sufficient of themselves to cause any obvious refrigeration of the body. I have already referred to the employment of quinine and salicylate of soda, and to the powerful influence they severally possess, when duly administered, in reducing temperature. The most powerful agent, however, in this respect, is the cold bath. I need not here go into the history of its introduction. It is sufficient to state that for some years past it has been very largely employed abroad, especially in Germany, in the treatment of enteric fever; and that lately it has been extensively adopted among us by some of those physicians who are connected with fever-hospitals. I have already referred to the admirable Croonian Lectures on Typhoid Fever by Dr. Cayley. In the last of them he discusses, with equal learning, knowledge, and skill, the use of the cold bath in this disease. He quotes statistics from foreign writers, which go to show that the mortality from enteric fever has been reduced by one-half among those who have been treated systematically by cold bathing; and he shows that, in his own hands, this mode of treatment has appeared to be almost equally successful. He argues forcibly that, by keeping the temperature systematically depressed from an early period of the disease, the intestinal lesions, and other morbid processes which are going on in the body concurrently with them, and which collectively bring on asthenia, impede recovery, and hasten death, are kept under; and that dangerous complications are hence less likely to ensue. He admits, however, that relapses appear to follow this kind of treatment in much larger proportion than they follow other plans of treatment; and he concludes a powerful argument by urging that the treatment by cold bathing should, at any rate, receive a fair trial. I am sure that any opinion or advice of Dr. Cayley's will be received with respect by all who know him, and by all who read what he writes; and I should be sorry if a course of treatment which has his sanction be not fully tried in this country by those who have the opportunity of trying it. I confess for myself, however, that I am very much in the same frame of mind as he acknowledges himself to have been in a little while ago; and that I am not yet fully satisfied of the great advantages of cold bathing. I am not absolutely convinced by his arguments, that the lesions attending enteric fever are kept in abeyance by reducing temperature. I know that, under the influence of the bath, delirium disappears, and the patient's condition seems to improve for a time; but I recollect how exactly the same kind of thing used to occur in cholera patients in whom injection of fluid was made into the veins; and how that practice, once much vaunted, has practically been abandoned. And I must acknowledge that, without being able to explain them away, the statistics, honest though they doubtless are, do not satisfy me. The result, in fact, seems too good to be true. If the mortality of a disease be diminished one-half by a particular kind of treatment, the benefit resulting from that treatment ought to be apparent to the most casual observer; it ought, like the effect of salicylate of soda on rheumatism, or of quinine on ague, to be utterly beyond dispute. And yet Dr. Cayley speaks with great caution of his own results. The results which I have witnessed in my own practice have not—at any rate, in my opinion—been favourable. I admit that I have not resorted to the systematic use of the cold bath at all extensively, and that during the last year or two I have scarcely employed it at all; but, two or three years ago, those of my hospital patients whose temperatures ran high were submitted to this plan of treatment. Some of the patients did well, and I was inclined to attribute the improvement which followed the baths to the baths; but two cases occurred in rapid succession in which I thought, perhaps erroneously, that the baths were instrumental in causing death.

The first case was that of a young man who had the disease severely, and a very high temperature; the baths were systematically employed, with the usual immediate effects; but suddenly, after they had been continued for some days, he passed into a state of collapse, with rapid breathing and great duskiness of face, and I assumed, notwithstanding that there was no abdominal pain, that perforation of the bowel had taken place. He lived for two or three days more, and at the *post mortem* examination I found that, though there was extensive bowel-disease, there was neither perforation nor peritonitis, but the lungs were in a condition in which I never recollected to have seen them before in enteric fever. They were small, collapsed, almost devoid of air, and of a deep slate colour. There was no pneumonia or oedema. I attributed his collapse and his death to the condition of his lungs, and I could not avoid attributing the condition of his lungs to the use of the baths. The other case was also that of a young man; and although he was very ill and had a high temperature, I demurred, after my recent experience, to treat him with baths. Nevertheless, I left it to the resident assistant-physician to employ them, if, in my absence, circumstances arose to make him think it desirable. The boy died, and at the *post mortem* examination his lungs were found in precisely the same condition as those in the previous case. Believing that the patient had not had baths, I observed

half jokingly to the resident assistant, who was present, that if only baths had been employed I should certainly have attributed his death to them. His answer was that they had been employed.

