while we shall be saved the mortification of having unfulfilled promises, and disappointed expectations, thrown in our teeth.

* Meeting of the Social Science Congress. 1863.

CHAPTER III.

THE PROPAGATION OF CHOLERA.

When observers in Europe learnt that a new disease had sprung up in the Delta of the Ganges, and when they found that it was progressing, regularly and slowly, across the continent of India, and, as it were, step by step, it was natural for them to conclude, that this *nova pestis* was propagated, the same as the old one was believed to be, by contagion. When, also, the epidemic, by a sudden bound, traversed the intermediate sea, which appeared to say to it, thus far shalt thou go, and no farther: and when it spread along the shores of the Red Sea, and of the Persian Gulf, until it reached Asia Minor, on the one hand, and Egypt on the other, a strong confirmation appeared to be given to the above conclusion. Although the epidemic extended to the ports of Asia Minor and of Egypt, and although it had thus confronted Europe, as it were, those who were watching its progress were surprised to find that, instead of spreading farther in that direction, the disease suddenly disappeared. As it was precisely here, in the centre of commercial traffic and intercourse between Asia, Africa, and Europe, that a contagious disease might have been expected to spread, a somewhat startling negation appeared to be given to the doctrine of contagion. When, also, it was afterwards discovered, that the epidemic cholera was progressing regularly and slowly across the continent of Asia, by a route along which there is scarcely any traffic, and

among a scattered population, between whom there is little, if any, intercourse; and when, seven years after it had appeared on the shores of the Mediterranean, the epidemic suddenly sprung up in the Southern Provinces of Russia, even the advocates of contagion acknowledged that the phenomenon was inexplicable.

"Of all countries in the old world, Russia," as M. Moreau de Jonnes has justly observed, "seemed to be the least exposed to the irruptions of the epidemic cholera. Of her European provinces, the nearest to the Delta of the Ganges, where the malady first appeared, are 1,200 leagues in a direct line, and more than 2,000 by the track of the ordinary communications. Her situation in the highest latitude necessarily limits the period of the hot season, and produces an extreme degree of cold during the winter; which has the double effect of diminishing the duration, and the violence of foreign contagions—a necessary condition for the existence of which is an elevated temperature. Her commercial relations do not extend to tropical regions, nor even to eastern countries, whence all the pestilential maladies imported into Europe come. Lastly, her population, scattered over an immense surface, and less in density than the inhabitants of Belgium or Lombardy, in the ratio of one to ten, is, in comparison with all other parts of the continent, that which offers the fewest facilities for the propagation of contagious maladies; and yet, such is the course of events, and the uncertainty of human affairs, that the Russian provinces are the first which experience the fierce attacks of the Asiatic cholera; and it is by them that this remarkable scourge first invaded the countries of Europe."* The above quotation from a work, written expressly to show the contagious nature of the epidemic

* Rapport au Conseil superieur de Santé, sur le Cholera Morbus.

cholera, not only proves that this disease, like all other general pestilences, pursues its own course, uninfluenced by accidental circumstances, or human agency; but also shows, at the same time, that even the advocates of the doctrine of contagion do not attempt to explain many of the anomalies which these diseases present, when regarded according to this theory.

One other kind of proof may also be mentioned. It is well known that, in London, the disease, in its first visitation, was principally confined to particular parts of the town,—as the banks of the river, and more particularly the southern side of the Thames; while also the mortality from the epidemic cholera, in this densely crowded city, only amounted to about 3,000—the very lowest rate that has been observed in any town visited by the disease, and little more than what some cities have lost in a single day, with only one-third the number of inhabitants. Now these facts can never receive elucidation from the doctrine of contagion; for if the disease were infectious, there can be no reason why it should not have been propagated as readily in one part of the town as in the other, especially as cases were not wanting in any situation; for although the malady prevailed principally in certain situations, no part was entirely free. The difference observed was simply this:—In the locality where the epidemic prevailed in its greatest intensity, two-thirds of the inhabitants of a house, a street, or a district, were cut off or attacked by the disease; while, in the more favoured spots, only one in a house, in a street, or one street in a district, was attacked. This phenomenon, at present inexplicable, as one author has remarked, was not observed in this spot alone; for Constantinople—where the causes to which the exemption in London had been attributed do not exist—was similarly spared, without its being possible to discover the reason

of this mysterious immunity.* That it was not produced from the less intensity of the disease is proved by the fact, that the relative mortality of the epidemic was the same in London as in other situations: for of two persons attacked one died. As it seems difficult to understand why the inhabitants in one house, or street, or town, should not be as capable of propagating the malady as those in other situations—when, as in London, no means were taken to prevent communication between the sick and the healthy—we must conclude, that the spread of the disease from country to country, and from house to house, is due to other causes than human agency.

It is no less a fact, that the epidemic commenced in the centre of France, and before any of the towns on the frontier had been attacked: while it was impossible to refer the origin of the disease in Paris to the least communication with an infected town, or with infected individuals. Its simultaneous appearance, in fact, among numbers of individuals at the same moment, and in that class of persons who had the least intercourse with strangers, plainly showed that the doctrine of contagion could never account for the origin of the disease in that capital. "We unhesitatingly avow our conviction," remarks the editor of the "Lancet"—a work, be it observed, that had previously advocated the doctrine of contagion—"that it would be worse than frivolous to discuss the proposition, that some other influence than contagion was concerned—and mainly concerned—in the excitement of the disease in the French capital; and has since contributed powerfully and fatally to its propagation." In addition to this, the profession in India, including the three Medical Boards, had pronounced against

* Relation du Cholera Morbus de Londres, par Halma Grand, D.M.P.

the contagious nature of cholera several years before. When appealed to, by the authorities, on the termination of the third visitation in 1821, there were only three among the 200 or 300 medical men, then in India, in favour of the contagiousness of the disease, and two of these modified their opinion afterwards.

Subsequently to this, and before the epidemic had reached England, the organic theory of disease, as it has been termed by me, had been launched into the world. As might have been expected, this doctrine was applied to the new disease as well as to the old ones. Mr. Grainger, one of the Inspectors of the Board of Health, during the prevalence of the cholera, in 1848-9, observes: "It is, then, in filth: that is, in decomposing organic matter, that the main predisposing causes of epidemic diseases are to be sought out—filthy alleys, filthy houses, filthy air, and filthy persons."* Dr. Greenhow, another Inspector, writing in 1854, states that, in addition to the other known causes productive of cholera, "the effluvia arising from collections of night-soil were by far the most influential." Mr. Simon, also, remarks: "Now here is a remarkable cause of death. These gases, which so many thousands of persons are daily inhaling, do not, it is true, in their diluted condition suddenly extinguish life.....But, in their diluted state, as they rise from so many cesspools, and taint the atmosphere of so many houses, they form a climate the most congenial for *the multiplication of epidemic disorders*, and operate, beyond all known influences, in impairing the chances of life."† The same writer adds, in another place, "The one great pathological fact, which I have sought to bring into prominence for your knowledge and application is this—that the epidemic

* Report of the General Board of Health, for 1848-9.

† First Annual Report to the City of London, 1849.

prevalence of cholera does not arise in some new cloud of venom, floating above reach and control, high over successive lands, and raining down upon them, without difference, its prepared destruction of death; but that—so far as scientific analysis can decide—it depends on one occasional phase of an influence, which is always about us—on one change of materials, which, in their other changes, give rise to other ills: that these materials, so perilously prone to explode into one or other breath of epidemic pestilence, are the dense *exhalations of animal uncleanness*, which infect, in varying proportion, the entire area of our metropolis."*

That these conclusions are entirely erroneous, may be inferred from what has gone before. A few facts will show this. It has been already mentioned that, in Jamaica, the epidemic cholera prevailed to the greatest extent precisely in those situations where decomposing matter, both animal and vegetable, existed to the smallest possible extent. Of all the West India Islands, the ravages of cholera were, probably, the greatest in Barbados—a coral island, with scarcely any alluvial matter excepting in a few valleys in the interior. But the disease was as virulent on the limestone hills as in the valleys, if not more so. When the epidemic broke out in Bridgetown, the capital, many of the inhabitants fled to Bathsheba, a small watering place on the windward side of the island, situated on some high and perpendicular cliffs, washed by the waters of the mighty ocean. The disease, however, broke out here subsequently, and, with such fury, that the visitors and the residents fled panic-struck from the place. Similar facts have been recorded in other parts of the world. In a paper read before the Academy of Medicine in Paris, Dr. Willemin, the author, mentions that, "at Boulac, Egypt, the northern part of the town is composed

* Fifth Annual Report to the City of London, 1853, p. 35.

of large, spacious, and well-ventilated houses, wherein the wealthier part of the community dwell. The southern division is occupied by the poorer classes, and the houses are closely built together, ill-ventilated and very dirty. Strange to say, the northern district has suffered the most by the cholera; and the southern, seemingly so unfavourably circumstanced, has had but few cases. The same difference was noticed at Cairo, when the north-eastern division, rich in numerous plantations, has been severely dealt with: whilst the Jewish quarter, where *filth abounds amazingly*, was hardly touched by the disease."

Similar conclusions may be drawn respecting the emanations arising from that particular matter, about which so much has been written and such direful accusations have been made. When, however, we search for facts, instead of depending on fancies, or theories, we shall find that this matter is as innocuous as all other knds of organic matter, in the production of cholera. When the epidemic commenced in Port of Spain, Trinidad, there were fifty-one black and coloured patients in the hospital, viz., fifteen Africans, and thirty-six Chinese and Coolies. The ward of the latter class was on the ground-floor, and Mr. Mercer, the surgeon, in his Report, states: "Its means of ventilation are very inadequate indeed, for the air which traverses it, is, to say the least, most poisonous and unhealthy. This is occasioned by the immediate proximity of the large privy on its eastern side, being only eight or ten feet distant from it, and where *the constant emission of the deadly and miasmatic vapour* is continually pervading the atmosphere of the whole building, but more particularly the Chinese and Coolie ward." Having, during my visits to this hospital, experienced the non-odoriferous effects of these emanations, I can bear testimony to the fidelity of the above statement. Now mark the result. Among

those chiefly exposed to these deadly emanations, as they are termed—the noses, in actions against the judgment, generally gaining the day—there was only *one* death: among the others, ten—or 6 per cent. of the one class, and 27 of the other class. So, also, during the severe visitation of cholera in Paris, in 1832—the most severe that was witnessed in Europe at that period—the nightmen formed *the only class*, with the exception of the charcoal porters, that escaped the ravages of the disease. Hence one French writer has wickedly demanded—for to ask such a question is, of course, downright heresy—was there anything antidotal to the poison of cholera in such emanations? As the object of this work is the cause, not the *treatment*, of diseases, it is unnecessary to give an answer to this question: we may leave that for another occasion, and a different subject. There are other theories —modifications of the above—to which the same arguments and objections will apply. One is that of Professor Pettenkofer, who considers that cholera is a product of contagion and of putrefactive miasm combined. According to this writer, the contagious elements exist principally in the form of vapour; but he allows, of late, that the cholera germ, or ferment, may also be present in drinking water. The local miasm is produced in damp, porous, and polluted soils—abounding in putrescent matters. "When the two come together, their offspring will possess all, and more than all, the malignity of both its parents; but their concurrence is necessary for its existence." If so, it will not be difficult to show the fallacy of the worthy Professor's theory. Independently of the facts already adduced, if the presence of putrescent miasmata be necessary for the production and propagation of cholera, how are we to account for the prevalence and fatality of the disease on the calcareous plains of Upper

India and of Persia, in the sandy deserts of Arabia, and on the snow-covered steppes of Russia—where organic matter either did not exist, or where, if present, its decomposition was impossible? As the organic theory is utterly futile and erroneous, and as Dr. Pettenkofer states that the presence of putrid miasms are necessary for the production of his virus of contagion, the whole theory necessarily falls to the ground, notwithstanding that it has received much countenance and support in this country. It is, in truth, very similar to that, once propounded by Mr. Simon, who inferred, that the cholera germs were a sort of ferment, or zymotic matter, transported beyond the seas; and that the addition of this leaven to accumulations of organic matter produced a compound to which the evil is to be ascribed. "That which seems to have come to us from the East," observes Mr. Simon, "is not itself a poison, so much as it is a test and touchstone of poison. Whatever in its nature it may be, this, at least, we know of its operation. Past millions of *scattered* population it moves innocuous. Through the unpolluted atmosphere of cleanly districts it migrates silently, without a blow; that which it can kindle into poison lies not there. To the foul, damp breath of low-lying cities it comes like a spark to powder. Here is contained that which it can swiftly make destruction—soaked into soil, stagnant in water, griming the pavement, tainting the air—the slow rottenness of unremoved excrement, to which the first contact of this foreign ferment brings the occasion of changing into new and more deadly combinations."* When Mr. Simon penned the above eloquent lines, he forgot, or, else, perhaps, was unaware, that the epidemic cholera committed the greatest ravages, precisely in the two countries in which the population is the most scattered, viz.,

* Fifth Report for the City of London, 1854.

Arabia and the Caucasus. On the other hand, the mortality was the least, in the two towns in which the population is the densest, viz., London and Constantinople. In the Caucasus, out of *a scattered population* of 24,000, two-thirds, or 16,000, were attacked and 10,000 died. In London, there were only 14,000 deaths, at one visitation, out of a population of 2½ millions. Had the explosion of this choleraic powder magazine produced as many victims in London as in the Caucasus, in proportion to the population, 830,000 would have been destroyed. It was not in the Caucasus only that this result was observed. As has been remarked by me on a previous occasion, while narrating the events connected with the diffusion of cholera in Jamaica; "it was not in the large towns, but in the small villages and in the solitary hut on the mountain,—where the air is uncontaminated with the breath of man, where the water is derived, pure and unpolluted, from its source, and where decomposing matter did not exist, excepting in the smallest possible quantity,—that the epidemic cholera committed its greatest ravages, and prevailed in its greatest intensity." *

In addition to these theories, a somewhat novel doctrine, as already mentioned, was started in 1854, by the late Dr. Snow—a theory that has been adopted and advocated by a number of writers, from that time to the present. Dr. Snow considered, that "the *materies morbi* of cholera is something, which passes from the mucous membrane of the alimentary canal of one patient to that of another; this it can only do by being swallowed!" Hence the further inference, that the cholera evacuations of patients become mixed with the water used for drinking, and for culinary purposes; either by permeating the ground and getting into wells, or by running down channels and

* Statistical Report of the Epidemic Cholera in Jamaica.

sewers into rivers.* It was to the introduction of this specific matter that the water in the well in Broad-street, and also that distributed by the Southwark Water Company, became, according to this writer, contaminated: thus differing, like Dr. Budd, from Mr. Simon, and his *filth* theory. "In my opinion," remarks Dr. Snow, "mere impurity in the water would not cause cholera, unless it were of a specific kind—unless, in fact, the impurity had proceeded from a cholera patient, from infection." † Dr. Snow also inferred, that the germs of the disease passed into the system in other ways, as with the food, from the soiled hands of nurses and others—a very nasty idea, as well as a very nasty theory. These, according to more recent writers, are not the only means by which the cholera germs are propagated. Mr. Simon, who also adopts this theory, but whether in addition to, or in the place of the previous ones, it is somewhat difficult to say, states, that "the infective influence of choleraic discharges attaches to whatever bedding, clothing, towels, and the like things have been imbued with them." ‡ Other writers consider, that the germs are diffused in the air, and that they are thus carried from the sick to the healthy: so that by the aid of water, air, animate and inanimate objects, the propagation of cholera must be as easy as it is certain. The only wonder is, not that we have the cholera, but that we are ever without it, more especially as it is stated, that "even a single case of disease may, if local circumstances co-operate, exert a terribly infective power on considerable masses of population." §

We have thus returned, by a curious sort of coincidence, to the doctrine of the middle ages, thus verifying the

* On the Propagation of Cholera.

† Report to the Vestry of St. James's, Westminster, 1855.

‡ Ninth Report to the Privy Council, p. 229. § Ib., p. 230.

saying of Bacon, that medical men move, like the horse in a mill, in a circle; returning always to the point from which they started. Unlike the horse, they do not appear to effect much good during these revolutions: there being, according to his Lordship, *much iteration and small progress!* Not only have we returned to the doctrine of the middle ages, but we shall soon, no doubt, follow their practice. The thin edge of the wedge has, in fact, been already inserted; for the authorities have now power to remove the healthy and isolate the sick, during outbreaks of cholera.* The corpse also is to be buried "with *the earliest possible despatch*," which means, we may presume, as soon as the breath is out of the body, in accordance with the practice adopted by the inhabitants and the negroes in Jamaica. The same rules and directions apply to ships, which are to be placed under the orders of the vestry, local board, or nuisance authority—a very proper designation! With quarantine, isolation of the sick, speedy burial, and its concomitant, the dead cart, all that remains will be to paint a *red cross* on the infected houses, or places, as in days of yore, with the inscription under; "Lord have mercy upon us." This means, Lord have mercy on the poor devil, thus isolated, and cut off from all human aid, and all human consolation! Perish the thought!

Such being the opinions of Mr. Simon, we need not be surprised to learn, that the outbreak of cholera at Southampton, in 1865, was referred by him to importation. "In the summer of 1865," remarks Mr. Simon, "cholera approached us from the Mediterranean: Egypt had been badly *infected* by Mohammedan pilgrims, returning from Mecca: the infection had spread along the lines of steamboat communication, which diverge from Alexandria, as a

* See Mr. Simon's Ninth Report, *loc. cit.*

centre, to all the most considerable ports of the Levant and of southern Europe; and, presently, as was expected, a first wave of the infection touched our shores. For the first time in our experience, the attack was on our south coast. Into Southampton there came, on July 10th, and at intervals afterwards, very *suspicious* arrivals from Alexandria, Malta, and Gibraltar." * This is in accordance with an axiom previously enunciated—viz., that, "contagious current on the continent of Europe must be deemed virtually current in England." † This, we are further informed by Mr. Radcliffe, is to be accepted as an *axiom* in State medicine.‡ Not being aware that there was a contagious current on the Continent of Europe; and not having been initiated into the mysteries of State sanitation, it is necessary to pause a short time, in order to inquire what the facts are on which these official *dicta* repose. As, according to Mr. Simon, the infection came originally from Mecca, it will be better to go at once to the *fons et origo mali*, in order to verify the validity of the preceding conclusions. The history of this outbreak has been given by Mr. Radcliffe, in the Report just alluded to; and, also, by Messrs. Adams and Welsh, Army Surgeons, then stationed at Malta.§ From these Reports, we glean the following facts.

Pilgrims to the number of 200,000 had assembled at Mecca, early in the spring; soon after which cholera broke out among them, carrying off, it is asserted, from 10,000 to 15,000. That the cholera was not imported into Mecca, but that it arose there spontaneously, we may infer from the following facts. The ceremonies do not

* Twelfth Report, 1870, p. 28.

† Eighth Report, p. 43.

‡ Ninth ditto, of the Medical Officer of the Privy Council, p. 228.

§ Army Medical Report for 1865.

last many days: and the principal one — the Kurban Bairam, or Feast of Sacrifice—took place on the 4th of May. But the cholera had appeared as early as March at Makhalla, a port on the Red Sea; and it was here that some pilgrims from Singapore were attacked on their arrival. These pilgrims, amounting to 1,066, nearly all Javanese, had arrived in two sailing vessels—the *Persia* and the *North Wind*. "The disease," as we are informed by Dr. Sawas Effendi, "broke out on board, after touching at Makhalla, and after the crew and passengers had partaken largely of fish of a bad quality, and of brackish water." * According to Mr. Calvert, H.M. Consul at Jedda, the epidemic was prevailing, at the same time, at Hodeida, another port in the Red Sea. We have thus proof, that the cause productive of the epidemic cholera was in operation in this part of the world before the arrival of any pilgrims. As such, if that cause—no matter what it may be—was in operation in other and adjacent towns, the probability is, that Mecca was brought under the influence of the same malign agency, although it might not manifest its effects until a later period. If so, there can be no difficulty in accounting for the outbreak. The disease arose here spontaneously, the same as in other places; and the pilgrims were attacked, in common with the inhabitants, possibly before, being more predisposed than the latter, for a variety of reasons.

The pilgrims fled when the disease broke out, and, as a matter of course, many of them took the seeds of it with them: they fell sick on the road, or in the nearest town. Mr. Consul Calvert, in a letter to H. B. M.'s Consul-General in Egypt, states, that the mortality was great among the pilgrims on the road from Mecca to Jedda. The survivors arrived in this town on the 10th of May,

* Sur la Marche et le Mode de Propagation du Cholera en 1865.

but the epidemic was then prevailing there, many of the European sailors and Lascars having died previously, as, also, some of the Turkish soldiers in garrison. The disease, consequently, was not carried into Jedda by the pilgrims. The same result, no doubt, occurred on other roads taken by the pilgrims, but no account has been transmitted to us of these occurrences. The majority of the pilgrims go to Egypt, and thence to their several destinations; and the first ship, with 1,500 passengers on board, arrived at Suez on the 19th of May. The Egyptian Government had taken the precaution to send medical men to Suez, to examine the pilgrims on their arrival; and they reported, that no indications of cholera existed among them. Some deaths had occurred on board, during the voyage, but the cause was not known. We only learn, that the captain and his wife were both attacked on the 21st, two days after the arrival of the ship at Suez.* After this, we hear nothing more of the pilgrims, not a single case having been reported among the thousands that arrived in Egypt: had there been any, we should most assuredly have been informed of the fact. On the 2nd of June there was a case of cholera, with *a resident* in Alexandria; on the 5th, two others; and, on the 11th, the existence of the disease in Alexandria was officially announced. By the end of the month, nearly all the towns in the delta of the Nile had been attacked—Cairo, Rosetta, Damietta, &c.—spreading, at the same time, beyond the Egyptian boundaries, to Malta, Smyrna, and Constantinople. As the epidemic thus broke out in Egypt, so soon after the arrival of the pilgrims, the Contagionists of course affirm, that the germs of the disease

* It has been stated, on the authority of a private practitioner in Egypt, that one of the pilgrims was attacked with cholera on the journey from Suez to Cairo. This is not improbable.

were brought there by them. Had this been the only visitation experienced in that country, it might be somewhat difficult to disprove the assertation. But Egypt has been invaded by cholera on numerous occasions since 1831, the first outbreak: while, in the majority of instances, there were no pilgrims to carry the infection—the epidemic having appeared at a different time of the year to that of the pilgrimage to Mecca. This will be rendered apparent, by an inspection of the Table now added.

TABLE 13.—Date of the Assembling of the Pilgrims at Mecca, and that of the Outbreak of Cholera in Egypt, in the following years.

Years.	Outbreak in Egypt.	Feast of Sacrifice.
1831.	July.	18th May.
1848.	24th June.	10th November, 1847.
1850.	25th July.	20th October, 1849.
1855.	4th June.	24th August.

We thus find, that in only one of the above instances did the outbreak of cholera, in Egypt, occur soon after the dispersion of the pilgrims at Mecca. In two of the other instances, the assembling of the pilgrims at Mecca occurred seven and nine months before the epidemic made its appearance in Egypt: while, in the third instance, the cholera actually appeared in Egypt two months before the arrival of the pilgrims at Mecca. In these instances, therefore, these unjustly accused and maligned persons could have had nothing to do with the propagation of the cholera in Egypt, whatever may have been the case in the first example, that in 1831. It is also to be remarked that the epidemic sprung up about the same time of the year, in each instance; the same when there were pilgrims as

when there were not. This shows, that this modern scourge observes its own laws, irrespective of human agency and accidental circumstances. If, therefore, the disease could spring up spontaneously in Egypt at one time, it could do so at another: and this is the only logical deduction to draw on the subject. The simultaneous arrival of the pilgrims, and the outbreak of the disease, must be regarded, not as a cause, but merely as a coincidence. In fact, the epidemic only pursued, in 1865, the route it has invariably followed from the commencement, in the first outbreak as well as in the last; that is to say, it progressed from south to north. We never hear of its pursuing the opposite course. And why is this? Simply because it is a law of the disease: one that cannot be explained by a reference to the doctrine of contagion. If pilgrims and others can transport the germs of the disease from south to north, they, or others, would be able to carry them in the opposite direction. As human traffic and commercial intercourse are not, like the magnetic needle, directed always to one point, we may conclude, that the epidemic cholera was not transported from Mecca to Egypt, by the pilgrims in 1865.

We will now trace the course of the disease westward; the epidemic having spread, this year, contrary to its previous habit, to the northern shores of the Mediterranean, to Italy, France, and Spain. If this were by infection, nothing could have been more erratic than its course. Malta was attacked on the 20th of June, quarantine having been established on the 14th for all ships arriving from Egypt. Previously to this, however, or, between the 1st and the 14th June, thirteen steamboats had arrived, with passengers on board; the majority pilgrims bound for other places,—while a certain number landed on the island. It has been concluded that the

disease was imported by these passengers, the maxim of the Contagionists being *post hoc, ergo propter hoc;* although none of them were affected with cholera, either at the time or subsequently. There are, nevertheless, a few facts, that militate against this conclusion. The island of Rhodes, situated between Alexandria, Smyrna, and Constantinople, in all of which places the epidemic prevailed, entirely escaped,—not a case having occurred among the inhabitants, numbering 33,000. And yet, 222 sailing vessels, and 66 steamboats, with 2,618 passengers on board, and with crews amounting to 2,501 persons, arrived there in the course of two months. With the exception of one case in the Lazaret, with a person who had landed the day before, having arrived in a ship from Alexandria, no attacks are mentioned among either the passengers or the crews of these vessels. The island of Mitelin also escaped, although 70 steamboats anchored there, and disembarked 775 passengers. She also received, in her Port, 235 ships, with crews amounting to 1,420 men: but these vessels were all placed in quarantine. These are not the only anomalies that occurred. At Trieste there were eighty cases and sixty deaths: and although the epidemic prevailed slightly in some of the surrounding villages, the disease did not spread beyond. A quarantine of seven days had been established here, as, also, at Ancona. Nevertheless, there were 3,763 attacks of cholera, and 2,108 deaths, in the latter town. So that, the disease entered where a strict quarantine was established, and did not spread, where no precautions were adopted to prevent its diffusion—at least at Trieste. At Ancona, the epidemic spread into the province. Another curious anomaly is, that the epidemic commenced at Marseilles at the same time as in Egypt—the beginning of June. The outbreak at Marseilles has been ascribed to the arrival of a ship

there (the *Stella*) on the 11th June, from Alexandria, with ninety-seven passengers, of whom sixty-five were pilgrims bound for Algiers. Now this ship left Alexandria on the 1st June, some days before the cholera broke out there. As such, if the disease was carried to Marseilles by the pilgrims, they must have imbibed the seeds of it at Mecca, four or five weeks previously. Added to this, as none of these men were sick on their arrival, we are bound to infer, that those seeds which, when scattered over the good town of Marseilles, produced such fatal results, had remained dormant and innocuous in the bodies of the pilgrims all this time. Did these men, we may ask, in all simplicity, possess a charm, like the snake charmers, against poison, and the poison of cholera in particular? If so, their secret would be worth knowing, aye, and worth purchasing, too, at any cost. Besides, it has been before stated, that the poison of plague does not lie dormant in the system more than eight days—and the incubation of cholera is probably less. The infection, therefore, could not have been conveyed to Marseilles by the pilgrims. That it could not have been, will be rendered certain by the following particulars. According to Dr. Grimaud de Caux, there were several deaths from attacks of cholera before the arrival of the *Stella*—on, or before, the 9th of June.* Dr. Sèlim-Ernest Maurin says, on the 7th,† "These cases exhibited all the characteristic symptoms of Asiatic cholera—blueness of the skin, suppression of urine, raucous voice (*vox cholerica*), coldness, &c. As such, the disease, instead of having been imported, must have arisen spontaneously in Marseilles. This inference is confirmed by other facts. The cholera broke out at Toulon, a purely naval arsenal; which has little communication with other ports, and to which neither com-

* Gazette des Hopitaux, 17th Oct., 1865. † Ib., 30th Sept.

mercial nor passenger ships resort. And yet the ravages of the disease were as great here, if not greater, considering the difference in population, than at Marseilles. In the latter town, the deaths were 1847; in Toulon, 1,282. At Arles and Aix, also, the epidemic committed greater havoc, than at the port of its presumed disembarcation. It spread through the south of France, and reached Paris on the 13th October. This appears to have been its boundary northwards, with one exception. The disease broke out at Altenburg, a town about twenty-four miles from Leipzig, but did not extend beyond. Perhaps, the Contagionists will be kind enough to inform us, why so infectious a disease was confined to this single spot, the inhabitants not having been placed in quarantine, and no wall having been built around the town?

We will now turn to another part of the world. In Spain, the first place attacked was Valencia, which the disease reached on the 3rd July—seven days before it broke out at Gibraltar. The selection of this place is another anomaly of the epidemic, regarded by the doctrine of contagion; for this town could have had no direct communication with Alexandria, or other infected port. What is called the Port of Grao, situated a short distance from Valencia, is merely a beach, at which only small coasting vessels think of touching. All vessels and ships bound to this part of Spain go to Alicante, where there is a good harbour. And yet here, where you might have expected the first cases to have occurred, the epidemic did not make its appearance until September—two months afterwards. The epidemic spread into the interior of Spain, and prevailed rather extensively; 486 towns, villages, and hamlets having been attacked, before the subsidence of the disease on the 1st of November. Madrid was attacked on the 9th of October; the outbreak

being so sudden that it can only be compared to some violent explosion. Within the *first three days*, there were 3,000 cases, of which number 1,000 proved fatal. The termination of the epidemic was equally sudden, having ceased entirely on the 15th—six days after its appearance. To talk of infection, in a case like this, would be sheer nonsense.

With the mention of another anomaly, we may close this part of the subject. As is well known, a large number of the pilgrims, that pass through Egypt, go to Algiers. As such, we should have thought, that the first cases of cholera which occurred on the western shores of the Mediterranean would have been in Algiers. But the epidemic did not make its appearance there until September—three months after the arrival of the pilgrims. Really this disease must be a very ill-mannered one, thus to set at defiance all the laws of contagion, and the rules expressly laid down for its guidance by the Contagionists.

Having thus endeavoured to prove, that the epidemic cholera was not introduced into Egypt, and other countries, by the pilgrims from Mecca, we may now return to the point from which we started, in order to ascertain if the disease was imported into Southampton, in 1865, as stated by Mr. Simon. Fortunately, a special Report on this outbreak has been drawn up by Dr. Parkes, and no better person could have been employed to carry out such an investigation. From this Report, we glean the following particulars; merely observing, that the disease was supposed to have been introduced by one of the steamers belonging to the Peninsular and Oriental Company, on board of which there had been some cases of cholera. According to the returns furnished by the Peninsular and Oriental Company to Dr. Wiblin, the Medical Super-

intendent at this port, it appears that, of twenty-three steamers which sailed from Alexandria for Southampton, *viâ* Malta and Gibraltar, at a time when cholera was committing the most serious ravages amongst the population of those places, there had been only seven deaths from cholera; and these had occurred on board the *Ellora* and the *Nyanza*.* Three of these deaths were on the outward bound passage, between Marseilles and Alexandria; the other four, on the voyage from Alexandria to Southampton. Two of these were on board the *Ellora*, one death having occurred two days before she left Alexandria, on the 28th June, and the other, two days after. The remaining two deaths occurred on board the *Nyanza*, during the voyage from Alexandria to Southampton. They were both passengers, one of whom died on the 9th July, the day after leaving Alexandria; and the other, on the 17th July, at Gibraltar—five days before the arrival of the steamer at Southampton. It is thus apparent, that there were no cases of cholera on board the regular steamers, at the time of their arrival at Southampton. But Dr. Parkes states, on the authority of Dr. Miller, the surgeon, that there were thirty-one cases of diarrhœa on board the *Ellora* during the voyage—the last case having occurred two days before she entered the docks. According to Dr. Wiblin, there was diarrhœa on board the *Nyanza* also, but the number of cases was not known. Still, it does not appear that any one landed suffering from diarrhœa. In answer to a question from Dr. Parkes, Dr. Wiblin replies: "From the most careful inquiries instituted, I am unable to ascertain that any cases, either of cholera or diarrhœa have been landed at the Port of Southampton."†

* Report by Professor Parkes, M.D., on the Outbreak of Cholera in Southampton, 1865. Eighth Report of the Medical Officer of the Privy Council, p. 425.

