

(seven of which were spent in the tank) she lost 7 lb. During the second period of four days in the tank she gained 1 lb. The net weight of the fifth case fell from 97½ lb. to 92 lb. during six days in the tank. In the sixth case the net weight fell from 115 lb. to 95 lb. during the first twenty-seven days in the tank, and during the remaining four days he gained 3½ lb. In the seventh case the net weight fell from 127½ lb. to 117½ lb. in ten days. The loss of weight in these cases has been very considerable, but the wasting did not appear so extensive as I have previously seen it in severe cases of typhoid fever. I hope in future, with improved dietary, that the wasting will be much less.

8. *Skin*.—The horny layers of the palms of the hands and soles of the feet get quite macerated, but on the skin of the body generally there is very little effect, with the exception of a slight roughness and elevation of the papillæ.

9. *The tissues generally*.—There is marked diminution in the dehydration of the tissues, which takes place in all febrile conditions. This is very apparent in the case of the tongue, which maintains its proper size and keeps moist. I know that a great many eminent authorities state that there is a retention of fluid in the system during the febrile process. There is an intra-vascular retention, especially in the veins, to fill up the parietic vessels; but a retention in the tissues is quite inconsistent with a high temperature. Dropsical conditions follow the fever, and are associated with a feeble circulation. The intra-vascular retention hampers the action of the heart, whereas the improved vascular tone and tension associated with the use of the tank diminishes the bulk of fluid in circulation and increases the action of the heart.

Such, gentlemen, are the effects which you may hope to derive from the use of the tank in typhoid fever. It is not a specific for that or any other disease, but it is a specific treatment for the patient; by placing him in an improved environment his system is better able to adapt itself to and overcome the altered conditions attendant on the fever. You may say that the number of cases is too limited to draw conclusions, but if I can only induce you to put the method in practice we will soon accumulate a sufficient number of cases to satisfy the most ardent Baconian. Inductive philosophy is all very well in its proper place; it helps us to take stock of our progress and to remove any excrescences which may have grown on our theories; but, so far as I am aware, no advance either in science or anything else has ever been effected by this process of reasoning. If the advantages of the tank had not been apparent to my senses I would have discontinued it long ago, without waiting for the accumulation of statistics. This is essentially a method of treatment for severe and moderately severe cases; all or nearly all mild cases get well with little or no treatment except that of rest in bed, with a regulated diet. You can see such a case in the hospital at present under my care, which has not been in the tank although there was one vacant during the progress of her disease. This treatment has met with the approval of my colleague, Dr. Dickinson, senior physician to this hospital, to whose valuable opinion I attach much importance. Dr. Wallace, who has had the active care of the patients, says that if he contracted typhoid fever he would be treated in the tank, and I believe his opinion is shared by many others. As for myself, I have been so often "in hot water" that I should take very kindly to it.

PAROXYSMAL HURRY OF THE HEART.

By SAMUEL WEST, M.D., F.R.C.P.,

ASSISTANT PHYSICIAN TO ST. BARTHOLOMEW'S HOSPITAL; SENIOR PHYSICIAN TO THE ROYAL FREE HOSPITAL.

(Concluded from p. 642.)

CASE 2. *Sudden cardiac pain; collapse; pulse 250-300; relieved by nitrite of amyl; no murmur at time; extreme morbus cordis found on recovery; history of syphilis and fever; two previous attacks eight years and six years ago respectively*.—William Y—, aged thirty-two, a commercial traveller, was admitted into St. Bartholomew's Hospital almost in a condition of collapse, and with a pulse which numbered 300 beats in the minute. The attack came on suddenly on the morning of admission. He became faint at 11.30 A.M., had much pain in the præcordium, in the back between the shoulders, and down the left arm. Some brandy was given

