newly-born children that the milder irritants produce less visible results, and are more circumscribed in their operation than in older children.

Dr. Faye says that he cannot express a distinct opinion on the point as to whether the continued application of irritating matter to a limited surface would produce immunity of the skin generally, nor how long the local immunity would last. All his experience had reference to the treatment of cases; and that treatment was discontinued when the cases were convalescent. From the facts which had come under his observation he is able to affirm that, after the application of the ointment as described, the skin on a large surface of the body—as, for instance, the nape of the neck and the back—becomes callous to its action, or, at the most, shows signs of a superficial soreness, which very soon dries up. And he also mentions that, after such application, the same irritants applied to other parts of the skin do not act so powerfully as those previously used.

From these facts, occurring under his own observation, Dr. Faye draws the conclusion that the repeated application of the ointment of corrosive sublimate, or of croton oil, or of other simple irritating agents to large surfaces of the skin, will lead to similar results as those obtained in Paris by the repeated application of blisters. This immunity, however, does not extend through the whole texture of the skin; for if the smallest quantity of the same irritating substance which no longer produces any irritation on the surface be injected into the texture of the skin, a small abscess will without fail follow. Dr. Faye mentions that Dr. Danielssen had found similar results to follow the repeated application of antimonial ointment on different parts of the bodies of patients affected with leprosy in the hospital at Bergen.

Dr. Faye wishes to draw a marked distinction between substances which produce irritation of the skin only and those which act chemically upon the tissues. The effects of the first class gradually cease; those of the latter continue as long as the skin remains alive. But he particularly dwells upon the fact that immunity of the surface of the skin does not necessarily imply immunity of other parts or of the general system. A very interesting illustration of this fact occurred during the last year. There was a patient of Mr. James Lane's in the Lock Hospital upon whom Professor Boeck supposed he had produced immunity by repeated inoculation. Mr. James Lane was good enough to allow me to test for myself whether immunity had really been produced. I inoculated some matter below the cuticle, a little deeper than the inoculations which had previously been made, but not deep enough to produce bleeding. The result was that in due course a small, well-defined, circular chancre, with sharp edges and a suppurating surface, was produced.* The surface-immunity, then, in this case did not imply an immunity for other parts. It was the surface of the skin only, and not the general system, which had become proof against one particular form of irritation.

To the list of observers above mentioned may be added the name of Dr. Rennie, whose experiments and observations Dr. Faye had not seen when he wrote his essay. These observations are referred to at some length in *THE LANCET*, on the 7th of April last, p. 362, and they entirely confirm Dr. Faye's experience; the only difference being that Dr. Rennie's cases occurred almost exclusively in adults, Dr. Faye's chiefly in children.

If, then, the observations already made, supported as they are by different physicians in the most remote parts of the world, should be confirmed by future experience, we have in the phenomena of so-called syphilisation only another manifestation of a common law. The surface of the skin becomes proof to a greater or less extent against the irritation of specific puriform matter, as it would against the repeated action of blisters, corrosive sublimate ointment, or croton oil. But in none of these cases does the immunity to a particular irritant imply that that immunity extends to other textures besides the immediate surface of the skin; nor that irritants of a different nature would not produce their ordinary effects upon the skin itself; nor, again, that the same action might not be again produced upon the skin so soon as the immunity should have passed off. As far as our present knowledge goes, it would, therefore, appear quite possible for a patient to become proof against the further inoculation of specific purulent matter on the surface of the skin and yet to become infected with

another kind of syphilitic matter in the ordinary way, either through the skin or through some other part.

It would, of course, militate against the idea of any fresh matter in such a case being taken through the skin, if it could be shown that all syphilitic matter was essentially of the same nature; but even then other ways would still be open through which infection might occur.

That all syphilitic matter is essentially of the same nature was one of the facts that Professor Boeck attempted to demonstrate during his visit to England. He believes that if some secretion be taken from an indurated sore and inoculated, and a soft suppurating sore results, it is conclusive evidence that the poison in the two cases is the same. The evidence hitherto adduced in England on this point is most unsatisfactory, and the cases recorded are most inconclusive. It is now upwards of sixteen months since Professor Boeck paid his visit, and during that time, so far as we may learn from any published records, in two cases only has the matter been taken from an indurated sore and inoculated so as to produce a lineal series of chancres. These two cases (a sufficiently meagre list for a period of sixteen months) have already been sufficiently discussed in the pages of *THE LANCET*. But some observations may not be out of place with regard to the mode in which recent experiments on inoculation have been conducted.