† Loc. cit., p. 425.

Had it been otherwise, however, we should then have found it difficult to account for the spread of the disease in Southampton: none of the patients first attacked having had any relation, or communication, with the ships, or shipping. The first case was that of a female, and she was not attacked until the 12th of August, twenty-one days after the arrival of *the last* of the two infected ships. Then, again, there was no fresh attack, until the 22nd of September—an interval of forty-one days. How, then, did this patient receive the infection? It could not have been from the ship, nor yet from the first person attacked. Were it shown, that these patients had been in communication with each other, it would be impossible to infer, that the one had imbibed the infection from the other, the time being too long for the poison to lie dormant in the system. But we are told distinctly that this patient had not been near any other. Dr. Parkes observes: "As already stated, it cannot be shown that Rose, Westcott, or Hembery (the first three persons attacked after the young woman on the 12th August), had been near any other patients. All inquiries have failed in tracing directly their sources of exposure."

While, on the one hand, personal contact with infected persons could not have been the cause of the outbreak, so, on the other, the contamination of the water could not have been the exciting agent. Dr. Parkes remarks: "The supply at Southampton is from the chalk, and is quite free from deleterious matter. As the service is continuous, there is no local contamination from cisterns:" and, we may add, from any other source, as the dejecta of cholera patients. Besides, as "all the persons attacked drank the town water, which was consumed also by the other 53,000 persons in Southampton," the disease would not have been thus limited in its range had the water

become contaminated. Nor could the germs of the disease have been preserved in the soil from the overflowing of cesspools, and the extrication of deleterious miasmata into the surrounding air, for all the houses in Southampton are sewered: while "it so happens," remarks Dr. Parkes, "that the sewers act better, in that part of the town where cholera prevailed, than in some wealthier parts. In most of the old small houses, the old privies outside the houses have been converted into water-closets, which have been well trapped.....Every evacuation was at once washed away and carried into the sea." The cases, in fact, were too much scattered to refer the disease to any local cause; at the same time that they broke out almost simultaneously at separate points, far removed from each other. The earliest cases—always excepting the first case in August—were at Weston Common, a small hamlet about two miles from Southampton: the next, in Southampton, and then at Bitterne, a village two miles from the town and one and a-half from Weston. There were also cases (6) at a later period at Itchen, a small fishing village, independently of those in the suburbs of Southampton—at St. Deny's and Freemantle, situated to the south and west of the borough. This is not how an imported and infectious malady would have spread: the first cases ought to have been, not in the neighbourhood, but in the town itself, and with those in direct communication with the ship. From this it should have spread, like radii from the centre to the suburbs; but the directly opposite course to this was pursued. Then, again, if the disease be so contagious, that a ship, on board of which there had only been two cases of cholera, could infect a town, how are we to explain the exemption of the 52,940 persons out of the 53,000 inhabitants of Southampton? Instead of two patients, whose bodies had been com-

mitted to the deep some days before the arrival of the ship; there were here sixty cases, or *foci* of infection, as the term is, scattered among a large population. And yet, with the exception of sixty, all these 50,000 persons escape an attack; without it being possible to refer the exemption to any apparent cause. Referring to the cessation of the disease at Weston Common, Dr. Parkes observes: "These five cases were all that occurred in this locality. Nothing whatever was done to arrest the disease, and there were numbers of persons in the adjoining houses, who must have been susceptible, yet it did not spread. The causes of the cessation lay, therefore, in no preventive action."—(P. 401.) Had the cholera been "stamped out," according to the last patent method of preventing disease; and had the 52,940 uninfected persons in Southampton been sacrificed on the altar of scientific ignorance and popular credulity, we could have understood the cessation of cholera in this town. At present, however, it remains as much a mystery as the origin of the disease.

That this importation theory is an inconsequential one, will appear to be more particularly the case, if we turn to other facts. In the quarter ending June, 1865, there had been 32 deaths from cholera, and 706 from diarrhœa, registered in London. These are merely the deaths, how many cases of cholera there were we have no means of ascertaining. One of the latter has been recorded, and it will be interesting to give the particulars, as showing that some of these cases, if not all, were really attacks of Asiatic cholera. In May, 1865, a case, described as "Asiatic cholera" was admitted into Guy's Hospital under the care of Dr. Wilks. This case was reported in the "Medical Times," * and the reporter stated, that he had employed this term, because "it was such a case, as would have been so called during the prevailing epidemic."

* June 3, 1865.

And he added: "Dr. Wilks said, that, *every year*, he saw one or two such cases, but seldom so early in the year as this." Whence, then, did these cases receive the infection? Not from Southampton, nor yet from Alexandria, for they occurred not only before the arrival of the steamer at the former port, but some of them before the outbreak in Egypt, which did not commence until the beginning of June. Nor could they have received the germs of the disease from any other source, as the epidemic did not make its appearance in the South of Europe until July, and in the north, until September. Granted that the infection had arrived by electric telegraph, or otherwise, from the ports of the Red Sea, infected in April and May, we should, even then, be at a loss to account for the cases and deaths that occurred previously to this. Independently of the cases already mentioned, there were 934 deaths from cholera, and 16,432 from diarrhœa, registered in England, and 156 from cholera, and 2,364 from diarrhœa, registered in London, in 1864. As this was actually before the epidemic appeared in Egypt, and before it recommenced in Russia or any other country in Europe, there was no possible source to which we could look for the importation of the disease. Besides, if the disease had been introduced by infection, it would have spread by infection, after its arrival. But this was not the case, as the cholera did not become epidemic in England until two years after. Under these circumstances, the only conclusion to be drawn on the subject is, that the epidemic cholera arose spontaneously in England in 1864, and in Southampton in 1865.

As will probably have been remarked, nothing has been said of the manner in which the infection was conveyed in the preceding instances. The truth is, these writers have been entirely mute on this part of the subject, although

it is an important one. There is wisdom sometimes in silence. In the next visitation of cholera, that of 1866, these writers were not so discreet: they have given us a full and minute account of the manner in which the germs of the disease were supposed to have been conveyed, from point to point in London. We shall therefore be better enabled to test the value of their arguments and conclusions: and to ascertain, whether the infection of cholera be really conveyed in the manner pointed out by these theorists.

As will be remembered, the epidemic expended its fury, principally, on the East-end of London, in the area supplied by the East London Water Company. This coincidence was not likely to be passed over by those who adopt the water theory, and by those writers who are prone to conclude, that coincidence is cause. Attention was first directed to the circumstance, in the weekly Returns of the Registrar-General; and Dr. Farr and other investigators having inferred, that the water had become contaminated, in some way, four different Commissioners were appointed to inquire into the validity of the charge. These were, the River Commission, and those of the Privy Council, of the Board of Trade, and of the "Lancet." A New Zealander, or other stranger from the Antipodes, might have inferred, from the apparent alarm and excitement thus occasioned, that such a visitation had never occurred before. So far from this being the case, there was really nothing peculiar in this outbreak; while it was less severe than the three previous ones. Although the history of this visitation has been honoured with a special report by the Registrar-General, and another by the Medical Officer of the Privy Council, drawn up by Mr. Radcliffe, the facts themselves lie in a nutshell: the rest is all hypothesis.

It appears that the water, then supplied by this Company, was drawn from the river Lea, at Lea Bridge—a point considered to be free from the influence of the tide, and of the sewage discharged into the river lower down. The water, after being filtered, was stored in two covered reservoirs: and was thence distributed to the houses supplied by the Company. In addition to the above, there were two open reservoirs, but the water in these, drawn from the same source, was never used, excepting in cases of emergency. Thus, there was no way of accounting for the contamination of the water, under ordinary circumstances: but, then, some extraordinary circumstances were discovered. It transpired, that the supply in the ordinary reservoirs being short, water was admitted into them from the open reservoirs, on *three* different occasions, or *days*—in March, June, and July. It was therefore inferred, that the water in the open reservoir had become contaminated in some way; and that the distribution of this infected water had spread the disease over the district. This was the first supposition.

It was next found, that a sewer, which discharged its contents into the river at Old Ford, was situated near to one of the open reservoirs—within 600 yards. Another inference was therefore drawn, viz., that some of the sewage had percolated through the banks of the river into this open reservoir, and contaminated the water. Here, then, we have supposition No. 2.

As it so happened, this sewer was connected, by a drain, with the houses at Bromley, in which the two first cases of cholera, in this district, occurred. As a matter of course, it was at once decided, that the dejections from these patients had contaminated the water in the river, as, also, that in the reservoir—the open reservoir—by percolation through the bank of the river. This was No. 3

supposition: and it was on these three suppositions, that the charge of impurity, or infection, of the water rested. Although the chain of causation would thus appear to be complete, and although it may be a very pretty chain to look at, it is made, not of metal, but of straw; and will break asunder at the first trial of its strength.

Assuming that the water in the river did percolate through the bank into the reservoir—and this would be the case, according to the laws of hydrostatics, if the reservoir were empty, or the level of the water lower than that in the river—what right had these theorists to infer, that any portion of the *dejecta* would find its way into the reservoir? The infectious matter would not only be dissolved in the whole contents of the sewer, but be speedily diffused in the surrounding water of the river. As, also, this river is a tidal one, it would either be immediately carried out to sea, or else be carried up by the flood-tide beyond the reservoir. When it returned, it would be so diluted, that it might puzzle even Dr. Farr to calculate how small a quantity would be contained in a gallon, or even a ton of water. As, also, only a very small quantity of water could find its way into the reservoir, and this only under exceptional circumstances, it is almost morally impossible that any portion of the dejecta of these patients could have found its way into the reservoir.

It is also to be remembered, that the water, in the open reservoir, was only used on a single day, after the two first cases occurred, the latter end of June: so that we are forced to believe, if the conclusion of these writers be a correct one, that the distribution of this water, contaminated with so small a portion of infectious matter that it is impossible to define the quantity, had infected the whole district. Then, again, the epidemic was not confined to the area supplied by the East London Water

Company: but was spread over the greater part of London, only to a much less extent—the deaths, during the first five weeks, in the East London District having been 2,394, and, in the other districts of London, 434. In what way then, it will be asked, did these persons receive the germs of the disease? According to Dr. Farr, the spread of cholera over the rest of London was due to one or more of the following circumstances: "The elements of the disease, in 1866, must either have been diffused (1) by personal contact, (2) by translation through the air, (3) or by dissemination in the vapour of sewers or (4) by the various waters." It was to the influence of the latter, more particularly, that Dr. Farr attributed the spread of the disease from the east to the west end of London. He remarks: "It may appear, at first sight, impossible, that the cholera flux of one or more patients should produce any effects in the waters of a river like the Thames. But living molecules, endowed with the power of *endless multiplication,* are inconceivably minute; and may be counted by *millions in a drop of water.*" This, of course, is the germ theory, the fallacy of which has been already pointed out. This endless multiplication, as then shown, would be fatal to the doctrine.

Allowing, however, that the hypothesis is a correct one, we shall then have to inquire how it happened that any of the inhabitants, supplied with water from the East London Company, escaped the disease? Severe as the visitation was, in this part of London, the deaths only amounted to 7 per 1,000 of the population: whereas only 7, according to the above theory, ought to have been left alive. Then, again, how are we to account for the limitation of range of the disease in the other parts of London, notwithstanding that *foci* of infection existed in every district? Exclusive of the East-end, the total deaths in London, in

1866, from cholera and diarrhœa, were 4,410. Allowing three cases to each death, there would thus be 13,000 infected persons scattered over the various districts of London, irrespective of the East-end. Including the latter, there must have been nearly 20,000 patients affected either with cholera or diarrhœa that year; and the disease, we are told, is propagated as readily by the latter affection as by the former. As such, if two patients could infect 20,000, how many, we may inquire, would 20,000 infect? A schoolboy can answer the question; while other persons will know, without making the calculation, that London, if this doctrine were true, would have been half depopulated, in 1866, by these anti-Malthusian entities! But whatever may be the nature of the virus, or the specific matter productive of the disease, it will be easy to show that the infection was not conveyed in the water to those districts supplied by the Thames Companies. Had such been the case, we should have found that, next to the water of the Lea, the ravages of the disease were the greatest in the districts supplied with Thames water. But the reverse is the case, the rate of mortality being lower in these districts than that of any of the districts supplied by the other Companies. This will be rendered evident by a glance at the following Table, extracted from the Registrar-General's Tables.

TABLE 14.—Rate of mortality in the districts supplied by the following Water Companies *:—

	Per 10,000.
East London	70.50.
New River †	8.72.
Kent Company, viz., Wells and Ravensbourne	15.30.
Southwark and Lambeth	5.89.
Other Thames Companies	3.60.

* Cholera Report, p. 54. † Draws part of its supply from the River Lea.

Although the rate of mortality is thus high, in the area supplied by the East London Water Company, it does not follow that the higher rate was caused by the state of the water. If the cholera had been propagated in the manner now pointed out, the waters of the Lea would necessarily have been the first contaminated. As such, the first cases ought to have occurred in the area supplied by this water. So far from this being the case, there was a death from cholera on the south side of the Thames nearly a month before the two attacks in the East London districts: while there were several others in other parts of London during the interval. This is rendered evident in the following Table.

TABLE 15.—Date and Residence of the following fatal cases of Cholera and Choleraic Diarrhœa, in London, in 1866 *:—

Date.	Residence.	Age. Years.	Disease.	Deaths.
May 28	North-street, Walworth	10 m.	C.	1
„ 31	Charles-street, Bethnal-green	5	C. D.	1
June 1	North-street, Marylebone	4 wks.	C. D.	1
„ 2	Duke's-lane, Kensington	Adult.	C.	1
„ 6	South-street, May Fair	6 m.	C. D.	1
„ 12	Archibald-street, Bromley	8 m.	C. D.	1
„ „	Dawson's-place, Mile End	11 m.	C. D.	1
„ 13	Swan-court, Newington	55	C.	1
„ 21	Old Bethnal-green-road	—	C. D.	1
„ 22	Wellington-place, Holloway	Adult.	—	1
„ 23	Nursery-place, Walworth	7 wks.	C.	1
„ 25	New-street, St. James's, Westminster.	Adults.	C.	2
„ 27	Priory-street, Bromley †.....	Do.	C.	2

Some of these deaths, it is true, were occasioned by choleraic diarrhœa, as it is termed; but such cases are as genuine attacks of cholera as the more characteristic ones.

* Extracted from Table 24 of the Registrar-General's Report.

† These were the two patients to whom all the mischief was ascribed!

This form of the disease prevails principally with children: and it is observed, not only at the commencement, but at the height of the epidemic. The susceptibility of children to the malign influence of the disease is so great, that they succumb before the stage of collapse becomes developed. Hence, also, the reason why they are frequently attacked before the epidemic becomes general, and before those of maturer age.

Then, again, if the spread of the disease were due to the contamination of the water in the river Lea, by the two patients referred to, the greatest number of cases, after the death of these individuals, ought to have been in the East London area. But here, also, the facts are in opposition to the theory. From the 1st to the 7th of July there was not a single death in the East London districts, while there were 9 in the districts supplied by the other Companies, as shown in the following Table.

TABLE 16.—Deaths from Cholera, on the following days, in the water-fields of the several Water Companies.*

Date.	G. Junction, Middlesex, and Chelsea.	Southwark and Lambeth.	New River.	East London.	Kent Company.	Total.
July 1	1	1	1	—	—	3
„ 2	—	—	2	—	—	2
„ 3	—	—	—	—	—	—
„ 4	—	—	—	—	—	—
„ 5	—	1	2	—	—	3
„ 6	—	1	—	—	—	1
„ 7	—	—	1	1	—	2
„ 8	1	2	—	—	—	3

NOTE.—In addition to the above, there was one death in Hammersmith on the 29th, and another in Clerkenwell, supplied by the New River Company, on the 30th of May.

* Extracted from Table 23 of the Registrar-General's Report on Cholera, p. 42.

Other facts lead to the same conclusion. Had the contamination of the water, in the river Lea, been the sole cause of the propagation of the cholera, something like uniformity ought to have been observed, in the different districts to which this water is distributed. But nothing can be greater than the variation in the rate of mortality in the sub-districts supplied by the East London Water Company in 1866. The deaths varied from 171, to 10,000 living, in the district of St. John, George-in-the-East, to seven in East Haggerstone, and to only *three* at Stamford-hill. This is not all. In some of the sub-districts supplied by this Company, there were no deaths, no attacks. Mr. Radcliffe remarks:—"At Lower Forest, Wanstead, Walthamstow, and Buckhurst-hill, east and north of Stamford-hill, across the Lea, no deaths directly traceable to the epidemic took place." Leyton and Leytonstone, also supplied with water from Old Ford, suffered only to a slight extent late in the autumn. "Finally, in North Woolwich, which obtains its water solely from the same source, *not a death* from cholera, and *not a case* of the disease, so far as I can ascertain, occurred."—(P. 324.) The immunity of these districts, to which the same water was supplied, as to those where the rate of mortality amounted to 170 and 155 per 10,000, is an anomaly that can never be explained by a reference to the doctrine now under review. It would be easy to explain the exemption of individuals, on the supposition that they did not drink the water: but the exemption of whole districts cannot be referred to such a cause. Besides, there is precisely the same anomaly with respect to individuals as with districts—those who partook largely of the water escaping, while those who did not drink it were attacked. Dr. Letheby, in his Report to the Commissioners of Sewers, observes:—"While there is ample proof of the propagation of choleraic disease by certain

well-waters of London, there is, at present, no evidence that the public water supply has had anything whatever to do with the localisation or spread of the recent epidemic. Already, in the Eastern parts of London, where the disease was most fatal, the medical officers of health have reported to this effect; and have shown that the largest mortality from cholera was of persons who were *not* water-drinkers, and that teetotallers and others, who drank largely of the East London water, in its unboiled state, have been singularly exempt from the disease. At the City of London Workhouse, at Bromley, where the inmates partook of well-water alone, there were twenty deaths from the epidemic in one week: whereas, at the East London Workhouse, at Hackney, which is supplied with the East London Company's water, there has not been a single death from cholera." * This evidence is the more valuable, as it comes from a gentleman who is, or was, a believer, as we have seen, in the production and propagation of disease by means of impure water. It is not necessary to pursue this part of the subject farther: it must be evident, from what has gone before, that the spread of the cholera in London, in 1866, was not due to the contamination of the water, either of the Lea or of the Thames.

We might have arrived at this conclusion, *à priori*, even supposing that cholera dejections be infectious, or that they contain the germs of the disease. It is well known that many morbid substances which, when introduced directly into the blood, would be attended with fatal results, can be taken into the stomach with impunity. Thus, carnivorous animals can take, without any ill-effect, not only putrid but diseased substances of every kind.

* On the Quality and Quantity of the Water supplied to the Metropolis in the year 1866.

This has been amply demonstrated by some experiments conducted at the Veterinary College at Alfort in France. In these experiments, M. Renault has shown—1. "That the dog and the pigeon can eat, without danger, all the products of secretion, no matter of what kind: the remains of carcases, cooked or not, proceeding from animals affected by contagious diseases—that is to say, the glanders, gangrene of the spleen, hydrophobia, the contagious typhus, the pneumonia of horned cattle, and the contagious epizooty of fowls. 2. The same result was obtained with fowls, with the exception of the last disease, when no deductions could be drawn, in consequence of their being necessarily placed, at the time, in an epidemic atmosphere." * As regards the exemption of carnivorous animals from any ill-effect, when fed on putrid substances, it must be referred, as previously remarked, when considering the effects of ordinary putrescent matter, to the anti-septic properties of the gastric juice—a property which is also peculiar to the gastric juice of man. Hence we may infer, that any morbid matter existing in the dejecta of cholera patients, and which found its way, subsequently, into the stomach of other persons, would be decomposed and rendered innocuous.

There are, it is true, certain experiments, which would appear to negative this conclusion. The cholera fluid has been given to animals with the effect of producing, in some of them, effects somewhat similar to cholera, and of which a few have died. But all deductions drawn from experiments conducted at an epidemic period, during the prevalence of cholera, and with animals as liable to the disease as man, are utterly valueless. Rats and mice—the latter having been more particularly the subject of

* Etudes Experimentales et Pratiques sur les Effets de l'Ingestion des Matières Virulentes. Lu à l'Institut, le 17 Nov., 1851.

experiment—would seem to be peculiarly susceptible to the epidemic influence. At Port of Spain, Trinidad, not a rat was to be seen, on the cessation of the cholera; although they swarmed previously, and infested every store and warehouse. When also we know how slight a circumstance will bring on an attack of cholera with man—a fit of indigestion, a dose of medicine, fatigue, and even mental emotion—we cannot be surprised at the animals subjected to these experiments having similar attacks. It would have been somewhat strange, if they had not; while we may infer, that paper and fluid, saturated with other matters besides cholera dejecta, would have produced precisely the same effects. These inferences are apparently confirmed by the results obtained by Dr. Sanderson, in the experiments he carried out in 1866. These experiments were conducted in the months of September, October, and November. In the two former months, many of the animals—mice and guinea-pigs—sickened, and several died, with indications of alvine disorders, more or less marked. But of three experiments conducted in precisely the same way, with twenty-eight mice, in the month of November, no morbid effect was produced.* In the first two months the epidemic was prevailing; in the latter, it had ceased.

That the germs of cholera are not contained in drinking water, and that the disease is not propagated by this means, is the conclusion at which those medical men in India, who have had the best opportunity of forming a correct opinion on the subject, have arrived. Dr. Bryden, attached to the Statistical Department of India, and who has been engaged, for some years, in investigating the diseases of that country, remarks :—"Everyone feels,

* Ninth Report of the Medical Officer of the Privy Council, p. 450.

that it would facilitate much the study of the phenomena of cholera, could we hold as a truth, and not as a theory, the constant or frequent presence of the cholera germs in the evacuations; and could we trace to this, as a source, the infection of localities, or the poisoning of the water supply. In this country, we act upon the belief, in the transmissibility of cholera in such a manner; and the precautions used against the possibility of infection, from such a source, have been elaborated to the last degree; and yet, it is a melancholy truth that the liability of our cantonments and regiments to cholera, in its worst form, is as great now as it has been at any time during the past fifty years, and that the *absolute mortality is on the increase.** † That the theory is not true, can be shown by other circumstances. In the first place, cholera sometimes appears under circumstances, in which it is morally impossible, that the water could have become contaminated, in the way now alluded to, or in any other way. At Cadiz, and at the Island of St. Thomas, both which places have been ravaged by cholera, rain water is alone

* Report on the Cholera of 1866-68, p. 207.

† "The health of the British army in India," remarks Dr. Farr, "has already been improved: and its losses by cholera, although still too great, are no longer what they were in the years before the mutiny.....To render the generation of great epidemics of cholera rare, nay, *impossible*, India has only to carry out the measures which have proved *efficacious in England!*" (Cholera Report, 1866, p. xc.) The wish, in this instance, must have been father to the thought, as the following facts will show.

"In 1869, the proportion of deaths from cholera, in India, was 16·46 per 1,000 of strength; the general mortality being 42·89. This is the highest death rate that had been observed for the previous eleven years, with the exception of 1861, when a great epidemic of cholera passed over the north of India. In that year, the gross mortality was 45·93 per 1,000, and that for cholera 23·73." —Sixth Report of the Sanitary Commissioner for India.

used for domestic purposes, the wells being brackish. It is stored, either in brick wells, or in cisterns, into which the excreta could not possibly find an entrance. Again, the water at Port of Spain, Trinidad, and also at Kingstown, St. Vincent, has been brought from the neighbouring mountains—distant five or six miles—and as there is a constant supply, and the water is not stored, its contamination by the means under consideration would seem to be impossible. Yet the disease presented as severe a form—as I can bear witness—in both these towns as in other parts of the West Indies. Stronger proof even than this can be adduced. Cholera suddenly broke out on board the *Undaunted* frigate, on her passage down the China Sea. This continuing for some days, and a number of the crew having died, the surgeon recommended the captain to change the course of the ship. This was no sooner done than the disease ceased—not a case having occurred afterwards. A similar instance has been recorded since. The ship *Gertrude* left Calcutta on the 21st May, 1859, with a party of invalids in medical charge of Dr. Tulloch. The cholera was prevalent at Calcutta when she left, but all on board continued healthy, until the 15th June, when in lat. 4° S., and *twenty-four days' non-communication* with the shore, and hundreds of miles from the land, an invalid was attacked with cholera, and died the next day. The chief officer of the ship, the strongest and healthiest man on board, was attacked on the same day, and died in three hours and a-half. Another invalid, a boy, and a sailor, were also attacked and died—the last on the 25th. The visitation then ceased. In these cases it is certain that the water could not have been the cause of the outbreak, otherwise it would not have ceased so suddenly. Those beautiful little anti-Malthusian entities, that these theorists discourse on so pathetically, would

have gone on increasing and multiplying until all the tanks in the ship had been filled with them, and all the water contained therein contaminated. In that case, instead of five persons being attacked, every man on board would inevitably have been brought under the malign influence of the same operating cause. The same remarks will apply to the probability of the germs of infection having been carried in some other way, as in the clothes, &c. Such an argument, although a fallacious one, might be used in the case of the *Gertrude;* but as regards the *Undaunted,* there was no cholera at the port she had just left—Macao—nor had she been within the focus of the disease previously.

There is another circumstance which would seem fatal to this water theory. If the germs of cholera were diffused in water, and if the spread of the disease were due to the multiplication of these germs, the disease ought to present a greater and greater degree of malignancy the longer it continued. As these germs or entities, increase, as we are told, so rapidly, that two patients are sufficient to infect a whole town, in the course of a few weeks, the water ought to be nearly saturated with them, when there are 20,000 or 30,000 infected persons. It is precisely, then, we should infer, that the disease would assume its most malignant and fatal form—the quantity of poison contained in a glass of water being a hundred or a thousand times greater than at the commencement of the outbreak. The contrary, however, is the case, the cholera almost invariably presenting a more malignant form at its commencement, and a milder form towards its termination. For example, of ninety-eight patients admitted into the Hotel Dieu, Paris, during the *first three* days of the visitation, in 1832, no less than ninety-three died—95 per cent. So, also, of the 509 cases of cholera admitted into

the London Hospital in 1866, the mortality was at the rate of 85 per cent. in the first week (July 10th to 17th), and only 39 during the last week.* Then, again, it is precisely when these entities have increased to the greatest possible extent; and when there are millions in every drop of water, that the disease declines and subsides more rapidly than it rose.

With such facts before us, we may conclude, without much risk of error, that this *infective* water theory, like the organic one, is unsound, illogical, and false. As regards the fact, that the disease prevailed principally in the area supplied by the East London Water Company, this should have been regarded merely as a coincidence, not a cause. It is characteristic of the epidemic cholera, not only to attack one district in preference to another, but to attack one locality at one visitation, and another at the next, leaving the former untouched: and this, too, when the water supply and all external circumstances remain the same. If, however, we are to look to local causes, in all these instances, for an elucidation of the phenomenon, there will be no end of theories, false assumptions, and false accusations. For instance, suppose that the cholera should prevail exclusively in a particular street, or one side of that street, as sometimes happens: and that a particular baker, or butcher, supplies those houses, and not any others. Are we to accuse them of having poisoned the inhabitants of these houses? We may as well do so, as to accuse the Water Companies: the amount of evidence that we possess, in the one case, being about the same as in the other—mere assumption. This point has been very well illustrated by Dr. Letheby. Referring to the prevalence of cholera, in the area of the East London Water Company, he remarks: "I might make an exact comparison of

* Clinical Lectures and Reports, vol. iii., p. 437.

a similar kind: there are two Gas Companies supplying the East-end of London, and it is a remarkable fact, that, in the district supplied by the Commercial Company, cholera has existed; and in another district, supplied by the City of London Company, the cholera has not existed. There is just the same amount of coincidence and parallelism between these two cases, as between the Water Companies." * In fact, if we go on in this way, we shall be plunged at last in the same quagmire as the Board of Health in Jamaica, which assigned forty different exciting causes for the production of cholera in Kingston—beginning with pigstyes and ending with vagrants! And yet, the mortality in this town was four or five times less, than in some other localities in the island, where not one of these causes happened to be in operation.

Having thus rejected the water theory, it only remains to ascertain, if there be any other way of accounting for the spread of cholera in the East-end of London. The sewers, or the emanations from them, have sometimes been accused of producing cholera, as well as typhoid fever. But this could not have been the cause in this instance. Dr. Farr remarks: "In the water-closet system, the cholera flux in vapour, if it is not sometimes generated, is sometimes distributed in sewers, and is driven into the dwellings of the people. But, then, it so happened that, in East London in 1866, several districts in the group, so heavily visited by cholera, lie in the particular region which then derived no advantage from the contemplated low-level sewer." †

Baffled thus in our inquiries, and being unable to

* Evidence before the Select Committee on East London Water Bill, p. 426.

† Report, p. xvi.

account for the diffusion of the cholera, in London, the next thing is to inquire what the cause of the outbreak was? "It may be assumed," remarks Dr. Farr, "that the cholera, in its Asiatic form, was brought into England in 1866." Mr. Radcliffe has drawn the same conclusion. "The history of the epidemic in its entirety points, therefore, to the transmission of the disease to the metropolis from localities previously visited by it in Western Europe." And yet the writer adds, while narrating the particulars of the two first attacks in the East-end of London: "So far as the family knew, and that knowledge was such as to justify them speaking with tolerable positiveness, neither the man nor his wife had had any communication, incidental or otherwise, with individuals arriving from infected districts on the Continent. They both worked at a brush manufactory in the neighbourhood, and had not left their residence for some time previously, with the exception of having taken a walk in Greenwich Park some days previously."* More than this, Mr. Radcliffe states: "That he is unable, after diligent and protracted search, to bring forward any facts, which would establish the direct dependence, by transmission, of the recent outbreak upon the outbreaks previously occurring in Western Europe." And it is then added, in another place: "The freedom of the metropolis and the Port of London, from imported cases of cholera, prior to July, 1866, notwithstanding the incessant and rapid intercourse maintained with infected districts of France and Holland, is certainly remarkable: but the facts known represent *our ignorance* rather than the accuracy of our knowledge." Precisely so! a truer word than this was never written. The arguments and the facts brought forward by Contagionists, in support of their doctrine, are, in reality, evidence, not of

* Loc. cit., pp. 285 and 288.

the amount of knowledge, but of the amount of ignorance that they possess on the subject! The Anti-contagionists, perhaps, may be able to throw a little light on the subject, by seeking for the cause of the outbreak, not in other countries but in our own.

