

A short note on peritoneoclysis, hypodermoclysis, and vesicoclysis in cholera : a paper read before the Bombay Medical and Physical Society, February 6, 1885 / by Cameron Macdowall.

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A SHORT NOTE

ON

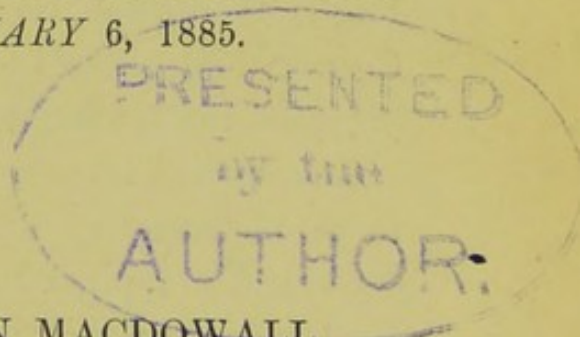
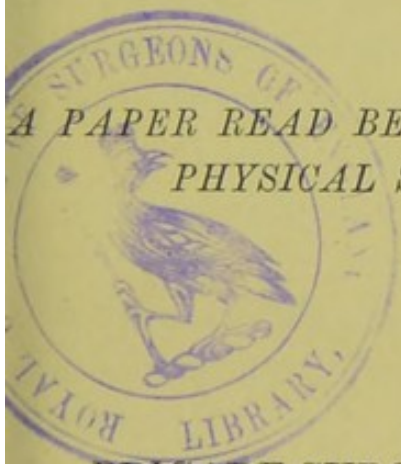
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PERITONEOCLYSIS,
HYPODERMOCLYSIS, AND
VESICOCLYSIS

IN

C H O L E R A .

A PAPER READ BEFORE THE BOMBAY MEDICAL AND
PHYSICAL SOCIETY, FEBRUARY 6, 1885.



BY

BRIGADE-SURGEON CAMERON MACDOWALL,
Bombay Army.

LONDON :

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1886

To
SIR JOSEPH FAYRER, K.C.S.I., ETC.

AND TO

DR. B. W. RICHARDSON, F.R.S., ETC.

GENTLEMEN,—

As you, Sir Joseph, were, I believe, the first to prove by experiment that one of the most characteristic appearances in cholera secretions can be produced by purely inorganic poisons, and as I consider that this fact should be steadily kept in mind, I venture to give it prominence (though not immediately connected with the scope of these short pages) by begging leave to make this dedication, without, at this distance, being able to ask permission, to him who first made that fact known.

I also beg to dedicate this little pamphlet to you, Dr. Richardson, as you were, it now appears, the first to practise Peritoneoclysis, etc. (and that successfully), for cholera, in England, although the fact was unknown to Drs. Purvis and Cooney and myself, when we severally and independently proposed the same treatment in the *Lancet*.

The hurried manner in which I have been obliged to write of these methods of treatment, some of which have been so successfully applied in Italy, by the distinguished physician, Dr. Cantani, on the suggestion originally made, it would seem, by the illustrious Paccini, is owing to the desirability of quickly bringing them into general notice. This must be my excuse for begging you to accept the dedication of so imperfect a performance. Two such names may attract such attention to it as may be useful to suffering humanity.

I beg to remain, Gentlemen,

Yours, etc.,

BOMBAY,

C. M.

January 29th, 1886.

ON NEW DEPARTURES IN THE THERAPEUTICS OF CHOLERA, WITH A SKETCH OF A VISIT TO TOULON AND MARSEILLES DURING THE EPIDEMIC.

I.

AT the request of the President of the Medical and Physical Society of Bombay, viz., that I should give a sketch of my visit to Toulon and Marseilles in July and beginning of August last, 1884, and with the permission of the Surgeon-General, I have put together the following notes :

At the end of 1883, the *Lancet* had published, October 13th,* a proposition of mine to use large injections of warm water, plain or *medicated*, or milk, etc., either subcutaneously (after a plan suggested long ago by a Bengal surgeon, whose name I cannot ascertain) or into the *peritoneum*, as a perfectly easy and safe means† of reviving the circulation and the secretions, and prolonging life, in the collapse of cholera, until the poison of the disease should be eliminated or (if a living colony of germs) die out, as Dr. Koch maintains, it, after a short time, *does*. This last operation, 'Intra-peritoneal injection,' I thought I

* *Vide* Appendix.

† In contradistinction to venous transfusion, which requires a complicated and often unsafe apparatus never at hand! Here all that is needed is a hollow needle (one of Dieulafoy's aspirators will do), sharp or blunt, a piece of india-rubber tubing to connect it with a large common pewter syringe, or even a common dispensary funnel. (See Plate, Fig. 1.) A hospital apprentice can perform the operation easily and without danger.

was the first to suggest. It appears, however, that Dr. Benjamin Ward Richardson had tried it with success in a single case of cholera, the last he saw, as far back as 1854. I immediately wrote to the *Lancet* acquiescing in Dr. Richardson's statements, and congratulating myself on having hit on the same idea as himself, and on one that had proved successful in his hands, although he had only an opportunity of trying it once. I may remark that I was very kindly received by Dr. Richardson when at home, and he showed me the hollow probe which he had used. He had also experimented, not in cholera cases, however, on causing water to be absorbed by injecting it into the cellular tissues, and into the urinary bladder. This last he had found less effectual as a means for causing the absorption of water. It was lately recommended in the *Lancet* by Drs. Purvis and Cooney, and Dr. Purvis also, it appears, thought of peritoneoclysis independently. (*See Appendix.*)

Intra-peritoneal injections of large quantities of *defibrinated blood** have been employed successfully in France, Italy, and Germany as a remedy in pernicious anæmia by Dubar and Remy, Mosler, Golgi, Bizzozero, etc., etc.,† and it was seeing this fact recorded in the French and German journals, and afterwards in the *Lancet*, that made me think of it for cholera. It should be remarked that only one case of its causing fatal inflammation on being too soon repeated is on record. I confess, however, the simpler and safer plan of injecting large quantities of warm medicated or plain water, milk, defibrinated blood, etc., into the cellular tissues of the limbs, thus causing an *artificial anasarca*,

* 400 grammes = x oz. ; also albumen in solution 'well borne and inoffensive and absorbed.' Milk should be tried (?).

