nary phthisis was admitted, one of the nurses and one servant had died from the disease. Within the last few years, he had noticed that phthisis was certainly more prevalent among widows of men who had died of phthisis than among the other population; but he dared say there were causes accounting for this, without saying the contagion produced it.

Dr. BERNARD O'CONNOR (London) said that, as to the existence of the bacillus tuberculosis there could be no doubt; but if he were asked if the bacillus were the cause of phthisis, he would unhesitatingly reply, "not necessarily so, any more than any minute irritant might be supposed to be an indirect cause." He believed that bacilli might be derived from each of two persons; the one apparently healthy, the other consumed by tuberculosis; the former he regarded as an unfavourable and the latter as a favourable ground for the further propagation of these micro-organisms. In the first case there was a barren soil, in the second case a marked receptivity on the part of the patient for these particles; and it was this element of receptivity that physicians attempted to imply in the phrase "tubercular diathesis." Dr. Powell had considered the question of the communicability of phthisis, and he had adduced the cases of the nurses and attendants at the Brompton Consumption Hospital, as evidence against the entertaining the idea of such transmission. He (Dr. O'Connor) had, however, been struck by many of the statements forwarded to the Collective Investigation Committee respecting this subject, and his own personal observation has led him to the consideration of evidence which he could not wholly ignore. He had several times contemplated cases of phthisis apparently transmitted from the slowly dying and presently dead husband to the wife. He had never met with a case of supposed transmission from the wife to the husband, or to any other member of the family. He could only explain this, in part, by the attendance of the wives on their husbands. He had frequently observed that cases of undoubtedly hereditary phthisis seemed to run a much longer course than instances of the same disease occurring in those whose family history was not quite so definite. He thought that all must confirm Dr. Powell's statement that abdominal symptoms in phthisis were now very much less marked, and of less frequent occurrence than seemed to have been the case formerly. With regard to the effects of antiseptic inhalations of different substances, he could not agree with all that had fallen from Dr. Ward Cousins. Antiseptic inhalation, whether by means of Coghill's, or Mackenzie's, or of any similar instrument, continued day and night for weeks and weeks, had, in some cases in his hands, been followed by marvellous improvement in the general bodily condition; the cough had been eased, lessened, and, in some instances, stopped; hæmoptysis had disappeared; nocturnal perspiration had been, after a time, almost forgotten; the appetite had been regained, and the weight, in the vast majority of cases, had been considerably augmented. He had generally found that patients strongly objected to inhalers. At the same time, he never lost sight of the importance of driving the patient into the open air wherever he might live, of making him respire deeply, and of limiting himself, if he could do so, to sixteen or eighteen respirations in the minute. At the same time, he was directed to give up alcohol in all its forms, and to drink milk freely. Professor Gairdner had acquitted cold winds of any share in the causation of phthisis. He might have given them the verdict, "Not proven." He (Dr. O'Connor) thought that the cold winds (and more especially the north-east wind) had an indirect effect in the direction of favouring the advance of phthisis, by causing ignorant persons to keep their windows sealed during the prevalence of these winds. The majority of dwelling-houses were constructed on most faulty principles; and, unless a house were well ventilated by means of Tobin's tubes, or of some such contrivance, it was absolutely necessary that the windows should be kept open at the top, so as to allow the escape at the upper part of all the deleterious particles arising from the respiratory tract, the skin, etc., of the occupants of the room, as well as of the results of the incomplete decomposition of coal-gas, and the large quantity of sewer-gas which entered most dwelling-houses. Professor Gairdner stated that, from the miner's condition, it was plain that re-breathed air was not causative of phthisis. He thought that Dr. Gairdner meant to say that it was not the carbon existing in previously breathed air that was chargeable with the offence, but something else; otherwise his statement would almost presuppose that re-breathed air contained nothing but carbon. As regarded Niemeyer's views in connection with catarrhal pneumonia, he thought that the whole of his teaching might be summed up by a single quotation, "Some phthisical patients may become tuberculous."

