above. Here operations were out of the question owing to the dust and flies. The mules were quartered in a gully close by, and the plague of flies was quite beyond description.

As very many of the men had artificial teeth, and these were constantly getting broken or lost, it was necessary that some steps should be taken to stop the wastage of men from this cause. We had a dentist attached to the ambulance, so we sent to Egypt and got a complete outfit for making dentures. In a small dug-out beside the dressing station the dental officer, with two mechanics, kept the teeth of the whole division in repair, and also made new dentures for those who required them, either from loss or breakage of teeth. The biscuit was so hard that artificial, or sometimes even natural, teeth were liable to injury.

In conclusion, I would like to make quite clear that, so far as we could see, the Turks did not deliberately fire at the medical personnel. The position we occupied on the beach was such that we could not expect to be spared. Indeed, for the greater part of the time we flew no flag, being of opinion that to do so was unjustifiable owing to the proximity of various points of military importance.

The tows of wounded going off to the hospital ships and the hospital ships themselves were never shelled. Only once to my knowledge was a tow of wounded fired on, and that was by a machine gun which was presumably manned by Germans. This incident occurred immediately after a successful attack in which the Turks had lost very heavily and it was not repeated.

THE RECRUIT'S HEART.

BY

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AFTER the outbreak of war numbers of young men were sent to me for my opinion, as they had been rejected for the army because of some affection of the heart. Most of them were perfectly healthy, and their rejection was due to the presence of some manifestations, physiological in character, which the medical examiner had taken for a sign of disease. I drew up a memorandum embodying the more prominent signs which were liable to be misunderstood, and giving in brief compass simple methods for differentiating the manifestations of a healthy heart from those due to disease. As I was desirous, like every other man, of contributing what little my special knowledge might help the common cause, I submitted this memorandum to the head of the medical staff at the War Office, scarcely daring to hope that any notice would be taken of it. It came, therefore, as a surprise when Sir Alfred Keogh suggested the issuing of the memorandum to the medical examiners of recruits.

In the memorandum I had endeavoured to give, in clear and concise language, the results of long years of careful investigation, because in the circumstances any more elaborate statement would not be likely to receive much attention. Many people have told me that they have found this memorandum of the greatest service, but others have criticized, and in doing so have contrived to muddle up that which I tried to make clear. Under ordinary circumstances I should not have taken notice of these criticisms, because it was evident that the critics neither understood the grounds on which the statements in the memorandum were made, nor appreciated their practical significance. But in the *JOURNAL* of November 20th (p. 744) Sir James Fowler has felt called upon to muddle up things still more, and as he occupies an official position connected with the War Office, and it seems that doubtful heart cases are referred to him, his remarks call for notice.

In dealing with his comments I must go outside the subject under immediate discussion, and show that Sir James Fowler's views are not based on observations that really bring the essential facts to light.

The manner in which I acquired the information which is summarized in the memorandum was as follows: When I entered general practice in 1879, I did so with the airy confidence of the young man fresh from the medical school imagining that I was well equipped for my work.

In a very short time I realized that I was ignorant of the very essentials of my work. In heart work, like other young men, I flattered myself on my cleverness with the stethoscope in detecting and naming all sorts of murmurs, but when it came to telling their significance, I was completely at fault. I turned to my books for instruction, to find that this very essential knowledge had been ignored. Realizing my ignorance of the meaning of the simplest symptoms, I set myself the task of finding them out.

To do this, I took careful note of my patients who presented any abnormal sign. I carefully differentiated each phenomenon from allied conditions. I noted all associated phenomena. I kept a large number of people under observation, watching them for a great many years, noting how they bore themselves through the strain and stress of life, in their labours and in their illnesses. As years went on, I found it necessary to seek other methods of investigation, and had to devise them. In this work I followed a large number of individuals for ten, fifteen, twenty, or even thirty years to find out the significance of some sign.

I was continually analysing my results and testing my conclusions, so that, at last, out of all this work, some clear issues were perceived. I was able to recognize why certain people with murmurs and irregularities enjoyed perfect health and lived laborious days with no sign of weakness; I was able to recognize also why others drifted to death or lived in a state of impaired health; in other words, I was able, in a great many cases, to make good that deficiency in my education, and was able to estimate the value of many signs and symptoms. I am still engaged in this work, for every day I realize my ignorance.

I want each reader who is capable of reasoning to consider the meaning of the methods I pursued. Knowledge in medicine is not limited to the perception of an abnormal sign, nor to the detection of its cause, but should include the knowledge of what effect the cause of an abnormal sign has on the individual's future life. Every reasonable person must accept that as a prime necessity in a physician. By what other means than that which I adopted could this necessary knowledge be acquired? It is because physicians do not adopt these methods that the profession is absolutely ignorant of the prognostic significance of any sign that may be present for a long period of years.