The subject of a clean lancet need not again be referred to. There is one point, however, which appears not to have attracted the attention of those who have conducted the experiments—namely, that the inoculations, after they were performed, were left uncovered. It has every now and then happened to me to see spontaneous auto-inoculation occur in patients with suppurating syphilitic sores. This has occurred without any scratch or abrasion of the skin having been previously noticed; and it is certainly not less likely to occur where the skin has been abraded with the point of a lancet, and where the irritation thus produced may lead the patient unintentionally to scratch the irritated parts. It has also several times occurred to me to see a patient who had been many days in hospital without leaving the house, and who had no syphilitic symptom on admission, become affected with a soft suppurating sore. It is of course difficult to prove in what way such affections are communicated; but when we reflect that in Lock Hospital's patients may often necessarily use the same basins and the same towels, it is not surprising that such communication should occasionally take place. If this, then, may take place on skin apparently sound, it is evident that it may also occur on skin from which the cuticle has been removed by the lancet. This, therefore, among other sources of error, must be guarded against before a few cases can be held to illustrate a general law.

Fournier, in his experiments, found that indurated sores furnished an auto-inoculable secretion in the ratio of 2 per 100. Rollet's experiments gave 6 per 100. The proportion of cases in England, as far as any published records are concerned, do not appear to furnish any very different results. It is said, indeed, that by continually irritating an indurated sore, a secretion is at last produced which, as a rule, will produce a lineal series of inoculations. The only proofs of this assertion hitherto published in England are, as far as I know, the two cases above mentioned, and in one of these the sore suppurated from the commencement, and no irritating application was used. From these two cases three series of inoculations were performed. In one only of these had the process of long-continued irritation been applied to the indurated sore, and in that instance the patient was at the same time inoculated repeatedly with the matter from an ordinary suppurating sore. The first abortive attempts at inoculation from the indurated sore and the successful inoculations from the suppurating sore were alike left uncovered, as in the other recent experiments on inoculation.

Savile-row, January, 1867.

ON

CERTAIN POINTS RELATING TO CHOLERA.

By HENRY WM. FULLER, M.D. CANTAB., F.R.C.P.L.,
PHYSICIAN TO ST. GEORGE'S HOSPITAL.

It may appear strange at this late period of the year to invite a discussion respecting cholera. The epidemic, it may be said, has almost subsided, and years may elapse before another opportunity occurs of investigating its natural history. But I

* The secretion from the pustule which preceded this sore was inoculated by Mr. James Lane, in the usual very superficial way of performing the operation. Two imperfect pustules were produced, and from these no further inoculations with the mode adopted could be made. Had the subsequent inoculations, however, been made to the same depth as the original one, a lineal series might, in all probability, have resulted.

confess that I entertain a different opinion. The course of former epidemics of cholera, viewed in connexion with the salient features of the present visitation, is opposed to such a supposition. On all former occasions the full force of the malady has not been felt until the second year, and the experience of our neighbours across the channel has shown that in this respect the disease is observing the course it has always taken. Last year it committed sad havoc in Paris, and this year again its ravages were repeated. In London it has now visited us for the first time, and there is no reasonable ground for supposing that its visit will not be repeated next year. Indeed, many features of the outbreak we have just witnessed appear to show that we may expect a more general and more serious epidemic next year. This year the disease has nowhere prevailed in an epidemic form, except over a limited area at the East end of London, but it has gradually spread over the whole metropolis, and its progress has not been entirely arrested, even by the advent of cold weather. It has shown itself generally throughout the United Kingdom, and though its attacks have been few, and in most cases isolated, both in London and elsewhere, they have been virulent in their nature and rapid in their course, as if the seeds of a malignant form of disease were present, and only needed some peculiar condition of the atmosphere to call them into full activity, and ensure a serious outbreak of the epidemic. In short, if we may judge of the future from the past, there is too much reason to anticipate that next year we shall have ample opportunities of investigating the disease, and of testing any theory as to its origin, nature, and mode of spreading.