There are two ways of cooling patients by baths: the one is by means of what is sometimes termed the graduated bath, the other by means of the cold bath. In the former case, the patient is immersed in water, the temperature of which varies from 90 to 100 degrees, and is reduced gradually while he is in the bath to 65 or 70 degrees; in the latter case, he is at once plunged into a bath, the temperature of which from the beginning is made to stand at 65 or 70 degrees. In either case, the patient should remain immersed for a time varying between ten minutes and half an hour, or until he feels cold and shivers, and his temperature has been reduced by two or three degrees. It is important to recollect that the temperature continues to fall for some little time after removal from the bath. The bathing should be repeated whenever the temperature has again risen, and in many cases needs to be repeated as often as every three hours. The graduated bath is that which alone we have employed at St. Thomas's; but it is much more troublesome of application than the other, and it takes a longer time to reduce the bodily temperature. It is a less severe remedy, however, and may be preferably employed, as Dr. Cayley suggests, for old people, and patients who are extremely prostrate, and for those who have organic disease of the heart or lungs. The cold bath is preferred, as a general rule, by foreign physicians and by Dr. Cayley. Those who employ the baths habitually, commence its use in any case of enteric fever, as soon as the temperature in the mouth or rectum has attained an elevation of 102.2 degrees, and then carry on the treatment systematically as long as febrile temperature is maintained.

In conclusion, gentlemen, let me state briefly the treatment to which I should like to be subjected if ever, unfortunately, I should become affected with enteric fever. I should like to be placed in a cool, well-ventilated room, and covered lightly with bedclothes; to have a skilful and attentive nurse to look after me; to be fed solely with cold milk, unless vomiting should demand the addition to the milk of medicine calculated to allay vomiting. If diarrhoea became troublesome, or ever there was much pain or tenderness in the cæcal rings and in the bowels, I should like to be treated, not with laxatives, but with opium, given either by the mouth or by the rectum. If constipation were present, I should, excepting in the first week, like to have enemata only employed for its relief. In the event of intestinal hæmorrhage coming on, I should like to have ice to suck or ice-cold fluids to drink, cold compresses to the belly, and cold injections into the bowels; and, though I am sceptical as to their efficacy, I should still choose to have astringents, and more especially lead, given to me at short intervals. If perforation should take place, let me have large and repeated doses of opium. Stimulants I should prefer to be without early in the disease; later, however, and during convalescence, I should like to have them in moderation. As to the cold baths, I would rather not have them; but I would, nevertheless, leave it to my physician to exercise his discretion in the matter. I would leave it also for him to decide, according to circumstances, whether alcohol should be administered to me in large quantities. I would prefer not to be treated at a temperance hospital.

ON THE ACTION OF PANCREATINE UPON FAT, AND THE PROPER FORM IN WHICH TO USE IT.

By HORACE DOBELL, M.D.,

Consulting Physician, late Senior Physician, to the Royal Hospital for Diseases of the Chest.

I HAVE watched with much satisfaction the reinforcement, under the flag of Dr. W. Roberts, of the interest in the medical use of pancreatic preparations, first excited by my papers in 1864-6; and, hoping that nothing but good could come of Dr. Roberts's campaign, I have hitherto carefully abstained from intermeddling.

After carefully restudying Dr. Roberts's contributions, however, I find myself, at last, constrained to come forward to prevent more harm from being done by one part of his statements, than would be compensated for by all the good that could accrue from the rest. He has done excellent service by bringing prominently forward his experiments, confirmatory of the wonderful effects of the pancreatic secretion in digesting starch and albuminoids, and especially in formulating his results in the very useful recipe for "peptonised (pancreatized) milk-gruel", which seems somehow or other to have fascinated the professional mind more strongly than the forms for pancreatized suet and milk; pancreatized milk, egg, and arrowroot; pancreatized milk-cocoa; and pancreatized nutritive enemata, published by me from time to time (1870, 1872, 1875, 1878, 1880).