If we extend our view beyond Priory-street, Bromley, and to a date long anterior to the attack of the two unfortunate victims of cholera, we shall find that the disease was prevailing in a number of districts in England, as well as in London, from the very commencement of the year. This is shown in the following Table:—

TABLE 17.—Deaths from cholera and diarrhœa, in 1866, in the following months, in London and in England.*

1866.	London.		England.	
	Cholera.	Diarrhœa.	Cholera.	Diarrhœa.
January .	3	50	13	693
February .	3	80	14	676
March . .	1	67	11	744
April . .	4	1	11	606
May . .	6	67	88	669
June . .	20	170	88	929
Total . .	37	435	225	4,317

It is thus apparent, that there were cases and deaths from cholera, as well as diarrhœa, in England and in London, in the month of January, and in every subsequent month until the outbreak, the end of June. Independently of the cases of diarrhœa, there were, during this period, no

* Extracted from the Registrar-General's Report. Tables 19 and 20, pp. 38 and 39.

less than 225 deaths registered as cholera in England. As we have no account of the epidemic prevailing on the Continent of Europe, until the middle, or the end, of April, none of the cases, that occurred previously to this date, could have received the infection from abroad. Besides, is it not the height of absurdity to infer, that one or two persons coming from a distance can infect a whole town, or a country, when several hundred patients, scattered among the whole population, are unable to produce the same effect. Calculating three attacks to each death, there would have been, during the six months previous to the outbreak in London, 786 cases of cholera, dispersed over England. And yet, the disease does not become general, until the end of this period. We have also seen, that the cholera prevailed in England the year previous, at Southampton and elsewhere—the cases, in the former locality, being acknowledged, by all persons, to have been genuine attacks of Asiatic cholera. Notwithstanding, the disease is confined to this town, although not placed in quarantine, and although no means were adopted, that could possibly have prevented the diffusion of an infectious disease. In addition to these, there were no less than 1,291 deaths from cholera, registered in England, in the same year—all isolated cases, and scattered over the whole surface of England. According to the rule laid down by the advocates of this doctrine—that one patient can infect thousands—the deaths from cholera that year in England, ought to have been, not 1,200, but 1,200,000.

It will, of course, be said, that these were not genuine cases of Asiatic cholera; that they were merely attacks of English cholera, or of choleraic diarrhœa. In one sense, no doubt, they were cases of English cholera, for they sprung up spontaneously in this country; but, then, many of them, if not all, were identical in type with

Asiatic cholera. Independently of the fact, that the type of the disease is different to that of the old English cholera, the latter was a comparatively rare disease, and seldom fatal. If we look through the bills of mortality, for the last century, and the commencement of this, we shall find very few entries for cholera. But there is not a year, since 1832, in which there have not been numerous deaths from cholera, although the disease has only prevailed epidemically three times since then. "Every year," it is stated, "since 1837, when the causes of death were first registered, a certain number of deaths from cholera have been recorded in the registers of the various districts in the country." Again, while referring to the scattered cases of cholera, in 1865, Dr. Farr remarks: "A certain number are mentioned by the Registrars *in every division* (of England), excepting the North Midland." . . . . "One case at Willsden, in Yorkshire, was registered 'Asiatic cholera'; another in the Rillington sub-district, fatal in twenty-four hours, presented *all the features of Asiatic cholera!* All such cases are of ordinary occurrence, and are inexplicable by those who deny the spontaneous origin of sporadic cases."* The same results have been observed since the outbreak in 1866. In 1868, no less than "1,498 deaths were ascribed to cholera, and, among these, were many cases, that could not have been distinguished from the Asiatic type."†

There were other sources of presumed infection besides these. Not only is the poison supposed to be contained in the true cholera stools, or the rice water evacuations, but even in those of the preliminary diarrhœa. "It is," remarks Mr. Simon, "characteristic of cholera, not only of the disease in its developed and alarming form, but equally of

* Registrar-General's Report, 1865, p. xlv.

† Report of the Registrar-General for 1868, p. 199.

the slightest diarrhœa, which the epidemic influence can cause, that all the matters, which the patient discharges from his stomach and bowels, are *infective;* and that, if they be left without disinfection, after they are discharged, their infectiveness, during some days, gradually grows stronger and stronger." Here, then, was a still greater source of infection —thousands instead of hundreds of *foci*—not only in 1866, and previously to the outbreak in that year, but also every year from 1832 to the present time. In 1870, no less than 25,311 deaths from diarrhœa and 815 from dysentery were registered in England : as well as 1,065 from cholera. But the cholera, nevertheless, has not become epidemic, although these cases of diarrhœa, there can be no doubt, are produced by the same cause as that which gives origin to the epidemic, or Asiatic, cholera, as it is erroneously termed, for the disease is no more Asiatic now than it is European. Cases of diarrhœa were comparatively rare previously to the advent of the epidemic cholera—as has been conclusively shown, in the previous chapter, with respect to London; and we may draw the same conclusion with respect to England generally. The type of the disease also is different now to what it was formerly : it then assumed, at least in the majority of cases, a dysenteric form; now, that of diarrhœa—two very distinct complaints. Instead of the disease being introduced from abroad, it thus appears, that it is constantly present in our midst. Our neighbours, therefore, might just as well accuse us of infecting them, as for us to accuse them of importing the disease into this country. And why should not cholera spring up spontaneously in England, the same as in India, in Russia, and in Egypt? Is there anything peculiar in those climates, with the exception of temperature, that is not to be found in England? Nothing. As to heat, that is not necessary for the production of

cholera: it not only commenced in England in the middle of winter, on its first invasion, t it broke out in Russia, with the thermometer 20° below e freezing point.

That this modern scourge does, and can, spring up spontaneously, irrespective of climate, locality, or soil, we have proof of by the outbreaks of cholera on board ship. Two examples of this have been already given: another was afforded in 1866, which it may be as well to refer to, as the outbreak was ascribed to a very different cause. Cholera, in a severe form, broke out on board an emigrant ship—the *England*—during the passage from Liverpool to Canada: and, as the greater number of emigrants were foreigners—German and Dutch—it was inferred, that they had brought the disease with them. This is the conclusion at which Inspector Lawson, then stationed at Halifax; Dr. Trench, the Medical Officer of Health at Liverpool, and Mr. Radcliffe, have arrived. When, however, we come to analyze the facts connected with this outbreak, and reported by these writers, we shall be bound to draw a very different conclusion.

The *England*—a screw steamer—sailed from the Mersey on the 28th March,* 1866, having on board 807 passengers—the half Dutch or German. These had arrived, a few days before, from Rotterdam. The vessel touched at Queenstown on the 29th, and took in 393 additional passengers, making, with the crew, about 1,200 souls in all. Five days after leaving Queenstown, a Dutch boy, aged 12, who had been ill for the two previous days, was found dead in his bed in the morning, from what appeared to be an attack of cholera. On the evening of the same day, Thomas Welsh, an Irishman, was attacked, and died in twelve hours. Within a few hours of his death, five

* In Mr. Radcliffe's report it is *May!* This is an error or misprint.

more were attacked, and the numbers then increased so rapidly, that when the ship arrived at Halifax, on the 9th, there had been 150 cases and 46 deaths. The disease continued among the emigrants until the end of April, the last cases being mild ones: up to which time, there had been 280 deaths and from 500 to 600 attacked. How a sudden explosion like this could be referred to infection, it is somewhat difficult to understand; more especially as the Irish, who had certainly not come from an infected locality, were struck down indiscriminately with the foreign emigrants. Besides, had the latter brought the seeds of the disease with them, it ought to have produced some effect previously; as an interval of, at least, fourteen days must have elapsed from the embarkation of these emigrants at Rotterdam to the outbreak on board the *England*. Independently of these reasons, the cholera, as Mr. Radcliffe allows, did not break out in Rotterdam, until the end of April, and we have no account of its having prevailed anywhere else previously. The only conclusion, therefore, to draw on the subject is, that this ship, like many others, picked up the disease in mid-ocean. That she did so, would appear certain from other facts. Another ship—the *Virginia*—sailed from Liverpool on the 4th of April, seven days after the *England*—with emigrants. On the 12th, 3 of these died of cholera: 8 more died on the following day, and 7 on the next—the 14th—thus making a total of 18 deaths in three days. On the 22nd, when the passengers were removed to the *Falcon*, quarantine ship at New York, the deaths had amounted to 50. Between this date, and the cessation of the disease, on the 23rd of May, there had been 55 more deaths—thus making a total of 105. On the 12th of May, in the same year, the s.s. *Union* sailed from the Mersey, with 437

emigrants on board; of whom 231 were foreigners. On the 18th, a Dane was attacked, and died; and, on the next day, an Irishman and a Danish woman. Between this date and the 29th, when the ship arrived at New York, there had been thirty-four victims in all.

Such is a brief history of these outbreaks. As will have been remarked, the number of cases and the deaths were the greatest in the *England*, which left the Mersey in March,—a month before there was any outbreak of cholera, either in this country or on the Continent. The smallest number, on the other hand, was in the last ship, which left in May, when there might have been some chance of infection, if the cholera can be propagated by such means. It must also have struck others as a remarkable circumstance, that the epidemic should have broken out on board two of these ships, exactly six days after leaving Liverpool; and, in the other, eight days after. It will appear still more singular, when the following particulars are added. Dr. Trench remarks: "It is worth mentioning, as a coincidence, to which, however, *no importance* whatever can be attached, that the geographical position occupied by the *England*, the *Virginia*, and the *Union*, at the period of their first cases of cholera, was *almost identical*—being about latitude 48° 50″ N. and longitude 28° 40″ W." * Although Dr. Trench attaches no importance to the interesting fact, thus made known, it appeared to me to be so important, that I was about writing to the owners of these ships, in order to ascertain the position of each at the time of the outbreak, when the above statement met my eye. It shows, in fact, that certain spots, in the midst of the wide ocean, are pestiferous, the same as on the land; of which examples have been already given. Instead of the emigrants carrying

* Medical Report for Liverpool, to June, 1866.

the seeds of the disease with them from Holland, as Mr. Radcliffe concludes—although the first emigrants actually left Liverpool a month before the cholera broke out at Rotterdam or any other place—the poison must have been wafted into these ships, on the spot where the epidemic commenced, and from another and a very different source. What this source is, will be considered hereafter; it is sufficient now to remark, that if the epidemic cholera can spring up spontaneously, in the middle of the wide Atlantic, there can be less difficulty in concluding, that it may arise spontaneously on shore—in England and in other countries.

With these facts before us, it will appear somewhat extraordinary that a letter, addressed to Mr. Simon by Mr. Radcliffe, "On the recent Diffusion of Cholera in Europe," should have been recommended by the former gentleman to be forwarded, through the Foreign Office, to other Governments. The object of Mr. Radcliffe's letter is to point out the *danger* that threatens Europe from the formation of certain railways, between the north of Persia and the south of Russia—the writer inferring, that the cholera is invariably propagated from country to country by human agency. As the letter has been printed at the Foreign Office, we may presume that Mr. Simon's request has been complied with. The responsibility of such a step is not slight. With some Governments, the missive will be a harmless one: not so with others,—with those that, like Turkey, Persia, and Egypt, rule over semi-barbarous people. If the practice, naturally flowing from the establishment of the doctrine of contagion, should be adopted and carried out, the evils that would ensue will be ten times greater than those arising from the disease itself. And this might follow, as these Governments would have a right to conclude, that an official document

like this contained, not only the opinions of the writer but those of the majority of the profession in England. The step will appear the more impolitic and unwarranted, if it should be shown that the conclusions drawn by Mr. Radcliffe are altogether and entirely erroneous.*

We will now turn to the letter itself, in order to ascertain what the facts are on which Mr. Radcliffe bases his conclusions. Referring to the prevalence of cholera in Russia in 1869-71, the writer states, that these outbreaks were observed to follow upon the prevalence and activity of the disease in north Persia. Hence he infers, that the one is connected with the other, and that the disease was imported into Russia from Persia by human agency. We are informed, however, that the epidemic "could not be traced as an extension across the Russo-Persian frontier (the general route which the disease has taken), at least as far as official researches go," but to a new route. This route "which, within the *last few weeks*, has had a railway opened for its western half, and of which the eastern half is *in course of having* the same accommodation provided for it; and which, traversing Transcaucasia from east to west, and having Tiflis at about its mid-point, brings the Black Sea into free communication with the Caspian and with Northern Persia." Hence, as a continuous railway communication was established, in 1869, between the principal port in the Black Sea and North Germany, Mr. Radcliffe concludes that, "With the completion of

* In France, a question of this kind would have been submitted to the Academy of Sciences, who would have appointed a Committee, composed of men best qualified to offer an opinion, in order to draw up a Report on the subject. But we manage things differently in England, although there is no reason why we should do so. Why could not the Royal Society take upon itself the functions of the Academy of Sciences?

the Transcaucasian Railway, it may be anticipated, that this will follow (to use, Sir, a modification of one of your own phrases applied to the relation of the Continent to this country in respect to infectious diseases), viz., *contagious current in Persia will become current in Europe.*" * Leaving the future, for "sufficient unto the day is the evil thereof," let us return to the proposition first laid down, viz., that the cholera was transported, by human agency, from Persia to Russia in 1869. No account has been given of the arrival of travellers or others in Russia suffering from the disease: not so much as a single fact in proof of the inference. Mr. Radcliffe only infers, that some traveller *might* have taken the infection from Persia to Kiev, where the disease commenced. Of course he might, if the cholera be infectious, and if the infected individual travelled fast enough. But it has not been yet proved that the cholera is infectious: on the contrary, if the arguments and conclusions before drawn be of any value we must conclude that it is not. We will, however, assume that the cholera is infectious; and follow Mr. Radcliffe in his endeavour to trace it from Persia to Russia.

As the epidemic was not prevailing in the intermediate regions, the only inference is, that it had been transported from the borders of Persia to Kiev. But the journey from Persia to Poti, on the Black Sea, and thence to Odessa by steam, must be rather a long one, especially as the railway is not yet completed. The disease could hardly have remained latent in the system all that time. Then, again, the epidemic commenced at Kiev, 300 miles from Odessa. There is a railroad, it is true, between the two towns; and, as such, Mr. Radcliffe infers that this

* Letter to Mr. Simon, on the recent Diffusion of Cholera in Europe, p. 9.

unknown traveller—this wingless messenger of death—went direct from Odessa to Kiev. As, however, no account exists of the arrival of a traveller, either at Kiev or at Odessa suffering from cholera, it is further inferred that the attack commenced after the arrival at Kiev of this imaginary being. Here, then, we have assumption upon assumption: and the most important deductions drawn without so much as a particle of evidence in their support. Had such an event as this occurred, the Russian authorities would certainly have been cognisant of the fact; but they are entirely silent on the subject. We need not look to them, however, in order to disprove the hypothesis: the facts, with which we are acquainted, are sufficient to show its absurdity. If the poison of cholera be so potent, that it can be carried from Persia to Kiev by a single person, and then infect the inhabitants of this town, how was it that the persons in the steam-boat, and others in direct contact with the infected individual, escaped an attack? There must also have been more travellers than one who passed by this route at the same time, and subsequently; some of whom, we may presume, rested at Odessa and other places. But they failed to carry the infection with them, although one human being must be as capable of transporting the seeds of the disease as another, provided only that he comes from an infected locality. But the cholera did not commence at Odessa until the autumn, although it spread over the greater part of Persia in the interval—between the outbreak at Kiev and the outbreak at Odessa. Then, again, if the cholera were transported from Persia to Russia, and by a healthy person, how was it that this highly infectious disease was not carried from Kiev to Odessa, with a railroad and constant communication between the two towns? Is it easier to carry the seeds of the disease

2,000 or 3,000 miles from south to north, than 200 or 300 from north to south?

There is another circumstance to be taken into consideration. The disease commenced at Kiev in the spring, subsided, and then broke out again in August, when it became general. How is this phenomenon to be accounted for by the doctrine of contagion? If the cholera be so infectious a disease, why was it not propagated to the other inhabitants of the town, and to other districts, in the spring, as well as in the autumn? The truth is, we must look to some other cause than importation for the outbreak of cholera in Russia in 1869. Fortunately, we have not to look far. Dr. Pelikan, the Director of the Imperial Medical Department of Russia, states, in a communication made to the Academy of Medicine in Paris, through Dr. Fauvel, that the visitation of 1869 was merely a *recrudescence* of that of 1865. Narrating the particulars of the outbreak in 1869, the Doctor remarks: "It is *not* a new epidemic due to an importation from Persia, as is *erroneously surmised* at Constantinople; it is only the tail—*ce sont les queues*—of the epidemic of 1865, which, the same as we have observed previously, affords proof of a great persistence (*ténacité*) of the disease in our country." * But Mr. Radcliffe refuses to accept this theory of recrudescence: why, it is somewhat difficult to say, excepting that this opinion is in opposition to his own. Dr. Pelikan ought to know, better than Mr. Radcliffe, whether the cholera arose spontaneously in Russia that year or not. That it did so, we may infer from the fact, that cholera is constantly present, as far as our information goes, in every country that has been invaded by the disease. It is so in India, it is so in England, and it would appear to be so in Russia.

* Bulletin de l'Académie de Médecine, T. 36, p. 694.

According to Dr. Mariscani, a Russian physician, the deaths in Russia from cholera, in the undermentioned years, were as follows :—*

Years.	Deaths.	Years.	Deaths.	Years.	Deaths.
1847	116,501	1854	13,892	1865	3,178
1848	668,092	1855	131,107	1866	88,352†
1849	6,688	1856	17,823	1867	617
...	...	1857	4,727	1868	38
...	...	1858	1,937	...	...
...	...	1859	1,794	...	...
...	...	1860	5,081	...	...

It is thus apparent, that the cholera prevailed in Russia every year, from 1854 to 1860, and from 1865 to 1869, to a greater or less extent. It is also probable, nay, almost certain, that, if a complete system of registration existed in that country, we should have found deaths from cholera every year since 1829—the first visitation. As such, Dr. Pelikan is quite right in affirming, that the outbreak, in 1869, was merely a recrudescence of that in 1865; the same as the visitation of 1866 was a recrudescence of that of 1855—there having been, as we have seen, cases and deaths in each of the intervening years. To affirm that a disease, which is already prevailing in a country, has been re-imported, would be as logical as to infer that the corn, which springs up every year beneath our feet, has been imported, or the seeds of it, from Russia.

Having thus discussed the cause of the outbreak of

* Gazette Médicale de Paris, May 13th, 1871.

† This year the cholera broke out at Stettin, next at St. Petersburg, and then extended to other provinces, as far as Moscow. Instead of spreading from the south to the north, the epidemic took the opposite course, commencing in the north, and then spreading from west to east, and from north to south.

cholera in Russia, in 1869, we will now endeavour to ascertain how the disease originated in Persia. This, as we are informed, is by infection from India. "The great trade route between Persia and India, through Afghanistan, by way of Herat to Meshed, has been the track chiefly followed in previous invasions of Persia by cholera." And it is then added, "Facts are not wanting to show, that Persia was infected from India in 1867-70, by *re-importation* from India, along the lines of traffic just referred to, and *not* by a re-kindling of the embers of the old epidemic of 1865-67." The facts to which Mr. Radcliffe alludes are the following: "In 1867, cholera, which had broken out with much fierceness among the multitude assembled at Hurdwar for the great annual religious Hindu fair *(fête?)* held there, followed in the track of the pilgrims returning northwards into Afghanistan, and towards the close of the year it was prevalent in Cabul. Again, in 1869, it became epidemic over the whole of the Bombay Presidency and Northern India, and had a new spread into Afghanistan." Granted; but, then, what could these outbreaks have to do with the diffusion of cholera in Persia and in Russia: the disease having existed in both these countries from 1865. That this was the case in Russia has been already shown: while the same fact will be evident, if we turn to Persia.

In 1865, the cholera prevailed in the Persian Gulf, and spread along the Tigris and Euphrates rivers, as far as the Turco-Persian frontier. It appeared again in 1866, but did not prevail to any great extent. In 1867 (the year when it appeared among the pilgrims at Hurdwar) it broke out again; as, also, in 1868, to a still greater extent, continuing to the end of the year, and advancing to nearly the Turkish frontier. It reappeared in the spring of 1869, and extended over nearly the whole of Persia, and even

into Turkey, having crossed the frontier in the autumn. Here, then, we have precisely the same facts as in Russia, an epidemic prevailing every year, and, actually, for two years before the time when it is said to have been imported; or, as Mr. Radcliffe terms it, *re-imported*. This re-importation theory is, most assuredly, a somewhat absurd one; and appears very much like taking coals to Newcastle, or putting a torch to houses, when an extensive fire is raging in a town. In addition to this, the route by which the infection is said to reach Persia, is not that which the epidemic takes from India to this country. In its first invasion, in 1821, the epidemic broke out at Muscat, and then spread along the shores of the Persian Gulf northwards. The cholera had not then passed the Himalaya Mountains, or appeared in Cabul: as such, it could not have been propagated in that direction. Besides, Muscat is in lat. 23°, long. 58° E., while Cabul is in lat. 35° and long. 70°, so that the disease, in order to reach the former place, must have turned off at an obtuse angle, and have proceeded in a south-westerly direction, instead of a northerly one—its usual route. It will sometimes return, as the French say, *sur ses pas*, but only on *the line* it has been previously pursuing: it never goes off at a tangent or pursues an erratic course. Nor could the disease have reached Persia by a more direct route, there being no traffic across the great Sandy Desert that separates Persia from the upper provinces of India. The fact is, the epidemic cholera, when it left the shores of India, passed by two well-marked and distinct routes —one by the Persian Gulf, and the other by the Red Sea. Granting, that the cholera reaches Persia by the route described by Mr. Radcliffe—although it does not follow, that it would even then be by infection—how are we to account for its propagation along the last-mentioned

route? It cannot be by Afghanistan and Persia, for the disease sometimes prevails in Egypt when it does not in Persia, and *vice versâ*. The usual course is this. When the epidemic prevails in the north of India, it passes on to Persia: when in the south, to Egypt. In what way, then, does the cholera reach the latter country? By importation, it will be answered. That would be a very good answer in the present day, if the disease be really infectious; but it will not account for the first outbreaks of cholera in Egypt, at a time when there was scarcely any traffic, or intercourse, between this country and India. Nor could it have been brought by the pilgrims, for the outbreaks of cholera, and the assembling of the pilgrims, were not, as we have previously seen, coincident, excepting in one instance. If, therefore, the epidemic cholera could pass from India to Egypt, without human aid, it could also pass, in the same way, from India to the Persian Gulf. This is the route which the disease took on its first invasion of Persia; and this is the course which the cholera has taken, as we have a right to infer, on all occasions since. It is also the route by which the epidemic reached Russia, having spread from the north of Persia to Astrakan as early as 1823, although it did not then become epidemic. We may therefore conclude that the importation of the cholera into Persia from Afghanistan in 1867-70, is simply a chimera of Mr. Radcliffe, for which there is not the slightest foundation. All the facts with which we are acquainted point to the opposite conclusion.

Not only was the cholera imported into Persia, according to Mr. Radcliffe, but it was also imported into Cabul by the pilgrims returning from the great Hurdwar fête in 1867. As this statement has been made over and over again, until, at last, it has been received almost as an acknowledged truth, it will be not only instructive, but

important, to inquire on what foundation this statement reposes. This is the more necessary, in consequence of its having been stated, that these pilgrims spread the disease over different parts of India, as well as Afghanistan.

The meeting this year was a duo-decennial one, when there is always a much larger assemblage of pilgrims than in other years. On this occasion, there were, it has been calculated, three millions present,—the area of the encampment being twenty-two square miles. It is as well to premise, that Hurdwar—the Gate of Vishnu—is a native village, situated about forty miles from the station of Saharunpore, on the southern slope of a mountain range, called Siwalik, and at the mouth of a gorge. It is through this gorge, that the Ganges issues from its source in the Himalayas into the plains of Hindostan. The spot is considered sacred, and annual pilgrimages have been made to it from time immemorial, in order to dip in the virgin stream; it being considered, that those who first wash in the sacred waters are cleansed from all their sins. The gathering commenced early in April, and all went on well until the 13th, there having been only 1,367 cases of slight ailments previously. On that day, eight cases of cholera were sent to the hospital, and there were eleven more between that date and the 15th, or nineteen in all. There were no other attacks, and for the simple reason, that the pilgrims dispersed on the first intimation of danger: not a single one remained on the 15th: the camp was a desert!

Such being the facts connected with the outbreak, the question arises, how was the disease produced? It could not be from insanitation, as, independently of the fact that, on some previous occasions, when no precautions had been taken, cholera did not make its appearance, the

greatest precautions had been adopted in this instance. "Large sums of money were expended by the authorities; *Latrines* were established, the dry-earth system was adopted, and the disinfected excreta were regularly removed. Precautions were taken to prevent dead bodies and other injurious matter being thrown into the river; and overcrowding in the camp was prohibited. Two hospitals were established for contagious diseases: and six for ordinary diseases, besides two dispensaries."* Such being the case, it has been almost unanimously concluded, that the outbreak cannot be ascribed to the absence of sanitary precautions: or, to the presence of decomposing matter. But as there would appear to be only two alternatives, in the opinion of the majority of modern theorists, it has been inferred, that the disease was imported. But how, and whence? Cholera, it appears, existed in the Upper Provinces the latter end of 1866, and in the beginning of 1867. It prevailed at Burtpore, Terai, Pergunnah, not far from Hurdwar, and in the camp of Sir Jung Bahadoor —pilgrims from which places were, of course, present in the camp: and it is to them that the introduction of the disease has been ascribed, and in the following way. Dr. Cutcliffe states: "At the bathing *ghat* there actually was a pen full of bathers, who had been subjected to the predisposing causes of the disease, and who had, amongst them, people fresh from a place infected with cholera, if not themselves actually, at the time, infected."† Hence, the outbreak has been referred to the contamination of the water of the Ganges by the germs of cholera. By infected, Dr. Cutcliffe means, that the pilgrims carried the seeds of the disease in their system: not that they were labouring

* Fourth Annual Report of the Sanitary Commissioner with the Government of India.

† *Idem.*

under the disease at the time. That could not have been possible: in the first place, because a person suffering from cholera in India, where the course of the disease is so rapid, would not have gone to the river to bathe; and, in the next, because, as we have seen, there were no attacks of cholera until the 13th—the day after the great bathing day. It follows, therefore, that the water of the Ganges, if contaminated at all, must have received the infection from the bodies of the healthy, not from the sick—a most preposterous idea. Were such a result possible, India would have been half depopulated 'ere this: and many other countries as well. Allowing, however, for the sake of argument, that the cholera can be propagated in this way—by means of the healthy—it then remains to be seen, whether the infection could have been received from the water of the Ganges.

The Ganges, it should be remembered, is not a lake, nor yet a tidal river, but a mountain stream, flowing in one direction only, and with some velocity. Supposing, then, that a certain number of these pilgrims were infected, and that the cholera germs were diffused by them in the surrounding water, during the time of their ablution; how could these germs, we may ask, affect those who were bathing in the same place and at the same time, or, subsequently? The infected water would flow away, the next moment, beyond their reach, never to return. In fact, these individuals might have given the same answer to their accusers, that the lamb, in the fable, gave to the wolf: "How could we have contaminated the water which you drank; seeing that it did not flow from us to you, but from us to another and a distant region?" In order to have produced any ill effect, it would have been necessary for the infected pilgrims to have bathed in the stream above the ghats used by the other pilgrims. Even, then,

there would have been only a bare chance of any of the other pilgrims drinking any portion of the infected water, during its rapid passage past the bathing-places. It would, indeed, have been impossible to propagate the disease, in the manner now pointed out, unless the whole of the water in the Ganges had become contaminated from its source to Hurdwar. But that was impossible, the banks of the river, above the gorge in the mountains, being uninhabited.

Another explanation has been offered. There was a storm at Hurdwar on the 11th with heavy rain. This continued during the night; and it has been concluded that excreta, with cholera germs, were carried into the river, together with the surface-washings of the camp. In this way, not only the contamination of the water in the river, but also the outbreak of the disease, as has been conjectured, can be readily accounted for. Nothing, indeed, can be easier, when theorists, instead of searching for facts, jump to conclusions, and allow their imaginations to get the better of their judgment. To show the fallacy of this conclusion, a few words will suffice. The storm took place on the afternoon and evening of the 11th, but the bathing did not commence until the following day. Now long before the pilgrims had entered the water, all the matter that had been carried into the river from the camp and surrounding surface, would have been hundreds of miles away. The water that the pilgrims then drank, and in which they bathed, must have been derived from a locality far removed from any source of contamination, or, at least, from either of the sources now pointed out. We may therefore reject the hypothesis of the outbreak at Hurdwar having been caused by the contamination of the water of the Ganges; and as no other explanation has been given, it is highly desirable to ascer-

tain if a solution to the problem cannot be obtained. This, perhaps, is not so difficult as it may at first sight appear.

It is to be remarked, that the valley in which the camp was situated is swampy, covered with jungle, and reputed to be very unhealthy—a chosen seat of malaria. It would have been surprising, therefore, if disease of some kind had not broken out among this vast, heterogeneous mass. If, also, the cholera was prevailing in that part of India at the time—and such was the case, as we have seen—and if the epidemic influence were present in the valley, an outburst of the disease was all but inevitable. That this influence—no matter what it may be—does exist, if not constantly, at least frequently, in the uninhabited districts of India, the same as the habited, we have had abundant evidence from the first rise of the epidemic cholera to the present day. This was more particularly the case formerly, when troops were obliged to march across the uninhabited districts of that vast peninsula, instead of being conveyed, as they now are, by rail. For instance, it has frequently happened that certain corps on march have encamped for the night on a particular spot—the men being at the time free from all disease. Before the morning numbers have been attacked with cholera, and many have died. Either from design, or accident, the camp has been broken up, and the healthy and the sick removed to another locality ; *this is no sooner done than the disease ceases.* Thus, a light-infantry regiment, returning from the Deccan war to Bombay, being attacked by the epidemic at its bivouac, an havildar stated to the commandant, that *there was no cholera a few hundred yards further on, beyond the Nullah;* the camp therefore was broken up, and the regiment, *carrying the sick along with it,* marched beyond the morbific boundary : and *the plague was stayed.* We are also informed, by Dr. Henderson,

that the 13th regiment of infantry, to which he was attached, together with the 38th and 48th, encamped on a low marshy spot, near to Patnago, in 1825; in the morning, one officer was attacked with cholera, and in twenty-four hours twenty men were carried off. On the following day, the corps removed *to a higher ground*, a mile and a-half off: *and*, from this time, *no more cases of cholera were observed* in the army. An example of the same kind, but on a larger scale, was afforded by the army under the command of the Marquis of Hastings; which was attacked with the cholera, in Bundlekund, during the first year of the prevalence of the epidemic. This division of the grand army had encamped on the banks of the Sinde, immediately after which the disease appeared; commencing however in its usual insidious manner, by attacking only the lowest orders of the camp followers. But in a few days, and as it were in an instant, the disease burst forth with irresistible violence:—"Unsubjected to the laws of contact and proximity of situation," to quote the writer of the Bengal Report, "which have been observed to mark and retard the course of other pestilences; it surpassed the plague in the width of its range, and outstripped the most fatal diseases hitherto seen in the destructive rapidity of its progress. In the course of a week, it had overspread every part of the camp, sparing neither sex nor age, in the undistinguishing virulence of its attacks—the old and the young, the European and the native, fighting men and camp followers, were alike subjected to its visits; and all equally sunk, in a few hours, under its most powerful grasp. It was then wisely resolved by the Commander-in-Chief to change the encampment, *in search of a purer air and a healthier soil* (the reasons assigned for the measure in the official Report); and although the line of march was covered with

the dead and the dying—men dropping from their horses, or falling while marching in the ranks, as if struck by a cannon-ball—they succeeded, after a few intermediate halts, in reaching, on the 19th, the high and dry banks of the Betwah, at Erich; *where they almost immediately got rid of the disease:* for not a single severe case occurred after the 22d. The disease was at its height on the 14th; and in one fatal week, of seven thousand fighting men, seven hundred and sixty-one fell victims to the disease; while it was conjectured that eight thousand of the camp followers, or about one-tenth of the whole, were cut off." Independently of the sudden and simultaneous outbreak of the disease among so large a body of men, and its extension *in a few days* over every part of the camp; the circumstance of its as sudden cessation on the division reaching another and a different locality—*notwithstanding that they carried along with them the sick and the dying*—can never be explained by a reference to the doctrine of contagion.