It is not my intention on the present occasion to enter into a discussion of the various theories which have been broached respecting cholera, but I will simply state my opinion that the existence of a distinct *materies morbi*—a cholera germ—probably of fungoid origin, which can be conveyed from one place to another, and possesses the power of reproduction in each locality under certain atmospheric or other conditions, is alone capable of explaining many of the facts which have been observed in regard to the disease. Amongst these may be mentioned the infrequency of its invasion; its occasional sudden outbreaks—outbreaks at once so sudden, so widespread, and so severe as to be inexplicable except on the supposition of some pervading morbid influence; the vast extent in some instances, and in others the limited area of its attacks; its predilection for low, dirty, ill-drained, unhealthy localities; the immunity of towns above a certain level; the extraordinary differences which have been noted in its period of incubation; the remarkable uniformity of its duration in an epidemic form in any locality; its spread generally in the direction of the wind, but sometimes in the very teeth of the wind; its apparent contagiousness or communicability in one case, and its non-contagiousness in another. Admitting, for the sake of argument, that cholera is due to the agency of some fungus, there is no one circumstance in the history of the disorder which is not susceptible of explanation. Nor is the theory far-fetched or improbable. The recent and very interesting researches of Dr. Salisbury have proved almost to demonstration that ague is produced by the spores of one of the species of palmellæ; and the experiments of Dr. Flint and others are scarcely less conclusive as to the fact of measles being caused by the presence of a peculiar fungus developed under certain circumstances on straw. It seems probable, therefore, though it yet remains to be proved, that cholera and all so-called zymotic and epidemic disease are referable to the noxious agency of some minute fungoid growth—an influence which would explain the whole history of the disorder.

This theory of cholera not only serves to unravel much of the mystery in which the disease has been enshrouded, but it forms an intelligible basis for preventive and curative measures. Knowing as we do how slight an alteration in the degree of light, heat, moisture, and other circumstances will promote or prevent the growth of fungi, how certainly the decomposition of animal and vegetable matters favours their development, how readily their spores may be conveyed from one place to another, and how unlikely they are to take root and develop unless they meet with a congenial nidus—it is quite intelligible that the disease should prove contagious in one instance and in no degree contagious in another; and it is also obvious that by taking measures to render the atmosphere pure, and to keep it in a condition unfavourable to fungoid growths, we shall be taking the most effectual steps to keep off the disease or to check its progress.

This brings us to the question of quarantine on the one hand, and of hygienic precautions on the other.

It must be admitted, I think, that cholera is not ordinarily

contagious. Instances without number have been placed on record in which cholera patients have been massed together in buildings imperfectly ventilated, and where adequate cleanliness was simply impossible, and yet doctors, nurses, washerwomen, and attendants have wholly escaped infection. Instances are within the knowledge of every practitioner in which the disease, though virulent in certain houses, or sometimes in a particular portion of a house, has not evinced any tendency to spread, although free communication has been kept up between those houses and persons resident in the immediate neighbourhood. The same has been witnessed on a larger scale. One town, or one portion of a town, has been decimated by cholera; whilst another part of the town, or another town a few miles off, in daily and hourly communication with the infected localities, has remained perfectly free from attack. On the other hand, it would appear that the disease is communicable under certain unknown conditions. In many instances its invasion has been traced to the arrival of a ship from an infected port, or to the visit of an individual from an infected district; and many curious cases are recorded which seem to prove that the malady may be imported into a house, and may then be communicated not only to the inmates, but to the washerwomen and others connected with the family resident out of the house. In short, the history of cholera leaves no room for doubt that the disease is not contagious under ordinary conditions, but may become so under exceptional circumstances.

This remarkable difference in the contagiousness of cholera is not inconsistent with the result of observation respecting other diseases of the zymotic or epidemic class. On the contrary, it appears to be the rule rather than the exception, and it is quite in accordance with what might be expected if the disease acknowledges a fungoid origin. For whilst at one time, or in one locality, the condition of the atmosphere may not be favourable for the transmission of the fungoid spores, nor the bodies of those in contact with the patient fitted for their reception and development, at another these conditions may be reversed, and the spores may be suspended in vast numbers in the air and water, and may find the bodies of those who are about the patient fitted by electrical or other agencies to become suitable seats for their growth. This is just what was observed by Dr. Salisbury during his investigation into the action of certain of the palmellæ in the production of intermittent fever, and it is quite consistent with what we know in regard to the growth and spread of fungi generally.