But it must be remembered that, while we have excellent means of digesting albuminoids and starch in pepsine and vegetable diastase, and, therefore, are not thrown upon the pancreas for these purposes for want of any other agent, this is not the case with regard to the emulsification (digestion) of fat by the pancreatic method.* Here we are left entirely destitute if deprived of pancreatic action.* There is no other known means of arriving at the same sort of emulsification of fat as that produced by pancreatisation. Yet it is this special action of the pancreas on fats—so far the most precious of its powers—that Dr. Roberts disposes of, as though it were of the least importance to medical practice, in the following passage. "I have not had an opportunity of examining the behaviour of pancreatic juice with fatty matter, and cannot, therefore, speak of its properties; but it is singular, if, as alleged, the effect of pancreatic juice and pancreatic tissue on fat is due to the presence of a soluble ferment, that the extracts of pancreas possess none of the same power.....I could not satisfy myself that any of these extracts possessed any special power of emulsifying fat." (*Lumleian Lectures*.)

No one who has experimented much with the pancreas will be surprised that an experimenter should be so charmed by the almost miraculous celerity and completeness of its amylolytic and proteolytic action, as to be in danger of neglecting the other properties of the secretion. But the emulsifying power of the pancreas is so remarkable, so unmistakable, and so important, that I am certainly astonished at Dr. Roberts' statement.

I hope shortly to publish my private journal and correspondence relating to the experiments made by me, and for me, since 1863; but in the meantime I append to this article a few extracts from them, plainly showing the effect of the pancreas and of pancreatine on fat. Since the first experiments were made by Mr. Heathorn and myself in 1863, hundreds of thousands of pounds of fat have been made into pancreatic emulsion, simply by the action of the pancreas. There is no gainsaying this practical demonstration of the power of the pancreas to emulsify fat. But it does not therefore follow that pancreatic extracts, essences, and liquors, have the same power; and what I wish most conspicuously to bring before the profession is, that Dr. Roberts is quite right in stating that they have not. These fluid extracts, essences, and liquors, of pancreas, were all tried by me years ago, and rejected as useless on this very account. They possess the amylolytic and proteolytic properties—which are easily obtained, and are convenient and useful for the digestion of starch and albumen—like pepsin and vegetable diastase—but they do not, in a sufficient degree to be valuable, possess the peculiar special property of the pancreas, that which no other substance at present known possesses, viz., the power of converting fat without saponification into a true permanent emulsion, capable of admixture with water. The property which I described and demonstrated to the Royal Society in 1868, and which was referred to by Dr. Letheby in his Cantor Lectures at the Society of Arts, shortly afterwards, was as follows.

"More than twenty years ago, Bernard proved what Valentin had long before suspected, that pancreatic fluid was concerned in the digestion of fatty matters; but he fell into error in supposing that its action was to saponify the fat and to set free the glycerin. Here is a specimen of glycerin and of lead soap obtained from the fat upon which the pancreatic fluid had previously acted, showing that saponification had not been effected. The true action of the pancreatic secretion is, evidently, to break up the large granules and crystals, and globules of oil and fat, into myriads of minute particles of from the 1-3000th to 1-15000th of an inch in diameter. In this way, the fat is emulsified and converted into a milky fluid which mixes freely with water.....We are indebted for this knowledge to Dr. Dobell.....When the fresh pancreas (that of the pig) is rubbed down in a mortar with twice its weight of hog's lard, it rapidly emulsifies it."

Dr. Roberts speaks of its being "alleged" that this "effect of pancreatic juice and pancreatic tissue on fat is due to the presence of a soluble ferment"; but he does not say who alleges this. In the introduction to my book on Tuberculosis, in 1866, I said "We found that there are several active principles of the pancreatic secretion all essential to its proper functions. There is no one of these taken singly which completely represents the true properties of the healthy pancreatic juice"; and in my paper to the Royal Society in 1868, I stated that up to that time, "all attempts to isolate the several properties of the pancreas into separate products had failed—no one of such products having been found to possess in perfection the property of acting upon fat in the manner described in this paper as peculiar to the pancreas. By the term 'pancreatine' therefore, I desire to represent the entire properties of the pancreas in a convenient form for keeping for experi-

ment and for administration as a remedial agent." The only preparation with which I am acquainted which answers to this description is the pancreatine powder of Savory and Moore—practically, powdered pancreas. This was the outcome of the experiments made for me by Mr. Heathorn and Mr. Schweitzer; and it cannot be too emphatically stated that when the object is to assist in the digestion and assimilation of fat, the liquid pancreatines must be rejected, and no other preparation used except the pancreatine powder.