† Who also injected the spleen, see *Revue de Medecine*, 10th June, 1881, quoting *Deutsches Archiv* ; also Hamond, Kusmaul, Leyden, Kaczorowski of Posen, *Deutsches Med. Wochenschrift*, 1880.

which is steadily absorbed (I have tried it and found it act admirably), to be a more manageable remedy in disease than producing an artificial dropsy by injecting into the peritoneum; although Dr. Richardson assured me that the fluid seemed to be absorbed very rapidly, and the patient scarcely knew what he (the operator) was doing to him. I lay on the table the hollow needles used for the purpose. That for injecting or *transfusing* into the peritoneum slightly differs from that used by Dr. Richardson (who handed to me what he had at hand), in being blunt at the end, where there is an eye on the side like a catheter, instead of being open at the *end*. It is pushed through or into the peritoneum at the bottom of a small lancet incision on the *linea alba*, according to Dr. Richardson's directions given verbally to me. The other and larger hollow needle (for convenience it fits over the other,) can be used alone for injecting the cellular tissue. It is simply to be thrust under the pinched-up skin on the outside of the limb selected. I injected all four limbs in my case (1874), and the man apparently quite recovered. The voice, the urine, and even the appetite returned, as also the colour of the dejecta. He, however, was a man who had long suffered from cardiac dyspnœa, amounting to asthma; his face had been for years of a slightly cyanosed appearance, and I noticed that the breathing still remained embarrassed. He died on the second day. Other remedies were not neglected, but the case was not a favourable one for judging of the efficacy of the remedy owing to pre-existing disease. Had we thought of injecting the peritoneum, which is the *last* resource, the case might have ended differently, especially if we had used nutritive fluids. It seems that this injecting of the cellular tissue with large quantities of water (not over one-fifth of the weight of

the body, however—say twenty pints—*Richardson*) is being used now with success in Italy under the name of *hypodermoclysis*, and that it was first recommended by the great Italian physician and anatomist, Paccini, the discoverer of the tactile (paccinian) bodies, the first describer and delineator of the virgulated or comma bacillus, spirillum or vibrio, which he called ‘*Microbio cholorigeno*,’ which Bristowe and Carter (*Lancet*) saw and described also independently, and which Dr. Koch hopes to have *proved* to be what Paccini *said* it was—the cause of cholera,* in consequence of its alleged pathognomic appearance under *cultivation*,† viz., the ‘*Luft-blase*,’ or air-space or bag it forms above, as it (the colony) sinks in the gelatine, etc., etc., of a test-tube, or the ‘*hohl raum*’ *vacuole*, which it forms round itself in a gelatine disc on a glass slide, ‘as if it had the power of evaporating as well as liquefying the gelatine’ (Koch). May I be now permitted to remark (although this is not the subject of the paper) that it seems to me rather premature to assert that other *bent* or ‘*comma*’ bacilli, found elsewhere than in cholera, as in the Mouth (*Lewis*) in Chol. Nostras (*Finkler* and *Prior*), are identical with the one described by Paccini, Hassell, Bristowe, Carter (spirillum-joints), Koch, etc., until they have been observed *under cultivation*?‡ But

* See *Lancet*, August 2, 1884; Paccini’s words are:—‘Un organismo semplicissimo e di estrema tenuita che io appellero Microbio-Cholorigeno.’ In his whole lecture, which has been published, he describes it as ‘bent, and even sometimes like the letter s.’

† Cultivation and attenuation of bacteriae seem both to have been invented by Pasteur.

‡ All that Professor Koch condescends to say in the last *Deutsche Med. Wochenschrift*, 6th November, just come to hand, is as follows:—‘Had Lewis taken the very slight trouble to introduce the bacillus-holding saliva into nourishing gelatine, then would he have immediately known that his comma bacilli, in a neutral or weakly alkaline flesh-water (meat infusion), peptone gelatine, *generally do not grow*, whereas

as the bacillus or 'plasmode' of ague is certainly not contagious (though possibly inoculable), it seems that, according to overwhelming evidence, the cholera bacillus is not contagious either. (*See II.*)

However that may be, it is clear that the idea of subcutaneous injection of quantities of fluid recommended by a Bengal surgeon and tried by me, or mine of injecting it into the peritoneum, instead of only being seemingly rather eccentric propositions, are indeed the inventions of such distinguished men as the great Italian Paccini, and the well-known hygienist and physician, Dr. B. W. Richardson. I for one congratulate myself on having thought of one of these methods as a remedy for cholera, before I had ever heard of Paccini or Richardson having done so. Being therefore persuaded that Toulon or Marseilles would be a good place, if not too late, to try and introduce the treatments, I started for these places during my late leave of absence in Europe. I should here state that after I returned thence I found a copy of a letter from the British Ambassador in Paris, stating that he had recommended me to the good offices of the French authorities. It was forwarded to me by the India Office, which had kindly interested itself in my self-imposed trip.

I now return to my visit to Toulon and Marseilles.

On my arrival I found two distinct parties in contention as far as the propagation of cholera was con-

the cholera bacilli without exception develop therein.' As yet this scarcely disposes of Dr. Lewis's facts.