Admitting, then, for the sake of argument, that the cholera germ may be, and sometimes is, imported, and that the disease is apt to prove contagious, what view should be taken respecting quarantine? My own, I confess, coincides with the practice hitherto pursued in this country. If it could be shown that the disease could not arise without the importation of its germ through human agency; that the germ loses its vitality in fourteen or twenty-one days, the limit of the time to which quarantine usually extends; and that the disease, when it does occur, *commonly* proves contagious,—then I maintain that we should be bound, at all hazards and at whatever inconvenience, to enforce the strictest quarantine. But facts are decidedly opposed to these suppositions. The records of cholera teem with examples of its invasion without any previous communication through human agency with an infected district; and ships at sea have been attacked with cholera though the disease did not exist at the port from which they sailed, and though they have not touched land for many weeks,—circumstances which seem to show that the seeds of the disorder may be conveyed by the air as readily as by human intercourse. Further, it appears doubtful whether the cholera germ loses its vitality within twenty-one days, or within any specified limited period; for quarantine, even where most strictly enforced, has proved ineffectual to prevent the importation of the disease. On the other hand, in this country, where quarantine regulations are not enforced in respect to ships arriving from ports infected with cholera, the disease has been imported and *has not spread*. The conclusions therefore seem inevitable, that the seeds of cholera may reach a country through the medium of the atmosphere; that when they are so introduced, quarantine, however strict, cannot prevent the invasion and spread of the disease; and that when they are not so introduced, but are imported directly through human agency, quarantine is seldom needed to prevent an outbreak of the disease, inasmuch as the malady will not spread in an uncongenial atmosphere, and therefore in most cases, with certain precautions, may be strictly limited in its effects.

The influence of sanitary arrangements in warding off the advent and spread of cholera is a subject which, equally with quarantine, demands careful consideration, lest, on the one

hand, the importance of sanitary measures should not be sufficiently recognised, and, on the other, it should be regarded as greater than it really is. No one can doubt that defective drainage, the emanations from sewers, the gaseous exhalations from decomposing filth of all kinds, impure water, over-crowding, imperfect ventilation, and other agencies of this kind, promote in some way the spread of the disorder; and that intemperate habits, deficiency of food, the depressing passions, and other circumstances which lower the general health, exert a similar influence. They do so probably in part by depressing the nervous power and lessening the resistance of those exposed to their influence, and in part by giving rise to a congenial nidus for the development of the cholera germ. Be this as it may, their action in encouraging an invasion of the disease, and in promoting its spread, cannot be doubted; and it is impossible, therefore, to praise too highly the precautions which have been taken to carry out proper sanitary arrangements, and to meet the disease at its very outset. But in many instances so much importance has been attached to these measures that their true bearing has been mistaken, and defective sanitary arrangements have been looked upon by many persons as the real cause of cholera. Especially has this been the case in regard to the water-supply, which has been held up, not only in the reports of the Registrar-General, but even in some of the medical journals, as the essential cause of the epidemic. So strongly has this notion taken hold of many members of our profession, that cases of cholera have been published as very remarkable and noteworthy because, forsooth, they have occurred in persons who have not tasted water. Doubtless foul water is a frequent source of disease, and observation has proved that it exerts a powerful influence on the spread of cholera. I am satisfied, however, that it does so not simply, as many persons suppose, by introducing into the stomach the excreta of cholera patients—for it has yet to be shown that the rice-water evacuations will produce cholera when swallowed,—but by proving a means of conveying into the system the *materies morbi* (sometimes probably derived from the excreta of cholera patients, but more commonly from the atmosphere), and at the same time by lowering the tone of the system, deranging the stomach and bowels, and so preparing the body for the reception and development of the cholera germ. But whether this view be adopted or not, it is vain to argue that water, however foul, is the essential cause of cholera. It is drunk in all its impurity when the epidemic is not present, and does not produce cholera; it is swallowed, tainted with choleraic excreta, when the epidemic is amongst us, and the disease ceases nevertheless after the lapse of three or four months—the usual limit of its duration *now*, as it was before the water was suspected, and at a time, therefore, when it was drunk freely up to the last. Doubtless water is *one* means of conveying the *materies morbi* into the system, but it is nothing more; and though the outcry which has been raised respecting our water-supply must benefit the community by tending to the cleansing of our rivers and to our obtaining purer water for domestic purposes, it will do little towards the discovery of the true source and nature of the cholera poison. Nay, more; the facts which have been discovered respecting the pernicious influence of foul water may even retard our progress if they are permitted to serve as the ground for stifling inquiry into the nature and source of the cholera poison. So, too, house-to-house visitation, the isolation of the sick, and other so-called preventive measures, however good and valuable in other ways, and no one can for a moment doubt their value, have little, if any, direct influence in checking the progress or duration of the disease. They may, and they probably do, diminish the mortality and mitigate the distress attendant on an epidemic, and they lead to the cleansing of many places, and the removal of many nuisances, which would otherwise remain as plague spots in the neighbourhood. But the decline of the disease, which is so often placed to the credit of the “stamping out” system, as pursued by the house-to-house visitors, is attributable to the natural declension of the disorder and not to human agency. The natural history of cholera shows that its duration in an epidemic form in any locality is limited to three or four months, and that the outbreaks which occur in the present day are not of shorter duration than they were when the disease first visited the country, and before the present “stamping out” system was commenced. Further, it is doubtful whether the intensity of the epidemic has been diminished by the measures adopted, though there is reason to believe that its mortality has been lessened by the precautions taken to treat the disease at its very outset.