Extracts from private Journal on experiments made for me, and by me, with Pancreas and Pancreatine.

September 1863.—"To prepare pancreatic emulsion, take the pancreas of the pig immediately after its removal from the body, and after separating all external suet, dissect out the lobes, of which the pancreas is composed, avoiding, as far as possible, the rupture of the ducts which intercommunicate with them.....The pancreas, so dissected, will probably weigh about one ounce avoirdupois.....This is to be well pounded in a mortar, and one ounce and a half of tepid distilled water added.....set aside to infuse in a water oven for thirty minutes, at temperature 100°; on removal, strain through muslin, and while still warm, mix with oil or fat in a fluid state, either by agitation in a bottle or trituration in a mortar."—(Signed) A. Heathorn.

To this I noted, that "the emulsion so made, under a quarter of an inch object glass, appears peculiarly equal; the oil-globules being less than half the size of a blood-corpuscle. This emulsion, treated with pepsin, and hydrochloric acid and water, remained complete two days after such mixture."

September 1864.—"The emulsion with lard-oil (made by the same process) is satisfactory.....it is a far more elegant pharmaceutical preparation than our former emulsion of beef fat."—(Signed) A. Heathorn.

August 23rd, 1864.—"The total amount of emulsion consumed by the patients of the Royal Hospital amounts to 156 lbs. avoirdupois; requiring 468 pancreases to emulsify the fat. The thirty-three patients consumed 105 lbs., requiring 315 pancreases to emulsify the fat."—(Signed) A. Heathorn.

October 29th, 1864.—"I have recently succeeded in obtaining from the pancreas some of the secretion, viz., pancreatic fluid. It possesses very active properties in emulsifying oil, and is, I think, one more proof of the reality of the pancreatic emulsion. The specimen I have preserved for your examination."—(Signed) A. Heathorn.

July 24th, 1867.—Specimens of pancreatine and pancreatic emulsion were exhibited at the Royal College of Physicians at my request, and after the exhibition, Mr. Julius Schweitzer wrote, July 27th, 1867: "I made this specimen of pancreatine on purpose for you.....It will digest sixteen times its own weight of lard. For carrying out the experiment I take, say, one grain of pancreatine, rub it up in a mortar, with about two drachms of distilled water, and add sixteen grains of lard, and mix the whole; I stir it together occasionally, till I find that the water has been absorbed, when I add a little more water, and, as before, stir it occasionally. It is best to commence this experiment in the evening, and to leave the mixture undisturbed all night; next morning the pancreatine will be found to have acted on the lard, the mixture readily mixing with water; and the above quantities will require about three ounces of water to form a smooth thick creamy emulsion. This emulsion still possesses the power to destroy the blue reaction of starch paste, made of two grains of starch-paste, and three or four ounces of boiling water. By itself, one part of this pancreatine destroys the blue reaction of starch paste made of eight times its weight of dry starch.....The pancreatine does not decompose lard, it seems solely to alter the molecular condition, adding, perhaps, a little water, but rendering the single minute globules into which it splits up the fat, able to suspend and emulsify the water. The glycerine is still left in the fat, and can be extracted from the first crude emulsion by means of ether, and combining it with oxide of lead.....This pancreatine also acts on albumen, but the extent of its action I have not yet ascertained.....All these experiments are made at the suggestion of Dr. Dobell."—(Signed) Julius Schweitzer.

On the next day, July 28th, Mr. Schweitzer wrote to me: "I tried the experiment of digesting white of egg, boiled hard, with pancreatine. I took one part of pancreatine and twenty parts of boiled white of egg. I found that this was somewhat too much, but at a rough guess I should say that it dissolved about sixteen or seventeen parts of the coagulated white of egg. It is a curious coincidence that one part of pancreatine emulsifies sixteen parts of lard; digests seventeen parts of dry starch, and made into paste, sixteen parts of boiled white of egg."

October 6th, 1868.—"The pancreatine exhibited at the Royal Society to illustrate your paper (December 12th, 1867) is exactly the same as what is sold to-day. It is perfectly good, and possesses the

* See my paper to the Royal Society, No. 97, *Proceedings*, 1868.

same properties now as then.....Pancreatine is a very wonderful thing, and not half understood or appreciated at present."—(Signed) Julius Schweitzer.