With regard to Finkler and Prior's bacillus, its behaviour in gelatine seems to differ from Koch's. So also, apparently more or less, do a host of others: Flugge's *cheese* bacillus. Lewis', and the bacillus found in Indian tanks (!).

cerned, if not so pronounced with regard to other points. The surgeons of the Navy almost all seemed to be of opinion that cholera exists normally in Europe, especially the South, under the attenuated form of *Cholera Nostras*, *Cholerine*, etc. (which they do not confound, as the public do, with the bilious cholera of autumn). They maintain, and the civilian practitioners allow, that cases of severe diarrhœa and vomiting, with no pronounced signs of bile, and which are occasionally accompanied by cramps and a certain amount of collapse, are of annual occurrence. But they maintain that these cases, which are rarely fatal, are not cholera. This last the naval surgeons seem to doubt, as also do some distinguished authorities in Paris, etc. The Navy say that the transport *Sarthe* had been cleaned out, *even to her ballast*, before leaving Saigon, and that she was free from cholera for more than forty days. The civil branch of the profession smile and ask if the admirals and commodores, etc., of the Fleet were likely to imperil the services of the military and naval reliefs, and derange the clockwork on which depends the safety of an empire, by putting back the ships, or publishing questionable cases of disease, or allowing doubtful diagnoses to be entered otherwise than '*confidentially*.' The Navy retort that the first cases appeared amongst the *peasantry*, and that the Navy was comparatively free, that England, in constant communication with India and Egypt, remained free, etc., etc., etc., till the controversy becomes a mass of indigestible statements and undigested facts.

As a mere general practitioner, *I*, of course, am not qualified to give an opinion one way or another, and my visit not being official, I could get no information *officially*. But I have great difficulty in believing that cholera is really endemic anywhere but in the East.

Nor is it even *possible*, I believe, to fix the origin of the first case of recognised *cholera* in the South of France. I myself confess that I hold my judgment in suspense. I think it is generally admitted that contagion is not proved yet, though the disease seems to stick to places, to bodies of men who have been subjected there to some as yet unknown influence, and that these bodies of men should be moved about to get rid of that influence. But we have yet to learn whether these admissions be absolutely true.

To show what novelties await us, Professor Koch asserts, and we have no reason whatever to doubt him, that drying *kills* the comma bacillus in about two hours, and that disinfectants do *not* kill, but only arrest putrefactive decomposition. Moreover, that as the '*Fäulniss bacterien*,' or bacteria of decomposition, appear simultaneously as the comma bacilli disappear, it is assumable, or to use Professor Koch's own words, 'I might assume that if one brings comma bacilli into a very foul solution containing many other bacteria, and especially putrefactive bacteria, all products of decomposition (*stoff wechsel producten*) they do not develop, but *soon die*.' He allows that further research is desirable, but hints that disinfectants which simply arrest putrefaction (and such he declares all those he has tested in practically useful strengths) are worse than useless. Disinfections of railway carriages and stations, etc., were soon given up, save, erroneously perhaps, as deodorizers. (*Deutsche Med. Wochenschrift*, 7th August, 1884.) In the same paper (the week after, August 11th) it is stated also that in pure water ('*reinem wasser*') the comma bacilli die in twenty-four hours—not so in soup, milk, etc., so that not only moisture but a proper *pabulum* will revive and preserve them. This is consoling for water-drinkers ; but, *per*

contra, I fear it does not hold good for *typhoid* germs.

For a short time in Toulon watering the roads was given up; but as the dust which ensued proved quite unbearable, it was resumed. I will finish this part of a long rambling paper by stating that corrosive sublimate, 1 part in 100·000, was found to *check* the growth* of the bacilli (Koch). It would be interesting to know if stronger solutions do not *kill* them. As early as 1863-64, I injected subcutaneously three cases, if I remember rightly, with $\frac{1}{12}$ th of a grain Hydr. Perchlor. in m. xxx water. One case died; the others recovered, to the best of my recollection. The records were lost during the Afghan War. It appears now (*Annuaire Thérapeutique*) that $\frac{1}{3}$ th of a grain can be injected for two or three or more days with perfect safety in syphilis. I certainly did not inject enough to be of use. It is clear that in a pint of warm water (which is what I injected into each limb under the cellular tissue) $\frac{1}{3}$ th of a grain could be injected without any difficulty or danger whatever, or else it could be thrown into the *peritoneum*.† I would also suggest milk being used in the latter case especially. I do not know how milk would be borne in the cellular tissues,

* Check, not *kill* apparently. But if we revived the secretions the checked bacilli would be evacuated.

† But as the bacilli are not found in the blood or peritoneum, but in the stomach and intestines (when gastric juice and bile are *deficient*—hence the old indication for calomel?) it would be advisable to give the patient perchloride solution (weak) by mouth frequently, and by lavements, or even to inject by a fine needle into the intestine itself, while sustaining the circulation by large injections of warm fluid under the skin, etc., at the same time. With regard to bile destroying the bacilli, Professors Nicati and Rietsch tied the bile duct in dogs, and injected the bacilli directly into the duodenum. The dogs are said to have died of cholera. Guinea-pigs died without tying the duct. But less formidable operations will kill dogs, and especially guinea-pigs.

but in the peritoneum it is borne well, also defibrinated blood, etc. Now as the stomach and intestines in cholera are no longer capable of *absorbing* anything almost, but are really become *secreting* and excreting organs only, I regard the injection of warm water or milk with about one grain of common neutral table salt to each one ounce,* whether medicated or not, into the cellular tissue (?) or the peritoneum, as the most rational future resource in cholera. In *anæmia* the injected milk-corpuscles have been found to nourish or perhaps replenish and replace the deficient blood-corpuscles, and in cholera these corpuscles are partly necrosed, so that their replacement by others as the supplying their immediate want of nourishment is equally obvious; and the stomach and intestines cannot do this. It was with such ideas that I took the trouble of going to Toulon and Marseilles, and here on the very threshold of a series of perhaps fruitful experiments I fear that, like the traditional 'Knife-grinder' of Mr. Canning, I have 'no story to tell.'