It has also frequently happened, in India, that two corps on march have encamped for the night near to each other, but *on different ground.* One corps has been suddenly and immediately attacked with the disease, while the other has not presented a single case. The sick battalion observing this, shifts its encampment, and takes up a position alongside the healthy one; and, although they carry with them the sick and the dying, the disease does not spread to the healthy division: notwithstanding that the intercourse between the two corps has been unrestricted, not only between the healthy, but also between *the sick and the healthy.* As some of the corps thus attacked had never been within the focus of the epidemic before, they could not have carried the germs of the disease with them from any other or infected source. Nor, on the

other hand, could they have derived the disease from any human source on the spot where it first commenced, as it so happened, that they encamped, not in an inhabited, but in an *uninhabited* region. Nothing can show more clearly than these facts, that it is *places*, and *not* persons, that are infected at epidemic periods—a conclusion which will enable us to understand the cause of the outbreak of cholera among the pilgrims at Hurdwar without reference to the contamination of the water, or the infectious character of the disease. This, however, is not all.

Not only was the cholera supposed to have been imported into Hurdwar, but it is also said to have been exported into other and distant parts of India, by the fugitive pilgrims. That many of them were attacked subsequently is undoubted: it would have been strange if they had not been, seeing that they had fled from an infected locality; and that some of them would and must have imbibed the seeds of the disease before their flight. Had they remained, they would have been attacked on the spot: as it was, they were attacked elsewhere. In this there is nothing singular, nothing unaccountable: the only singularity is, that the subsequent outbreak of cholera in other parts of India should have been ascribed to infection—to the infection carried in the bodies of these pilgrims. According to the returns, in 35 districts out of 51 subsequently visited, the pilgrims were the first attacked, and then the residents. In the other 16, the residents were attacked, not the pilgrims; or, else, before the latter.

Leaving the former districts, for the moment—those in which the pilgrims were the first attacked—let us inquire into the cause of the outbreak in the latter sixteen. As no attempt has been made to trace the origin of the disease to importation, in these districts, we may conclude, that

there was some general cause in operation productive of these particular outbreaks. This inference is confirmed by other facts. Independently of the visitation at Hurdwar, cholera had prevailed, as before remarked, at several other places previously; thus showing that the epidemic influence had been in operation months before it appeared among the pilgrims. More than this, the epidemic was *due*—actually due—that year in the Upper Provinces. Not only can the epidemic cholera be predicted, but it has been actually predicted, in numerous instances, long before its advent. Two years before the outbreak, Dr. Bryden wrote to the authorities to say, that cholera would certainly break out in the Upper Provinces of India in 1867, and warned them of the consequence. Dr. Bryden remarked: "Speaking from the experience of 1857 and 1862, I had fixed the 20th April as the date at which a cholera, distributed within the western division of the epidemic area, was due to revive." * It showed itself at Hurdwar on the 12th, and he adds: "The dates and distribution of the cholera, and the Tables for 1862 and 1867 are as nearly as possible reproductions, one of the other, as far as concerns distribution within the western division." † Hence the great precautions that were adopted at the Hurdwar *fête*. There is another circumstance to be taken into consideration. The epidemic spread, not only to the districts near to Hurdwar, but, also, beyond the confines of India; and beyond the influence of the pilgrims—in countries in which the population is chiefly Mohammedan. Thus, 8,000 perished in Cabul, and 6,000 in Cashmere. Added to this, the epidemic did not follow the principal route of the pilgrims, that is, from north to

* Letter to the Sanitary Commissioner for Bengal, 28th July, 1865.

† Report on the Cholera, 1866-68.

south, for the Hindoos inhabit, principally, the Southern Provinces of India—and this is a purely Hindoo, not a Mohammedan, *fête*—nor did it spread in all directions, like radii from a centre, as we should have expected, had the pilgrims been the conveyers of the poison. On the contrary it took, as is usually the case, a linear direction across the Continent of India. "It travelled from east to west," said the Commissioner of Allahabad, "as I believe to be *universally* the case." It commenced in Nepaul in April, cutting off 3,000 of the inhabitants of the valley, before its termination, and then spread in two different directions—one south of the Jumna, and the other between this river and the mountains—into the Banda district, *leaving the Doab untouched.* By looking at the map affixed to Dr. Bryden's Report, it will be seen, that the cholera area, for 1867, forms an irregular triangle, the base of which is turned to, and extends as far as, Cabul, while the apex rests on the northern boundary of the province of Bengal. It thus appears that the whole of Bengal, and nearly the half of the western portion of the peninsula of India, remained exempt, the very reverse to what we should have expected, had the disease been propagated by the pilgrims—these being the very districts principally inhabited by the Hindoos. It is thus apparent, that this disease not only springs up spontaneously, without any known, or assignable cause, but it pursues its own course, irrespective of human intercourse and human agency.

As to the attacks of the pilgrims, after their flight, and before those of the residents, a satisfactory explanation can be given, without reference to the doctrine of contagion—by a reference to the laws regulating the operation of malaria on the human system. In districts where this destructive agent exists, it is well known that strangers,

and persons coming from another and a more healthy locality, are frequently attacked with disease; while the inhabitants remain entirely free. Thus, it is death for a European to sleep for a single night in many of the pestiferous spots of intertropical climates; notwithstanding that the natives reside there with impunity; or, at least, if affected at all by the same malign influence, are not subject to those acute diseases and severe attacks which strangers experience. It has been before stated, that the residents within the tropics, or, rather, from lat. 10° to 30° N., are not usually subject to yellow fever: but strangers are almost certain to be attacked. At St. Domingo, in 1803, two-thirds of the French army perished, victims to yellow fever; while the negroes, the creoles, and the old colonists, preserved the most perfect health. It is, in fact, a wise provision of nature, that the human frame should be able to bear, if not with complete, with at least comparative, impunity, the operation of many morbid agents, when slowly and gradually brought under their influence; although they would produce disease and death in those suddenly exposed to their injurious operation. This circumstance will enable us to explain the otherwise remarkable fact that, during the prevalence of epidemic diseases, persons coming from other towns—or, in other words, strangers—are often the first affected. "Who," remarks the Editor of the Lancet, "would have believed, before 1832, that Lyons, a damp, unclean, and unhealthy city, full of workmen, the majority of whom are in a wretched state of want and filth, would escape the scourge? And how strange now, that the disease (the cholera) has made its appearance, that it should be confined to that portion of the inhabitants, who make but a *temporary residence* in the city, and who are decidedly in far

better condition, in every respect, than the bulk of the population." * The reason is clear.

From the facts which have been collected, by numerous writers, we are bound to infer, that the epidemic influence, whatever that may be, is in operation for some time—in fact many weeks, if not months—before it produces any severe effects. This phenomenon was particularly observed during the prevalence of the epidemic cholera in Europe: for not only was the severe form of the malady preceded for many days by slight attacks of diarrhœa, but certain anomalous symptoms, indicative of derangement in the digestive organs, were observed to prevail for many weeks, before the epidemic manifested itself in its most severe form. To this particular affection the term *cholerine* was applied by the French.† As, therefore, the cause, productive of these effects, seemed to be thus slowly brought into operation—as proved by the mildness of the symptoms first observed, and their gradual increase until the disease was ushered in in its acute form —we may conclude, that those who had been gradually accustomed from the first to the influence of the morbific cause would be longer able to resist its operation, than those who were suddenly exposed to the injurious effects of the deleterious agent. On this point I can speak from personal experience; for having followed the epidemic cholera for some years, and having paid frequent and oft-repeated visits to districts in which malaria abounds, it has generally happened, after I have been residing for any time in other and more healthy localities, that I was, on

* "Lancet," January 8th, 1850.

† It was remarked by one writer that, on the first invasion of the cholera in Berlin, many persons, who did not before know where their stomach was situated, then complained of indigestion.

my arrival in an infected district, attacked with the prevailing complaint—no matter whether it was epidemic or endemic; that is to say, the cholera, fever, or dysentery. As, however, this frequently occurred on my first arrival, and before I had seen a single patient, or had come in contact with those who had been in communication with the sick, it was impossible to suppose that the cause of the disease resided in the bodies of men.

With such facts before us, it is not difficult to understand why the pilgrims were the first to be attacked, in the districts to which they fled, more especially as they would be predisposed to an attack from other causes. As the majority of persons who make this pilgrimage, from religious motives, and as a matter of duty, are very poor, and ill able to afford themselves the necessary comforts or food, on the journey, they would be more predisposed to an attack than others, better fed and better housed. Exposure to the air, as we shall find hereafter, is the most powerful exciting cause of the disease.

Having thus endeavoured to show, that the arguments and conclusions advanced by those writers, who advocate the contagious nature of the epidemic cholera, are entirely erroneous, we may be allowed to draw the following inferences. 1st. That the epidemic cholera is neither contagious nor infectious. 2ndly. That it is not propagated from individual to individual. And, lastly, that its propagation, or rather progression, from country to country, is due to some unknown cause, *not* to infection. We have thus arrived at the point from which we started, or, rather, at the conclusion drawn fifty years ago by those most competent to form a correct opinion on the subject. It is to be regretted, therefore, that modern theorists, instead of depending on home experience, which is abolutely worthless—for we know nothing of the ravages of the

epidemic cholera in England *at present*—had not recurred to that of India. Had they done so, we should not have now had to discuss the question of the infection of cholera, or of its propagation from country to country by human agency. Hear what Dr. Bryden, who has been cradled, as it were, with the monster; and who has lived in contact with it ever since, says on the subject: "The most careful and skilled physicians have succeeded each other, in the charge of the great cholera hospitals of Calcutta, for fifty years back; and *not one* of them has yet been able to make up his mind to the belief that cholera is a thing communicable to those around the suffering patient."

CHAPTER IV.

EPIZOOTICS; OR, DISEASE IN THE BRUTE CREATION.

The earliest records extant prove, that disease in the brute creation has always been the accompaniment of pestilence in man. Thus, the plague of blotches and blains, which afflicted the Egyptians in the time of Moses, was a companied by a murrain. The Grecian Poet has also recorded the same combination, excepting that, in this instance, the epizooty preceded the epidemic in man—

> "On mules and dogs the infection first began;
> And, then, the vengeful arrows fixed in man."

The same order was observed in the time of Romulus, when a great plague, according to Plutarch, swept off many of the Romans—the cattle having been destroyed the previous year. Another cattle plague raged in Rome, under similar circumstances, in the year 355 of the Roman era; and to such an extent that processions were formed, and offerings made to the Pagan gods, to avert the evil. But we need not go back to so early a date as this for examples: murrain in cattle having been the invariable accompaniment of the black death, during the whole time of its prevalence in Europe. More than this the murrain continued to prevail in England for a century after the plague had ceased. From this time to the advent of the epidemic cholera, no epizootic murrain had been observed in England; but there have been several since then,

unfortunately. These epizootics observe precisely the same laws as the epidemics of the human race. They arise suddenly, prevail for a certain time, and then disappear; but only to return, after a longer or shorter interval, and to manifest the same series of phenomena. This will continue for long periods, generally speaking for centuries, as was the case with the murrain that accompanied the black death. Another characteristic of these epizootics, as well as of epidemics, is their *progressiveness*: or propagation from country to country. More than this, epizootic murrains have usually commenced in the East, and then travelled slowly across the Continent of Europe to the West. It has been this circumstance which has caused these pestilences to be regarded, the same as those in man, as contagious. This was more particularly the case with the epizootic murrain of the last pestilential epoch: and it is the case with that of the present epoch, or the cattle plague. As this is the most important question of all—more important perhaps than that of contagion in man—it is highly necessary to endeavour to ascertain if the conclusion be a valid one. As few facts of any value have been recorded, respecting the epizootic murrains of former epochs, it will be better to confine our investigations to the cattle plague, the evidence respecting which is as complete as such evidence, perhaps, can be. The principal reasons that have been advanced in support of the contagious nature of the cattle plague are the following:—1st. That the disease does not belong to this country, but to Russia. 2ndly. That the disease is, or was believed to be, confined to the *bovine* race. At an adjourned Meeting of the Committee of Norfolk Agriculturists, held in Norwich in August, 1865, Mr. Read, M.P., in the chair, Professor Simonds observed: "They must recollect that this was a disease

not belonging to England. It no more belonged to us than the Asiatic cholera, yellow fever, or any other of those diseases, belonged to us. It was a disease specially belonging to Russia, Austria, and the Danubian Provinces, Bessarabia, and the whole of the countries lying eastward. The countries on the west side of the line were strangers to the disease save and except upon its introduction. Sometimes it went into Prussia, sometimes into Bohemia, and sometimes into Bavaria; but it never went there except upon the introduction of cattle, in whose systems the disease was either incubated at the time or the cattle themselves were positively in a state of disease. It was, however, scarcely possible for the disease to be introduced in the latter way, because the sanitary regulations of those countries were such as to prevent animals in that state being allowed to pass. Therefore it could only be brought by animals after (before ?) the ordinary period of incubation had passed, and before the disease had begun to declare itself."

The Professor, in these instances, was most unfortunate in his comparisons. The epidemic cholera, as must be evident from what has gone before, belongs as much to England as to India: while the plague was scarcely known in Egypt until after it had ceased to prevail in England. To listen to certain writers, we might conclude that England was a country into which nothing impure, nothing unholy had ever entered: and where disease and death are unknown—a second Paradise in fact. To show how erroneous this conclusion is, at least as regards the diseases of cattle, the following brief account of the murrains that have prevailed in Great Britain, previously, will suffice to show:—

Thus in A.D. 218, there was a "mortality of all the reptiles, cattle, and horses in Great Britain." *Iolo.* (Welsh MS.)

In Ireland, in the reign of the Monarch Bodhibhadh—A.M. 5001, or A.D. 36—there was so fatal a murrain, that only one bull and one heifer, it is stated, survived.—*Census of Ireland.*

A.D. 466.—"A pestiferous smell in the air killed both man and beast." A famine prevailed at the same time. The same result is stated to have occurred in Scotland in 480.—*Short.*

A.D. 547.—The disease called the Yellow Pestilence, which nearly destroyed the whole population of Wales, was said to have spread to the cattle this year. It attacked beast and reptiles.—*Book of Llandaff.*

"In 561 and 582, a great mortality among the cattle in Ireland."—*Census of Ireland.*

A.D. 682.—"A mortality (*Ar* slaughter) upon all animals in general throughout the world, for the space of *three* years, so that there escaped not one out of the thousand of any kind of animals."—*Census of Ireland.*

An epizooty in 689 attacked the cattle in Ireland: and in 694, there was, to use the language of the day, "a great morren of cattle throughout all England." In the following year the same disease prevailed in Ireland. The scarcity in the latter country was so great that "for three years together men and women did eat one another for want."—*Annals of Clanmacnoise.*

It is again stated, in the Annals of Ulster, that "a mortality of cattle raged in Ireland, in the Kalends of Feb., 699." And it is then added: "man ate man." Short states that in 700, and the three following years, there was famine in England, produced, no doubt, by disease in cattle, as there was great destruction among black cattle in Saxonia in 699; and in Ireland in 700, 701, and 707, with all kinds of cattle.—*Census of Ireland.*

In 760 commenced the great epidemic period of the eighth century, characterised by pestilence in man, and murrain in cattle. It continued for 20 years. The following years, viz., 765, 70, 72, 76, 77, and 78, are specially referred to as years of great mortality, in Ireland, among cattle—*bovina mortalitas.* That in 707 is called *Bo-ar-mor*, or the cow mortality. Disease in the cattle produced famine in 772 and in 778, as it is added, "the penury of victuals and the mortality of men from want ceased not."—*Annals of Ulster and Chronicon Scotorum.*

In 810, "there happened the greatest mortality among horned cattle in Britain that is on record."—*Welsh Annals.*

The year 823, according to Short, "was fatal to both cattle and men in England; and in 868 to cattle." * In 884, there was a murrain again among cattle, destroying great numbers of them; and another in 897, which continued for three years, with pestilence in the human race.—*Anglo-Saxon Chronicle.*

During this century no mention is made of any murrain in Ireland until 898, when a great mortality occurred among all animals. This disease, or others, ravaged Ireland during the whole of the next century, at short intervals, viz., in 903, -8, -18, -50,† -53, -55, -60, -81, and 92. In the previous year (981), an epizooty of a cutaneous character, previously unknown in Ireland—called "Moiligarb"—broke out, and continued to prevail for six years.

Curiously enough, while Ireland appears to have been nearly exempt from murrain in the ninth century, England was equally so in the tenth—no mention being made of any visitation from 897 to 986. But, in the latter year, a mighty mortality took place among the cattle over the whole island of Britain.—*Welsh Annals.*

In 987, "two pestilences, previously unknown to the English—to wit, fever of man, and plague of cattle, the latter called *scitta*, in English, but in Latin *Fluxus interneorum*—made their appearance."—*Short.* The same disease, according to this author, prevailed among men, not having been observed previously for 200 years.

Coming to the next century—the eleventh—we find the years 1016 and 1040 specified as murrain years in Ireland. In the latter year, there was an epidemic, with great mortality among swine—"the first recorded in the Irish annals."—*Census of Ireland.*

Short states that, in 1041, so much cattle perished in England, "as no man remembered before." There was, also, "a great dearth and death of people and cattle" in 1047 and 1049, and in 1054, "there was so great a murrain among cattle, as no man remembered for many years before."—*Anglo-Saxon Chronicle.*

In 1078, or 1080, commenced the epidemic period of the latter part of the eleventh century. It continued for thirteen years, both in England and Ireland, and was attended by epizootics affecting oxen, sheep, and swine.

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* Offer's Annals of the Reign of Alfred the Great.

† "In this year there was a great mortality among bees—the first of the kind mentioned in the Irish annals."—*Census of Ireland.*

In 1092-4 a murrain prevailed in England, in common with France, Italy, and Germany.

The twelfth century was ushered in by murrain in cattle, in 1103; and in 1110 there was what Short terms "a bloody battle of wild fowls, wherein great heaps were slain:" the battle being, we may presume, not with each other, but with pestilence and death. This is confirmed by the next account, for he states that, in the following year, the tame fowls died and fled into the woods, while murrain prevailed with the cattle, and fish died in the water.

In 1115 there was a great mortality in Ireland, among men, beasts, and birds; and in the following year a famine, "during which the man sold his son and daughter for food, and the people even ate each other."—*Census of Ireland.*

Murrain prevailed in England, also, in 1112, -15, -22, -25, and -31. In the latter year, "there was so great a pestilence amongst animals, over all England, as had not been in the memory of man. It fell chiefly on swine, so that the man, who had possessed 200 or 300 swine, had not one left." This murrain, together with pestilence among men, lasted twelve years, from 1131 to 1146.—*Anglo-Saxon Chronicle.*

The years 1133 and -34 were memorable in Ireland, also, by the great murrain of cows—the greatest that had been experienced for 432 years. "It left but a small remnant of the cattle of Ireland." Murrain is again mentioned as having prevailed in 1154, but this is the last recorded, in this century, in Ireland. In England, a grievous and pestilent mortality of men and cattle is stated, by Short, to have occurred in 1171 and 1187.

In the next, or thirteenth, century, the number of epizootics was less than in the preceding. They commenced in 1207, in which year men as well as cattle were attacked in Ireland. In England, the murrain did not commence until 1217. There was also a great mortality among the cattle and sheep, in England, in 1224 and -25, and in Ireland, among the cattle. This murrain commenced in the East, and then spread through Hungary, Austria, Italy, France, and Germany to England. Precisely the same phenomenon was observed in 809-10, with the murrain that prevailed in England, and in France, and in which nearly all the cattle in Charlemagne's army perished. It came from the East, say the chroniclers of that period, and spread to the West.—*Chroniques de St. Denis.*

Short says that in 1240, a sore and heavy disease fell on man and beast, and the fish on the English coast died. In 1252, there was a murrain among cattle in England, and particularly in the Fens: while, in 1254, there was "such a murrain of sheep that, in many places, above half died."

No mention is made of any other epizooty until 1275; which year is memorable by the appearance of the "rot" among the sheep—"the first," according to Short, "that was ever known in England." It spread over the whole kingdom, and lasted twenty-five or twenty-eight years, "till it left very few sheep alive."

1302.—A great loss of cows (*Bo-dhith*) in Ireland, and a slaughter (*Ar*) upon all the beasts this year. In 1308, there was another murrain in Ireland.—*Census of Ireland.* According to Lingard, an epizooty raged among the cattle in England in 1315. And in 1321, there was a great destruction of cows, throughout all Ireland, "the like of which was never known." The murrain continued to prevail in 1322, and returned in 1324, attacking, in the latter year, oxen and kine of all kinds. In 1335, there was "a murrain of cattle and a dearth of corn" in England.—*Knighton.*

1338.—In this year, the most part of the sheep in Ireland perished, being, according to the writers of the Census of Ireland, the first *ovine* epizooty that has been recorded in that island.

1348.—This year was memorable for the commencement of the black death in England, and for the prevalence of a severe epizooty among cattle and other animals. "They died," says Short, "in holes, furrows, and ditches, in innumerable multitudes over the whole kingdom." The disease among the cattle continued during the following year not only in England but in Wales, "which," according to one authority, "was the origin of taking gold in payment for cattle from Englishmen."—*Iolo.* (Welsh MS.)

1360-62.—Short remarks, that there was, in the former year, "a very great death of cattle and horses:" and Stow states that, in the latter year, "men and beast perished in England in divers places."

1370.—"Great mortality in cattle—the like seldom heard of."—*Short.*

1443.—In this year, the second epizooty of sheep, in Ireland, is recorded, and the third among bees: and, in 1445, a murrain among cattle. In 1450, there was "great loss of cattle," and in 1473, "a great destruction of cows in Ireland."

In 1500 and -4, another severe murrain with great destruction of cattle prevailed all over Ireland and also in England. In 1504, a great drought, with failure of the crops of grass and hay added to the loss by murrain. Herds of cattle, driven out in the morning, lost two-thirds before their return in the evening.—*Mansfield Chronic. Book* 1, *p.* 402.

Webster states, that the distemper destroyed much cattle in England in 1514 ; and a great mortality is mentioned as having occurred among the cattle in 1543. Mutton was 2s. 6d. a pound, and the Lord Mayor was prohibited from having more than *seven dishes* at dinner, and the Sheriffs and Aldermen *six*, under a penalty of 40s. for every supernumerary dish !—*Maitland's London.*

A great mortality of men and cattle in 1572 in Ireland, and in England, in 1581.—*Short.*

No particular epizooty appears to have prevailed from this period either in England, or in Ireland, until 1682. But, in this year, there was, according to Short, "a grievous murrain among cattle both here and abroad. It was a disease styled *angina maligna.*" The same murrain prevailed in Ireland the following year. Dr. Stare states : "The contagion was observed to make its progress daily, spreading near two German miles in twenty-four hours. This, they say, was certainly observed by many curious persons, that it continually, without intermission, made progressive voyages, and suffered no neighbouring parish to escape : so that it did not, at the same time, infect places at great distances. They added, that cattle at rack and manger were equally infected with those in the field." •

1687.—According to the writers of the Census Report for Ireland, the last epidemic period of the 17th century commenced this year, and lasted until the year 1700. It embraced epizootics, pestilence in the human race, and partial famines, with plagues of insects.

In 1711 commenced a severe murrain in England, as, also, in Europe. It was at its height in 1713, and, in 1715, spread over those parts of the country that had been previously spared. Short states, that it prevailed, at the same time, in Holland for two or three years, and carried off 300,000 cows, oxen, and bulls.

In 1716 there was a general rot among sheep in Ireland ; and, in 1736, a pestilential distemper made sad havoc of the cattle and

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• Phil. Trans., vol. 13, p. 93. A.D. 1683.

swine in the south of Devonshire. In 1741, again, there was, "death of horses, cattle, and sheep."—*Short.*

1745.—The memorable murrain which raged this year in England commenced in Turkey, and then spread over the whole of Europe. It continued to prevail for twelve years, in spite of all the precautionary measures that were employed, and in spite of the adoption of the free use of the pole-axe.

There was a slight return of the plague in 1770 and 1781, but it was not very general, or very fatal: since which time, the great epizooty, or plague murrain, of the previous epoch, has disappeared entirely in England.

It is thus apparent, that although epizootic murrains, the same as pestilence in man, have been almost entirely unknown in England for the previous century, they were as common formerly as they now are in Russia—possibly more so. It is no less apparent that these murrains sprung up spontaneously. That the murrains which prevailed in England and Ireland, during the first ten or twelve centuries of the Christian era, arose spontaneously can admit of no doubt. There could have been no importation of cattle at that period from the Continent, and none from England to Ireland, or the reverse. If, therefore, epizootics arose spontaneously in these islands formerly, no possible reason can be assigned why they should not do so now.

With respect to the second proposition that was laid down, viz., that the disease is peculiar to the bovine race, the argument was this. The disease being confined to the bovine race, the poison productive of it must be generated in the body of the ox, and be thence propagated from individual to individual. This argument was maintained with great warmth at the commencement of the disease in England. "Contagious typhus," observed M. Renault, Director of the Veterinary School at Alfort, in a Memoir written at the time, "is a

malady peculiar to the bovine race. Other species and man may then go with impunity, for themselves at least, into the most active centres of contagion." The same conclusion was drawn by Mr. Simonds, and other writers in England. This opinion was combated by me in a brochure published at the moment; it being impossible to draw any deduction from this fact, as it is characteristic of all epidemics to attack by classes. This has been already shown to be the case in the human race: and the same phenomenon has been remarked in the brute creation. Thus, in the murrains that prevailed in England in 694, and in Ireland in 776 and 797, the cows were principally, if not exclusively, attacked. The same fact is mentioned in 1133 and 1321: but, in 1088, the oxen perished (*ar-mor*), whereas, in the previous year, the cows died (*Bo-ar*, cow mortality). At another time, also, sheep will be attacked, and the oxen be spared: or these classes will be spared, and horses or swine, or dogs, be attacked. Dr. Layard, also, while referring to the cattle plague of 1745, says:— "I have seen sheep, hogs, young pigs, horses, and dogs in the midst of the infection, without being in the least affected by it." This was also the case with the disease that affected the *black cattle* a few years before, and which did not affect other species. The father of medicine also mentions the same fact, and Lancisi confirms the opinion of Hippocrates: as also does Dr. Mead, in his discourse on the plague. It is unnecessary now to rest on analogy, in order to show that no argument can be advanced from the limitation of the disease to one class, or species, as the conclusion received a speedy refutation by the extension of the cattle plague to sheep. On Sept. 25, 1865, Mr. Simonds sent in a Report to the Privy Council, stating that he had visited Mr. Temple's farm, in

Norfolk, agreeably to the instructions he had received, some lambs belonging to the above gentleman having been attacked with a disease resembling cattle plague. At that time, 46 had either died, or been killed, and 18 were in a precarious state, out of a flock of 120. The Professor states:—"My examination of the lambs showed that they were unmistakeably the subjects of the plague. The symptoms agreed, in almost every particular, with those observed in cattle affected with the malady, and the *post-mortem* appearances were also identical." It is then added, that the diseased parts of one of the lambs killed, having been sent to the Veterinary College, "these were examined by my colleague, Mr. Varnall, who at once recognised the special changes of structure which are caused by cattle plague."* This announcement caused great consternation among the agriculturists; more especially as several other flocks of sheep were attacked about the same time, in different parts of the country. Some Veterinarians, and the majority of the practical men, denied that these were genuine cases of cattle plague: but there can be little doubt, after the evidence adduced, that some of these outbreaks at least, if not all, were due to the same cause as that which produced disease in the bovine race. The fact has been since confirmed by a variety of independent observations. A disease, similar to the cattle plague, has been observed and recorded in Bohemia, in Galicia, Austria, Hungary, Poland, and Turkey.† In a Report of Professor Seifman, of the Veterinary College, Warsaw, are the following remarks:—"The Rinderpest was, at first, considered to be confined to

* Circular of the Privy Council, Oct. 5, 1865.

† Report of the Veterinary Department of the Privy Council, p. 299.

cattle, but it has now, however, been found to extend to sheep.* A work was also published in 1864, by Dr. Röll, of Vienna, with the title, "Die Rinderpestähnliche Krankheit der Schafe und Ziegen" (the disease resembling rinderpest among sheep and goats); and a Report was also published in Turin the same year, entitled "La Malattia delle pecore e delle capre simigliante alla Peste Bovina." We have thus proof afforded us, that one of the principal arguments adduced, in favour of the contagious origin of the cattle plague, falls at once to the ground.

Notwithstanding these facts, it was concluded, at the time, that the cattle plague was imported into this country: and that opinion is still maintained. When this importation theory first sprung up in England, it is impossible to say. As, however, its congener, in the human race, was considered to be contagious, we may be certain that the murrain of that epoch would also be regarded as contagious. If so, its propagation from animal to animal would also be referred to contagion. Such was the fact. Still, we hear nothing of the "importation theory," by the writers of that period, until the severe visitation of 1745: the reason being, doubtless, that no cattle were imported from abroad at previous periods — England having always been a great grazing country.

According to some writers, this murrain was introduced into England from Holland, by the skins of some diseased animals, purchased there: although a law existed in Holland, that the skins of all animals, that died of the disease, should be buried.† Dr. Mortimer, who has

* Circular of the Privy Council, October 5th, 1865.

† Bates, Surgeon to George I., who has written on the cattle plague of 1714, remarks, incidentally, while referring to the murrain of

written on this particular visitation, states, that it was to be referred to the importation of two calves from Holland, by a farmer at Poplar, who wished to improve his own breed! When the Contagionists differ, who shall decide? The best plan, in such a case, is to conclude that neither the one story nor the other is true, otherwise there would not be this discrepancy. The two calves, in all probability, were Dr. Mortimer and the Poplar farmer: the one for thinking of sending for calves to improve the breed of his cattle, at the very time when a severe murrain was raging in Holland; and the other, for believing such a tale! Although the attempt has not been made by the writers of that period, later writers have attempted to prove, that the previous murrain—that of 1714—was also imported. One of these is Mr. Youatt, a veterinary surgeon. After stating, that this epizooty—which, like all the others, when prevailing epidemically, had first appeared in the East—had been introduced into Italy by a Dalmatian ox, he adds, "that the malady reached Piedmont in 1714. From Piedmont, it easily found its way into France. All the provinces in the south of France, and (those ?) bordering on Germany, were devastated by it. And now its progress was rapid and murderous to a fearful degree: for, before *the end of the year*, it had reached Brabant and Holland, in the latter of which at least 200,000 cattle perished, and *it had crossed the channel to England*." Let us now turn from this imaginative account to hear how a plain tale will tell—one founded on facts, not on romance. By referring to the short account previously given of this murrain, it will be seen, that it commenced in England, not in 1715, as would have

1665, that "the King of Prussia, and *the States of Holland*, issued decrees commanding them (the dead cattle) to be buried *upon pain of death*, with other severe penalties."

been the case, had it been imported from Holland, but in 1711. More than this, the epizooty was at its height in 1713, actually before it had reached Holland, while it only spread, in 1715, over those parts of England that had not been previously visited! And yet, it is stated by Messrs. Simonds and Browne, in the Report before referred to: "No sufficient account has been left as to the manner the disease was introduced into the neighbourhood of London, but of its importation from the Continent, and probably from Holland, no reasonable doubt can be entertained."* What a pity it is that writers do not take the trouble to search for facts, before drawing conclusions on so important a subject. These are all the accounts extant of the importation of murrains into England from the Continent at former periods. We will therefore turn to the most recent example of alleged importation, the evidence respecting which can be more easily sifted.