Dr. POWELL, in reply, said he wished to say a word with reference to one point, and that was thoroughly to endorse all that had been

said as to the best cure or the best treatment for phthisis. He should attach no special importance to the treatment of phthisis by inhalations. He thought that it was perfectly certain that inhalations of such strength as could be used had no earthly effect in killing the bacilli, but had much the same sort of influence on them as a London fog had upon us. The bacilli did not like them, and did not flourish so well with them as under other conditions; but they still flourished quite too well for the patient's welfare.

ON THE CONTAGION OF PHTHISIS.

Read in the Section of Medicine.

By J. HENRY BENNET, M.D.

WHEN the question of the contagion of phthisis was submitted collectively to the profession, I did not sign the paper sent to me, not feeling able to answer it either affirmatively or negatively. At the same time I then felt, and still feel, that my opportunities for professional observation during the last quarter of a century have been so great and so exceptional that my experience must be worth recording. About this time last year I published, in a Paris medical journal (*La Semaine Médicale*, July 3rd, 1883), a communication on this subject, which I believe exercised in France some little influence in stemming the present current of professional opinion, and I now reproduce the facts therein contained.

There is no ignoring the fact that the doctrine of the contagion of phthisis is rapidly gaining ground with the junior members of the profession, especially abroad, and that its general adoption would lead to much that is distressing to the patients, to their relatives, and to society in general.

If it were true that all or nearly all the cases of phthisis observed have their origin in contagion, as asserted by some prominent writers, such as M. Debove, the Paris hospital-physician, the fact must be accepted, however disastrous in its social bearings. It is asserted that, in a person having a constitution hereditarily predisposed to consumption, or prepared by anæmia from whatever cause, exposure to the breath and to the emanations of a consumptive individual leads to the inoculation, to the development of phthisis, and that most, if not all, cases of phthisis thus originate, that is, in contagion.

If this statement be correct, we cannot avoid the inevitable sequela. We must adopt the same preventive precautions with our patients as we do with those suffering from scarlatina or small-pox. Phthisis, however, is so much more universal a disease, causing, as it does, the mortality of from one-seventh to one-tenth of those who die in large northern towns, that the result of such a pathological discovery would be infinitely more trying to society. Indeed, social relations would be all but disorganised, a consumptive person would be considered like a leper in olden times, one to be separated from his family, to be isolated, shut up. He must be refused admission into hospitals or asylums. He would have to live months, years, in a tent. His clothes should be destroyed, and whether he dies or recovers, the home which he has inhabited should be burnt. It would be perhaps a charity to mankind to kill him at once, like an animal attacked with rinderpest; for, as the duration of the ordinary forms of phthisis may extend over years, during that period every time he breathed he would be filling the atmosphere with the germs of disease, wafted by the atmosphere to be scattered far and near. Such is the logical sequela of the doctrine of the contagion of phthisis carried to an extreme, as it is now being carried by many, especially in Germany and in France.

Fortunately, clinical experience shows that this doctrine is not the expression of facts; that, if phthisis be communicable from one individual to another—contagious, in a word—it is only so to a very limited degree, and under very exceptional circumstances. The history of the Consumption Hospital at Brompton, since its foundation, as brought forward by Dr. Theodore Williams, at the Worcester meeting, is in itself conclusive. Although the hospital has always contained large numbers of consumptives, in every stage of the disease, the mortality from that malady among the large staff of nurses, attendants, medical men, etc., has never been greater, scarcely as great, as that of the population of London in general.

My own personal experience corroborates these data. I went to Mentone in 1859, twenty-five years ago, in an advanced stage of phthisis, recovered in a very unhopèd-for way, and have practised there ever since. I was the first, or nearly the first, to draw the attention of the profession, at home and abroad, to the special adaptability of the winter climate of the Genoese Riviera to the treatment of chronic chest-disease, owing to its dry, sunny, equable character,

neither hot nor cold. From that time, it has been frequented by great numbers of consumptives, of all nationalities. Thus, I have had the opportunity of studying the disease, every winter, in many sufferers belonging to the middle and higher classes of all the nations of Europe.