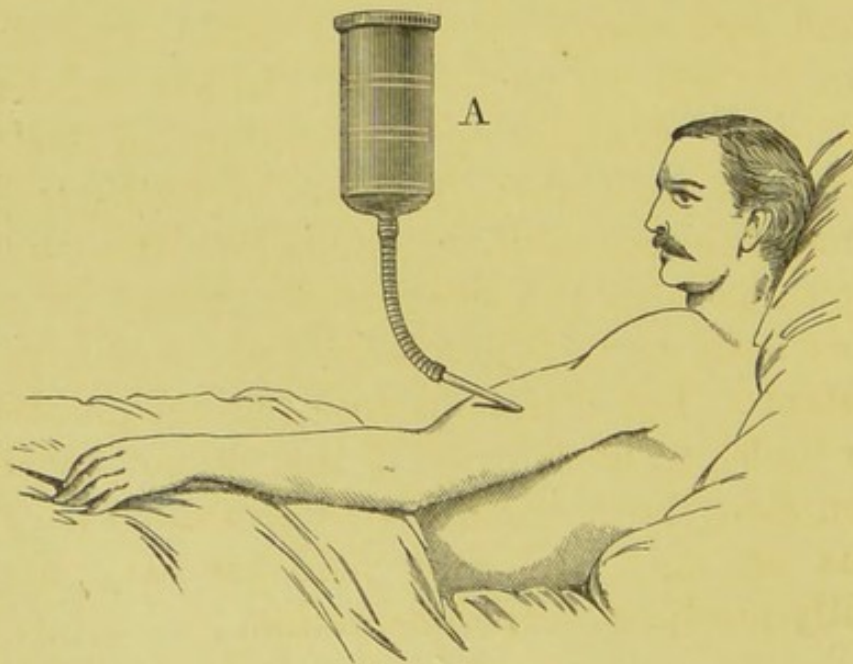
In spite of the amiable *empressement* of the medical men to find a suitable case for trial in the hospitals, I found none in the state of collapse during my visits. The epidemic was, in fact, approaching its end, and what cases were brought to hospital were already in a state of reaction; and those in the hospitals were in the state of consecutive fever or else convalescent. When the epidemic burst out at Naples, my leave in Europe was just about to expire. I had put off my

* Dr. Kronecker, in the *Deutsche Med. Wochenschrift* shows that this greatly adds to the safety of large warm-water injections. (*Deut. Med. Wochenschrift*, Aug. 7th, 1884.) He found that any alkali, as Soda Carb., etc., was highly dangerous. But with weak kitchen or table salt (*kochsatz*) he revived dogs that had been bled and deprived of two-thirds and even one-half of their blood. Stronger or weaker solutions than one or two grains to one ounce of fluid will not do.

departure for Toulon whilst waiting for an answer from Government as to whether they were inclined to *send* a Medical Commissioner. After waiting for some time I started on my own accord, and though, as I have said, introductions were most kindly sent me by the Government, the real opportunity was lost—I was too late. What cases continued to occur were in the suburbs, often, nay generally, in the middle of the night, and were thus unavailable to me, or when brought to hospital were in the reaction stage.

I venture to suggest, in conclusion, that two hollow needles like those which I have shown, one blunt and one sharp, together with a piece of india rubber tubing to connect it with a common large syringe without its piston, or else a funnel, be supplied to hospitals.

Fig. 1.

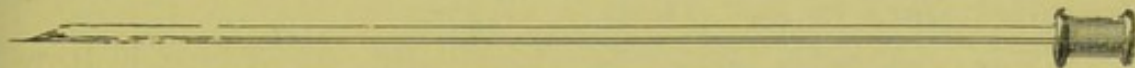


A is a common hospital pewter enema syringe without its piston and with cover removed.

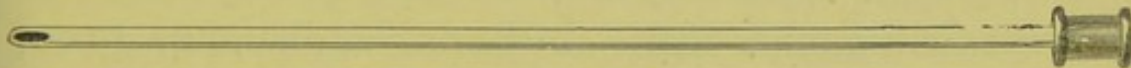
If the fluid flows too slowly by gravity alone the piston can be used, but very gently. It is clear that a common funnel can be used the same way as the syringe, thus, Fig. 2, and the fluid poured in; but it flows very slowly, and patience or the syringe and piston will be required. For injecting into the peritoneum, the blunt needle, or rather canula, is safest after a small lancet incision into the *linea alba*. A catheter would do.



Fig. 2.



Hollow Needle No. 1, sharp pointed.



Hollow Needle No. 2, blunt at end, with an 'eye' on side, as chosen by me at Krohne & Co., Duke Street, Manchester Square, London.

Cost of both, fitting over one another, 12 shillings.

Injection into the veins can NEVER be tried on a large scale, and not more than 6 to 9 cubic centimetres of fluid can be injected per second, without embarrassing and seriously diminishing the action of the heart. (*Kronecker*, as cited above.) This of itself renders all our former experience of injections into the veins very suspicious, as also the dangerous mixture of alkalies that has been used. On a large scale accidents would be constant. But injections into the cellular tissue of all four limbs, to be *followed*, if that do not succeed, by injection into the *peritoneum* of warm nutritive fluids,* offer us an easy and vast field for very easy and eminently *practicable* experimentation.

* In the following order of preference:—1. Defibrinated blood, oz. x. 2. Solution of white of egg, perhaps milk. 3. Plain water, never forgetting Professor *Kronecker's sine qua non* of gr. i. to ii. of common salt to each oz.

II.

As I have already stated in a note, the experiments of *Pietsch* and *Nicati* were such formidable surgical operations in themselves, and so likely to cause septicæmia, that even they are not conclusive, especially in the case of guinea-pigs. I think it was Prof. Syme who, seeing a guinea-pig *die before* chloroform was administered and the vivisection begun, said laughingly to Prof. Bennett, the experimenting physiologist:— ‘The sight of our knives has been too much for it; it’s dead of fright.’ Indeed, mere rough *handling* has been known to kill guinea-pigs. In Berlin laudanum is given them also, before operating, to favour the absorption of the bacilli. The alcohol alone is enough to kill them almost, if not the opium (as they are *graminivora*).