When the cattle plague broke out in the Metropolitan Market, the Contagionists jumped at once to the conclusion, that it had been imported: and as a cargo of cattle from Revel had recently arrived at Hull, it was further inferred, that the germs of the disease had been brought by those animals. Some of the cattle had been sold in the market three weeks previously, but none of them were diseased. One bullock had been sick on the voyage; but whether from sea-sickness, or some other ailment, is uncertain. That the cattle exhibited no signs of disease on their arrival at Hull, has been acknowledged by those who had the best means of ascertaining the truth—the Cattle Plague Commissioners. They observe, in their Report: "It is proper to add that we have been furnished with the original certificate, dated the 29th of May, and signed by

* Loc. cit., p. 238.

two veterinary surgeons who were charged with the examination of the cargo, that they had examined it, and that it was sound and free from disease, as well as with a subsequent declaration by them to the same effect." Nor did any signs of disease manifest themselves subsequently among these cattle. "Inquiry has been made," remark the Commissioners in their second Report, "of the several persons through whose hands the cargo of bullocks imported from Revel, in May, passed, after their arrival at Hull. . . . . Questions have been addressed to all these persons, and from their answers we have no reason to doubt, that all the animals were bought and slaughtered, so far as the purchasers could judge, *in a perfectly healthy state.*" In addition to this, it has been proved, by evidence that cannot be disputed, that the plague did not exist at Revel, or in the surrounding districts, nor had it done so for some years.* Indeed, as M. Schrader, veterinary surgeon in Holland, remarked, in a communication to Her Majesty's Consul-General in Hamburg, "the great distance of Revel both from Great Britain, and from the cattle districts of Southern Russia, seems to render it unlikely that diseased cattle should have reached England from that port without observation."† In opposition to these opinions, it is stated, in the recent Report on the Cattle Plague by Professors Simonds and Brown, that thirteen of the cattle contracted for were rejected at Revel by the agent of the purchasers in England. These had to be replaced by others, and the requisite number

* A document was sent to Mr. Irwin, the importer of the cattle, from Herr Maassen, First Veterinarian of the Province of Esthonia, and certified by Mr. Gerard de Somanton, H.B.M's Vice-Consul, in which it was stated that neither the cattle plague nor any other contagious disease had existed in the province, either in that, or in the preceding year. This document was inserted in the "Times," Sept. 8th, 1865.

† Loc. cit, p. 258.

was selected from a drove of forty-six. "No satisfactory account has been given relative to the part of Russia from which the dealers obtained the forty-six cattle, although *it was said*, that they had come from the neighbourhood of St. Petersburg." Such information is unnecessary, for the plague did not exist at that time in Russia; it only prevailed in the Western Provinces from January to July, 1864; and in the latter part of that year near St. Petersburg. Whence, then, could the infection have been derived? *Ex nihilo nihil fit*, has hitherto been considered a truism: but here are healthy cattle, that could not possibly have come in contact with the plague for months, accused of carrying the seeds of it to England. These writers add: "The entire facts of the case cannot therefore be considered as being irreconcilable with the opinion that the cattle plague was brought to England by the Revel cargo." Such a conclusion is not only irreconcilable with the previous facts but with subsequent ones.

If the seeds of the disease had existed in any of the bullocks, added to the Revel stock, we should have expected to find, that some of the latter would have become affected, more especially if the disease be only half as infectious as the advocates of the doctrine of contagion are *obliged* to make it, in order to substantiate their theory. And yet none of these animals, as we have seen, exhibited the slightest symptom of ailment up to the day of their death; although packed closely together on board a steamer for eight days, hide to hide, and mouth to mouth. Although the time was not so long for the other animals, there was more than sufficient for the incubation of the disease with the thirteen Russian cattle.* Independently

* Professor Unterberger says: "The *stadium incubationis* which

of the twelve or thirteen days that elapsed from the date of embarcation to the day of slaughter, several days will have to be added for the journey from St. Petersburg to Revel—a period double that which is supposed to be necessary for the development of the disease. Granting that the germs of the disease can remain dormant in the system thus long, and it is necessary to give these theorists a long rope—as long as from here to the North Pole—otherwise you are unable to catch them in the noose that will destroy both them and their hypotheses: there are still other facts, altogether at variance with the doctrine of contagion. The portion of the Revel cargo sent to London was in the market held on the 31st May. Of the 321 cattle imported, 146 were sold at Hull for *immediate* slaughter, and sent to Wakefield, Manchester, and Derby. The remaining 175 were sent to London, where they arrived on the 31st May, and were placed in lairs near the cattle market. On the following day they were, with the exception of twenty, sent into the market and sold. The twenty left, together with seven of the others, were sent to Gosport and Portsmouth, the same day, for the supply of the troops. The remainder were sent to the slaughter-houses in Newgate and Whitechapel. The whole, therefore, would have been slaughtered within two or three days of their sale, and three weeks, at least, before the plague broke out. More than this, with the exception of the Metropolis and Hampshire, the disease did not break out in any of the places to which the cattle were sent during the month of July, while it did not appear in Hampshire until the 16th of July—six weeks after the

begins with the day of taking, and ends with the breaking out of the disease, lasts about four or five days: sometimes, but *very rarely*, seven."—"Medical Times," Dec. 9, 1865, p. 627.

death of these unfortunate animals.* But none of the cattle that were in the market on that day, and that were brought into immediate contact with the Revel cattle, were attacked with the plague. The first cases of the disease, witnessed in England, were at Lambeth, on the 24th June, with two cows purchased in the Metropolitan Market on the 19th. The next were at Hackney, on the 26th, in Mr. Baldwin's dairy; and the third outbreak was among the dairy cows of Mrs. Nicholls, attacked on the 27th June: these animals having been also purchased in the same market on the 19th. We have thus proof that none of the animals, attacked with Rinderpest in England, had ever come into contact with the Revel cargo—the presumed source of infection. The Cattle Plague Commissioners, in their first Report, state: "No animal is proved to have contracted the disease in the Metropolitan Market from the 1st (of June)—the date of its supposed introduction—to the 19th—the date of its supposed transmission to the Islington, Hackney, and Lambeth cows." How, then, did these animals imbibe the seeds of the disease? Were the germs scattered over the market by these foreign carriers of infection with a premeditated design; and did they remain adhering to the soil or the pavement, for these three weeks, in spite of scavengers, the wind and the rain? This is the only conclusion to be drawn on the subject, if the infection were brought into England by the Revel oxen. But then there are certain circumstances that rather militate against this conclusion. If the cows taken to these three dairies imbibed the seeds of the disease in the market, how was it that no other animals were attacked at the same time? Was the virus concealed in some corner to which these cows only had access? Then, again, two markets had been held

* Veterinary Report, p. 259.

between the 20th of May, and the 19th of June: as such, if the germs of infection had been present, they would, we may presume, have been imbibed and carried away by some of the cattle assembled on these two occasions. But not a single ox, or cow, among the thousands that were in the market on these particular days was attacked with Rinderpest: and the fact could not have escaped detection had such been the case. We may therefore conclude—contrary to the *dictum* of Messrs. Simonds and Brown—that the cattle plague was *not* brought to England by the Revel cargo, and, consequently, that the animals first attacked in this country, did not receive the infection from this source.

Other suggestions have been made, as that by Dr. Letheby and some members of the Royal Commission. This is, that the disease was imported from Holland, a bovine malady having prevailed at Utrecht in May, 1865. But it has been since shown, that this affection was not cattle plague, which only commenced in July, and was not proclaimed by the Dutch Government until August. The Dutch, in fact, accuse us of importing the disease into Holland, and with more probability. Other suggestions, still more improbable, have also been made: but as we are dealing now with facts, not fancies, we may pass these by. Notwithstanding, Messrs. Simonds and Brown remark: "From all the evidence which has been collected, one fact *is proved*, viz., the foreign origin of the disease, although the precise place from which the infection was brought cannot be accurately determined." That is to say, these writers infer, that a thing is so, because it must be, or ought to be, so—a very logical way of arguing.

This determination, on the part of the Contagionists, to prove the importation of the cattle plague, is simply the

consequence of another and a foregone conclusion, viz., that the cattle plague is a contagious disease, and that it is propagated by contagion alone. Thus, Professor Seefman, in the Report before alluded to, says: "The contagious qualities of this disease exceed those of any known disease of man or beast, and for this reason, when once propagated it is extremely difficult to arrest its progress. The contagion is conveyed not only by infected cattle, but by those apparently sound, by pigs, poultry, hides, manure, bones, offal, straw, buildings, and the clothes of herdsmen or others; in fact, by every substance which has been in contact with the diseased cattle." At a Meeting of the Royal Agricultural Society, Mr. Simonds said, "he believed the sphere of infection around the disease was very limited, and he was inclined to think that the air itself did not become impregnated with the virus to any considerable extent; but that the secretions from the eyes and nostrils, and the *excreta* in particular, were charged with morbific matter. He believed that if they took a small quantity of that matter, on the point of a lancet for instance, or on the end of a stick, they could with that alone convey the disease from John o' Groat's to the Land's End." These opinions and conclusions were reiterated by the Cattle Plague Commissioners, in their First Report. They observe: — "1. The natural voidings of a diseased animal, as well as the discharges which come from its mouth, nose, and eyes, during the progress of the disorder, can be carried by men and animals so as to infect sound cattle, and in this way the disease is often propagated. 2. Both sheep and dogs can carry the seeds of the disease, so that they should be carefully looked after, lest, in having access to diseased cattle, they may attach to themselves portions of excrement or discharges, and communicate the contagion to

sound cattle. The farmer will do well to recollect that both sheep and goats take the plague in a virulent form, although they are not, perhaps, quite so susceptible to the influence of the contagion as horned cattle; but even when they do not take the disorder, the wool of the sheep and the hair of goats can long retain the morbific matter, and then transfer it to cattle. 3. The particles of the poison can be drifted by the wind to some distance, experience having shown that a space of considerably more than a hundred yards affords no protection." In addition to the above, Mr. Simonds stated, at a *conversazione* of veterinary surgeons held at the Freemasons' Tavern, that "hunting, he considered, in infected districts, was a cause of spreading disease—hounds running through pasture lands, which contained the excreta of infected animals, being a very likely medium of propagating the plague. It could also be propagated through the medium of birds —starlings being notorious for their habit of perching themselves on the backs of sheep. He was aware that farmers were large breeders of pigeons, which were apt to fly from one place to another in picking up their food. It had happened that disease had been communicated from one farm to another in a very mysterious manner, until it was discovered that large numbers of pigeons got their feet and legs covered with excreta in cattle yards, and afterwards visiting one place after another while feeding. The cause of spreading the disease, in short, might be a puzzle, but it could not be traced to a spontaneous origin." The Royal Agricultural Society, also, in their Circular, add: "Another fact of equal importance, but not generally known, is that the special poisonous material, or infectious matter, on which the disease depends for its existence, is multiplied to an extent scarcely to be estimated in the system of every fresh

victim ; so that it is quite possible for one diseased animal to be ultimately the cause of the death of thousands." And, lastly, we are informed by Dr. Letheby, in his Report to the Commissioners of Sewers, for 1865, that the disease is frightfully contagious. "The nature of the infectious matter is not yet discovered, but it is, to use the words of the Royal Commissioners, subtle, volatile, and prolific in the highest degree. It is living germinal matter, so minute that its particles are probably less than the one hundred thousandth of an inch in diameter."

Such are the conclusions that have been drawn by scientific men, on what is supposed to be a scientific subject. If, however, this be science, then science is not wisdom but folly : it is not the knowledge of facts and the acquisition of truth, but the diversions of the imagination and the promulgation of error. The preceding conclusions are not only the most gratuitous and the most absurd, but they are the most unreasonable and the most puerile ever put forth by either scientific or unscientific writers. We laugh at these crudities—begot by fear and propagated by ignorance—when we meet with them in the writings of the middle ages; and yet we are now called upon—solemnly and deliberately called upon—to accept these absurdities as truths in the middle of the nineteenth century. If the sun has not stood still during this long interval, it is evident that science has, or, at least, this branch of medical and veterinary science. Instead of adopting the Baconian method of reasoning, and proceeding from effects to causes, these writers have pursued the very opposite course. They first guess at the cause, and then try to account for the effects by a reference to this preconceived hypothesis. In this way, the theory is not bent to the facts, but the facts are bent to suit the theory.

Hence the absurd and ridiculous conclusions that have been broached on the subject.

According to the arguments and conclusions drawn by these theorists, the *virus* of cattle plague must exist in three different states—in a gaseous, a fluid, and a solid state. If this disease can be propagated from an infected to a healthy animal at the distance of a hundred yards, as we are informed, it can only be by means of the surrounding air. The poison, therefore, must be in a gaseous or volatile state. Then, again, if the virus be present in the secretions and excretions, it ought to be in a fluid state. On the other hand, if this infective matter can be retained by inanimate substances, and be transported long distances, it must then be in a solid state; otherwise it would be speedily dissipated in the surrounding air. That a vast number of substances exist in each of these conditions is undoubted; but, then, it is only under different circumstances, as of temperature, &c. But here we are asked to believe that the virus of contagion exists in these three states under precisely the same circumstances. But that is impossible. If the poison be of so volatile a nature, that it is given out by the lungs, and in such quantities as to infect healthy cattle at a distance, the probability is, that it would all pass off by that channel. In that case, the secretions and excretions would not be infectious. If, however, it were found, that the secretions and excretions were dangerous, we might then conclude, that the poison was not a volatile substance, and that the expired air of the sick would be innocuous. Still, these secretions would not be dangerous, unless applied to an absorbing surface; or unless introduced directly into the blood—a result that could seldom be witnessed. Then, again, if the morbid matter be contained in the secretions and excretions, it

would be separated and precipitated when the fluid part was dissipated, unless it were volatilised. If, however, it were not given out in this state by the lungs, with a temperature of 98°, it is not likely that it would be under the ordinary temperature of the atmosphere. We may therefore conclude, that the virus, if given out of the body in this way, would be left ultimately in a solid state. In this form, it would be no more dangerous than when in a fluid state; if anything, less so, as it would not, in that state, be very easily transported by either animate or inanimate objects. It matters not, however, whether the infectious matter be left on the surface, or be carried to distant parts, it could not be productive of any mischief, excepting under exceptional and almost impossible circumstances. To produce any effect, it would have to be introduced directly into the system; or, else, be applied to an abraded surface.

Such are the arguments that may be employed, provided the disease be contagious, and provided, also, that it be propagated by contagion. But that is the question. Is the cattle plague contagious, and is it propagated by contagion? These are questions that it is necessary to ascertain if possible; not only because the question is interesting in a scientific point of view, but also because it is important—highly important—in a social, nay, even political aspect. The prosperity of the agricultural classes, and the welfare of the whole population, may possibly depend on the right solution of this question.

If the germs of the cattle plague be of so subtle a nature, that they can be carried in the body of a healthy animal from St. Petersburg to Revel, from Revel to Hull, and from Hull to London; remain dormant in the market for three weeks, and then be transported to Norfolk by other cattle, we ought not to be surprised at the ravages

of the disease in England in 1865. As one writer has remarked: "The advocates of the theory of importation and contagion find themselves with such extraordinary cases to explain, and are compelled to recognise so many means of infection, that it may reasonably be asked whether contagion, under such conditions, can ever be possibly prevented?.....The plague may travel in a cart-wheel, a stable-bucket, a smock-frock, or a truss of hay. In fact, the risks recapitulated, and the precautions described, are so minute and manifold, that there was really little exaggeration in a suggestion gravely made the other day, in these columns—to wit, that as flies were known vehicles of the disorder, every cow should be put to bed with musquito curtains." * The wonder is, that a single ox or cow has been left alive: or, at least, that we were not reduced to the same condition as Ireland, at the commencement of the Christian era, when only one bull and one heifer were left alive—the bovine Adam and Eve of a new generation. When, however, we examine the statistical returns, we find, that only a fraction of the cattle, in this country, were attacked in 1865. The returns were not very accurate previously, but when the census was taken in March, 1866, there were then 4,785,836 head of cattle in Great Britain. If to these we add, the cattle that died—133,499, the number of diseased animals that were slaughtered—100,180, and also the healthy ones, kindly sent out of the world to prevent their being liable to the common ills of humanity, and amounting to 56,929, we shall have a total of 5,076,366; the number that must have existed previously to the outbreak. Of this number, 278,439 were attacked, being about 5 per cent. It thus appears that this dreadfully contagious disease did not produce such very serious results after

* The Times, October 6, 1865.

all, for there were 4,785,836 cattle still alive in Great Britain, on the termination of the murrain. How it was that all these escaped the contagion, flying about in all directions, we must leave the Contagionists to explain: it would be impossible for those not initiated in the mysteries of this doctrine, either to explain or understand it.

There is another circumstance, still more singular, and still more difficult of solution. This is, the immunity of certain districts, at the very time that others were as severely scourged. No less than twenty counties in England, Scotland and Wales remained entirely exempt, on the termination of the epidemic. And yet, the position of some of these counties was remarkable. Thus Westmoreland was completely surrounded by the disease; but there was not a single case within the magic circle. Had this county been surrounded with a high wall, it could not have remained more entirely exempt from the inroads of the disease. In Cheshire, on the other hand, 98,880 cattle were attacked. We shall of course be told, that this difference arose from the want of care and precaution in keeping out the disease. Granting that such were the case, although we might have inferred, that Cheshire, a great grazing county, would have been more careful than any other to keep out the infection—and she had ample time to do this, for the disease did not break out there until October, 1865—how are we to account for the slight ravages of the disease in some of the other counties, after the infection had entered? The per-centage of attacks varied in different counties from only ·002 in Inverness to 68·12 in Cheshire. In the first instance, only one animal out of 45,334 was attacked; but, in the other, 93,880, from a total of 137,808. In other instances, again, there was a low rate of mortality, even with numerous *foci* of infection. This will be evident by

a reference to the examples contained in the following Table, in all of which the ratio of attacks is less than 1 per cent. :—

TABLE 19.—In which the ratio of attacks, and the number of farms, etc., invaded by the disease, in the following counties, have been given.

Counties.	Stock of Cattle.	Number Attacked.	Ratio per 1,000.	Number of Farms Attacked.
Devonshire ...	184,077	221	1·20	37
Wiltshire	77,724	123	1·50	25
Aberdeen ...	133,451	316	2·30	39
Leicestershire ...	89,115	418	4·60	70
Durham	52,322	406	7·70	58
Hampshire ...	48,688	380	7·80	52
Cornwall	133,652	1,069	7·90	124

From this Table, it will appear, that the proportion of attacks bore no relation to the number of *foci* of infection, nor yet to the number of cattle that surrounded them. For instance, in the county in which the ratio of attacks is the least, the number of cattle in the surrounding districts is nearly four times as many as in Hampshire, where the ratio of attacks, with one exception, is the highest. Then, again, the ratio of attacks in Cornwall is only a few fractions higher than in Hampshire, although there were, in the former county, between two and three times as many *foci* of infection, and although the number of cattle was nearly three times as many. These are anomalies that cannot be explained by a reference to the doctrine of contagion; and to a disease which is, as we are told, so virulently contagious—unless it could be shown,

that the winds did not blow, in these counties, during the time of the visitation; that there were neither starlings nor pigeons kept, or other birds, or sheep, or goats, or anything in fact: that the herdsmen never left the infected district, and that the cattle were not visited by Inspectors.*

Although it be easy to account for the immunity of particular counties and localities, in which no cases have occurred, on the supposition that precautionary measures were adopted, it is not so easy to account for the immunity of particular districts, or of certain cattle, placed in the midst of the pestilence. An instance of this kind was afforded at Lord Granville's Dairy Farm in the Finchley-road. On the 13th July, five Dutch cows were purchased in the Metropolitan Market, and placed in quarantine at Child's Hill Farm—a mile from the other cow-sheds. On the 22nd—eight days after—one cow was taken ill, and died in five days: the others were taken ill in succession, and in another five days all had died or been killed. "Every precaution was used," says Mr. Penton, Lord Granville's bailiff, in a letter to his Lordship, "to prevent the spread of infection here: the men who attended the sick cattle were not allowed to go among the healthy ones, and *vice versâ*." Notwithstanding this, on the 8th August, five cows were attacked, within a few hours of each other, in the best built, best drained, and best ventilated shed on the farm; and in which no new stock had been added for nine weeks. These cows, together with the healthy ones, or which, at

* It appears to be the height of absurdity, to make such a fuss about the herdsmen and others, when the Inspectors, who visit the diseased animals, attend to them when sick and cut up their bodies when dead, travel over the country in all directions, and visit not only the diseased but healthy cattle.

least, had not exhibited any signs of disease, were immediately removed and slaughtered. But this did not stop the progress of the disease, for, on the 20th August, it broke out in another shed of thirty-five cows, some ten yards from the former one, and continued its ravages, taking from two to four cows daily, till they were all gone but two. Of these one was not *attacked:* the other which was a bad case is cured. All these cows were of the English breed: but in two other sheds, standing about forty yards from the infected one, and in which half the stock was English and half Dutch, no cases had occurred, when the letter was written in October. Mr. Penton also states, that he purchased twelve fresh cows on the 16th August, and placed them in the quarantine shed at Child's Farm, *where the first cases were observed:* and that these had continued quite healthy up to that time.

The above history is a very instructive one: for we have here three distinct outbreaks. Without waiting to inquire into the cause of the first outbreak, for this is immaterial, we find that the second occurred seventeen days after the former, and in a shed situated at a mile distance. As, also, every precaution was taken to guard against infection, we may conclude that the disease was not derived from the cows first attacked—more especially as the stock subsequently put into that shed remained perfectly healthy. But no matter whence derived, we should have expected to find, in accordance with the infectious and pole-axe theory, that the disease would have been "stamped out" on that farm; all the cattle in that shed having been slaughtered a few hours after the disease made its appearance among them. And yet, strange to say, it extended itself, twelve days after, to a neighbouring shed: thus showing, as was before remarked, that the pole-axe does not prevent the spread of the

disease. But the strangest part of the story is, that the disease was arrested after this; although, instead of handing over all the inmates of the shed to the tender mercies of the executioner, they were consigned to the veterinarian and others to be treated *secundum artem.* And yet there were, in this shed, thirty-four distinct *foci* of infection: while there were two sheds, filled with healthy cattle, within forty yards of them. These anomalies have attracted the attention of others. The Editor of the "Agricultural Gazette" remarks: "Lord Granville's herd was declared to have become infected by his herdsmen having habitually passed through the field, in which the cows belonging to a neighbour had died. They might have trod in the cow-dung there, and thus brought the poison with them. But it is a curious fact, which makes this theory untenable, that the neighbour, to whose field reference is thus made, stocked it again with cows within a few weeks after his loss; these have remained healthy ever since, and it was not till a fortnight after he had done so that the first cow in Lord Granville's cow-house suffered. Take another example: Mr. Collinson Hall, at Navestock, near Brentwood, is in the midst of the disease: neighbours have suffered all around him: his own farm is intersected in every direction with public roads. His principal cow-house is in the angle of a crossing of this kind, and men are in and out of it all day long, walking to it along the public roads, which are in direct communication with infected farms. His cow stock, upwards of 100, have been healthy hitherto. He has them in several separate lots, turning them daily into the grass fields, and keeping each lot always and absolutely apart from its neighbours. The only precaution used is to employ a herd boy with them, who never suffers any of them to approach the boundaries of the fields, whether they abut

upon the public road or only on a neighbour's land. Here is an instance where the risk of infection is a maximum, and yet the single cow of a clergyman in a neighbouring parish, living in a secluded paddock, and never approached except by the 'gardener,' subjected thus to a minimum of risk, has taken the disease and died." *

Passing from these anomalies, we will now endeavour to ascertain, if the cattle plague was propagated, as stated, from the Metropolitan Market to the localities first attacked. As previously mentioned, the first attacks in England were on the 24th June, with two Dutch cows that had been purchased in the Metropolitan Market on the 19th inst. Whence, then, did these animals derive the infection? It was not from the Revel cargo, for the reasons previously adduced; nor could it have been from contact with diseased animals, or infected articles, for not a single case of the disease had occurred in England up to this date. These cows must, therefore, have imbibed the seeds of the disease from healthy animals that had become infected from some unknown source—or, else, the disease must have arisen spontaneously. The latter is, in all probability, the right conclusion: for a bull, that had been on the premises for some time, was attacked on *the following day*—the 25th. As there had not been sufficient time for the incubation of the plague, after the arrival of the two cows, no other inference can be drawn than that the disease arose spontaneously in the bull, on the spot. If so, we may reasonably conclude, that the cows derived the disease from the same source; all other causes that have been assigned, being insufficient to account for the outbreak. As will be remembered, the next outbreak was in the dairy of Mrs. Nicholls, at Islington, on the 27th June: or, rather, at Hackney, on the day previous.

* Saturday, October 28, 1865.

No detailed account of the latter outbreak having been given, we will pass that over.

The cows first attacked in this dairy had also been purchased in the Metropolitan Market on the 19th—an interval of eight days—sufficient time to account for the origin of the disease, on the supposition that it arose spontaneously in the sheds, if not derived from infection in the market. As, however, these animals, like the former, had not been in contact with any diseased cattle, and as the market itself, for the reasons before given, could not have been a *focus* of infection, in the contagious sense of the term, the plague, in this instance, the same as in the former, must have arisen spontaneously. This inference receives support from the rapid extension of the disease, no less than seventy-four attacks, in three dairies, having been recorded on the 1st July—within a week. In these three dairies there were 151 cows, out of which number 19 had died, 45 diseased animals and 50 healthy ones had been killed, or 114 out of the 151. In Mrs. Nicholls's dairy, 16 cows had died and 69 had been killed in the first fortnight, out of 123 stock. As those first attacked had been in a shed by themselves, it being the custom at this dairy to put all newly-purchased animals in quarantine, for a time, it is difficult, if not impossible, to understand, how the other cows became infected in so short a time.

Still more difficult is it to account for the rapid extension of the disease to other cow-sheds. On the 8th July, 536 fresh cases, in 8 cow-sheds, containing 610 cattle, were reported. Of these, 99 had then died, 364 diseased animals and 55 healthy animals had been killed. On the 15th July, the plague had broken out in fifteen different localities, farms or sheds, extending from Lambeth in the south to Hendon in the north, and from Goswell-road in the east to Marylebone in the west. As all these outbreaks

could not be traced to the introduction of fresh cattle, the only solution to the problem is, that the disease, in these instances, arose spontaneously. There had been no time, in fact, for the propagation of an infectious disease from point to point, and from animal to animal. If an inoculator had gone round, with a pot of *virus* in one hand, and a lancet in the other, and had inoculated all these animals, the disease could not have spread with more rapidity. That the plague arose spontaneously in the dairy of Mrs. Nicholls, we may conclude from the remarkable fact, that in the murrain which appeared in London in 1714 the disease *broke out at Islington*, and at nearly the same time of the year. Mr. Bates, surgeon to George the First, states: "About the middle of July, the distemper appeared at Islington, and thereupon their Excellencies, the Lords Justices, having notice of it, were pleased to command that I should examine into the truth of the report of its being contagious, &c."* The murrain of 1745 also first appeared in the neighbourhood of London, whence it extended over the length and breadth of the land, and continued its devastating effects, with almost unmitigated severity, down to 1754-5. The same locality was again visited in January, 1867, as will appear from the following account: "A virulent outbreak of Rinderpest has just taken place in Islington, in the dairy of Mrs. Nicholls, Liverpool-road. The first case of the disease occurred on Monday last, and on being reported to the authorities a notice was issued by the Metropolitan Board of Works declaring the premises 'an infected place,' in accordance with the provisions of the 'Cattle Diseases Prevention Act, 1866.' Immediate steps were taken by those in charge of the dairy to separate the healthy cattle from those which showed symptoms of the disease, but without any beneficial effect, for on the following Thursday

* Paper inserted in the Philosophical Transactions.

four additional cases occurred. The infected animals were at once slaughtered by order of the Cattle-plague Inspectors, but the disease had taken root, and on Friday five more cows were attacked, and had to be destroyed. On Saturday last seven additional cases occurred, and fears being entertained of a further spread of the disease, it was decided at a Meeting of the Committee of the Metropolitan Board of Works, in whom power is vested in such cases, that all the animals on the premises, healthy or otherwise, should be destroyed. This order was at once carried into effect, and the entire of the stock in the dairy, consisting of forty-five cows and one bull, were slaughtered on Saturday evening." It would thus appear that Islington, or, at least, that part of it where the dairy was situated, is, like so many other spots on the surface of the globe, pestilential: and hence the repeated outbreaks of disease these.

The next place visited by the disease, after the three outbreaks at Lambeth, Hackney, and Islington, was not, as we might have inferred, in the immediate neighbourhood of London, but in Norfolk. The following is the account of the outbreak, contained in the Official Report:—"The plague appeared in this county about the 1st or 2d of July, and *seems to be* traceable to infected cattle brought from London, and exhibited in the market on Norwich-hill. The disease was recognised, about *the same time*, on several farms to which animals, *originally* from London, were conveyed."* And this is all the information we obtain from the Official Report on this important part of the inquiry—the point on which everything hinges, as regards the propagation of the disease by infection. We did not want to know whether the disease *seemed* to be traceable from London, but whether it actually was, and could be, traced. How these animals became infected

* Report by the Veterinary Department of the Privy Council Office, p. 14.

has not been mentioned, while it would puzzle a conjuror to tell. They left the metropolis a week before the first cases occurred ; and there could not have been, as we have seen, any source of infection previously. It ought also to have been stated how many cattle from London, or Norwich, had been received on these farms; and whether they arrived with the disease upon them : if not, how soon it had appeared after their arrival. We do not even know the date of their departure from London : although it has been stated by other writers, that they were sold in the Metropolitan Market on the 19th of June. Nor did it matter whether the other cattle attacked, at the same time on these farms, *originally* came from London or from Australia : they could not possibly have been in the way of infection previously. To these natural questions no information has been afforded. Being thus left without any official guide, we must try and extricate ourselves from the labyrinth in the best way we can ; and by our own light, instead of the light of others. It appears from the returns, that there had been in Norfolk between the 1st of July—the commencement of the outbreak—and the 8th, no less than 69 cattle attacked out of 72 on three farms. Of these, 22 had died and 15 recovered. We are thus gravely asked to believe, that a certain number of oxen sold in the Metropolitan Market on the 19th of June, sent to Norwich, and thence to the three farms, although exhibiting no signs of disease themselves, had thus suddenly infected three different herds,—22 of whom had died and 15 had recovered within the week. Is this probable ? Is this possible ? *Credat Judæus !* Allowing that the cattle from Norwich had brought the disease with them, and that they had been equally distributed on the three farms—not a very probable circumstance—it would be irrational to infer, that the remaining cattle attacked had received the infection from them. There

could not have been sufficient time. It was more like a sudden explosion from fire-damp, or similar cause, than the somewhat slow process of incubation from a contagious disease. Had it been otherwise, however, we should still have had to inquire how and where the cattle from Norwich had derived the infection? It could not have been in the Metropolitan Market, and no evidence exists of any case having occurred anywhere else previously. It matters not, therefore, where these cattle contracted the disease, whether before or after their arrival on the farms: it is certain that the disease in either case arose spontaneously in them. The probability is, that they imbibed the poison on the spot where they were attacked. We may draw the same inference with respect to the cattle said to have come, *originally*, from London. As the last arrivals from abroad had left the market on the 1st of June—and these animals must have been there antecedent to that date—it was impossible that they could have contracted the disease excepting on the spot where they were attacked.