During that long period, I have only seen one case (presently to be narrated) that I could refer to contagion; and even that case might be otherwise explained. I have studied with care the history, and the personal and family antecedents, of my patients, which was comparatively easy in the class of life to which they belonged; and I have nearly always found the explication of their disease either in an antecedent lowering of vitality, produced in a hundred different ways, or in hereditary predisposition, without its being necessary to have recourse to contagion. I may say that I have lived with numbers of these consumptives in the large hotel which I have always inhabited, and have attended shoals of them living in other hotels, or in small boarding-houses, or in villas. Often these consumptives passed their life, especially the continental ones, notwithstanding my persistent advice, in badly ventilated rooms, surrounded by relations and servants, who lived with them for many months consecutively, for half the year; and yet I cannot recollect a single case of evident contagion occurring at Mentone amongst them.

Sometimes phthisis attacks simultaneously or in succession several or all the junior members of a family. Were they all living together, we might conclude that one gave the disease to the other; but what proves that such is not the case is that often the sufferers are separated from each other by half the earth. One is in England, the other in Canada, a third in Australia. They have the same constitution, the same latent defective vitality, which gives way and wears out almost at the same epoch. Phthisis is merely the mode in which they die. If the contagion doctrine were true, I ought to have seen hundreds of cases illustrating it in these twenty-five years of practice in such an arena. The large hotel in which I have lived, often surrounded by them, as also the other hotels and boarding-houses on the Riviera, inhabited every winter by numerous consumptives, should be saturated with bacilli, with the elements of contagion, and ought to be razed to the ground. No summer cleaning or ventilating would purify them, and render them fit for habitation the following winter, were phthisis as contagious as it is stated to be.

The experience of my colleagues at Mentone, twenty-three in number, is the same as my own. At our Medical Society, of which I have the honour to be president, we have repeatedly discussed this question, and all agree with me that, with here and there an exception, they have not seen phthisis propagated by contagion. The few cases they have observed capable of bearing this interpretation have generally also been capable of being otherwise explained. Dr. Schnyder, the learned medical resident for twelve years of the well known baths of Weissengen, near Berne, in Switzerland, where many hundred consumptive cases congregate every summer, has only seen four or five instances during that lengthened period that seem to favour the doctrine of contagion, and those admit of other explanation. Yet at Weissengen they are aggregated all day in rooms small for their number, and are constantly spitting about, in-doors and out, as continental people do, without any disinfection being resorted to, at least until quite recently.

My own case, already alluded to, was that of a young healthy officer, who came from New Zealand with his wife—the latter in advanced decline. She had large cavities in her lungs, and spat, he told me, immense quantities of matter. They lived together in a small cabin, only a few feet square, day and night, with the window generally shut, for four months; and she died a few days after landing in England. A short time afterwards, he showed signs of pulmonary consumption, which drove him to Mentone the following winter. This looks like a case of positive contagion, for there were no consumptive antecedents in his family. A four months' residence, however, with a consumptive wife in an advanced state of disease, coughing up masses of pus, in a small, confined, unventilated cabin, is calculated to undermine the health of the strongest person, and to produce that very condition of lowered vitality in which tubercular or caseous deposits appear in the lung-tissue spontaneously, as believed until now, or under the provocation of accidental inflammatory conditions. Moreover, breathing such an atmosphere, loaded with morbid pus, there may be a direct inoculation of the lungs with the pus disseminated in the atmosphere, reproducing M. Villemin's experiments. The spray-injection into a dog's kennel for half an hour a day, for a few days, of a fluid mixed with the contents of a phthisical cavity, has developed caseous deposits in the dog; and probably any kind of pus, with bacilli or not, so breathed, might have the same result.

Considering the known results, as regards the lungs, of the inocula-

tion of any kind of pus or filth, or of their artificial inhalation, it is only a matter of surprise to me that phthisis should not be more frequently conveyed from the consumptive patient to those living with him. This surprise is justified, whether we attribute the communication of disease to the recently discovered bacilli, or to mere purulent infection.