I should here say something of the researches of Professor Emmerich (of Munich) in Naples. In perfectly sterilised receptacles (tubes) he obtained blood directly from moribund cholera patients, and *in it* discovered very small, straight bacilli with rounded ends. After death he found the same bacilli in almost *all* the internal organs, especially in the kidneys. The careful criticisms on all this, in the Berlin Medical journals, really add some weight to the discovery, such as it is.

I must here prominently call attention to the fact that all pathologists now agree that Vandyke Carter of Bombay was right when he long ago declared the *comma bacillus* to be the result of fission of some spirillum, and probably a mere phase or chapter of its life-history.

It has not been my purpose throughout this short paper to attempt anything like a history of the biology and morphology of comma-shaped bacilli; but as *peritoneoclysis*, *hypodermoclysis*, *vesicoclysis*, are all more or less based on what, in the language of practitioners of

medicine (and of statesmen also, as the acts of political parties abundantly show) is called the 'treatment of symptoms,' I am obliged to take some notice of bacteria while advocating such treatment of a disease alleged to be microbial.

But although a treatment of symptoms may not always aim at the immediate direct removal of the cause, it does so indirectly by aiding nature, and is by no means incompatible with the idea of a perishable microbial origin of cholera. Microbicide drugs also can be combined with the water of the injection.

But besides the fact of the disease not being apparently contagious from man to man, *i.e.*, by handling the patient, touching his tongue, pulse, etc., as all doctors do, there are other reasons for not being quite sure yet that Professor Koch's bacillus is the cause of cholera. In Ferran's so-called attenuated cultivations used for inoculation, there were seen commas, both simple and studded all over with granulations or buds, spirilli, cocci, and every form more or less described by Babès, Cornet, Petronel, Van Ermengem, etc.; and yet we are told that we know these cultivations, on which no efficient systematic Pasteurian attenuation had been apparently practised (save the addition of bile!). We are told, and we know, that these inoculations were generally negative: not always so, but sufficiently often to show that they were neither preventive nor infective. But now we are told that these buddings or 'granulations,' 'muriform' bodies, etc., which metamorphose the appearance of the comma bacillus and spirilli (from which latter, indeed, the commas arise)—we are told that these appearances are forms of *involution* or degeneration, and that they are *sterile* and not spores, nor 'pollenodes' nor 'gonidia,' etc. (*see* Doyen's admirable paper in the *Progrès Médical*, 4th July, 1885); that only the commas are contagious, etc.

But, as I have already stated, commas and spirilli, as well as granulations and muriform bodies, were found in Ferran's inoculating fluids. Moreover, a most competent observer, *Hueppe*, in the *Deutsche Medicinische Wochenschrift* of 29th October, 1885, comes forward now with the assertion that there is a persistent form, 'Dauerform,' of the comma bacillus (not a degeneration at all), which he calls arthrospores, and which are capable of leading in a new generation of commas, being, in fact, a true prelude to fructification (*Echten Fructifications Vorgang*). These spores, *Hueppe* says, resist destruction by drying, etc., for four weeks without being vital to the same degree as *endospores*. Now how could the mixed liquids of Ferran, where Van Ermengem and Gibier saw all these appearances without any evidence of true attenuation—how could these liquids have failed to produce cholera when inoculated, if the comma bacillus or any of its cultivations were contagious? It has been contended that it cannot propagate itself in the cellular tissue. But this seems a mere subterfuge. But whether the wondrous, painstaking investigations of Professor Koch, carried out at the imminent risk of his own life, have resulted in finding the true *microbe* of cholera, I am too great an admirer of his genius, and too little qualified, to dare give an opinion. But that is not the question. One thing we *do* know, and that is that a disease caused by an undoubted bacillus or *microbe* may be utterly non-contagious, though perhaps inoculable, viz., ague. No one in their senses since the late beautiful revelation of *Marchiafava* and *Celli*, to say nothing of *Klebs*, *Tomasi-Crudeli*, and *Laveran*, can for a moment deny the microbial origin of malarious fever; yet it is not contagious. Now if cholera is a microbial disease (and probably it is), overwhelming evidence goes to prove

that it is not contagious. Unless we perhaps swallow the dejecta from the patient, we can scarcely by any possibility get it *from him*. We may rub the patient and touch his tongue: we *don't get cholera* from touching *him*. And it is a most fearful injustice to the sick to spread the idea that we run any risk from touching the patient, instead of the locality, the water, excreta, etc. We then neglect to attend to either the one or the other, and the local causes increase, viz., foul earth, foul water, foul food, foul air, etc. The patient is abandoned, and treatment is looked upon as useless, or dangerous to the attendants. I contribute my mite to prevent these fatal errors.

It may be that I am rash in assenting too easily to the opinion generally entertained against the attenuation of the cholera virus by Dr. Ferran throughout the scientific world; but this bold and talented experimenter has only himself to blame, by not revealing the methods which he employs. It is different when we come to the teachings of the illustrious Professor Koch; and it was with the utmost diffidence that I ventured long ago to write to a medical periodical,* pointing out the discrepancy between cholera epidemics almost always appearing in the hottest and driest seasons, and the fact so admirably demonstrated by Professor Koch that the *comma bacillus* rapidly dies by drying at ordinary summer temperatures. I begged, in fact, to cancel my letter through the fear of being prematurely rash. But the veteran and renowned Pettenkoffer has since unhesitatingly asserted this discrepancy, and my experience of many epidemics during twenty-two years in India (and elsewhere) is that they generally arose in the hottest and driest months, and that not, as might be supposed, after occasional showers. I believe this will

* *The Lancet*.