It may appear singular to many persons that the first attacks, in all the preceding outbreaks, were with cattle purchased in the Metropolitan Market; and it has been this circumstance that has afforded to the advocates of the doctrine of contagion one of their strongest arguments. But the phenomenon admits of explanation without reference to the doctrine of contagion. It has been stated, in a previous part of this work, that when human pestilences prevail, strangers are frequently the first attacked. If that law is common to the human race, there can be no reason why it should not be equally applicable to the organism of the brute creation. If so, we can understand why the animals before referred to were the first attacked; more especially as there are circumstances favourable to

an attack with cattle that do not usually occur with the human species. These are, the fatigue, privations, and bad treatment that cattle undergo, when driven to and from these markets. Hence it is, that murrain so frequently appears in the provision parks that accompany armies: of which numerous examples were afforded in the great continental wars, at the end of the last, and the beginning of the present, century. The outbreak of disease, in these instances, has, of course, been ascribed to infection, and to the introduction of cattle from Hungary and Russia—a common, if not constant, occurrence in those wars. But this conclusion has been contested by other writers; and particularly by M. Delafond, one of the Professors at the Veterinary School of Alfort in France. He remarks:—"Does contagious typhus in oxen originate in Hungary as the plague in man originates in the East? Lancisi, Leclerc, Layard, Vicq d'Azyr, Paulet, Boniva, Leroy, and Metaxa all hold this opinion. Other persons, without contesting that origin, think that the disease may spring forth spontaneously in cattle of all countries, when they are exposed to the causes which create its development in Hungarian cattle. This opinion, put forth by MM. Rodet and Darboval, we share in. These are the causes which create the development of epizootic typhus. In places where a war of some duration exists, contagious typhus in cattle develops itself. These destructive scourges are inseparable. It is incontestable that the typhus always accompanies great movements of troops, and marches in the suite of provision of horned beasts which follow *corps d'armée*. Behold (says he) the causes of typhus. Its origin is known; its appearance has always been coincident with the disorders brought about by war. Thus, wherever the scourge of war is carried, there the typhoid scourge

declares itself." * It is to the operation of similar causes that we may ascribe the almost constant prevalence of disease among the steppe cattle: not to a particular organisation, or constitutional habit. These circumstances have been dwelt on by M. Renault, Director of the Veterinary School of Alfort, in his Memoir.† The writer observes:—"Whoever has read good German and Russian authors who have treated of typhus, and particularly of the excellent monographs of Lorinzer and of Spinola, knows that during a great part of the year more than 100,000 of those animals plough up the almost impracticable roads, which lead to the Crimea and Bessarabia, to Podolia, Volhynia, and to other provinces of the north-west and centre, attached to carts that transport into those countries the salt which is furnished in great abundance by the saline springs of Lower Bessarabia and Perecop, and which, on their return, take to Odessa the corn raised in those fertile provinces. Now, it has been demonstrated by the learned German veterinarian writers whom I have just cited, and it is now perfectly well known, that the manifestation of typhus which takes place so often in times of peace, whether in Russia, or on the Russian frontiers of Poland and Germany, in oxen from the steppes which are taken for slaughter, or in those affected by transport, had generally for principal determinating causes the climate, and the fatigues, privations, and exposures to which those animals were subjected on the long routes on which they travelled. Thus this fact has been laid hold of, to attribute to these circumstances exclusively the development of the malady in these animals, and to affirm that if bovine cattle of other countries had similar distances to go, with the same regime,

* "Police Sanitaire," pp. 159 and 338. Paris. 1838.

† The Contagious Disease in Bovine Cattle, &c., &c.

agglomeration, and fatigues as great, they would without doubt be attacked with contagious typhus." Not that these circumstances are the sole cause of the development of disease in these cattle: they are merely predisposing causes. In order to produce a specific effect, or disease, another cause—a specific cause—must be superadded. Were it not for the operation of such a cause, the cattle would merely become attenuated and lose their strength, or if they died, they would die of fatigue or starvation—not of disease. More than this, it would depend on circumstances—the epoch, the country, and the climate—what the disease might be, that would spring up under these conditions. For instance, Egypt raises few cattle, and she supplies the deficiency by importations from Kardofan and Senear. These convoys, amounting to 1,500 or 2,000 head of cattle, starting from Upper Nubia, have a distance of 400 or 500 leagues to traverse, part of the journey being across the desert. In consequence of the difficulty of providing food for so large a number of cattle, and the apathy and negligence of those who have charge of them, large numbers die on the road. M. Prince, who occupied a professorial chair in the Veterinary College at Choulerah, in Egypt, states:—"According as they went farther from the point of departure and fertile spots, these convoys marked more and more their passage by the bodies of the dead oxen which, at short distances, pointed out the road they had gone along; and when at last, touching nearly the end of their journey, the convoys stopped near Cairo, it is no wonder that their numbers were reduced by a third, a half, or even more. As to those which survived, they were, for the most part, very meagre and in the most pitiable condition. The disease which falls most frequently upon these beasts, and that which destroys so

great a number, is dysentery, complicated in many of them by bilious affections; but never did any one see that malady or any other assume, in these animals, the typhoid character. Never has it had, the least in the world, a contagious character."

These inferences allowed, we can understand why the cattle sent from the Metropolitan Market should have been the first attacked, after their arrival in these different localities. It was not because they carried the contagion with them, but because they were more predisposed than animals on the spot to be affected by the operation of the unknown cause productive of the disease. This conclusion would appear to be confirmed by the following communication. The writer remarks:—"The cattle herds in the Denny district (Scotland) are being rapidly decimated by the plague. The fatality is unprecedented. .... For a time, the pestilence was confined almost exclusively to the *Irish stock* at Dunepace and Bonnyfield, and in one or two byres in these districts: but now it is widening its hold with wonderful celerity, and every day brings to light fresh outbreaks. The plague has also broken out near Kerriemuir, in a lot of *Irish cattle* brought recently to the parish of Airlie." Again, it is stated, that "the plague has been carried to the vicinity of Gora Bridge by a lot of *Irish cattle*, taken in to pasture, after having been left over from the last Falkirk Tryst."* As the disease did not prevail in Ireland at that time, these examples prove that cattle, coming from a healthy place, are frequently attacked before the native stock. As, also, it would be unphilosophical to suppose, that there were two causes in operation, productive of precisely the same results, we must also infer, that the subsequent attacks were effects of the same cause, not of

* The "Farmer," formerly "Scottish Farmer," Nov. 8, 1865.

infection. In order to show how impossible it is to account for the propagation of the cattle plague by the doctrine of contagion, we may refer to the following anomaly. This is, the long interval that frequently occurred between the first and the second outbreaks. In Flintshire, the first outbreak occurred on the 29th July, 1865, when, out of 20 cattle on one farm, 5 were attacked, 4 died, and 1 recovered. Now the pole-axe was not brought into requisition here, either with the diseased or the healthy cattle: and yet there was no other outbreak in this county until the 2d of September—five weeks after. The same result precisely was observed after this second outbreak. Of 21 cattle on the farm, then invaded, 6 were attacked, 5 died, and 1 was killed. Although none of the healthy cattle were slaughtered, they were not attacked; while there was no other outbreak in the county until Oct. 21—an interval of six weeks. The same phenomenon was remarked in other counties. In Lincolnshire (parts of Kesteven) the first outbreak was on the 7th of October, among a herd of 83; of these, 2 were attacked, 1 died, and 1 recovered. None of the others were killed, and yet there was no fresh outbreak in this county until the week ending Nov. 8—an interval of six weeks. In another part of the county (parts of Holland) 2 oxen out of 14, on two separate farms, were attacked. One died, and the other recovered. The pole-axe was not brought into requisition here any more than in the former instance. Notwithstanding, there was no other outbreak in that county until the week ending Nov. 11—exactly six weeks. It will, no doubt, have been remarked as singular, that the interval between the attacks, in all these instances, was nearly the same—between five and six weeks. As this occurred in different localities, widely separated from each other, and in different months, it shows that the

propagation of this disease is to be referred to the operation of natural laws, not to accidental circumstances, or human agency. That the cattle plague is not propagated by contagion we may learn from the facts that have been observed during the prevalence of this disease among sheep. One instance has been already referred to, and there was another severe outbreak shortly after among Mr. Harvey's lambs at Crown Point, Norfolk. The following is the statement made by Mr. Simonds on the subject, at a Meeting of the Royal Agricultural Society :—" With regard to Mr. Harvey's sheep (lambs ?), which had numbered 2,060, and of which there were not at present more than about 500 alive, he would unhesitatingly state —and he would stake his professional reputation on that statement—that those sheep died of the cattle plague, and of the cattle plague alone." Such being the case, the question arises, how did the disease originate ? Mr. Simonds, in his letter to the Clerk of the Council after his official visit to Crown Point, Sept. 25, 1865, observed :—" In this instance, the malady was brought into the estate by the purchase of some cattle, which afterwards died from the disease, and which were, unfortunately, pastured with them at the time the disease manifested itself." But Mr. Woods, at a Meeting of the Wayland Agricultural Society, convened for the purpose of discussing " the nature and cause of the disease now prevailing amongst lambs," stated that " he had it on the strongest possible evidence, and that was from Mr. Harvey himself, in his own handwriting, that eleven lambs sickened and died before a single bullock was taken ill." As, however, the bullocks were attacked subsequently, it will, no doubt, be answered, that the infection was conveyed to the sheep by these animals, while the disease was in a latent state. It would be useless to attempt to argue

this point with men determined not to be convinced; for, as Hudibras says,—

> "He that's convinced against his will,
> Is of the same opinion still."

It will be better to show, by other examples, that sheep do not, and cannot, take the disease from bullocks, either before or after the attack. At the Meeting before alluded to, Mr. Woods read a letter written by Mr. Fulcher, bailiff to Lord Sondes, which was as follows:—"We have had several cases of plague in this neighbourhood. In most instances the diseased cattle have been herded with sheep, yet the latter have always escaped. Mr. Dack, of Guist, three miles from this place, who lost ten bullocks of *rinderpest*, informs me that at the time the beasts became diseased they were grazing in the same pasture with his ewes." And the speaker then added: "He now came to the most important part of the information connected with the cattle disease that he had yet heard. Count Nesselrode, a Russian nobleman, and an extensive landed proprietor, was staying at Merton Hall, and had authorised him to state to that Meeting, that in 1861 he lost the whole of his large herd of cattle from *rinderpest*. He had at that time 10,000 sheep running on the same land with the infected cattle, but not a single sheep was attacked with the disease. Count Nesselrode had entirely given up keeping cattle in consequence of the *rinderpest*, and kept sheep only, for it was found that they did not take the disease."

In order to ascertain, if sheep were liable to cattle plague, and if they could contract it from bullocks, some were placed in the Sanatoriums established at Edinburgh and Glasgow. The result was as follows: "The sheep which Messrs. Swan, of Edinburgh, placed in the Sanatorium there, have been in the same place now for many

weeks, with successive series of plague-stricken cattle, which have been fruitlessly subjected to treatment beside them and died, but they have as yet shown no signs of the disease." * This account is confirmed by the evidence of a young veterinary surgeon, furnished to Mr. Woods, who said: "He has been to Edinburgh (of which college he is a member), where he stayed a week, during which time he was fully employed investigating the disease—namely, the cattle plague—and dissecting animals that had died from the plague: he examined the sheep that have been in the Sanatorium for weeks, living with diseased beasts and feeding with them, even *eating the hay with the saliva of these beasts upon it*, and he pronounced them healthy." The result at Glasgow is still more conclusive. Professor M'Caul, in a report to the Town Council of Glasgow, after speaking of the diseased cattle, said: "I have also had five ordinary sheep—viz., two ewes and three lambs—and one African ewe sheep, in constant contact with cattle suffering under plague in its most severe form. They have been confined in the same boxes and eaten of the same food since Thursday, the 28th ult., and I also inoculated, with the discharge from the eyes and nostrils of plague-stricken animals, the African ewe and one of the lambs in the beginning of the week. None of them have shown any symptoms of disease. I have also kept five dogs—viz., a Scotch terrier, two retrievers, a pointer, and a mongrel—at the Sanatorium, and fed them almost entirely on the stomachs and intestines of cattle which have died from plague, selecting those portions which are most virulently affected. They have continued feeding on this since Monday, the 18th, and no symptoms of disease of any kind have shown themselves." On the other hand, it appears that sheep do not give the disease

* "Agricultural Gazette," Oct. 28, 1865.

to the bullocks. Mr. Garne, of Bushey Grove Farm, in a letter to Mr. Woods, states: "At the time I had several lambs ill—with what, to all appearance, has been called in Norfolk cattle plague—I put them in a close, and soon after having two cows calve, I turned them into the field with the lambs for a few hours every day, and they (the cows) took no harm."

If the preceding facts be of any value, we must conclude, that the cattle plague is not communicable from the ox tribe to sheep. As, also, the latter, when attacked with the disease, do not give it to oxen, we may infer, as a corollary to the above, that the propagation of this scourge, in the bovine race, is due to some other cause than contagion.

It may be said, that the disease in the sheep was not cattle plague. Allowing this to be the case, it does not follow that the cattle plague is contagious: quite the contrary. If the disease among the sheep was not cattle plague, it must have arisen spontaneously, as there was no similar affection, as far as we know, among foreign sheep. When, therefore, a disease, so similar to cattle plague that doubts exist on the subject, while the highest authority pronounces it to be the same, springs up spontaneously in the midst of our flocks, no possible reason can be assigned why the disease in the bovine race should not arise in the same way. It is only on this supposition, that we can account for the outbreak of cattle plague in particular instances. In a letter addressed to Mr. Helps, Clerk to the Privy Council, by Viscount Sydney, and dated Frognal, Foots Cray, Kent, Sept. 20, 1865, we find the following: "I had eleven beasts born and bred upon the estate from my own dairy cows in the park here, of various ages. They were near to the farm homestead, and away from every road or any possibility of contagion,

and the park is bounded by a wall and wide plantation. Moreover, no new stock of any kind has been brought into it this year. Shortly after we met at Osborne (7th of August last), one animal was seized and died under the hands of a veterinary surgeon, to whose place it was removed. In time all the others, after various intervals, became affected, and nine have died, and two are now recovering—indeed I may say have recovered. They were the two youngest of the herd. My dairy cows and their calves are here, half a mile from the farm—perhaps less, as the crow flies. They have never been affected in any way, nor have the milch cows of a tenant of mine, whose meadows and farm lie between the two herds. And, as far as I can ascertain, when the first bullock was seized no cattle were then affected in the neighbourhood nearer than 3½ miles." And his Lordship then adds: "The result, therefore, on my mind is, that although the malady is no doubt contagious, it must arise also from atmospheric causes over which no one has any control." Mr. Day, in a letter dated Horsford, Norwich, Aug. 13, 1865, and inserted in the "Mark Lane Express," states that, three weeks previously, five young Dutch heifers, belonging to a lot of twelve, which he had purchased the year before, were attacked with rinderpest: four died, and one recovered. The writer then remarks: "Now, these bullocks could not possibly have come in contact with any other stock, and this disease must have been in their system, either from their breeding or some other unexplainable cause. As long as these Dutch cattle are imported into this country, it is my firm belief, so long will disease attack our own herds." When Mr. Day expressed that opinion, he was doubtless unaware, that the cattle plague had only broken out in Holland about the same time that his own cows were attacked; and that the Dutch con-

sidered it had been imported from England. Viscount Sydney, in the letter now referred to, observes: "I have just returned from the Hague. In Holland, they think the disease was brought there by unsold animals, returned from this country." Another example of spontaneous outbreak occurred in the Zoological Gardens. A Piedmontese cow was attacked, on the 1st October, 1865, with cattle plague. She was sent to the Veterinary College for treatment, and recovered. On the 11th, a Tuscan bull was attacked, and was killed on the 14th. Another bull —a Piedmontese one—fell sick on the 12th, and died on the 16th. Eight days after, the fourth animal, a female yak, was attacked, and died five days after—Oct. 29th. All these animals occupied the same range of buildings, but the disease did not extend beyond. No cause could be assigned for the outbreak. Messrs. Simonds and Brown remark: "Here, again, the introduction of the disease could not be traced, but, *probably*, it depended on the flesh of diseased cattle brought to the gardens for the carnivora." * If these be the shifts to which the Contagionists are driven, in order to explain an outbreak of cattle plague, the sooner they abandon the doctrine of contagion, the better it will be for their reputation as men of science, and men of common sense. In the first place, we have an assertion, without proof, that the flesh of diseased animals was sent into the gardens; and, in the next, that the food taken into the stomach of the carnivora, or destined for them, produced disease in the bodies of the herbivora! Added to this, we know that the flesh of animals, that die of the cattle plague, is not *infective:* Dr. Crisp lived on this flesh, for some time, with impunity. The following cases are no less instructive.

The cattle plague had entirely ceased in Argyllshire the

* Loc. cit., p. 277.

end of December, 1865; but it appeared simultaneously on the 19th January at two new centres; on the 23rd, at two others: and on the 1st February, at a fifth. The Secretary of the Cattle Plague Association for that county, Mr. Barclay, thus sums up the facts connected with these outbreaks. Generalizing the facts of these five cases, it appears:—1. That during an interval of twenty-two days from the 28th of December to the 19th of January, there was no visible disease in the county. 2. That disease broke out simultaneously at two places twenty miles apart, and four days later disease appeared simultaneously at other two places each seven miles apart, and each seven miles distant from any previous seat of disease. 3. That no cattle or sheep (one lot of the latter only excepted) have been admitted into the county from the south since the beginning of December. 4. That no cattle had been received on any of the farms for months previous to the outbreak (except in the case of Mindurns, where one animal was received from a neighbour whose stock is still free from disease). 5. That no food other than the produce of the farm (oilcake alone excepted) had been used in any of these cases. In all the cases the animals first attacked were cows which had not received any oilcake. 6. That there is no ground for suspicion that the disease had been brought by persons or articles of any description, except in the case of the pack sheeting at Pitmillan.* Another instance has been recorded by Professor Armatage. He remarks: "A few days ago I was accompanied into Kent by Professor Bouley, from the Alfort Veterinary School, France, to examine and report upon the state of sixty of the finest dairy cows, all selected with the greatest judgment and care. Two days previously, they were

* The Times, Feb. 16, 1866.

reported by eminent men to be apparently free from disease. On our visit, two were affected severely. Here Professor Bouley was delighted with the beautiful arrangements and systematic order of everything; yet the introduction of the plague to this establishment, as well as those already mentioned, is a perfect mystery. No fresh stock had been introduced for months, and all communication had been cut off from motives of rigid determination to avoid, if possible, the disease." And the Professor then adds: "Unless we prove, that our beautiful green pastures are the dismal swamps and pestiferous marshes of the Russian or Danubian steppes, we fail to find material for the development of the germ by which the disease is generated."* The following account is no less interesting and instructive. "The case of alleged rinderpest in Cumberland, after the county had enjoyed a clean bill of health for *nine months*, has created great consternation. It is said that for nearly two months the cow had never been near any others whatever, and that, unless some infection had come in *the corn bags* from Botcherley Mill, where several cattle were destroyed at the time of the disease, 'it was a case of spontaneous origin, which is denied by the scientific world.'" † Allowing that the *virus* could retain its power all this time, still, as the corn-bags, sent to other farms, had not produced any other attacks, we may dismiss the conjecture, as to the cause of the outbreak, as futile!

That the cattle plague can arise spontaneously, we may learn from the outbreaks of disease among the cattle in the Isle of Man, and also in Ireland. From the Report of Mr. Brown, who was sent by the Government to ascertain the nature of the outbreak, we learn the following par-

* The Veterinary Review, Oct., 1865, pp. 506-7.

† Illustrated London News, March 20, 1867.

ticulars.* The disease commenced the end of May, 1866. Previously to the arrival of the Government Inspector, June 2nd, seven cattle had died and two had been killed. Three more died during the following five days, and there were three more ill at this time—the date of this gentleman's departure from the island. Whether any more cases occurred we know not: no statistical returns having been furnished for the Isle of Man. We must therefore depend on the facts contained in Mr. Brown's Report—rather a short one.

The first farm visited was Port-e-chee, on which a cow had been attacked five days previously; and as the symptoms, in the opinion of the local inspector, resembled cattle plague, the animal was killed. The stock consisted of thirty head of cattle, but none of these had shown any signs of disease up to the time of the visit. "The fact, therefore," remarks Mr. Brown, "of *no sign of infection* existing in the herd after five days, although not in itself conclusive evidence, was suggestive of *the absence* of rinderpest." This means, that as Mr. Brown has made up his mind that cattle plague is contagious, an outbreak of it cannot possibly occur, unless signs of infection be present, although the contagiousness of this disease has not yet been demonstrated. But to proceed. A visit was next made to a farm at Braddon, about a mile distant from Port-e-chee. One cow, of nine on a farm, had been attacked May 30th, "with symptoms which bore *some resemblance* to those of cattle plague: the secretion of milk was suddenly arrested, the appetite was lost, and *rumination was entirely suspended.*" In addition to these symptoms, there was a feeble pulse, with difficult respiration and extreme prostration. Mr. Brown then adds:

* Report of the Veterinary Department of the Privy Council, p. 290.

"*None* of the diagnostic symptoms of cattle plague were detected during the very careful inspection which was made of this animal.... The fact of the cow having remained for several days without the development of any of the usual indications of cattle plague, amounted almost to positive proof of the non-existence of that disease." So, because the disease had not become developed in its severest form—the symptoms previously detailed being all characteristic of cattle plague—the case is to be rejected: as if all cases are to present the same rapidity in their course and the same intensity in the symptoms. This is like certain medical men who refuse to acknowledge a case to be one of Asiatic cholera, unless collapse be present: *i.e.*, unless the patient be in the last stage of the disease, as though he could arrive at that, before passing through the others. Marrown was next visited. In this place, one cow was suffering from the prevailing epidemic. She formed part of a herd of twenty-three, all of which, with the exception of one, that showed slight symptoms of ailment, remained healthy. "Besides the symptoms, which were noticed *in the case at Braddon*, there was in this case, more decided evidence of cerebral derangement.....If the question of the existence of cattle plague had not previously arisen, there was *nothing* in the symptoms presented by this animal to suggest it." That is singular, as, in addition to the symptoms previously described and present in both cases—all indicative of cattle plague—it is not uncommon to meet with instances of cerebral affection in true and undoubted cases of this disease.

So much for the symptoms: we will now inquire into the appearances presented after death. In a cow that died at a farm at Peel, "the post-mortem examination, which was made on the following day, revealed the presence of lesions *closely allied* to those of cattle plague." Some of

these lesions were as follows. "The lungs presented *exactly* the emphysematous condition, which is almost uniformly present in cattle plague." The heart was filled with dark coloured, congealed blood, with large hæmorrhagic patches in the left ventricle. "This lesion has been represented as one of *the characteristics* of the disease in plate 23 of the Royal Commissioners' Third Report on the Cattle Plague." The *rumen* was distended with a large quantity of undigested food: and the *omassum*, or third stomach, "was hard and *tightly* packed with food" —the great characteristic of cattle plague. And lastly, there was congestion of the mucous membrane of the *abomassum*, "the appearances in which were *remarkably like* those seen in cattle plague." In addition to the fact, that the contents of the intestines were fluid, as is almost invariably the case in the last stage of the disease, the mucous membrane was congested; and there were those peculiarly dark red, or nearly black lines, termed "zebra markings"—"appearances so generally seen in the intestinal membrane in animals *which have suffered from cattle plague.*" This is not all. "The cœcum was particularly congested at the apex, and the lining membrane of the rectum had precisely the hæmorrhagic condition which is *invariably seen in plague.*"

After this account, and these extracts, the majority of persons, we might suppose, whose heads were not filled with a preconceived theory,—blinding their eyes and warping their judgment,—would have had little doubt as to the nature of the disease. Not so Mr. Brown. He remarks: "This inspection concluded the first day's inquiry, which had *entirely failed* to afford positive evidence of the nature of the epizootic." And it is then added: "On the following day, the problem was solved, by the post-mortem examination of the cow which was inspected

at Marrown; and which died on the evening of the day on which it was first seen. In this case, the morbid changes were identical with those seen (in the cow) on the previous day at Peel; and it was thus established, that the disease, which prevailed among cattle in the island was characterised by symptoms *entirely distinct* from those of cattle plague; and by internal lesions, which were, in every particular, closely allied to, and, in some parts of the organism, *absolutely identical* with those which are found in that disease." Here then is a solecism in terms as well as in reality: a disease in which the pathological phenomena are absolutely *identical*, while the symptoms were entirely distinct. Is such a case likely to be met with? Certainly not. We have already seen that the symptoms present were characteristic of cattle plague; the only difference being that they were not so strongly marked as in the generality of cases, while some few were absent. There is nothing unusual in this: the symptoms may vary, and do vary, not only at different stages of the disease, but in different individuals and under different circumstances. Take as an example the epidemic cholera. At one visitation, cramp will be the prominent symptom, the vomiting and purging being slight, or entirely absent, for a time: at another, there will be few or no cramps. In one particular locality in Jamaica, the first stage of the disease was wanting altogether, the attack being ushered in by *colic*, instead of by vomiting and purging. In other and rare instances, the patient will fall into a state of collapse, without having had either cramp, or vomiting, or purging, or only one solitary evacuation. But there can be no doubt as to the nature of the attack in all these instances: although all the symptoms characteristic of the disease were not present. Nor ought there to have been in the cases of cattle plague,

in the Isle of Man: for although some of the more prominent symptoms of this disease were absent, those present were, with all due deference to Mr. Brown, not distinct from, but identical with it. Fortunately, the question can be decided without reference to the symptoms: the *crucial* experiment being the lesions found after death. This test is always resorted to, when a doubt exists as to the cause of death, as in cases of poisoning: and it is always considered, not only satisfactory but conclusive. As the application of this test to the disease, in the Isle of Man, removes all doubts on the subject, we may conclude, although Mr. Brown does not, that these were genuine cases of cattle plague.

As regards the unwillingness of this gentleman to regard these cases as attacks of rinderpest, the cause is sufficiently apparent. It appears that animals had been attacked *simultaneously* in localities distinct and distant from each other—"at Kirk Andreas in the north: Peel in the west, and Port-e-chee in the east. An examination of some of the herds, a week after the disease had appeared among them, and destroyed one or two of the number, did not lead to the detection of the usual signs of *infection*." This means, that the remainder of the herds, not having been handed over to the tender mercies of the pole-axe, remained free from the disease: contrary to all the rules of contagion. More than this, "an inspection was made on June 7th of a cow (that had died the previous day), which had been pastured with two others, in a *perfectly isolated* position on a mountain, Brid Sleauwhallen, where it was, at least, exceedingly unlikely, that the animals would be exposed to the influence of infection." With two such startling facts before him, the simultaneous attack of cattle in different localities, without being exposed to any source of infec-

tion: and the non-infection of the cattle that were herded with the diseased animals; we might have inferred that Mr. Brown would either have changed his opinion, or, else, have hesitated to speak positively on the subject. But no: Mr. Brown concludes his Report thus. "From the evidence obtained, the conclusion was fairly deducible, that the disease was not of an infectious character, and, *by consequence*, was not cattle plague." That is to say, if the facts and the theory be at variance, the facts are to be rejected, not the theory. Precisely the argument made use of by Mahomed, who said: "If there be anything not contained in the Koran, it is nought; if only what is there written, it is useless." *Mais revenous à nos moutons.* Although differing on other points, we agree in one respect. Mr. Brown concludes a letter, addressed to the Lieutenant-Governor of the island, in these words: "From a careful consideration of the whole of the evidence at present before me, I have arrived at the conclusion, that the affection from which the cattle, in the Isle of Man, are suffering, is not rinderpest, but a disease depending entirely upon *local* causes." Whether produced by local or general causes is immaterial: it is sufficiently apparent, that the disease was not introduced into this island, but sprung up there spontaneously. It would seem to be no less evident, that the disease was really rinderpest, although the fact, whether it be, or be not, is equally immaterial, as regards the present inquiry. We have proof that a disease, so nearly identical to cattle plague, that it is difficult to say, whether it be, or be not, the same, sprung up spontaneously in the Isle of Man. Although not so prevalent, it was equally, if not more fatal. If, therefore, a disease like this can spring up spontaneously in a country, why should not the cattle plague? Is our ignorance of the cause, any reason, any excuse, for our

disbelief? It is not allowed to be so, in other matters, in which our faith is more severely taxed: nor should it be in this. If all the facts, with which we are acquainted, lead to the conclusion that the disease arose spontaneously, let us say so at once, honestly and candidly, no matter whether the facts be for or against our most cherished opinions. We can then, with a clear conscience, leave the question to time, or to other investigators, to clear up the mystery. To an unbiassed mind, it would not have been necessary to wait very long for an elucidation of the question: the facts connected with the next outbreak being still clearer and more decisive.

As no cases of cattle plague had then occurred, an Order in Council of the 25th of August, 1865, prohibited the landing in Ireland of cattle and sheep from any part of Great Britain: and the Cattle Plague Commissioners in their first Report, dated October, 1865, recommended other measures in order to prevent the *indirect* importation of the disease—by persons and things. As may be supposed, the freedom of Ireland from the murrain during 1865, was ascribed entirely to the adoption of these measures. Nevertheless, the plague suddenly broke out in April, 1866, at Drennan, near Belfast, County Down. The first case occurred the 8th of April, on the farm of Mr. McKee. It then spread to eight other farms in the immediate neighbourhood, between this date and the 5th of July. Out of 50 cattle on these farms, 29 were attacked: of these, 18 died and 10 were slaughtered, together with 21 healthy animals, and one that had been previously attacked, but which had recovered! Unfortunate animal! Her executioners must, we may surmise, have been labouring under the cattle plague themselves; and have had all the milk of human kindness, in their composition, dried up!

In this instance, no doubt existed as to the nature of the disease. The cattle had been examined by Drs. Mapother and Foote, who had studied the disease previously in England; by Professor Ferguson, at the head of the Veterinary Department, Dublin; and by Professor M'Call, who had witnessed the outbreak in Scotland. All these authorities pronounced the cases to be decided attacks of cattle plague. Professor Brown, who was sent over by the Privy Council, also concurred in this opinion. The only question that remained was, how did the disease originate. No attempt has been made to show that the plague was introduced by the importation of cattle into Ireland: we may, therefore, put that cause on one side at once. As such, all that could be done was to infer, that the plague was introduced *indirectly: i.e.*, by human beings. It would appear almost a work of supererogation, if not a mockery, to attempt to disprove such conjectures as these, unless supported by facts; but not a particle of evidence has been adduced to show that there was any communication, by human agency, between this part of Ireland and infected districts in England. The only circumstance mentioned is, that Mr. Marrow, a neighbouring agriculturist, who held property in Scotland, had gone over there, *the previous year*, from October 7th to November 11th—*five months* before the outbreak. More than this, Mr. Marrow states, in a letter: "I had such a dread of rinderpest, that I kept a suit of clothes in Glasgow, which I left there. I also got my herd, that had charge of my diseased cattle, a suit of clothes, and burnt the old ones at Glasgow." Added to this, Mr. Marrow did not visit the infected farms in Ireland until after the outbreak, and when eight cattle had already died. It is, in fact, the very height of absurdity to talk of the cattle plague having been introduced from England, or Scotland

by persons who had crossed the Irish Channel, and had, probably, a ducking on the voyage; and then to find that dozens of persons, in direct contact with the diseased animals—inspectors, doctors, veterinarians, owners, herdsmen, &c.—fail afterwards to spread the disease. Here was Mr. Brown, not only visiting the infected farms, but dissecting several of the diseased animals; and then travelling over different parts of Ireland to report on the state of the cattle. But we do not hear, that the plague followed in his footsteps.