In conclusion, I would beg that I may not be understood as

deprecating sanitary measures, nor as regarding the labours of house-to-house visitors as of little value, for no one recognises more fully than I do their great importance; but simply as wishing to direct attention to the true effect of such measures, and as urging the necessity of a strict investigation into the essential cause of cholera, which must be sought elsewhere than in water tainted with choleraic excreta and in the neglect of hygienic precautions.

Manchester-square, Dec. 1866.

ON FATTY DEGENERATION OF THE DIAPHRAGM.

By GEO. W. CALLENDER, F.R.C.S.,
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I AM not aware that any notice has yet been taken of this disease, or that it has been described in any systematic work on Medicine or on Pathological Anatomy. A few cases have been referred to* in which the diaphragm has become wasted in connexion with a decay of other muscles of the body, but these are widely different from those which I propose to relate, in which the muscle, for the most part in connexion with the heart, has been the seat of fatty degeneration, and, in some instances, by failure of its action has been the immediate cause of the patient's death.

These cases came under my observation some years ago—1855–58,—whilst I held the appointment of Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital, and consequently whilst it was my duty to make the medical post-mortem examinations. I publish them now because I am not infrequently hearing of cases of death from supposed fatty heart, but in which cases the earliest symptoms have been conspicuous for the remarkable disturbance and embarrassment of the breathing as distinguished from signs of failure of the cardiac action.

CASE 1.—My attention was first drawn to the occurrence of fatty degeneration of the diaphragm by the following circumstances:—J. M—, a female, aged fifty-nine years, married, was admitted into St. Bartholomew's Hospital on the 30th of December, 1855, under the care of Dr. Hue. There was nothing in her condition to indicate the speedy termination of her life, for she complained only, in addition to a slight bronchial inflammation, of being faint and ill, without being able to refer to any special symptoms. She was exceedingly fat, her breathing was shallow, and her respirations rapid. The bronchial affection was quickly recovered from, and she was on the eve of leaving the hospital, when, on the eleventh day after her admission, she was found by the night-nurse “struggling for breath,” and died almost immediately.

I examined her body eighteen hours after death. Although an abundance of adipose tissue existed wherever fat is usually present, the voluntary muscles retained a natural appearance. The heart and liver were far advanced in fatty degeneration, as also was the diaphragm, to an examination of which I was guided partly by the obscure circumstances attending her death and the story of the embarrassed breathing, and partly by the pale and mottled appearance of the muscle as seen through the covering of peritoneum. The other organs and the vessels of the body were natural.

CASE 2.—Some time later the following case came under my notice:—S. P. M—, a female, aged fifty years, single, was admitted into St. Bartholomew's Hospital, on January 1st, 1857, under the care of Dr. Hue, suffering from rheumatism of three weeks' duration, the present being her first attack. Inflammation of the pericardium, as well as of the endocardium, required especial treatment. She continued without any material change in the symptoms until the fourth day, when, at half-past two in the morning, she was seized with orthopnoea, with sudden and great depression as though from some severe shock, became livid, cold, and bedewed with a clammy perspiration. Within an hour she was almost pulse-

* By Cruveilhier, Trousseau, and others. In cases of progressive muscular atrophy the diaphragm is one of the last muscles to be attacked. In Cruveilhier's well-known case “all respiration was at the base of the chest, and was effected by the diaphragm and the abdominal walls.”—Archives Générales de Médecine, vol. i., 1853, p. 562.