HEPATIC ABSCESS OPENED ANTISEPTICALLY: DEATH.

By NEIL MACLEOD, M.B. Edin., Shanghai.

W. M., a merchant's clerk aged 39, complained, on November 1st, 1879, of a severe stitch-like pain in the right infra-axillary region, and of inability to lie on either side. He had resided in Shanghai without a change for fourteen years, had been a free-liver, had taken little or no exercise for some years, and had a specific history dating ten years ago. In July 1879, he had a severe attack of diarrhoea; in the end of August, an acute attack of dysentery; and in the middle of September he went to Japan, the stools being solid, and one daily. He continued well until October 22nd, when the dysentery returned slightly; and, on his return to Shanghai in the end of October, the motions were liquid, two or three a day, light yellow, foetid, and occasionally contained a little blood and mucus.

From the 1st till the 13th of November, in the morning, the temperature varied from 97.8 to 100.3 deg.; in the evening, from 101.8 to 103.8 deg., once reaching 104.6 deg. The pulse varied from 80 in the morning to 104 in the evening, and latterly he had had night-sweats. There was no rigor or shivering at any time. The motions were sometimes solid, sometimes fluid—one to three daily; and there was occasional vomiting. The hepatic dulness was increased, but not markedly so. On November 11th, he was removed from the country house where he was residing into the settlement, when abscess of the liver was suspected, from the increased liver-dulness, fever, sweats, tenderness and pain on the right side, and slight bulging just behind the anterior axillary line. Fifteen to twenty grains of quinine, given frequently, had no permanent effect on the temperature; and opium and chloral were given to procure sleep. Mustard, a blister, and heat in various forms, were applied to the side.

November 13th. I attempted a thorough systematic examination (but had to desist, as the patient seemed unable to bear it), with this result. The tongue was dry, with a white fur at the back part; no appetite, and nausea. Slight bulging was to be seen over the lower ribs in the mid-axillary line and behind it; and in this region there were flattening of the intercostal spaces, slight oedema, and a tender spot. In the nipple-line, comparative dulness stretched six inches from the fifth rib to half an inch below the costal margin; absolute dulness four and a half inches from the sixth rib in the same line. On tracing the line of dulness backwards, nothing abnormal could be made out in its direction. Breath-sounds were inaudible over the bulging area, where friction could be distinctly heard on deep respiration, which caused a stitch-like pain and a sudden check in the respiratory act. Pulse 90, feeble; face flushed slightly; apex-beat not displaced. Morning temperature 99 deg.; evening pulse 104; temperature 102.8 deg.

November 14th. Pulse 101; temperature 98.3 deg. At 9 A.M., with Dr. Little's assistance, I performed the following operation under chloroform, with antiseptic precautions. I passed the largest-sized Matthieu's aspirator-needle between the seventh and eighth ribs in the medio-axillary line, at the tender spot before mentioned. At a depth of little more than an inch, yellow pus flowed through the tube. Aspiration was immediately stopped; and, the cannula being left *in situ* as a guide, I made an incision on each side of it, and cut down until pus began to well up by the side of the cannula. Having removed the latter and enlarged the opening with a probe-pointed bistoury, fully a pint of thick yellow odourless pus flowed from it, with the aid of pressure in the epigastrium. A drainage-tube four inches and a half long, with a calibre of half an inch, and stout silk guards, was introduced; and the usual dressings applied, and secured by gauze and elastic web bandages. No attempt was made to prevent the entrance of air into the cavity, which allowed a probe to pass five inches in a direction right across the body, at right angles to the surface. At noon, the pulse was 80; at 4 P.M., 78, and compressible; temperature 97.2 deg. At 9 P.M., pulse 90; temperature 98 deg. The patient said he was very comfortable. The discharge, under the microscope, showed pus-corpuscles full of granules, much free granular matter, and many large cells about two or three times the diameter of a pus-cell, free and in masses, and also very granular.

November 15th. Pulse 90; temperature 97.8 deg. He passed an uncomfortable night, serum having soaked through in large quantity, at the edge of the dressing behind, and the bandages having been too tight. He had no pain. There was about an ounce, of thick shreddy pus in the dressing. The expression was no longer anxious. He had

one solid motion. Evening: pulse 108; temperature 100.8 deg. He passed a very comfortable day, and the appetite was very good.