him, but it did him no good. On admission, he complained greatly of the pain, was almost collapsed, extremely pallid, looked and felt faint, and was in a condition of considerable mental excitement and distress. He was unable to lie down, but could rest only in the semi-recumbent position. The respirations were not hurried, 18 in the minute, and there was no dyspnoea. The pulse could hardly be counted; at first the beats were over 300, but the rate varied a little from 250-300. The beats at the wrist were regular, but quite distinct, though feeble and small. The patient made no complaint of palpitation, and did not seem to know what was the matter with him, except his heart was going very fast. The cardiac dulness was a little increased, and the apex displaced to just outside the nipple line in the fifth space. The right border did not extend beyond the left edge of the sternum. The sounds were clear and there was no murmur audible or any friction. A capsule of nitrite of amyl was administered, with the result of relieving the pain at once, and ten drops of tincture of digitalis with ten drops of laudanum were given at once and repeated at intervals of four hours for three doses. The heart gradually quieted down, and at 8.30 P.M. was not beating more than 88, with steady forcible beats, though not quite regular. The slight dilatation had subsided, and a systolic murmur became audible over the whole præcordium, but not propagated into the axilla. It was heard also at both bases, but chiefly behind the sternum, and here it was propagated towards the right clavicle. This was the end of the attack. The patient was kept in the hospital for nine days, during which time there was no recurrence. The pulse varied from 76 to 84, and was usually regular and full, but was easily hurried by excitement. The only medicine given, after the attack had passed off, was some iron and nux vomica. By Feb. 4th the murmurs, which had been gradually becoming audible, proved to be two, one at the apex, audible also in the axilla, and occasionally faintly at the angle of the scapula; the other at the right base, and propagated upwards into the neck. Both murmurs were systolic in time, and no diastolic murmur could be detected. The patient regained colour somewhat, but when he left at his own request he was still pale and anæmic. He did not return, though he promised to do so if further troubled, from which we may conclude that he has suffered no further inconvenience. His previous history was this: The present was the third attack of this kind, the first coming on without any reason that he could give eight years ago. The attacks lasted for a few months, coming on at irregular intervals, and lasting only for a short time, similar in character to the present, but not so severe. He remained free then for about eighteen months, when he came under my observation, and after four weeks recovered. Since that time he had no return until the present attack. He had never had rheumatism, gout, or chorea, and, except for measles when a child, had been perfectly well. He had had gonorrhœa, but never syphilis, and there was no history of similar affections in any near relative. His breathing was good even on exertion, and he was capable of considerable effort without suffering from pain or palpitation of the heart or any other discomfort. By occupation he was a commercial traveller, and drank freely, though not to great excess. There had been no unusual excitement or worry to determine the attack, which seemed to come on quite at random and without warning.

In this case a single paroxysm of about nine hours' duration constituted the whole attack. But the first attack, eight years ago, had continued over several months, and the second over four weeks, so that the attacks appear to be getting less severe and less frequent. *Morbis cordis* proved to exist after the attack was passed, both the mitral and aortic valves being probably affected. I would draw attention to the possible history of syphilis in this case, as it is also present in one of Dr. Bristowe's patients, and may suggest the nature of the lesion in some of the cases. The effects of nitrite of amyl and of digitalis have also their importance in considering the pathology of the affection, as I shall show later. This patient also got well and returned to work.

CASE 3. *Sudden attack of severe palpitation and pain brought on by hurrying; pulse very rapid; respiration also rapid; no murmurs or other evidences of morbus cordis; respiration rapid for some time; pulse, except during attack, slow, due to digitalis probably; strong hysterical history; complete recovery*.—Nurse G—, aged twenty-six, while