Notwithstanding these facts, Mr. Brown still holds to the opinion, that the disease was imported, although unable to explain how. He remarks: "Whether the cattle plague was carried from England to Ireland by direct, or indirect, agency will, perhaps, never be known. One thing to be learned from it is, the *impossibility* of obtaining absolute security, under all circumstances, against the communication of infection, even by the adoption of the most complete measures of precaution." This may be received as an undoubted truism: not so the following. "Another important point," remarks Mr. Brown, "which arises out of the outbreak in Ireland, is the testimony which it bears to the entire efficacy of the 'stamping-out system,' the whole losses amounting to little above fifty cattle." Here, then, we have another example of the line of argument employed on these occasions. If the disease subsides, it is ascribed to the efficacy of the measures adopted: if it does not, it is referred to their non-employment, or their improper application—a very easy way of cutting the Gordian knot. As, however, it is not a very satisfactory or scientific way, it may be as well to inquire, whether, instead of cutting, we cannot unravel the knot.

If we turn to the history of other epidemic diseases,

more especially to those in man, we shall find, that they invariably subside after a certain time: they never prevail for indefinite periods. As no means are adopted to produce this result, the subsidence of the disease must be referred to some natural and universal law: which is applicable to all diseases, at all times, and under all circumstances. More than this, these diseases are limited in extent as well as in time: never prevailing over the whole globe, or scattered irregularly over its surface; here, there, and everywhere. Although the boundary lines are thus well marked, it may be observed, that an epidemic, before terminating, will send off a few offsets, at the end of its march. But the disease, in these instances, is always very limited in its range, and, generally speaking, milder in its form. An example of this was afforded in the previous chapter. As may be remembered, the epidemic cholera, in 1865, spread from the south to the north of France, and then subsided, with one solitary exception. It reached Altenburg, a town near Leipzig, but did not extend beyond, nor was any intermediate town, as far as we know, attacked. To what, then, would Mr. Brown ascribe the subsidence of the disease in this instance? Not, certainly, to the "stamping-out process," as, fortunately, for the interests of science, as well as of humanity, this method of preventing human diseases has not yet been adopted. What the march of civilization may effect hereafter remains to be seen. Let us take another example. As will be remembered, the extreme limit of the epidemic westward, during the same visitation of cholera, was Southampton. Here, also, it was very limited in its range, and did not extend beyond, although the 50,000 healthy inhabitants of Southampton were not "stamped out" of the book of human existence. When, therefore, we find that human diseases present precisely

the same phenomena as those in the bovine race, and that they subside without the use of the pole-axe, it is the height of presumption for these theorists to assert, that they can stamp out a disease like the cattle plague. They might as well tell us, that they had put out the fires of Vesuvius or of Etna, when the eruption from these volcanoes ceases! The only sound and legitimate conclusion to draw on the subject is, that the cattle plague, like the majority of other diseases, arose spontaneously in Ireland; and that its subsidence is to be referred to natural causes, not to human agency. The same conclusion has been drawn respecting the previous epizootic murrain. A member of the College of Physicians, who wrote a treatise on the cattle plague of 1745, observes: "The great mortality which has attended the distemper, has led many persons into a belief that it is infectious; but, for my own part, I am of a contrary opinion, for the following reasons. In the first place, then, the distemper has sprung up spontaneously in many places, which are very distant from, and have no communication with, each other, about the same period of time. 2ndly. It has raged, in some particular places more than others, and chiefly in rich grounds. Nay, it has been observed that, at a farm not far from town, the greatest part of the cattle died of the distemper; though, in the next adjoining one, they were not at all affected by it." 3rdly. "Many of the cattle, which were taken up in due time, and bled, escaped the disease; though they were kept in the same house with others, that were affected." The last reason assigned by the author is the fact that the disease was more severe and fatal during the frosts—the contrary, he thinks, to what is observed in purely infectious and contagious diseases. That the cattle plague not only arises spontaneously, but subsides naturally, without the intervention of

the pole-axe, may be shown by a variety of circumstances. In Gloucestershire, there had been no outbreak from the 10th of March to the 30th June, 1866, when three cows were attacked, in a herd of forty, on one farm. One of these was killed, another died, and the third recovered. None of the healthy cattle were slaughtered, and yet, there was no other outbreak in this county until August 11th—six weeks after. One cow was then attacked, in a herd of five, on one farm; but none of the others were either attacked or slaughtered. Three weeks after, another cow was attacked, among fifteen, on a different farm; but the remainder escaped both the pole-axe and the disease. The two cows that were attacked were also allowed to die a natural death, at the risk of scattering disease and death all around. Notwithstanding, no other outbreaks have been observed, in this county, from that time to the present. In Staffordshire, also, there was a solitary outbreak on the 15th December, 1866, although the epidemic had ceased the end of September. One bullock was attacked, among a herd of nine; it was allowed to die a natural death, but it failed to infect any of the others. They were not slaughtered, but lived to tell the tale, and to recount their victory over the pole-axe, and the—Inspector. Another outbreak occurred on the 16th February, 1867—two months after—one solitary cow being attacked, in a herd of fifteen, on one farm. This cow was killed, but the others obtained a respite, by Royal favour, we may presume, as it was in direct opposition to the Orders in Council. This clemency was not abused, for these cattle did not become infected themselves, nor did they infect others, no other cases of cattle plague having occurred in this county afterwards. The following facts are still more striking.

It has been before stated, that what are termed *sporadic*

cases—*i.e.*, single and isolated cases—are constantly observed after the plague or the cholera has prevailed epidemically, and, sometimes, for years after the apparent cessation of the disease. The same result has been observed with the cattle plague. As will be remembered, the murrain had ceased to prevail epidemically the end of September, 1866, the cases afterwards being comparatively few. In the last week of December, they only amounted to nine. But, in the following spring, a number of outbreaks occurred in different parts of the country: isolated cases that did not spread. For instance, there had been no outbreak in Shropshire from October 20th, 1866, but on the 25th May, 1867, one cow, in a herd of thirty-six, on a farm, was attacked with the murrain. She was killed, but none of the others: yet they escaped an attack, while no other cases were observed afterwards in this county.

Independently of the metropolis, in which two outbreaks occurred in 1867, after its total cessation in May, 1866, there were in all England, 100 cattle attacked, out of a total of 790, on twenty-five different farms. Of those attacked, thirteen died, and the rest were slaughtered. In addition to these, 247 healthy cattle were slaughtered, which, deducted from the total stock on these farms, together with the 100 attacked, will leave 443 animals that had been thus exposed to infection, to scatter disease and death all around. Strange to say, not a single case occurred afterwards, although these outbreaks all occurred in different localities and in different counties. Thirteen counties were invaded, and of this number only three—viz., Yorkshire, Derbyshire, and Lincolnshire, had more than one outbreak, while even these were in different parts of the same county. To what, then, are we to ascribe this singular phenomenon, the non-diffusion of the

germs of infection? We shall be told, perhaps, that the disease was "stamped out" by the slaughter of the infected animals, and of all those in immediate contact with them: the others being too far removed to receive the germs of infection. If this conclusion be allowed, then we must reject one half the arguments and conclusions drawn by these writers, in order to account for the production of the disease, under circumstances in which ordinary mortals, not possessed of the intuitive knowledge of the Contagionists, are unable to account for the outbreak. Even if we allow, that the disease was really and effectually "stamped out" in some instances—although there are only three, in which all the healthy cattle were destroyed,—we shall still be at a loss to account for the subsidence of the plague, in those cases in which this patent instrument for the prevention of disease, was not brought into use. In eight of the preceding instances, no healthy cattle were slaughtered, while, in two others, only one animal was sacrificed—offered up, perhaps, as a warning to the others to keep out of the way of infection! The instances referred to, with other particulars, are given in the annexed table.

As it is quite evident, that the subsidence of the disease, in the above instances, cannot be referred to the "stamping-out process," we may call upon the Contagionists to account for the phenomenon. We may also be allowed to ask them, to explain the origin of the disease in all these instances. It is nearly certain, that the common refuge, in all cases of doubt, the importation of foreign cattle, will not hold good, as it is not probable that fresh stock had been introduced in all these instances. Besides, had such been the fact, even in a single instance, it would have been proclaimed on the housetops, as an additional proof of the importation theory. Nor could the germs of

TABLE 20.—Showing the number of farms invaded by the cattle plague, in 1867, in the undermentioned localities, with the number of healthy cattle slaughtered on each.*

Date.	Locality.	No. of Farms Invaded.	No. of Cattle on them.	Number Attacked.	Healthy Cattle Slaughtered.
1867.					
Feb. 1 ...	Derby	1	37	1†	None
Feb. 2 ...	Pirehill (Stafford) ...	1	18	1†	None
Feb. 6 ...	Morley (Yorkshire) ...	1	15	2†	None
March 9.	Horncastle (Lincoln) ...	1	14	1†	None
April 6...	Coquetdale (Northumbrld.)	1	31	1†	One
April 20.	Barton-on-Humber ...	1	28	4‡	None
May 4 ...	Retford (Nottinghamshire)	1	14	1†	None
May 25..	Whitchurch (Shropshire)...	1	36	1†	None
May 25..	Yarm (Yorkshire)... ...	1	41	1†	None
Aug. 24..	Chester-le-Street (Durham)	1	26	1†	One

infection have existed on the spot, in all the instances at least, as of the twenty-five farms invaded only nine had been previously infected.

With these facts before us, we shall be able to draw some important deductions. It would appear: (1) That the cattle plague, in a certain number of cases, has sprung up spontaneously; (2) that the disease has not spread to the other cattle on the same farm, although none of the healthy animals were slaughtered; (3) that the disease subsided without human intervention, and without the aid of the pole-axe. These deductions allowed, we may also infer, that the disease arises spontaneously in all other instances; and that it would also subside spontaneously,

* Extracted from the Tables in the Report of the Veterinary Department of the Privy Council, p. 194, *et seq.*

† These were killed. ‡ Three killed, one died.

if allowed to do so, and without the intervention of the pole-axe. More than this, if the disease be not propagated from animal to animal at one time, it will not be at another. To conclude, otherwise, would be in direct opposition to the simplicity of Nature's operations, and to the axiom laid down by Newton, viz., a multiplicity of effects but *a paucity* of causes. We may go even farther than this, and affirm, that the production of precisely the same effects, by different causes, would be a physical impossibility. If so, all the restrictions placed on the importation and removal of cattle; their isolation; the slaughter of the diseased and of those of the healthy, that come in contact with them, must be utterly useless.

That these measures are unnecessary, if not injurious, can be shown in another way—by the result of experience, for this is, after all, the crucial test. As we have accounts of this disease for the last forty years, we shall be enabled to ascertain what the result has been of measures of repression, for these have been almost invariably adopted on the Continent. Leaving the question of the origin and propagation of the murrain, in the steppes of Russia—in what is termed the home of the cattle plague—we will confine ourselves to a brief summary of the extent and ravages of the disease, when it has passed its usual boundaries, or become epidemic.

The first account we have of this disease is in 1827-8, during the Russian war with Turkey. It extended, not only into the Principalities, but, also, into Hungary, Galicia, and Moravia. We next hear of it in Poland, in 1831-32-33, at which time it extended into Prussia, or, rather, into the department of Bromberg. In 1835, Moravia and Hungary were visited, while, in the following year, the plague extended into Austria, Bosnia, and Dalmatia. It is also stated, that there were some cases in

Southern Italy in 1837. The epidemic next appeared in Moldavia and Wallachia, in 1840, and committed great ravages. It prevailed, at the same time, in Asia Minor, and is said to have spread thence to Egypt; although it is more probable, that the latter outbreak was an independent one. In 1840, occurred the most severe visitation that has been recorded, the epizootic prevailing, at one and the same time, in Russia, Hungary, Austria, Moravia, Bohemia, Poland, Galicia, and Silesia. The disease committed great ravages in all these countries, Russia alone, it is asserted, having lost nearly a million of cattle. We hear nothing more of the murrain until 1848, when Podolia and Volhynia were invaded, and, in the early part of 1849, Poland and Galicia. It also extended into Austria and Hungary, and did not subside in these countries until 1851. The losses among the herds, on all these occasions, were very great.

As might have been expected, the murrain made its appearance in the Principalities, in 1854, during their occupation by the Russians. It also extended to the cattle of the Allied Army. The French army lost 8,000 beasts out of 17,000 at Samsoum; and the English 4,000 out of 10,000. It also spread that year into Volhynia and Podolia. The murrain was supposed to have been introduced into the Principalities by the Russian troops, or, rather, the cattle that accompanied them. Hence its extension into the Crimea and Turkey. That this was an erroneous conclusion, will be evident from the following statement. Mr. T. W. Mayer, veterinary surgeon, attached to the Land Transport Corps, in a letter to Mr. Simonds, remarks: "About the latter end of August reports reached us, that a dreadful murrain was destroying immense numbers of cattle in *Asiatic Turkey*." As the Russians did not and could not have carried the disease

into the latter districts, and as, no doubt, the same cause was in operation in the Principalities, we may conclude, that the Russian cattle, if attacked, picked up the disease on the spot. We may draw the same conclusion, respecting the cattle brought together for the use of the French and English armies. If there had been no cattle, there could have been no outbreak: although the cause, productive of the disease, might still have been in operation. In the same way, if there had been no French or English soldiers present, there would have been neither deaths nor wounds from the shower of Russian shells and balls. The only difference is, that, in the one case, we see the agent productive of disease and death, in the other we do not!

The epizootic was not confined to this part of the world: it extended northwards as far as Poland; in consequence of which the Prussian frontier was closed, as early as March, 1855. Troops were sent to prevent all communication with the infected districts; and the importation of cattle, hides, &c., strictly prohibited. As the disease approached, more rigid measures were adopted. All persons coming from Poland were disinfected at the frontier: gendarmes were stationed in the adjoining villages, and veterinary surgeons were appointed to keep all the animals in the district under surveillance. "Notwithstanding that these precautions were rigorously adopted, the disease crossed the Prussian frontier, and manifested itself, in November, in the 'circle' of Inowraclaw, and, shortly afterwards, in the 'circle' of Gnesen, near the town of Posen."* More stringent measures were then adopted, sentries being placed, together with gendarmes, round the infected farms, so as to prevent all communication with other districts. The traffic in cattle was forbidden: dogs were chained up, and the pigeons, fowls, &c., confined;

* Report of Mr. Consul-General Mansfield, March 29, 1857.

but as it is not stated that the starlings were shot, we cannot be surprised to learn that, in April, 1856, the murrain spread to the department of Breslau, in the province of Silesia, and continued there about seven months; but had scarcely disappeared, when it rose up suddenly in the middle of November, and attacked the villages of Braunau and Sulsch.* What the mortality was in Prussia has not been stated: we only know that in Poland 20,000 beasts, according to Mr. Mansfield, were sacrificed between May, 1856, and March, 1857. Rinderpest prevailed in Austria at the same time, 26,442 beasts having died or been destroyed in 1856. In addition to Poland, Austria, and Prussia, the murrain extended as far north as Lithuania, Kovno, and Courland; but what the exact losses were has not been stated. Mr. Bakewell, British Vice-Consul at Lübeck, states in an official despatch, dated May 30, 1856, "that the murrain was then raging to a fearful extent in Poland, notwithstanding the *stringent measures* that have been adopted by the Russian Government for putting a stop to it." In a subsequent despatch, dated September, Mr. Bakewell observes that the murrain had then extended from Poland to Meclenburg and Holstein.

In the following year, it reappeared in Silesia, and prevailed in several departments. Although the disease existed in this province the previous year, its reappearance was attributed to the introduction of two herds from Galicia, of the Podolian or Hungarian breed. To show the foundation on which these statements are made, the following particulars are added. Some of these oxen,

* It would appear that this was the first visitation of the cattle plague, as it is remarked in the Official Report, that murrain had not existed in the province for forty years. It is also stated, "that all means of ascertaining how the plague had been introduced failed."

four or six, all of which, be it observed, were quite healthy, were sent to an estate where they remained for a few days only. *Twenty days* after their departure, one animal of a herd of 19, among which they had been placed, was attacked with rinderpest. It was immediately slaughtered, together with the remainder of the herd. Supposing it be possible for a healthy animal to convey the infection to another, it would be ridiculous to conclude that the germs of the disease could have remained latent in the system for twenty days before they developed themselves: as such, the only conclusion to draw on the subject is, that the disease sprung up spontaneously. This inference is confirmed by the fact that, in other instances, the plague appeared on farms into which no imported cattle had been introduced. Thus a case occurred at Zawaiz, in the circle of Beuthen, at a perfectly *isolated* farm; and another at Wahlau, in the circle of Pless. "In these instances," remarks Baron Schleinitz, President of the Province of Silesia, "it was impossible to trace the cause of the disease to importation." The murrain had thus gained a footing in two of the Prussian provinces and one department, viz., Posen, Silesia, and Königsberg, notwithstanding all the rigorous measures that were adopted for its exclusion.

The plague continued to prevail in the eastern parts of Europe in 1857, and was found to be prevailing in Galicia by the Commission (composed of Messrs. Simonds and Ernes), sent from England to obtain information respecting this modern bovine plague. It does not appear to have been either very prevalent or very general this year. But in 1858, it was again both general and fatal, particularly in Russia—118,515 cattle having died, without reckoning other losses. The murrain reached Silesia and Bohemia; the outbreak, in the latter country,

being marked by the extension of the disease to sheep. Of 108 in a flock, 38 were attacked, 18 died, 16 recovered, and 4 were killed. Here, therefore, the same as elsewhere, the disease was less severe and less fatal than with oxen.

In 1860-3, the murrain prevailed almost uninterruptedly in all the eastern parts of Europe. It commenced in 1860, in Buckowina, Lower Austria, and Hungary; and reached Moravia, Galicia, and Wallachia in 1861. Galatz, in the last-named province, was placed in quarantine, so as to prevent the town cattle from coming in contact with those in the interior. It continued to prevail in Galicia in 1862, which, together with Poland, suffered severely from the ravages of the disease. It re-appeared in 1863, and committed great ravages in Austria and Hungary, as well as in Poland, being favoured, in the latter country, by the Polish insurrection. It also passed this year into Turkey and Italy, and broke out again in Egypt. In Italy, the murrain appeared first on the eastern coast, at Ancona, Pescara, and other places, and after extending into the interior as far as the Campagna of Rome, it crossed the Apennines and reached the province of Naples. In 1864, only isolated, and, as it were, local outbreaks of the disease were observed in eastern Europe, or rather in Russia, as in the Government of Kowno and in that of Volhynia. It appears to have entirely subsided in Austria, and all the localities visited the previous years. In 1865, a year for ever memorable on account of the irruption of the plague in England, two or three isolated and distinct outbreaks are recorded in Buckowina, Galicia, Lower Austria, and part of Hungary; but these did not occur until after the commencement of the murrain in England.

In the following year, 1866, the plague became more

generally diffused, commencing early in Hungary, and appearing subsequently in Buckowina and in Prussia, in spite of the military cordons and the rigid measures in force in that country. No information has been afforded as to the extent of the ravages of the disease in Prussia: we only know from a despatch, dated September, 1866, that the plague was then "on the increase." We also learn from another despatch, dated May, 1867, that "no cattle plague had existed for some months past in *the district of Königsberg,* or the adjacent Polish provinces." Whether the murrain had disappeared in all other parts of the kingdom the document sayeth not. Prussia is wise. She does not proclaim to all the world that a contagious disease is prevailing in the country in order that other countries should establish quarantine, and cut off all commercial intercourse! It prevailed in Russia in 1867, in the southern, western, and central provinces, and in a dozen or more Governmental departments of the Empire. It is stated that the ravages of the disease, this year, were unusually great, but no accurate account of the actual losses has been made public. In addition to Russia, the murrain, this year, spread, according to the documents forwarded to the British Government, over the whole of Eastern Europe, and also into Turkey.* As already stated, Hungary suffered from an outbreak of the plague in 1866: this continued during the whole of 1867, and up to the date of the above Report, 1868. From Hungary the disease appears to have spread into Lower Austria at the beginning of 1867, and to have "uninterruptedly continued its ravages down to the last week of September. In October, 1867, the country was declared free, but fresh outbreaks soon afterwards occurred, so that by the end of November,

* Report by the Veterinary Department of the Privy Council Office, p. 256. London, 1868.

or the beginning of December, five districts of Lower Austria were still the seat of the disease." In Galicia, according to the information received, and dated April, the murrain was then prevailing in fifty-eight villages, and in twenty different "circles" or districts of East Galicia. "Throughout the whole of 1867, Galicia continued to suffer loss from outbreaks of the disease *without intermission*, but with varying intensity." Buckowina, also, which had been invaded by the plague in 1866, suffered very heavy losses in 1867. When the malady commenced in Moravia is not mentioned: it is only stated in a despatch dated May, 1867, that the cattle plague which *had been annihilated* by the middle of April, had again been introduced by some Hungarian cattle! What a convenient thing this *re-importation* doctrine is! "From this time, *notwithstanding the slaughter of all the cattle on the respective farms*, the disease continued to show itself in different parts of the province up to the end of the year, with little hope of its being quickly exterminated."*

In November, 1867, Silesia was invaded. Many hundreds of animals were slaughtered, and all communication with the infected districts cut off by military cordons. *Notwithstanding these measures*, the disease gained ground, and by the end of the month, it was existing in twenty-seven parishes, and on seventy-seven farms. It is stated, that the plague had diminished at the end of the year, but it had not entirely subsided, while the losses were heavy. As it commenced so late in the year, the probability is, that it would take head again in the spring and continue its ravages during 1868. The catalogue is not yet complete. In April, 1867, the murrain suddenly made its appearance in Upper Franconia, Bavaria, introduced, *it is believed*, by some Austrian cattle.

* Loc. cit.

"At the outset of the disease, thirty-six animals were attacked, and these being quickly sacrificed, it was hoped that the further progress of the plague would be arrested. The contrary, however, proved to be the case, as the disease extended to the contiguous farms and villages. By about the middle of May, the plague had manifested itself in Lower Franconia, and the Duchies of Coburg, Saxe Meiningen, Saxe Weimar, Gotha, and other parts of Thuringia." The plague had thus obtained a footing, in spite of *cordons sanitaires* and sacrifices to the bovine Juggernauth, in the centre of Germany. How it got there we are not told: on this point, the oracles are dumb!

It is thus evident, from the preceding history, that the cattle plague has spread from country to country, on the Continent of Europe, in spite of all the restrictive measures that were adopted—measures that are carried out, in these purely military kingdoms, with a degree of strictness that would be impossible in any other. To show what these measures are, the following account, obtained by Messrs. Simonds and Ernes, during their visit to Germany, is now added: "In Bavaria the Commission became acquainted with the details of the law applicable to the plague. The regulations are far more stringent than any which have been adopted at home, but they do not essentially differ from those enforced throughout the Continent. They are as follows:—During the continuance of the pest, no cattle, dead or alive, are allowed to be brought across the frontier. Flesh, hides, entrails, horns, hair, and tallow of cattle, and bones, whole or crushed, of any animal, with their hair, wool, or bristles, are especially prevented crossing by the *cordon;* as are woollen cloths, scutchings of leather, feathers, farmyard manure, hay, clover, straw, and all other description of cattle fodder. When the disease occurs on a farm, the affected animals are not

removed from the sheds, but the apparently healthy are taken to the quarantine station. Each Commune is obliged to provide a station of this description, which is built of wood and divided into two parts, one for the doubtful cases, and the other for the supposed healthy. Commissioners, appointed by the Government for carrying out the law, have the power of allowing medical treatment of the animals, but the veterinary surgeon must remain in the quarantine, and receive all he requires at the end of a long pole. All churches, schools, and public-houses of the district are closed, so as to prevent the congregating of people together, and remove those inducements which might cause persons to come from infected farms. On the occurrence of illness among cattle from *other* causes than the pest, the Commissioners do not, as a rule, approach the animals, but, standing at a distance, they arrive at a decision as to the nature of the ailment, frequently ordering some food to be offered as a test of their freedom or otherwise from the plague. In those instances where the Commissioners enter the stable, they are compelled, before leaving, to wash their hands, &c., with vinegar, and have their clothes fumigated with chlorine gas. All dogs, cats, rabbits, domestic poultry, pigeons, &c., have to be kept in places of security and close confinement. If the disease exist in a village through which a high-road runs, the course of the road is turned, if possible; but when this is not practicable, then a guard accompanies the several travellers who arrive at the boundaries of the *cordon*, to see that they do not go upon infected premises. The *cordon* is frequently maintained by the peasants, none of them, however, are taken for this purpose from an infected village, but the selection is made from contiguous villages or farms where the cattle are healthy. As soon as the plague is observed

in a Commune, notices are sent to all the surrounding places so that precautionary measures may be immediately adopted by the owners of cattle. Each Commune has to provide a place for the burial of the animals which die or are slaughtered, and also a waggon and horses to carry them upon; and, on the disease passing away, the waggon is burnt, and the horses are washed with a solution of chlorinated lime. The place of interment is likewise enclosed, and not allowed to be disturbed for several years. On an inspection of supposed cases of the plague, all the animals which give indications of spasmodic twitchings of the muscles are ordered by the Commissioners to be taken at once to the burial ground, where they are killed, and interred with their skins on, these being cut in the usual manner. Occasionally, a special order of the Government permits the removal of the skins, which are then subjected to a disinfecting process under the immediate superintendence of the Commissioners. If only a few cases of the disease occur in a large herd of cattle, the Commissioners have the power to suspend the slaughtering of the supposed animals for a few days, in order to watch the result; such animals, however, have a value put upon them, which is to be paid by the Government in the event of their being killed. Should no animal fall ill within twenty days from the death or slaughtering of the last one, the quarantine is raised; but the cattle which have been liberated are not allowed to go near to others until they have been washed with a solution of chlorinated lime. On the discharge of the animals, the quarantine station is razed and burnt. The Commissioners have to report day by day every occurrence to the Government, and to give the fullest particulars, even to the names of the persons employed at the *cordon*, and the age, colour, sex, &c., of the cattle in the quarantine.

The hay on the farm at the time the pest broke out is not allowed to be afterwards used for cattle, but must be consumed by horses and sheep." It may also be remarked, that Mr. Simonds then considered the entrance of the cattle plague into England almost impossible; the direct importation of cattle from Russia and other countries being then unknown. "When we add," he observes, "that in the event of the disease spreading from Galicia, it would have to break through hundreds of military *cordons*, one after the other, before it could possibly reach the *western side* of the German States; and moreover, that for years past commerce has been unrestricted, with regard to the importation of skins, hides, bones, etc., of cattle from Russia, *all alarm*, we believe, may cease, with reference to its introduction into the British Isles."* The opening of a direct trade with Russia by sea, has afforded the Contagionists, as we have seen, an opportunity of ascribing the origin of the disease in England to importation. But the restrictions, on the Continent, have remained the same. And yet, the murrain has invaded all the countries on the western side of Germany—Holland, Belgium, and France—in spite of the hundreds of *cordons* through which it had to pass, and in spite of all other restrictive measures. The Cattle Plague Commissioners, in their First Report—October, 1865—remark: "In both France and Belgium importation from England has been prohibited, and *stringent and minute* regulations have been issued by the Government of each country with a view *to extinguish* the disease, wherever it might break out. These measures appear to have been successful." At the same time, certain French Veterinarians were kindly writing to us, in the midst of our affliction, urging us to follow their good example;

* Report of the Cattle Plague Commissioners, p. 83.

and implying, that the spread of the disease in England was to be ascribed to our want of care, or else to our stupidity. In an article on the murrain, then prevailing in England, M. Bouley remarks: "England, in want of meat (*affamée de viande*), showed herself, it is true, less strict than us on this point"—the importation of foreign cattle. But, he adds, "If Holland had only taken proper precautions, against the danger of importation of cattle *from England*, it is probable she would have avoided the disaster from which she now suffers."* Nevertheless, although our wise neighbours were so much on the alert; and had so much time to prepare for the enemy, he suddenly made his appearance among them, and that, too, shortly after the preceding remarks were written, or, at least, published.

The outbreaks, it is true, were few and isolated, but they are, notwithstanding, both interesting and instructive. The first occurred in the Departement du Nord, and in that of the Pas-de-Calais; but the date and other particulars have not been given. Strange to say, no account has been published in the French veterinarian journals, as far as I have been able to learn, of this outbreak. This silence is ominous, at least for the Contagionists. We only know, from the Second Imperial Decree, laying down stricter regulations for the prevention of the plague, that the disease was supposed to have been introduced by an infected animal, purchased at Malines on the 3rd of September, and that forty-three cattle had then perished. But, these attacks did not occur in one locality only, and among one herd. They were scattered over two departments. How, then, could this one animal have infected all these localities, more especially as the attacks were nearly simultaneous? Was this ox, or cow, or

* Recueil de Médecine Vétérinaire, Sept., 1865.

calf—it is not stated which—making a tour of the provinces, in order to sow the seeds of disease broadcast on the land? That must have been the case, if all these isolated and independent outbreaks were produced by this one animal. But as this is not probable, and as the importation of cattle, both from Belgium and England, was at that time prohibited, we may at once infer, that the preceding account is a fabulous one, and intended to explain what would otherwise be inexplicable, regarded by the doctrine of contagion. Had it been possible to have traced the cause of the outbreak to the arrival of this animal, we should have had a true, full and particular account of the acts and doings of the criminal, as was the case with those concerned in the second outbreak.

This occurred in the gardens of the Acclimatisation Society, in the Bois de Boulogne, and was supposed to have been caused by two gazelles sent from London. These animals, purchased by Mr. Jamrack, of Radcliffe Highway, on the 9th of November, 1865, on their arrival from India, were removed to his stables, and forwarded to Paris on the 13th. They arrived on the 15th, and were placed, according to M. Leblanc, the Veterinary Surgeon to the Society, in a box by themselves; but three days after, they were removed to a *loge* in the large stable, where fifty other ruminants were. On the 20th one of the gazelles was taken ill, and died on the 25th, and, on this day, the other was attacked, and carried off on the 30th—the disease in both animals being, in the opinion of experienced veterinarians, true rinderpest. On the 28th a dozen yaks and zebus, among the fifty ruminants, were *suddenly and simultaneously* attacked, with symptoms characteristic of the cattle plague. Others were subsequently struck down, so that, out of the fifty

ruminants, thirty-four either died, or were killed.* As regards the introduction of the disease by the gazelles, a few words will suffice to show its improbability. These animals had only just arrived from India and were placed in a stable, in which there were not, and never had been, any other animals suffering from the "cattle plague." As such, there was no source, whence they could have derived the germs of the disease. Added to this, if the source of infection had been in this stable, other animals would have been attacked; but Mr. Jamrack states, that he has never had the disease among his stock, either before or since. In fact, this gentleman is a naturalist, and an importer of rare foreign animals and birds, not of cattle. As to the attack of the gazelles before the other animals, this can be readily understood, by a reference to what may be termed the "law of susceptibility": they were strangers not only to the climate of France, but also to the climate of Europe. Hence they were the first to fall under the malign influence of the cause, that must then have been in operation, for both these outbreaks were, there can be no doubt, spontaneous. They were sporadic cases: had they not been, the murrain would have spread, and become general, the same as was the case subsequently. This did not occur until September, 1870, rather a long interval, but not longer than is observed in other instances.