It was stated last year, in France, in reply to remarks I published antagonistic to the doctrine of contagion carried to an extreme, that the reason for the absence of evident contagion in my practice was the thorough, even extreme ventilation I rigorously insisted on. I do not think that ventilation alone would explain the fact were contagion as common as it is stated to be; moreover, most other middle-aged or elderly physicians with large opportunities of observation have, I believe, arrived at the same conclusion, although they do not ventilate as freely as I do. But I accept the argument as one in favour of free ventilation in the treatment of phthisis as recommended by your learned townsman, Dr. MacCormac. It is extremely difficult to induce patients, and especially their attendants, to ventilate freely, to always leave a window or door open, summer or winter, so that the air of the room inhabited may be constantly changed. This I consider essential to the welfare of all consumptives. This fact has been admirably elucidated in the most irrefutable manner by Dr. MacCormac. He has quite proved to my satisfaction, in his valuable work entitled *Consumption from pre-Breathed Air*, that the breathing of pre-breathed air is the greatest factor in the development of phthisis. It is very far from being the only one, but in my experience, as in that of Dr. MacCormac, the principal one. With the fear of bacilli and contagion before their eyes, neither the patient nor his attendants will hesitate to ventilate sufficiently to constantly renew the air breathed. Free extreme ventilation is the most important means of preventing, and also of healing, pulmonary consumption, so we may welcome any mode of thought, any opinion, that leads to it. The present contagion ideas will undoubtedly be most beneficial in this sense.

During the last few years phthisis has become much more common at Mentone, among the girls and young women, than it formerly was, especially among the washerwomen, a numerous class; and contagion from the linen soiled by the sputa of consumptive patients has been suggested as the cause. I am disposed myself to attribute this undoubted increase of phthisis in a healthy southern locality to other causes; to the mere change in habits and occupations. When I first settled in this region in 1859, nearly the entire population was agriculturally occupied. All the young women of a village lying on the mountain-side a few hundred feet above a property of mine were thus engaged, worked at the soil with their father and brother, carrying stones and earth in baskets on their heads. They were fine, healthy, robust girls and women, fed on macaroni, olive-oil and wine, worthy to be *matres hominum*, mothers of men. I often had half a dozen working for me in this way, tripping lightly over the rocks and hill-sides, heavily weighted. I employed them at that time with some compunction, which I should not feel now. Times are changed; the Grimaldi girls have all become town workers, washerwomen, sempstresses, servants, and they begin to die of consumption. Is it not because they work in close badly ventilated damp rooms, instead of in the open air? Is it not again a question of pre-breathed air, not of contagion?

The present tendency of the medical mind is to send all consumptives in winter to a cold climate, in Europe to the upper regions of the Alps. It has become so general, that the Genoese Riviera last winter was all but deserted by them, and many hundreds were crowded in the Engadine hotels. This accumulation of hundreds of consumptives, in all stages of disease, in a few hotels, must necessarily greatly increase the probability of contagion.

It must be very difficult, if not impossible, to ventilate as freely, night and day, in a rigorous Alpine climate, as in the temperate climate of the Riviera, with the thermometer very seldom below 45° at night, and generally between 50° and 60° in the day. The atmosphere must, therefore, be saturated with germs and bacilli, and I must confess that, notwithstanding my clinical experience on the Riviera, I should be very sorry to spend the winter under such conditions, however strong and well I might be.

On the Riviera, I may mention that we really get mountain air during sixteen out of the twenty-four hours. From about four in the afternoon to eight in the morning, there is a down draught from the mountains, which, behind Mentone, are about four thousand feet high. Those who open their windows at night, and all my patients do, thus breathe mountain air two-thirds of the time. The other third, from eight to four, there is a sea-breeze, during the existence of which they breathe sea-air.

Recent researches on sea-air, published in the *Semaine Médicale*,

March 6th, 1884, p. 90, prove that the popular ideas as to the purity of sea-air are quite correct and scientific. Experiments were carried out on board the *Gironde*, a French mail steamer, between Bordeaux and Rio Janeiro, by the commander, Captain Moreau, assisted by Dr. Plantymansion. M. Moreau had previously studied the subject at the Paris observatory of Montsouris, under Dr. P. Minquel, from whom he received his final instructions. He came to the following conclusions.