hurrying back to the hospital from church on Sunday, April 7th, was seized quite suddenly with violent pain near the heart running down the left arm. It seemed to take away all her breath, and it was only with the greatest difficulty that she succeeded in getting upstairs to her room. Her breath seemed almost to stop, and she felt as if being suffocated. Though she retained consciousness she felt dizzy and thought she was dying. The house-physician, who was at once summoned, found her greatly distressed, very pale and anxious, restless, but not sweating. She was breathing rapidly and panting, and could only speak in gasps. The pulse was regular, but beating so rapidly that it could not be counted at the wrist; the artery was distended though the pulse wave was small. The vessels in the neck pulsated distinctly, but though the apex of the heart was displaced a little, no murmur could be detected. She appeared so ill that the greatest alarm was felt about her, but after the administration of brandy and ether with a little opium, and the inhalation of some nitrite of amyl, she rallied, the pulse and breathing quieted down, and the attack slowly passed off, leaving her extremely weak and prostrated. Ten drops of tincture of digitalis with some ether were ordered to be taken every four hours. During the night she slept but little; she was unable to lie down, and was not comfortable unless the shoulders were propped up and the head supported by pillows. She was very pale, but had lost her anxious expression. The respiration was laboured and still rapid, 36, but the *alæ nasi* did not dilate, nor was there much dyspnoea. The pulse was 78, and a little irregular. The apex of the heart was felt in the fourth space just inside the nipple, and the cardiac dulness was increased upwards to the third rib. The impulse was natural, no thrill was found or any murmur heard. The temperature was normal. The patient still complained of pain, but it was not severe, and more an ache than an acute pain. A small injection of opium was prescribed to give her sleep, and the following mixture administered every three hours: Tincture of opium five minims, spirit of ether and aromatic spirit of ammonia of each ten minims; to an ounce of chloroform water. The patient slowly improved, and by the 13th had lost all pain, but during this night she was alarmed by one of the patients, and the pain returned enough to prevent her from sleeping for the rest of the night. On April 16th she was, by her own urgent request, allowed to be moved on to a chair, but the simple movement brought on an attack of dyspnoea and cardiac pain, which lasted through the night and into the next day. On the 24th she was again moved on to the couch, and bore it well until she moved to get back into bed, when the exertion brought on another slight attack. The next morning the respirations were 50 and shallow, but the pulse only 70. The temperature throughout was subnormal. Henceforth one day was very much like another. As long as she lay still in bed she was free from pain, but a very slight effort or excitement caused it to return. The respiration was accelerated from 40 to 50 in the minute, and though the pulse was not when at rest above 80, exertion or excitement at once caused an increase. On May 6th this note was taken: The patient looks less anæmic, but the respiration is still rapid and somewhat panting; the cardiac dulness is still slightly increased upwards, and the apex cannot be exactly fixed. No murmur is audible, but the sounds are everywhere feeble and the action easily excited. Fifteen drops of tincture of perchloride of iron with five of tincture of belladonna were given three times a day, and a dose of opium or a small hypodermic injection was necessary at night from time to time. On the 16th the patient tried to walk a few steps, with the result that a severe attack of pain came on which was only relieved by ten drops of digitalis and an injection of morphia. The digitalis seemed on this occasion, as on others previously, to reduce the pulse-rate very rapidly, and the next morning the heart was beating only 58 and slightly irregular; the respiration was still rapid—viz., 44. A few days later the belladonna was replaced by five drops of digitalis, which was now tolerated, and seemed to do good. On the 25th the patient was allowed to go home to be nursed upon her own and her mother's request. I saw her again three months later. She had had no return of the attack, and, though not looking robust, seemed in fair health. She went back to nursing, and has continued, I understand, in active work since. I have to-day (March 10th) heard that she has recently been very dangerously ill again, but the nature of her illness I have not ascertained for certain, but it seems probable that it is a return of the old affection.

The family history was not good; neither father nor mother was strong. The father died of cancer at sixty-eight, and there was phthisis on both sides. The patient is one of eight children, of whom three died young, and one is mentally defective. The patient has never been strong; at eight she had scarlet fever; at fourteen pleurisy; at twenty-one she had rheumatic fever, which kept her in her room nearly the whole winter, and during which attack she suffered much from pain in the left side, but a pain of an entirely different kind from that in the present attack; a year later she had scarlet fever for the second time, which was followed by nephritis, from which she slowly recovered, and in the Christmas of the same year she fell a victim to typhoid; this she regarded as the worst illness of all, for she remained pale ever after, and for months was in a state of low nervous exhaustion, for which she was a patient in St. Thomas's Hospital, and was cured by massage. She gradually became stronger, and continued in fair health till she came to be a nurse in the hospital in January of the present year. Shortly afterwards she fainted twice, from overwork and over-anxiety she thought, and was troubled with a little shortness of breath at times on exertion. Except for this she continued in good health until the present attack, before which for a few days she had not been feeling well, but she attributed this to the extra fatigue and anxiety of night duty.

This case appears to fall into a somewhat different category from the others, inasmuch as, except during the attacks, the pulse-rate was slow, below the normal; while both during the attacks and between them the respiration-rate was always above normal. Something of the slowness of the pulse must, I think, be put down to the influence of digitalis, to which the patient seemed extremely susceptible. In considering the rapidity of the respiration the strong hysterical history must be borne in mind. The patient was at the time very emotional, but clearly suffered much real discomfort and alarm from the attack of palpitation, which I did not feel at all justified in referring to hysteria, except perhaps in the most indirect way.