November 16th. Pulse 98; temperature 99.4 deg. He slept well. The dressing contained about half an ounce of pus in shreds. Round the wound there was a little bulging and tenderness. Temperature at noon, 97.8 deg. Evening: pulse 108; temperature 101.4 deg. He had one motion.

November 17th. Pulse 96; temperature 98.4 deg. The discharge was bile-stained. There was quite a deep hollow in the right hypochondrium. Evening: pulse 114; temperature 101.8 deg. He had two motions.

November 18th. Pulse 98; temperature 97.8 deg.; discharge less, but enough to necessitate daily dressing. Bulging and tenderness were still present, chiefly behind the wound over the seventh rib. Evening: pulse 108; temperature 102.1 deg. He had two motions. For three days, the patient had taken ten grains of iodide of potassium thrice daily, peritonitis of the rib being suspected.

November 19th. Pulse 108; temperature 99.7 deg. I gave the patient chloroform, and slit up the bulging side of the wound backwards. Pus had burrowed backwards under the skin; and the rib could be felt thickened, but not bare. Evening temperature 103 deg.

November 20th. Pulse 108; temperature 97.6 deg. The wound looked healthy and filled with clot. The discharge was as yesterday, and still bile-stained, very thick. Evening: pulse 114; temperature 103 deg. Twenty grains of quinine were given in the afternoon.

November 21st. Temperature 98 deg. I suspected tension from accumulation of matter. By means of a glass funnel, an India-rubber tube attached to it, and a long glass nozzle, having a calibre of a quarter of an inch, the whole being filled with a saturated solution of boracic acid (antiseptic and unirritating), I drew out of the abscess an ounce and a half of thick curdy pus. The air was first expelled from the tube by keeping the funnel a little above the abscess-level; then, on introducing the tube into the abscess, and lowering the funnel beneath the level of the abscess, the apparatus acted as a syphon. Evening: pulse 114; temperature 102.4 deg.

From this date to December 1st, the pulse continued to rise gradually. The temperature in the morning varied from 97.7 to 99 deg.; in the evening, from 99.4 to 101.7 deg., being once 102.4 deg. The motions became loose and frequent, but were kept under control by means of small doses of Dover's powder and bismuth subnitrate. The dressing was changed daily; and the discharge soon ceased to be bile-stained, remained odourless, and varied from a quarter to half an ounce in twenty-four hours, latterly becoming of a livery colour. It contained aseptic. On the 28th, the probe passed three inches obliquely backwards and downwards. Twenty grains of quinine were given frequently, without appreciable effect on the evening temperature. The patient sat up all day, sometimes walked in the verandah, and did not seem to lose strength.

From the 1st to the 20th of December, the temperature in the evening varied from 99 to 102.5 deg., being usually about 101 deg.; in the morning, as a whole, below normal. Two severe rigors, with an interval of a week between, were each followed by a change from the liver-coloured discharge to a fresh yellow-pus, lasting for two or three days, and then becoming once more dark-coloured, remaining aseptic throughout. Strength began to fail, and the appetite was lost; and, finally, severe diarrhoea set in. All action ceased in the wound, and the walls of the abscess could be felt soft and rotten by means of the probe. Death occurred on the 20th. I may add that there were never any lung-symptoms present.

POST MORTEM EXAMINATION, twenty hours after death.—There was no smell in the wound. The interior of the body was warm. The organs were anæmic. The liver was very fatty, the left lobe being about twice its natural size. The back part of the right lobe, opposite and behind the wound, was occupied by a cavity nearly twice the size of a hen's egg, having what appeared to be prolongations backwards and inwards; one of these, nearly two inches long, admitting the finger; and all containing pus of a lighter colour than the cavity. The liver-tissue round the abscess was soft and extremely friable, and bands of fibrous tissue ran through the cavity. There was no abscess outside of this softened area. Firm adhesions were present opposite the abscess, over a space of about six square inches from the wound backwards. The peritoneal cavity was normal. From the friability of the liver-tissue in the neighbourhood of the abscess, and the firm adhesions on removal of the organ (which I took the precaution to slice from left to right *in situ*, removing each slice), it was impossible to determine whether it was a case of multiple or of single burrowing abscess. From the clinical history and the result, I am inclined to think it was a multiple abscess of the liver.

REMARKS.—This is the first and only case treated by the method