"At a distance of 100 to 200 kilometres (60 to 120 miles) the atmosphere directed on the sea by terrestrial winds is, in a great measure, deprived of the microscopic organism which it has raised from the surface of the soil—in a word, that the atmosphere rapidly purifies itself by its contact with the immense liquid plains of the ocean, favoured by constant ascending and descending currents, due to changes of temperature. Thus the sea's mission is to seize and destroy the germs which the land-wind throws on it; whence is explained the non-transmission of epidemics and contagious diseases from one region to another, when separated by wide extents of sea. Whenever they are thus transmitted, the germ may be presumed to have been brought by vessels."

Taking an average of the analyses made between Bordeaux and Rio Janeiro, eight in number, M. Moreau found the number of spores in a cubic metre of air to be 530—many, no doubt, furnished by the ship itself; whereas, at the Observatory of Montsouris, near Paris, the number, on an average of four years, was found to be 14,000. Scarcely any bacteria, bacilli, or micrococci were found—only, on an average, five or six in ten cubic metres of air. Thus, with a systemic wind from the sea, the air on the sea-coast must be all but absolutely pure, even more so than that of mountain regions. This will be good news for those who live on the sea-coast; for those who take their health holiday there; and for those who take refuge in winter in coast sanatoria. The latter seem on the eve of being unjustly dethroned in favour of mountain elevations. Instead of avoiding the shore, however, as invalids are now-a-days constantly told to do, they may continue to seek it, and to live if they like within a few feet of it in our English fashion. This I have ever done. I have had an instinctive prevision of these scientific facts; and, for the last quarter of a century, have pitched my winter tent as near to the sea as I possibly could. Believing in fresh air as firmly as your townsman, Dr. Mac Cormac, and being determined to carry out the physiology of respiration to its extreme scientific limits, I felt sure there was no better place for that purpose.

In conclusion, I do not deny the contagion of phthisis, far from it. I accept it in certain conditions and limits, bacilli or no bacilli; but I do not consider it to be the regular habitual cause of phthisis, as asserted by many. Like all the seniors, I am watching with interest the labours of our juniors, ready to accept any change of doctrine they may present to our notice the moment its truth is proved. Such doctrinal changes must be founded, however, on clinical experience, as well as on microscopical research. In the meanwhile, as I have repeatedly stated, the contagion-theory will help on powerfully to the general acceptance, in health and disease, of the physiology of respiration. It is still shamefully ignored in the routine of daily life, as well as in the sick-chamber, especially on the Continent, by medical men as well as by their patients.

INTESTINAL INTUSSUSCEPTION.

Read in the Section of Medicine.

By A. K. YOUNG, F.R.C.S.I.

Surgeon to the County Monaghan Infirmary.

I PRESUME it may be expected that, at a meeting of this kind, not only new theories, broad and extensive philosophical views over the unbounded fields of nature and of science, reports of the bold, the almost audacious, surgical practice of the present age, but even cases in practice where circumstances called for remedies of unusual kind, are expected (at least, will not be esteemed out of place), if such are brought under the notice of the Association, as they may exhibit by their narration many more interesting points than the success or failure. I shall, therefore, with your permission, proceed to lay before you, as concisely as I can, a case from one of my prescribing books, as being instructive and interesting in several aspects; as showing to what extent a remedy that is common enough may be used; the strength of the human intestinal canal to sustain such a trial; the power of the constitution to endure the presence of such a quantity of the powerful metal, mercury, for a length of time, with perfect unconcern at its presence, and without showing in the very least degree any

one of the usual symptoms, which the very smallest quantities, when in chemical combination, so constantly exhibit.