This outbreak is said to have commenced in one of the cattle parks established by the Germans at Wissenburg—a common result, the cause of which has been already explained. As a matter of course, it has been concluded, that the disease was introduced by these cattle. According to M. le Dr. Lheritier, a convoy of 700 Silesian

* Recueil de Médecine Vétérinaire, November, 1865.

and Hungarian cattle arrived at Wissenburg, in the middle of August. From this they travelled to Nancy, and were pastured near to this city, on the 1st of September. By the 6th many had died, and as others were sick, 111 were immediately slaughtered, and 200 more subsequently. The writer then adds: "*After* the 6th, cases of the plague manifested themselves with other cattle in the neighbourhood, and in the surrounding villages."* It is to be regretted, that Dr. Lheritier did not state the exact day, as this information is of great importance. M. Zundel, in another memoir on the same subject,† mentions that the plague appeared, in Alsace and Lorraine, eight days after the battles of Wörth and Forbach. It is probable, that this account is the correct one, as M. Zundel would appear to be an inhabitant either of Alsace or Lorraine, as his original memoir is in German, and was extracted from a German journal of those provinces. If so, it would be almost impossible to infer, even supposing the doctrine of contagion to be true, that the native cattle had become infected by the foreign. They could hardly have come in contact with the former by that time, following in the rear of an invading army; while it would have taken all the eight days for the incubation of the disease. We shall be strengthened in this conclusion, if, instead of confining our observation to France, we turn to Prussia, whence the disease is supposed to have been derived, or, rather from the Russian and Hungarian cattle introduced by her. It is mentioned, in the September number of the *Veterinarian* for 1869, that the "Cattle Plague" had been prevailing for some months in Poland. "Since then intelligence has reached us, that the disease had crossed the Prussian frontier, and made its appearance in

* Recueil de Médecine Vétérinaire, 1871, p. 1.

† Idem, p. 726.

the province of East Prussia." The writer then adds: "It was hoped that the *severe* measures of extinction, which the Government had adopted, would soon rid Prussia of the plague. The contrary, however, proved to be the case, as the disease manifested itself in other parts of East Prussia, several miles to the north-west of Ortelsburg (where it first appeared). From East Prussia, the plague spread into the province of West Prussia. According to the latest reports, the plague had broken out at Rusenburg, on the Vistula; Landsberg, on the Wartha; Frankfurt, on the Oder; Newark and Kustrin, in Brandenburg, and other places." We thus find that the plague was prevailing in Prussia a year before she imported any Russian or Hungarian cattle, at least for the army. In 1870, the plague extended still farther to the west and the south—to Saxony, Bavaria, Baden and Rhenish Prussia. It prevailed to a serious extent in the province of Leignitz, between Dresden and Berlin; while Landau, Kaiserlautern and Bergzebern, in the Palatinate of Bavaria, were centres of infection—fifteen parishes around Bergzebern having been invaded. These places were all invaded before the Prussian Army had crossed the Rhine—the provision park attached to the army of the Prince Imperial having been attacked at Kaiserlautern. At a later period, the murrain spread not only into the provinces on the south side of the Rhine, but also into those on the north side, as far as Coblentz and Cologne—where there had been no importation of foreign cattle, and where the transit of cattle was in the opposite direction. It is thus apparent, that the cattle plague had reached the frontiers of France before the war commenced; and before a single Hungarian or Silesian ox had entered the French territory. Its extension, therefore, into the latter country was only the natural consequence of the "law of

progression," that appears to regulate all epidemic diseases, both in the human and in the brute creation. Not only do these diseases invariably progress along particular lines of the earth's surface, on their first invasion of a country, but they invariably recommence at the point at which they terminated—no matter whether the interval be a few months, or a few years. Hence, the extension of the disease into the interior of France, "In spite," as Dr. Lheritier remarks, "of all the measures of repression that were adopted." Besides, if the murrain could not be kept out of Prussia, in 1869, we may be sure that it would not be kept out of France, in 1870; for there is no nation that adopts such severe measures of repression, as the Prussian. We had a proof of this in the indiscriminate slaughter of the cattle, in their provision park at Nancy.

It is unnecessary to trace the course and subsequent progress of the murrain. It is sufficient to add, that it spread rapidly into the interior of France, reaching Paris soon after the commencement of the siege. Referring to this event, the Editor of the "Veterinarian" remarks: "How it passed the Prussian lines, which beleaguer the unfortunate city, máy not, perhaps, be known, although we can easily conceive of many ways, by which the infection might easily be transmitted inside the fortifications." * What appears so easy of solution to this writer, must be a matter of perplexity to others. Had the balloons gone into Paris instead of coming out, we might have inferred that the *virus* of contagion had been introduced by them. As, also, the poison could hardly have been conveyed by the Prussian shells, for the virus would certainly have been destroyed during the explosion of the gunpowder in the bomb, we must remain in ignorance on this important question, until enlightened further on the

* November, 1870.

subject. From this time, to the end of August, 1872, the murrain gradually spread over all the fertile provinces of France. In October, 1871, the plague raged in forty Departments of the east, west, centre, and north of the kingdom. It continued to prevail during the remainder of the year, and was spreading with great rapidity, in January, 1872. In the spring, the plague appears to have subsided, to a certain extent, as there were then only nine Departments suffering from it. But it again took head in May, and particularly in the north of France. In the previous Report, only seven Communes were suffering from the plague in the Departement du Nord, but, in this month, there were twelve. Fresh attacks were also reported in June, in the Arrondissements of Dunkirk, of Douai, of Cambrai, of Valenciennes, and of Lille. After this, the plague appears to have declined, as few fresh attacks have been recorded since. This we might infer, as the murrain must have run its allotted course—two years—being a longer time than in England, as we only had sporadic cases in 1867. It is worthy of remark, that sporadic cases of the plague occurred in Belgium, during the greater part of the time that the murrain was prevailing in France; thus showing that the cause was in operation there—in spite of *cordons* and preventive measures—the same as in France, although to a less extent.

We thus find, that the nations on the Continent have not been able to prevent the spread and the ravages of the cattle plague any more than ourselves, although they were held up to us as examples for our imitation. For instance, at a Meeting of the National Association for the Prevention of Cattle Disease, held in 1865, Professor Gamgee stated "that the Committee, in considering the steps which ought to be adopted for the purpose of checking the evil, had carefully inquired into the course which had been

pursued in other countries that had been similarly visited. They found that in Egypt, where the disease had been left to take its own course, unnoticed or uncontrolled, it had carried off 80 per cent. of the whole of the cattle of the country, and that, in fact, the valley of the Nile had been cleared of cows and oxen. But in Austria, on the other hand, where the authorities had vigorously interfered to prevent the spread of the calamity, it had destroyed only 2 per cent. of the cattle." That Egypt suffered severely from the ravages of the cattle plague, the same as all other countries, is undoubted: while it is equally probable that Austria, in the same year, 1841, might be as lightly visited, for the intensity of the disease is not the same at all periods and in all places. But if Austria escaped that year, she has not done so at other times. She was severely scourged in 1844, and on subsequent visitations, as, also, in 1863, when Egypt was again invaded by the plague. The murrain commenced in Austria in 1862, and continued until the end of 1864, in spite of the vigorous interference of the authorities! Referring to this country, the Commissioners, in their Second Report, remark: "In 1862 the number attacked by the plague in the Austrian dominions was 296,000, of which 152,000 died. In 1863 it again invaded and overran not only Galicia, but the whole of the kingdom of Hungary, and its dependencies, the Bukowina, Dalmatia, Carniola, Lower Austria, Moravia, and Styria. Fourteen per cent. of the cattle in these countries took the infection, and the average mortality, as stated in Schmidt's 'Jahrbuch der Gesammten Medecin,' 1865 (p. 95), was as follows:—

	Per cent.		Per cent.
Hungary	65	Moravia	88
East Galicia	77	Lower Austria	92
Croatia and Slavonia ...	81·6	West Galicia	94
Military Frontier	83	Bukowina and Styria ...	100."

M. Clement, a Belgian veterinary surgeon, residing at Palin, in Hungary, stated, in a letter inserted in the "Indépendance Belge," dated July, 1864, that throughout Hungary 180,000 beasts had been carried off, within the three previous years!

It is thus evident, that measures of repression are utterly useless: the disease pursues its own course, in spite of all the efforts of man to arrest its progress. It continues, also, to return, and has done so, during the last forty years, in all the localities first visited, in spite of what is termed the "stamping-out" process. Of what use, then, it may be asked, are these measures, if they do not prevent the spread of the disease at the moment, or its return at subsequent visitations? So far from being of any utility, they are productive of great injury, and great losses to proprietors and to the public. There are the expenses in the first place, attendant on the carrying out of these measures, and the loss entailed on the owners of the cattle, by the slaughter of healthy animals,—a portion of which falls on the public, by the compensation made to the owners, and by the loss of so much food. Then, again, there is the stoppage of the cattle-trade, the impossibility of transporting an animal from one locality to another, and the consequent raising of the price of meat to the consumer—not so much from the actual losses incurred, as from the difficulty of obtaining adequate supplies. But these evils are trifling—as feathers in the balance—compared with the suicidal folly of slaughtering healthy cattle; for a greater act of Vandalism than this was never committed by a civilized nation. Tell it not in Gath; write it not in the Book of the Chronicles of England, that 56,446 *healthy* cattle were slaughtered, in Great Britain, during one irruption only of the cattle plague. And for what? To

prevent, as it is ironically termed in the official returns, the spread of the disease. And has it done this? Not in one single instance. All that it has done, all that it could do, has been to prevent a few more of the herd from being attacked; as it is only under exceptional circumstances, that the whole of a herd would be carried off. It has been shown, in the previous chapter, that the epidemic cholera is most severe and most fatal at the commencement of the outbreak; and that it assumes a milder form, after it has arrived at its height, and towards its termination. Precisely the same result occurred with the cattle plague. At its commencement whole herds were swept off, but the ratio of attacks diminished afterwards, and they would have diminished more and more had the opportunity for observation been afforded us. But it was precisely at that period, when we might have looked for a diminution in the proportion of attacks, that the pole-axe was brought into general use, and swept off, not only those that were, and those that might have been, attacked, but those, also, that would *not* have been. How many of these would have been attacked, it is impossible to say: but in several of the outbreaks that occurred, in 1867, when the pole-axe was not employed, only one in a herd, as we have seen, was attacked. This shows that the plague, not only diminished in frequency, but in intensity, towards its termination. Taking this fact into consideration, and reasoning from analogy—from the law of mortality in human pestilences—we may conclude that not more than a third of the healthy cattle would have been attacked. The rest were wantonly sacrificed to the veterinarian Moloch. The act, therefore, was not only a barbarism, and a sin, but it was a suicidal act: it was taking the children's food and throwing it, not to the dogs, for that would have been a more pardonable offence, but

to the worms! This, however, is only a portion of the evil, and a very small portion. It is not the losses we have already sustained, but the losses that we may sustain hereafter, which we have to consider, if this patent process for the prevention of disease is to continue in operation. Should the cattle plague return—as it will and *must*, and, probably, for the next five or six centuries—and at short intervals, as it has done in other countries, the result will be, not the stamping-out of the disease—as it will probably extend hereafter to other species—but the "stamping-out" of the cattle in these isles. That must be the inevitable result, unless the pole-axe be thrown, as it ought to be, to the bottom of the sea.*

Another question arises, in the consideration of this part of the subject. This is, whether means could not be adopted to prevent the healthy cattle being attacked, when the plague breaks out on a farm or among a herd. That this might be done, in certain circumstances, and to a certain extent, admits of no doubt. It has been before shown, that when human beings remove from an infected town, or locality, to another in which the causes productive of the disease do not exist, they remain exempt from any attack. There can be no reason, why the same means, if adopted with cattle, should not be productive of the same result, if all restrictions to the movements of cattle were abolished. It is one of the measures recommended by me, in the brochure before alluded to.

* This "stamping out" is only a return to the practice of remote and barbarous ages; and appears to have been adopted, at a very early date, in Ireland. According to a popular song, the cattle, in the time of St. Patrick, committed *suicide*,

"To save themselves from slaughter."

Wise animals! It is a wonder, that the English cattle have not followed their example.

This is not all. If the "cattle plague," as has been inferred, be not propagated from animal to animal, there can be no more reason for slaughtering the diseased than the healthy animals. In that case, there might be some chance of saving a certain number of those attacked, hereafter. It is said, that this murrain is incurable, and that no mode of treatment, hitherto employed, has been of any avail. That, unfortunately, is the fact. But, then, it does not follow, because we have hitherto been unsuccessful in our efforts, that we are never to be successful. If that argument had been employed, in the case of human diseases, the probability is, that the pole-axe would now have been the only remedy to be met with in our present *Materia Medica*, while the only use of the doctors, like that of the veterinarians, would have been to pronounce the sentence of death, whenever an unfortunate being happened to be attacked with disease. It so happened, in fact, that no remedy could be found for the epidemic cholera, for many years after it made its appearance; but, now, thanks to the non-employment of the pole-axe, there is no disease so amenable to treatment as this—provided only that the right remedy be resorted to, before the state of *confirmed* collapse! But, then, this result was not obtained, until after *three thousand* different remedies had been tried in vain. There can be no reason, why the same result should not follow with the "cattle-plague," if the opportunity only be afforded to the doctors and the veterinarians—there being many men of ability among the latter class, anxious and willing to try their skill in this bovine malady.

We have not yet done with this unhappy subject—this massacre of the innocents—thanks to the untold and manifold evils that the Contagionists bring upon the world. Not content with slaying the animals—for which

there might be some excuse if it were done out of a merciful regard for their sufferings—but they actually bury the flesh, and destroy it with quick lime; so as to prevent even the worms from having a feast on this forbidden food. And yet the flesh of these animals—the diseased as well as the healthy—might be taken with perfect impunity by man, as well as by all the carnivora. Of this fact, no possible doubt can be entertained. In a communication made to the Academy of Sciences, Feb. 27th, 1871, by M. Bouley, we find the following remarks.* After observing, that no instance is known of the propagation of "cattle plague" to man, with those who handle and dissect the bodies, and even with those experimentalists who have inoculated themselves with the morbid secretions, he adds: "No doubt can exist on this subject. There exists, then, as is apparent, an essential difference between carbuncular diseases, truly virulent and communicable by inoculation to man, and the plague of cattle, which is not communicable to him. Now, although carbuncular diseases be communicable to man by inoculation, nevertheless, the use of even carbuncular flesh as food is innocuous, when cooked. For a still stronger reason ought it to be so, with the flesh of animals affected with the plague, since this flesh does not contain, even when raw, any principle injurious to man. This is not a mere induction; the proof of the absolute innocuousness of the flesh of animals, affected with the plague (bovine), has been obtained." After stating that the people and soldiers were in the habit of eating the meat of cattle that had died, during the prevalence of the former plague murrain; M. Bouley remarks: "At this period, as M. Huzard mentioned to the Society of Agri-

* De l'Emploi de la Viande des Animaux, atteints de la Peste Bovine, pour l'Alimentation.

of August—twelve days after its arrival—the heifer was taken ill, and was killed on the 12th. Six days after this, or on the 18th, the steer was attacked, and slaughtered in due course. On the 21st, one of the calves in the adjoining field was taken ill and died on the following day—the other calf and the two cows being carried off in succession. The next outbreak, of which we have any account, occurred among a herd belonging to Mr. Berriman. These animals had been purchased in Hunmanby Market on the 19th of August, and were sent by road to Mr. Berriman's farm at Yapham, where they arrived on the 28th. On the same day, one of these animals was found to be dying; and several others were observed to be ill; but the true nature of the complaint was not ascertained until the 3rd of September. As soon as the nature of the disease was ascertained, the remaining cattle in the herd were slaughtered, and the farm at Yapham declared to be an infected place. But this did not stop the spread of the murrain, as we shall presently find.

Such is a brief outline of the commencement of this visitation; and it now remains to ascertain its cause. It is necessary to premise, that a vessel, the *Joseph Soames*, had arrived at Hull, on the 25th July, from Cronstadt, with fifty-eight oxen on board. Two, it is stated, had died on the voyage, and, in consequence, the cattle were inspected on the following day by Messrs. Simonds and Brown, who pronounced eighteen to be infected with cattle plague. At eleven a.m. on the following day, the 27th, orders were received to slaughter the whole, which was immediately done, all the animals being dead by two p.m. The carcases were put into two lighters, which were towed out to sea, at eleven p.m., and sunk. Under such circumstances, we might have concluded, that we should have heard no more of this Russian cargo. But

no, the bodies of these animals have been used as a peg, on which to hang the last shreds of a rotten theory. It is to their arrival in Hull, that the outbreak in Yorkshire has been ascribed. The hatches of the lighters not having been properly secured, the carcases of the animals floated again, and were washed ashore on different parts of the coast.* By some persons, the outbreak was referred to infection from these carcases; but the editor of the "Veterinarian," Mr. Simonds, states that, "The carcases had nothing to do with the introduction of the cattle plague into Yorkshire. We make this assertion on the fact, which has been clearly demonstrated, that the first outbreak occurred rather before than after any of the carcases had been thrown ashore. It," the outbreak, "is traceable, as are the others also, to the purchase of animals in Hull Cattle Market, on Monday, July 29th, and Monday, August 12th.....The ship, with the diseased cattle on board, was lying for several days (two) in the Humber Dock, close to the 'Forin,' and not 500 yards from the cattle market, and in spite of all that could be done, there was open communication between the vessel and the shore.....The precise manner of the conveyance of the poison has not been, and certainly, *will not be*, ascertained. No one, in short, saw one of the men, who had been engaged about the diseased cattle, rub his hand or coat against an animal in the market, or on its way

* On the 2nd August, one lighter, with forty carcases, was washed ashore at Hassoft, on the coast of Lincolnshire, near to Boston. On the 12th, one carcase was washed ashore at Crow Stone, between Leigh and Southend, and fourteen others were thrown into the Port of Wells and upon different parts of the coast of Norfolk. Of the fifty-six carcases, fifty-five have been found at places, where no cattle plague has since appeared; and one remains unaccounted for.

thereto. But when a centre of infection has been established, it is not usual to cavil much about the possible means of communication, between that centre and a place a few hundred yards distant."* This is, according to Mr. Simonds, the first link in the chain of presumptive evidence: the next is, the purchase in the Hull Market, on the 29th July, of the heifer and steer before alluded to. It is to the infection imbibed in the Hull Market that the attack of these two animals has been referred. The writer observes, it is not usual to cavil much about the means of communication under such circumstances. That may be the case, possibly, with certain persons; with those who take everything for granted emanating from authority: but not being of that temperament, and not being accustomed to consider official dicta as infallible—for the opinions expressed in the "Veterinarian" may be regarded as the opinions of the Veterinary Department of the Privy Council—I cannot help cavilling at assumptions and conclusions like these, more especially when unsupported by a single fact. In addition to this, these conclusions appear to be, on their very face, alike extraordinary and improbable.

The cargo of Russian cattle, as will be remembered, were slaughtered on the 27th July—two days before the market was held. If, therefore, the heifer, subsequently attacked, received the infection in the market, it must have been derived from one of two sources. Either the germs of the disease were blown from the *Joseph Soames* —moored to the dolphins, in the centre of the dock—into the market; or, else, they must have been conveyed, as Mr. Simonds suggests, by one of the men employed on board, rubbing his hand, or his coat, against an animal in the market, or on its way thereto—the animal being, we

* The "Veterinarian," October, 1872, p. 737.

may presume, the poor unlucky heifer. As to the first of these suppositions, we may conclude, that the germs of the disease, if blown into the market, would have affected, or infected, as the term is, more animals than one. But we have not heard of a single animal, excepting the heifer, among the hundreds assembled in the market on that day, having been attacked with *rinderpest*. We may therefore dismiss this supposition as fallacious: as we may the second, and for precisely the same reasons, more especially as there must have been several men engaged in slaughtering these animals. If one man could convey the seeds of the disease to one animal, the others could, and would, in all probability, have done the same—more especially if they did not wash their hands, or change their clothes, between Saturday and Monday, as must have been the case with the man who infected the heifer. Added to this, the heifer was not attacked until twelve days after its departure from Hull—four days beyond the time usually assigned for the incubation of the disease. We may, therefore, conclude that this animal did not imbibe the seeds of the disease in Hull Market.

But the strangest part of the story remains to be told. The outbreak, in Mr. Berriman's herd, has been already mentioned. Although there were, as we have seen, several attacks in the neighbourhood previously, no attempt has been made to connect this outbreak with the others. The reason is evident. One of the animals was attacked on the day of its arrival, and the others in quick succession. It was impossible, therefore, to ascribe the outbreak to local infection. Still, as the Contagionists are enabled to explain all difficulties, and have an answer to give to all doubts, another cause has been assigned. As the cattle came, not from Hull, but from Hunmanby Market, and from a farm where the plague had not appeared; it fol-

lowed, that the disease must have arisen spontaneously in them, unless it could be shown, that they had come in contact with an infected animal during their transit. This, it has been stated, was the case, and in the following way:—"On August 12th Mr. Taylor, near Bridlington, bought three steers, in Hull Market, and sent them to his place. One of these was killed, and the other two were sent, on the 19th, to Hunmanby Market and sold to Mr. Berriman of Pocklington. One of these was the first observed to be ill, and then all the rest of the herd were attacked, in *quick succession*."* As the two steers were on 'the tramp,' having been first sent to Hull and then to Hunmanby, we can understand that they would be more predisposed than others to be affected by the malign cause then in operation. But to infer, that the animal, attacked on the 28th August, had imbibed the seeds of the disease in Hull Market on the 12th—sixteen days previously, and sixteen days after the destruction of the Russian cargo—is so absurd and preposterous an idea, that it would only be a waste of my readers' time, as well as of my own, to attempt to refute it. If the heifer, that was in the market two days after the death of the infected animals, did not imbibe the seeds of the disease there, we may be certain that the steers, which did not arrive there until a fortnight after, could not have done so, more especially as none of the cattle that were in the market, on the intervening day—the 5th August—had an attack. It is, in fact, a second edition of the *Revel* cargo tale, only more romantic and sensational, and, consequently, less credible. As such we may infer, that the outbreak of cattle plague in Yorkshire was not caused by the importation of Russian cattle into Hull—circumstances that were unconnected with each other, excepting so far as showing

* The "Veterinarian," October, 1872.

that the same cause was in operation, at the same time, in Russia and in England.

This inference receives support from the fact, that there were other vessels which arrived about the same time, in other ports, with diseased animals on board, but they failed to spread the disease in a single instance. For instance, one vessel, with a cargo of twenty-five Russian cattle from Cronstadt, arrived at Deptford on July 17th. One died *the day after landing;* and the characteristic lesions having been found on a post-mortem examination, the rest of the herd were slaughtered, but not before symptoms of the disease had manifested themselves in some of them. Here then there was a greater chance of infection, the cattle having landed, and one animal having actually died in the yard; but no cases were observed in this part of England. Another cargo arrived at Leith on the 21st July. Three of the animals exhibiting symptoms of the cattle plague, the whole were immediately destroyed. The *Brigadier*, with seventy German, and eight Russian cattle, reached Newcastle on the 22nd July. One animal died fourteen hours after the arrival of the ship; and as another exhibited the same symptoms, it was killed, and the bodies of both examined. The characteristic lesions having been found, all the remaining cattle were slaughtered. It is worthy of note, that none of the German cattle exhibited any signs of disease, although they had been in close contact with the infected animals for some days. The *Viatka* also arrived at Deptford, on the 28th July, from Cronstadt, with diseased cattle on board; and the *Gipsy Queen* at Hartlepool, on the 29th, with a cargo of German cattle from Hamburg; several of which gave indications of cattle plague. All were immediately slaughtered. A second cargo—103 in number—was landed at Newcastle from the s.s. *Brigadier*,

on July 29th. They were all German. One animal appeared to be indisposed on landing, and as symptoms of cattle plague made their appearance soon after, all were destroyed. As the risk of infection, in some of these cases, was much greater than with the cargo lying in *the centre* of the dock at Hull, we may infer, independently of what has been advanced before, that the outbreak in Yorkshire was not caused by the arrival of infected animals from abroad. As such, the disease must have arisen spontaneously, there having been no other source from which the infection could have been derived. A strong confirmation of this conclusion would be derived from another circumstance, if true, viz., that there was an outbreak at Ampthill in Bedfordshire. The Under-Sheriff of Lincolnshire also reported an outbreak of cattle plague at Skeyness, a village on the coast near Louth. No particulars have been furnished of these outbreaks, from official sources, that I am aware of. This is to be regretted, as these cases, if actually cases of "cattle plague," would be alone sufficient to prove the spontaneous origin of the murrain in England in 1872. Fortunately, this evidence is not required: the facts previously adduced, are sufficient to prove the truth of this conclusion. If the murrain was not introduced by contagion, as has been inferred, it must have arisen spontaneously.

If the disease arose spontaneously, we must also conclude, that it subsided spontaneously. No other logical conclusion, in fact, can be drawn on the subject: for if there be a cause—although at present unknown and beyond the ken of human knowledge—sufficient to produce an outbreak of cattle plague, we cannot fail to infer, that the cessation of the effects are due to the cessation, or subsidence of the operating cause. But a different con-

clusion has been drawn on the subject. It is asserted, and is now generally believed, that the plague was "stamped out." That it was not, may be inferred from the arguments advanced in a previous part of this chapter. It is unnecessary, however, to refer to them; the history of this single outbreak is alone sufficient to disprove the assertion. Passing over the first cases—those of the heifer and steer, purchased in the Hull Market, and the miller's two cows and calves—we come to the outbreak among Mr. Berriman's herd. Of twenty-two animals, eight were attacked, seven died, and one was killed: as, also, the remaining fourteen healthy animals. With this sacrifice we might have presumed, that the plague would have been "stamped out." But no. Ten days from the slaughter of Mr. Berriman's herd, the disease appeared among the cattle belonging to Mr. Towse, Mr. Craddock, and Mr. Kirby, at Yapham. On these premises thirty animals were killed. It then appeared at Belthorpe and thirty-nine animals were killed. On the 1st October the plague appeared at Tangfoss, outside the infected district, among Mrs. Bielby's cattle. Ten days after this, it broke out at Mr. Burneby's farm at Garrowby, five miles from Pocklington and two from any known centre of infection. On October 14th a cow belonging to Mrs. Dale, of Skirbenbeck, two miles from Garrowby, was attacked, and on October 18th, "a case occurred at Belthorpe, which has since been followed by two other outbreaks, leading to the destruction of eleven cows and eight calves, besides the animals attacked."* These were not the whole of the outbreaks, as there had been no less than twelve on the 21st September—three weeks after Mr. Berriman's herd was attacked—viz., three at Pocklington, two at Bridlington, two at Sowerby, five at Patrington, and a doubt-

* The "Veterinarian," November, 1872.

ful one at Bridlington. There was also an outbreak at Yapham on the 23rd September, among a herd of thirty-seven animals, all of which were immediately slaughtered. Still, isolated cases continued to occur, as we have seen, to the end of October. Of what use, then, is this stamping-out process, and this sacrifice of so many healthy cattle, if the disease be not arrested? It will be answered, no doubt, that the disease was stamped out eventually. This, however, is an assumption that no one has a right to make. If the disease was not stamped out, in the first instance, it is illogical to conclude, that the employment of the same means had been efficacious in the last. Preventive measures were not, it is true, carried out so strictly in the first outbreak, when the heifer was attacked; but they were, in the second, which, as will have been remarked, was an independent outbreak, and entirely unconnected with the first outbreak. It is stated, in the Circular issued by the Privy Council, "In the outbreak at Pocklington (in Mr. Berriman's herd), the local authorities, as soon as they received information, lost no time in exercising the powers entrusted to them. They at once caused all the animals in the infected herd to be slaughtered, and declared the farm at Yapham to be an infected place; together with a circuit of one mile, from the field in which the infected cattle were." And it was further stated, in the "Mark Lane Express," "the footpaths have been stopped, and disinfectants used in all the *gateways:* and a *cordon* of police has been established round the infected circle." Great energy has been displayed by the police to have the disease "stamped out." But, if these severe and energetic measures were insufficient to prevent the extension of the disease from Mr. Berriman's herd to those of Mr. Towse, Mr. Craddock, Mr. Kirby, and others, what was there to prevent its ex-

tension to other districts? Nothing whatever, as we should have a right to conclude, that the restrictive measures which had failed, in the majority of cases, would fail in all. Added to this, we can explain the subsidence of the outbreak, by a reference to natural laws, instead of to the doctrine of men, so liable to error.

It has been already stated, that there were sporadic cases of cattle plague in England, in 1867. If there were sporadic cases in 1867, there can be no reason why they should not occur in 1872; if the same cause were in operation. That it would be, we might infer from the fact, that the cattle plague was prevailing to a considerable extent, not only in Eastern Europe, but in France. If, therefore, this disease be produced by a general cause, as has been inferred, it was to be expected, that some of the effects would also be observed in England, although, as it appears, to a less and almost inappreciable extent. Hence the cause of the outbreak in Yorkshire; and hence the subsidence of the disease, for when this general cause ceased to operate, the effects would cease at the same time. That the cause productive of the cattle plague subsided about this time, we have proof from the cessation of the disease in France; few fresh attacks having been recorded, in that country, after the month of August or September. We may, therefore, fairly conclude, that the murrain would have subsided in Yorkshire, precisely the same, if the pole-axe had never been brought into requisition: while the use of this instrument is not only a great social evil, by the unnecessary sacrifice of so many cattle, but it is an injury, also, to the progress of science, and the acquisition of veterinary knowledge. As long as the pole-axe continues in use, so long shall we be in ignorance of the laws which regulate the rise, continuance, and decline of the cattle plague: as

we should have been of the laws which regulate human pestilences, if this instrument had been resorted to for their prevention. For instance, in the visitation of plague in London, in 1603, the deaths amounted to 36,000; but, in the following year, there were only 900; and in 1605, only 400. So, also, in 1666, the year after the last visitation of plague in England, 1,998 persons died of this disease in London: 35 in 1667, and only 14 in 1668. Deaths from plague were recorded, for a few years after this, but the number never amounted to more than five annually: thus showing that the disease never spreads, or becomes general, when these sporadic cases occur. Now suppose that the stamping-out process had been in vogue then; and that it had been resorted to on either of these occasions. How the Contagionists would have boasted of their success, and of the benefits they had conferred on mankind, by thus "stamping-out" the plague. The church bells would probably have been set ringing—not in London, as there could have been nobody left to ring them, but in the country—cannon, in all probability, would have been fired, and a *Te Deum* sung to celebrate the "victory" over the plague. And yet, how erroneous would have been the conclusion, how wicked, how diabolical the act. Nevertheless, this is precisely what has been done in the cattle plague—the only difference being that, instead of 50,000 or 60,000 human victims, 50,000 oxen have been sacrificed on the altar of scientific ignorance and scientific presumption.

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IN THE

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AND

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