The affection is one of the middle period of life, for out of the twelve cases recorded by Dr. Bristowe and myself ten occurred between the ages of twenty-five and fifty. The liability of the two sexes is the same. The chief interest of this affection centres, of course, in the question of its nature—that is to say, whether it depends upon an organic lesion of the heart or not. A nervous affection it certainly must be, but the question is whether this nervous irritability is itself due to organic disease of the heart. Dr. Bristowe concludes that it is not, and describes it as a functional disease, a conclusion which was supported by the only post-mortem obtained. Still, it seems to me that there is a good deal to be said on the other side. In the first place, extreme rapidity, such as is met with in these cases, is very rare in ordinary palpitation, and in exophthalmic goitre, where palpitation and rapidity of action are so common, it is most unusual for signs of cardiac failure to develop in the way that they developed in so many of Dr. Bristowe's cases.

Now, in one of Dr. Bristowe's cases and one of mine there was distinct evidence of antecedent valvular disease. In another case of Dr. Bristowe's the patient, a lady aged forty-nine, was only four weeks ill and died. This case reads to me very much like one of acute myocarditis, such as I recorded some years ago, and I think we might fairly presume, in the absence of evidence to the contrary, that the heart was not sound. Another case occurred in a patient of sixty-five, so that the possibility of cardiac lesion in connexion with atheromatous vessels cannot be excluded, although it is true that this patient was remarkably active three weeks before his illness. Of the remaining eight cases two died of gradual cardiac failure with the usual symptoms, and one suddenly, apparently of cardiac syncope. One of Dr. Bristowe's patients and one of mine referred the commencement of their attack to a strain.

If there be a heart lesion, the next question that arises is as to its nature. It is clear that no valvular lesion will account for the affection. It remains, then, that it must be a lesion of the myocardium. As I have already said, one of Dr. Bristowe's cases reminds me very strongly of the cases of acute myocarditis I recorded some years ago, for in them also the pulse was very rapid, and there was the same susceptibility to the action of digitalis, which was noticed in two of the present cases. It is, of course, very difficult to diagnose, or indeed to exclude, affections of the myocardium; but it

is remarkable that in several of the cases there is a history which would explain such a lesion if it were present. Thus the first and third of my patients had had rheumatic fever, and the second probably syphilis; while two of Dr. Bristowe's had also had syphilis. For these various reasons I am inclined to the belief that the cases of paroxysmal hurry of the heart are not due to functional disturbance alone, but to an organic lesion, and that that lesion is in all probability in the muscular substance. It may well be in some cases a form of chronic interstitial myocarditis, consequent in some cases on past rheumatic pericarditis, or in others on syphilis, being thus related to fibroid disease of the heart. I admit, of course, that proof is impossible, and that such a conclusion must at present remain a matter of opinion.

I have thought it well to bring these cases forward because there appear to be but few on record, though probably the affection is not so rare as might be supposed. Whether rare or not, they appear to me to constitute an important group to which less attention has been drawn than it merits.

Wimpole-street, W.

THE PREVENTION OF RABIES AND HYDROPHOBIA.

REMARKS ON CERTAIN PHASES OF THE SUBJECT.

BY SURGEON-GENERAL C. A. GORDON, M.D., C.B.,
HONORARY PHYSICIAN TO THE QUEEN.

THE subject of rabies and hydrophobia has recently been considered in two very different aspects—namely, by the *National Review* for December, 1889, and by the *New Review*, Nos. 6 and 7, referred to in THE LANCET, No. 3459. The divergence of opinion thus shown to exist among capable observers and writers being absolute, it may here be well as a possible step towards a surer footing to allude briefly to some at least of the points in respect to which such difference is most pronounced. Opinions differ as to the existence of rabies as a definite and specific disease due to one particular cause, and to it only, or as an attendant condition in the course of various maladies. That the condition so described has in particular instances arisen idiopathically is attested by observers whose statements are beyond question. The reliability as a crucial test of intracranial inoculation as practised in Paris is by no means generally accepted; on the contrary, effects identical in character have followed similar inoculation from healthy dogs with innocuous matters and from the preliminary operation in the absence of any inoculation. That rabies has its periods of epizootic prevalence and its periods of dormancy, or absence, is generally acknowledged. So also is the circumstance that these periods bear no apparent relation to the adoption or otherwise of measures specially directed against the condition or disease so named. The affection may be endemic—that is, limited to particular districts or localities, their conditions differing in no respect from those of adjoining localities from which it is absent. The ratio of animals affected after having been bitten seems to vary in different outbreaks and localities. Nor does the theory of specific poison and of germs in relation to rabies meet with unquestioned acceptance. The affection itself having yet to be proved specific in its nature, so the speciality of its products has to be demonstrated. With respect to germs, “we, as clinical investigators, will not follow the pathologists into finding micro-organisms in almost every disease, and in assuming that because a micro-organism is *post hoc*, therefore it is *propter hoc*.”¹ Nor does M. Pasteur assert that he has discovered a special microbe peculiar to rabies.