In 1876, on May 2nd, James M., aged 59, a farmer, married, and having a family, presented himself at the County Monaghan Infirmary. On referring to the books of the institution, I find this short entry made on his admission: "Obstipation from intussusception of the intestine; no evacuation from the bowel for two weeks." On referring to my prescribing book, I find he was treated in the not unusual way: by large enemata of soap and water, castor oil, and opium; by which means only very occasionally slight traces of feculent coloured matter were obtained, possibly only intestinal secretion.

On the morning of May 6th, finding no response to the careful perseverance in these remedies, I thought I would try another, and, if possible, aid mechanically the liberation of the obstructed part. Accordingly, I had the long enema-tube connected to a strong elastic one proceeding from the waste-cock of the water-tank on the roof of the house about 30 feet above our heads. The tank-water was agreeably tepid from the rays of the very hot sun to which it lay exposed. When the enema-tube was passed into the rectum as high as it could be without violence, at 10.45 P.M., I turned the tank-water into the tube, the great pressure of which I hoped, by acting steadily, continuously, and without the jerking unavoidable in the ordinary enema-apparatus, might possibly dilate the containing and liberate the contained portions of intestine. This very enormous and steady pressure I maintained for nearly five minutes without the slightest effect.

May 7th. Notwithstanding the large quantity injected last night at near 11 o'clock, he did not pass anything, nor even return the water injected under such unusual circumstances. The belly was swollen, but he said "it did not feel more so than it did on the previous morning." The obstruction would seem to be in the left hypochondrium. The tongue was moist, and only slightly coated. There was no great thirst. He took very little nourishment, being, as he said, "afraid to do so, as he was full enough." He urinated, but not abundantly. He had no hiccup nor vomiting. The pulse was as usual, i.e., exceedingly irregular and uncertain. He slept well. At 2.30 P.M., another high-pressure enema was given, and, whilst the abdomen was full of water, I well and carefully kneaded it, and especially at the left hypochondrium, where there seemed to me a doughy hardness, as if from blocked-up faeces; the result was four or five bits of feculent matter, such as a child of four years old might have passed. At 4.30 P.M., another high-pressure enema was given. After this third enema, he expressed himself as feeling "rather comfortable." There was no further result.

I had hoped this powerful pressure of water, which presses equally in all directions, would have the effect of distending the containing part of the bowel; and that its upper pressure, at the same time, might force up or dislodge the contained or strangulated portion; but, after the fair and careful trials I made, I gave this up, and bethought me of another force to act in the opposite direction. At 6 P.M., I passed the tube into the stomach, and, by aid of a small glass funnel, I decanted two pounds of quicksilver into the stomach slowly. In an hour afterwards, he said "I feel something like colic-pains." This passed off in a short time. At 10 P.M., on my visiting him, he produced a stool such as a child of five or six years old might have evacuated. It was of a greyish colour, semi-consistent, as much as would fill a goose egg, not foetid. There was not the least particle of mercury with it. He had urinated pretty fairly, as usual. There was no abdominal pain whatever. The residue of the mercury, all the quicksilver I possessed—namely, one pound and two drachms, making altogether three pounds and two drachms—I introduced, as in the first instance, through the glass funnel into the stomach. He did not complain of any abdominal pain whatever. He was sent to bed, and directed to lie on his right side for an hour; after which he might turn as often as he felt inclined.

May 8th. At 10 A.M., he was reported "not to have had a bad night by any means." He had ejected from the stomach a quantity of dark coffee-ground-looking material or tough phlegm, along with some curdled milk and remnants of broth. There was not the least appearance of fecal matter. He complained of more uneasiness in the belly, which was slightly painful to manual examination. There was no attempt at defecation. Tongue moist. He was sweating slightly. Pulse fuller than on yesterday. I ordered a large oleaginous enema, with thirty drops of tincture of opium every hour. At 2 P.M., his countenance seemed anxious. He was lying on his left side, as he said "it pained him very much to lie on his right side." He complained of much pain over the right iliac region, which was much increased by pressure. The abdomen was tense; the pulse strong and full; the tongue was moist. Between this time and 5 o'clock, he had two large enemata of soap and water, castor-oil, and a drachm and a