be found to be statistically a fact. Indeed, rain often, though not always so, puts an end to the visitation. But I myself have never seen cholera begin during the rains. Deceptive as statistics generally are, I believe they have settled or can settle this question. If then Professor Koch's *comma bacillus* be really *sui generis*, and behave specifically in cultivation (which by Dr. Klein seems lately to be contested), its unquestioned and rapid death after exposure to a summer temperature shows that it may not, after all, be the cause of cholera. But I venture to suggest that it also settles one question which even at the Berlin Cholera Conference seemed to remain undecided, viz., Can this bacillus infect the system by being inhaled? Professor Koch, I maintain, has proved to us that it cannot, since it dries and dies in an ordinary summer atmosphere. Indeed, it is evident that it cannot be 'blown about' and inhaled until it is first *dried* and *ergo dead*. This I maintain is a logical deduction, and is at all events consoling (if this bacillus be a cholera bacillus); and with the formulation of such deduction I hasten to conclude my rather disconnected notes.

APPENDIX.

INTRA-PERITONEAL INJECTIONS IN CHOLERA.

To the Editor of 'The Lancet' (Oct. 13th, 1883).

SIR,—The great importance of this subject must be my excuse for troubling you; but I will endeavour to be as brief as possible.

Hypodermic injections of drugs, though the best way of administering them in cholera, not having proved a cure for the grave Asiatic forms of that disease, it is evidently our duty to look for some other weapon to combat the malady when these fail. They have been largely tried. I believe I was the first to do so in Poona, Bombay Presidency, in 1863-66 (*vide* 'Deputy Inspector-General Murray's Résumé of the Treatment of Cholera from all Sources,' published by the Bengal Government). I tried, of course, morphia, atropia, Condy's fluid, solution of bichloride of mercury, chloral, and subsequently ammonia, bromide, etc., with (as shown in a letter published by you on October 16th, 1880) very varying success. In that letter I mentioned that I injected large quantities of warm water subcutaneously into the cellular tissue of the arms and legs in order to procure slow absorption of fluid, since rapid absorption by intravenous injection of fluids had failed ever since the days of Hamilton in Edinburgh, when it was exhaustively tried both with plain water and saline mixtures. The plan of injecting fluid into the cellular tissue, and thereby inducing an artificial dropsy, was no invention of mine, but of a Bengal surgeon, whose name I unfortunately cannot recall, and who based his theory of success on the fact of dropsical patients having been said to recover both from dropsy and cholera when attacked by the latter disease. Mine was the first case in which the plan was tried, I believe; and although the patient had long been a sufferer from dyspnoea, chronic bronchitis, and embarrassed heart, we believed that the almost miraculous resurrection which took place would be permanent. He died, however, on the second day, like the cases so graphically

described in Edinburgh by Begbie. This case was, moreover, a most unfavourable one. The man had had the 'asthmatic voice' for years, and was always slightly cyanotic. Whether the injection and slow absorption of fluid into the treacle-like cholera blood (which itself is incompatible with life) will, by giving time for the cholera poison to be eliminated, ever prove a cure, in contradistinction to the failure of rapid intravenous injection and absorption of fluid, is a matter yet to be decided. But I regret that a perhaps natural timidity made me refrain (this was the sixth epidemic I had witnessed—namely, in 1876) from employing what now appears to me to be likely to prove a much more effective weapon—that is, injection of warm water, milk, etc., or nutritive or medicated solutions, into the peritoneum.

Bizzozero and Golgi declare that the injection of defibrinated blood even is in general devoid of danger. Professor Mosler had a fatal case on repeating the injection after twelve days, but attributes it to the too early repetition of the operation.* Dr. Kaczorowski of Posen has had four absolute cures from debility and anæmia by injecting about 400 grammes of defibrinated blood (repeated in one case). All appear to have been absolutely confined to bed before the operation restored them to perfect health.† Dubar and Remy have shown that albumen in solution is absorbed by the peritoneum, and that it is well borne and inoffensive. It is evident that the operation, especially with simple water which has been well boiled and allowed to cool to a comfortable temperature, is quite feasible, and holds out some prospect of success.

In the case where I injected the cellular tissue of the four limbs, I simply used the medium-sized needle of a Dieulafoy's aspirator, previously attached by a piece of india-rubber tubing to a large hospital pewter enema syringe. For the peritoneum it would probably be advisable to divide the integuments with a lancet on the linea alba, and to take out the piston before thrusting in the hollow needle, so that the flow of water through it, the full syringe being held at a higher level, would to some extent push aside the intestines and prevent injury to them.

Surely if this operation has been judged advisable in chronic cases of debility in a malady like cholera, in which no time is to be lost, and when, quite apart from true cholera poison, the want of fluid in the blood stops the circulation, secretion, etc., and can kill *per se*—surely in this malady such a procedure is eminently called for, even if only after subcutaneous injection of

* *Deutsches Archiv*, 25th April, 1883.

† *Deut. Med. Woch.*, 1880.

large quantities of water into the cellular tissue of the four limbs. It infallibly brings the patient out of the collapse, and, acting gradually, may keep it away till the poison be eliminated or exhausted.

I am, Sir, yours truly,
CAMERON MACDOWALL,
Brigade-Surgeon I.M.D., Principal Medical Officer,
Sept., 1883. Quetta, Afghan Frontier.

TREATMENT OF CHOLERA.

To the Editor of 'The Lancet' (Feb. 2nd, 1883).