Direct evidence that rabies is an infective disease is not forthcoming, and meantime analogy is opposed to the theory of its being so propagated. That hydrophobia in man is not the same affection as rabies in dogs may be taken as demonstrated by fully qualified observers. To assume, therefore, that the “poison” of the one affection would produce the other, or *per contra* protect against it, would be to believe that in the realm of disease the course of nature through all other departments of organic life was departed from, and “like” produced that which is “unlike.” In his earliest experiments of inoculating rabbits with saliva of rabid animals, M. Pasteur declared

that the resulting disease in the rodents was not true “rage.” In that opinion he was supported by MM. Galtier and Vulpian, while M. Guérin gave to it the name of “la rage Pasteur,” declaring at the same time that in the ordinary course of events rabbits never suffered from rabies. Subsequently the disease produced in them was variously described as “septicæmia” and “cerebro-spinal meningitis.” Notwithstanding what has been adduced to the contrary effect, Friach and other capable observers assert that dogs inoculated in the laboratory have not proved refractory to “street rabies,” and that those inoculated intracranially have seldom escaped with life. The latter method has no relation whatever to the manner in which rabies is believed to be conveyed by one dog to another. According to statistics furnished by M. Pasteur, out of twenty-five “vaccinated” dogs five died, equal to 20 per cent., and of five unvaccinated three died, equal to 60 per cent. But many dogs remain unaffected by rabies, even when hydrophobia virus has been injected into their blood, and the proportion which suffer from that affection as a result of bites, according to the *New Review*, is only a fraction over 3 per cent., as against the figures of M. Pasteur. But, as expressed by that *savant*, “experiments performed on animals are in their results contradictory of each other.” Of the cases recorded as of hydrophobia, their characteristics differ among themselves. A large number of those so designated have, doubtless, followed bites by dogs, rabid and non-rabid, even when the part bitten was protected by clothing or other covering. But outbreaks of hydrophobia often follow those of rabies at such intervals of time as to indicate an absence of direct relation between them as cause and effect, and in a very considerable number of recorded instances hydrophobia has occurred in the absence of a bite or other physical injury. Besides these and various other circumstances adverse to the theory that the affection is due alone to rabic virus, it is contrary to experience in reference to the action of contagious poisons that the intervening period between inoculation and development of results should be so variable, and often so protracted, as it practically is in hydrophobia. Mental emotion and temperament also play important parts in respect to the occurrence of the affection, or, at least, a train of phenomena undistinguishable from it. In cases of wounded men, also, traumatic tetanus has simulated hydrophobia, and in certain recorded cases symptoms similar in character to those which have followed bites or other injuries have occurred in the absence of either.

Whatever be the theory in accordance with which the anti-rabic method of inoculation has been adopted, its practice is equally opposed to analogy. Variolous inoculation, originally employed in China, had for its object the communication of small-pox to the human subject while still young. But we now neither inject the virus of scarlatina nor of diphtheria. In vaccination the material used is in its nature specific, resulting from natural vital process; it is taken from the living subject, it produces specific results, and protects against an analogous but not identical disease. In the Pasteurian method so-called the material employed is the result of disease artificially produced by means of violence; it is taken from the dead body, and bears no relation in its nature or property to vaccine properly so called. According to the theory of germs and specific poison, either the material used as an antidote against rabies must be weaker than that which has to be combated, or it must be stronger. On the former assumption it is against experience to find the feeble overcoming the powerful. If the latter, it amounts to supplementing an already dangerous amount of poisonous material in the system by an additional quantity of poisonous material—a practice opposed alike to analogy and to reason. Moreover, neither is rabies of the laboratory identical with or analogous to hydrophobia.

From the circumstance that repeated changes in and modifications of the anti-rabic inoculation have taken place, it is practically impossible to appraise the precise value or otherwise of the several methods temporarily used. But, according to the report of the Special Commission, “deaths have occurred under conditions which have suggested that they were due to the inoculations rather than to the infection of the rabid animal” by which the person had been bitten. That the system of inoculation which bears his name was deemed by M. Pasteur to be unnecessary appears by a letter addressed by himself to Dr. Rigaut, dated Nov. 16th, 1884, to the effect as

¹ Brit. Med. Journ., July, 1884.