SIR,—In justice to the Indian Military Department, to which I belong, and which, with the Home Medical Department of the Army, has (especially in Egypt lately) had to bear more than the usual allowance of unmerited official and unofficial, responsible and irresponsible obloquy, and which is apt to be looked upon, even by the medical press, as a mere routine-work branch of the profession, unsolicitous of medical research, I beg respectfully the favour of your publishing the following. When I recommended intra-peritoneal injections in cholera, in the *Lancet* of Oct. 13th, before Dr. Purvis or Dr. Cooney wrote to you a week later courteously alluding to my prior proposition and recommending another procedure (vesical injection), neither they nor I were aware, I am sure, that Dr. B. W. Richardson, amongst his many admirable experiments, had tried their proposal of vesical injection, or *mine* (*Lancet*, Oct. 13th, 1883), of intra-peritoneal injections in cholera. Indeed, Dr. Cooney makes no allusion to intra-peritoneal injections, and Dr. Purvis did not think it would be wise to try them, save at first on the lower animals. I proposed this proceeding for cholera, and in utter ignorance of Dr. Richardson's work, relying on the following authorities, who had successfully used it—viz., 1st, Professor Mosler, Messrs. Bizzozero and Golgi,* who have also injected the spleen, as have also Hamond, Kusmaul, Leyden, etc., and once fatally. 2nd, Dr. Kaczorowski of Posen.† 3rd, Dubar and Rémy, whose work was carefully brought before the British public in the *Lancet* itself. Had I known, as stated in your article, in which you make no mention of me, but, by a pardonable mistake, attribute the revived proposition to others, that Dr. Richardson had

* *Revue de Médecine*, June 10th, 1881, quoting *Deutsches Archiv*.

† *Deutsches Med. Wochensch.*, 1880, quoted in *La Thérapeutique Contemporaine*.

experimented in the matter, I would most certainly have quoted so eminent an authority, and I humbly apologize for my ignorance. But most indubitably this idea of injecting the peritoneum for cholera occurred to me independently. I am glad to hear that Dr. Richardson's case recovered, and I believe the future successful treatment of cholera lies in that direction. The ingenious suggestion of Drs. Purvis and Cooney (which it appears was also originally Dr. Richardson's) of injecting the bladder, should of course be tried first; but I confess I have much less hope of its success. But, in Heaven's name, let something of the sort be tried. All drugs (save a timely morphia injection perhaps) during a long experience have appeared to prove almost useless, including chloral, the carbolates, etc., *ad nauseam*, figuratively and physically. I hope Dr. Richardson will again take the matter up, since it appears to be his own.

I am, Sir, yours obediently,

CAMERON MACDOWALL,

Bombay Army.

QUETTA, Dec. 14th, 1883.

INTRA-PERITONEAL INJECTIONS IN CHOLERA.

To the Editor of 'The Lancet' (Feb. 9th, 1884).

SIR,—I crave a small space in your next issue, and with your kind indulgence hope to show Dr. Macdowall that my idea of injecting fluids into the peritoneal cavity was not taken from his communication published in the *Lancet* of Oct. 13th, as he implies in his letter published in your issue of to-day, my letter having been sent in for publication on Oct. 9th—*i.e.*, a few days before his letter appeared in print. I was entirely ignorant of the employment of intra-peritoneal injections in any disease whatever at the time; and Dr. Macdowall's communication was the first I saw bearing on the subject, and it was with very great pleasure that I read his able advocacy of the above-mentioned method of treatment in Asiatic cholera.

My motive for suggesting such a plan of treatment was founded on the physiological fact that in the peritoneal cavity we have a ready means of communication with the great intra-abdominal lymphatics, and through the latter with the general circulation—a fact of no small importance in the collapse stage of cholera, when we desire to influence the circulation speedily, and act perhaps more directly on the intestinal canal than we can do by the mouth or by intravenous and hypodermic injections.

One other point, which I must notice as briefly as possible. Dr. Macdowall thinks that I had some hesitation about the safety of this method of treatment. I may say I had no doubt as to its safety; or, more correctly, I considered it as safe an operation as any could be where the peritoneum was involved, provided always that we use non-irritant injections—*e.g.*, pure ascitic fluid, blood-serum, or pure water at a suitable temperature. It was with reference to the exhibition of the remedies (in solution) by way of the peritoneal cavity that I suggested experiments on the lower mammalia, in order to test their safety in such a mode of administration; for it is only by such experiments that we can learn the tolerance by the peritoneum of remedies injected into its cavity—a subject which, as far as I am aware, has not been investigated as yet, and which I hope to work at the first opportunity I have.

I am, Sir, your obedient servant,
GEO. CARRINGTON PURVIS, M.B. Edin.

LONDON, *Feb. 2nd*, 1884.

HYPODERMIC INJECTIONS OF MORPHIA IN CHOLERA.

To the Editor of 'The Lancet' (Oct. 16th, 1880).

SIR,—In justice to the whole medical profession in India, I hasten to make public what I know of our experience of the above-named treatment in Asiatic cholera, as Mr. Hardman in his exceedingly interesting communication to your columns of the 2nd inst. seems to desire. I believe I was the first to try so obvious a mode of treatment in 1865, with the *post hoc* result that six cases out of fourteen got well. This you were good enough to publish in the *Lancet* in 1872, and was first made known in Deputy-Surgeon-General Murray's compendious Report on Cholera. The average success is scarcely above the usual one in severe epidemics. These cases, also, were all of very severe type. But most often deaths were, as usual, in the secondary fever stage. How far this was the result of the treatment is uncertain, for fatal *coma-vigil* is a common ending in the malignant disease. Reaction took place in more than half the fatal cases, and I have always found the morphia injection favour it. But it is of much more interest to inquire whether the cases cited by Mr. Hardman with so much careful accuracy were cases of choleraic diarrhœa or rather (for the term

is now abolished in official reports) of the diarrhœa stages of true cholera. For if 'choleraic diarrhœa' mean simply a diarrhœa bearing a certain *resemblance* to cholera, and that is the most likely to be the case, morphia is the remedy which Mr. Hardman paints it to be. Still, although it is almost certain that diarrhœa simply *resembling* cholera cannot become such, nevertheless morphia (of course, hypodermically), by diminishing exhaustive discharges and obviating blood-thickening, and perhaps as a stimulant, etc., practically, and combined with other remedies, is found to be one of our chief resources. It has been used, abandoned, and returned to over and over again. But it is not an antidote to the cholera poison (or *force*, as some thinkers have it, in the sense that heat and electricity are forces). Mr. Hardman well and truly describes the features of diarrhœa resembling cholera, but unconsciousness is noted in the collapsed stage of only three out of the thirteen. Again, cramps were only noted in five cases. Cramps are also seen in bilious diarrhœa, etc., and collapse (mentioned in three of the cases) in many diseases. Rice-water stools, which are quite *sui generis*,* are only mentioned in one case. The purging in three others is either 'watery and colourless' (this is quite different from rice-water) or 'yellowish'; the other dejecta apparently were merely loose, 'violent purging,' etc. None of the cases presented the peculiar burning anguish over the præcordia, nor the suppression of urine, so universal in cholera. But whether the disease were cholera (at least in some of the cases) or not, the treatment does Mr. Hardman great credit. The reason why it is not so successful in malignant cholera is that it is not alone 'the purging that kills,' I fear; I say it with the utmost deference. It is not, at all events, the mere loss of water and salts that kills, for intravenous injections have been extensively tried since Mr. Hamilton's experiments in Edinburgh in 1833. I injected large quantities of warm water subcutaneously, not into the veins, after the plan suggested by a surgeon in Bengal. As in Hamilton's cases, every symptom disappeared only to return and kill before we had time or heart to inject more. I think I sent you the cases. It is true this man had long been troubled with chronic bronchitis and embarrassed heart. Nevertheless, as I saw a gentleman who never gave opium lose *all* his cases (16), and as the most wonderful recoveries sometimes follow a full opiate, purging must be allowed to be a factor in assisting the fatal collapse. If opium simply locked up the poison and caused subsequent uræmia, then *all* the cases taking

* I think it was Sir Joseph Fayrer who produced in dogs something like rice-water dejecta by acute poisoning with arsenic.

opium would get worse and die, in a severe type of the disease, which is, as shown by Macnamara and most unprejudiced writers, utterly at variance with experience. At the very time that the anti-sedative practitioner lost all his cases a colleague saved two 'without treatment,' he said, but when hard pressed, he allowed that 'of course, they had had opium at the first onset.' I fear that the undue dread of this medicine has made us unduly sparing of it. I have known a gentleman severely reprimanded for using it. This was in the Madras Presidency, he told me. It seems almost incredible. A soft pill (to ensure its retention by gravity, Wylie's) of calomel should be given with any injections, to encourage the depurative functions. Chloral cures cramps, but does not relieve capillary spasms in the lungs, nor, apparently, does nitrite of amyl (not extensively tried). Injection of ammonia, if strong enough to do good, causes such terrible sloughing in cholera as to be practically useless, and in any case does not seem to have had a pronounced success. I saw numbers die, after reaction, under its use. I injected bromide of ammonia late in one case (but it was a relapsed one) without appreciable effect. Diuretics, etc., must never be neglected. The cold douche helps reaction. I have not seen a case subsequently. Since my first experience as resident medical assistant in a temporary cholera hospital in England I have seen five epidemics in India, besides constant sporadic cases, which must be my excuse for troubling you, and writing too dogmatically.

I beg to remain, Sir, yours truly,
 C. MACDOWALL,
 Bombay Army.

THE CHOLERA IN FRANCE.

To the Editor of 'The Lancet.'

SIR,—Before leaving for Paris, I send you a line to say that on visiting the hospitals, both of Toulon and this place, I was agreeably surprised to find all the patients either convalescent or in the stage of consecutive fever in which the intra-peritoneal injections were not called for. No admissions in the algide stages of this disease presented themselves whilst I was there. For some time, however, these injections into the peritoneum have been exercising the minds of practitioners here, through quotations from the *Lancet* having found their way into the foreign press; and since Dr. B. W. Richardson's first essay in

1854 many authors appear to have written on the subject. The chief difficulty seems to have been the want of directions for the employment of the method. In tapping for dropsy, the volume of ascitic fluid would protect the intestines; but in injecting the peritoneum for other diseases, it would seem necessary, as Dr. Richardson has done, to make a small lancet incision and scratch down to the peritoneum, and then push in a blunt cannula. Such a cannula, blunt at the end, with an eyelet like a catheter at one side, I have left with Dr. Duranty, one of the chief physicians of the cholera hospital established at the Palace of 'Pharo,' at the entrance of the harbour here; and Dr. Bassano, one of the house-surgeons, and the doctor himself, have promised to give me careful details of any cases which they may be able to treat by intra-peritoneal injections. All seem anxious to try them, intra-venous injections neither having succeeded in this epidemic, nor being, unless with exceptional apparatus, unattended with risk. The almost sudden influx of fluid by the veins seems to be unfavourable. I also strongly recommended the injection of the subcutaneous cellular tissues with warm water, so as to produce artificial anasarca, for both cases of dropsy and anasarca have been known to recover when attacked by cholera, apparently through the absorption of their effusions. To Dr. B. W. Richardson, it appears, we are indebted for this method, though others may have thought of it independently. Drs. Cooney and Purvis have recommended vesical injections, but Dr. Richardson, who tried this long ago, did not find the absorption to be satisfactory. Besides, so dangerous is the reabsorption of urine, that the catheter has often to be employed.

The epidemic seems to be passing away. The deaths registered are chiefly of cases admitted long ago in the stage of secondary fever. The admissions seem now to be few.

Dr. Koch has recommended, I hear, the non-watering of the streets, as he believes desiccation kills the 'microbia.' (?)

All the medical officers, sisters, and attendants, including ladies of rank, the British consulate, chaplains, etc., display the utmost courage and devotion.

I am, Sir, yours truly,
CAMERON MACDOWALL, F.R.C.S. Edin.,
Brigade Surgeon, Bombay Army.

MARSEILLES, *August 1st*, 1884.