Compare the method I adopted to acquire a knowledge of the meaning of symptoms with that of Sir James Fowler, as recorded in his article in the *JOURNAL*. At the time he wrote this article (twenty-five years ago) he was placing on record the basis of a belief which has apparently been his guide up to the present. The article itself is characteristic of the writings of that period—vague, immature, insisting on the immaterial, and neglecting entirely the essential. It is evident from the fact that Sir James Fowler now quotes this article that he is unaware of the great advance that has been made during the last twenty-five years in affections of the heart.

To bring clearly before the reader the futility of Sir James Fowler's conception, let him consider what is the essential question which the doctor has to answer when the individual presents himself for examination—the recruit for instance. A great many people do not realize what this essential is. If a murmur is detected, the essential question is, What bearing has the cause of this murmur on the recruit's future? Or, more precisely, Does this murmur indicate heart failure or does its presence foreshadow its occurrence? These are really the imperative questions (directly asked or implied) demanded from every medical examiner.

If the reader will turn to Sir James Fowler's article and take that portion written in his youth, or the whole matter with the comments added in his maturer years, he will find that he can get no suggestion of help to answer these essential questions, nor is there the slightest indication how the value of a murmur should be estimated.

As illustrating how firmly established is the superstition that a heart to be normal must be free from murmurs and irregularity, I cite two recent cases, not by any means exceptional.

In August, 1914, a lad of 19 was sent to me by his doctor with the statement that he had been rejected for the

army because of valvular disease of the heart. I examined him, found his response to effort good, the heart normal in size, and a well-marked systolic murmur at the apex. I gave him a certificate saying that the heart was perfectly healthy, and that the murmur was physiological, indicative neither of disease nor impairment. I told him to take this certificate to some other recruiting centre than that at which he had been rejected. This he did, was accepted, joined the army, went through his training, carrying his pack and enjoying his route marches. In course of time he was in Gallipoli in the trenches leading a strenuous life and feeling fit. One day in May a shell exploded near him and he was blown over and became unconscious; he was picked up and taken into hospital. After he recovered consciousness he felt quite fit, but, unluckily, a medical officer auscultated his heart, detected a murmur, and at once diagnosed valvular disease of the heart. He was kept on his back for a considerable time, shifted from hospital to hospital, repeatedly examined by medical men, and given treatment, till finally he was examined by a Board in August, 1915, and invalided out of the service because of valvular disease of the heart. He called to see me a few weeks later; the heart was exactly in the same condition as it was a year before, normal in size, with a physiological murmur, as sound a heart as there is in the British army.

A short time ago a highly placed medical official brought his son to see me, as the lad suffered from irregular heart which was resistant to treatment. I found the irregularity to be of the youthful type, and I explained very minutely to the medical official that the irregularity was purely physiological, and I demonstrated to him by what means I separated this form of irregularity from all others, and told him how it was frequently present in the most healthy hearts. A week later he wrote asking me if I could not give him some drug, such as digitalis or strophanthus, to remove the irregularity, as it still persisted. I again wrote, clearly explaining that the condition was a purely physiological one. Nevertheless, he replied that he himself had given the lad a tonic which he hoped might "cure the irregularity."

NOTE ON

A SUPPOSED SOLUBLE TOXIN, PRODUCED IN ARTIFICIAL CULTURE BY THE BACILLUS OF MALIGNANT OEDEMA.

BY

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SOME months ago we made some experiments on the nature of a toxic substance occurring in the filtrates from cultures of the bacillus of malignant oedema, an organism which has acquired special importance during the war, as being one of the spore-bearing anaërobic bacteria concerned in the production of gas gangrene from soil-infected wounds.

Our observations were limited to the cultural filtrate from one particular strain, for the culture of which, as well as for a quantity of the reputed toxin, with which many of our experiments were made, we are indebted to Mr. J. B. Buxton, F.R.C.V.S. The strain was originally obtained from a case of gas gangrene, and its correspondence in all diagnostic points with the true bacillus of malignant oedema has been confirmed by several bacteriologists. From the muscles and areolar tissue of guinea-pigs dying of infection with an organism of this type Roux and Chamberland expressed a juice which, after sterilization by filtration, killed other guinea-pigs with acute convulsant symptoms. Besson prepared a similarly toxic filtrate by growing the organism on sterilized meat, which the digestive activity of the organism rapidly liquefies. The filtered juice of a five or six days culture kills medium-sized guinea-pigs with acute symptoms in a dose of 3 to 5 c.cm. given hypodermically. It is a juice of this kind which has been regarded by Besson and others as containing a characteristic toxin produced by the organism, and attempts have been made to prepare an antitoxic

serum by immunizing animals against it. A broth culture of the organism, however abundant the growth, will not yield a filtrate of such potency.

The material used by us was obtained by growing the organism for five to six days at 37° C. on minced ox-heart, previously sterilized in the autoclave. The culture was started in hydrogen and provision made for the escape of gases, which the culture soon produced in abundance, by a tube leading from the cultural jar into a trap filled with sulphuric acid. The semifluid, evil-smelling product was filtered first through paper-pulp, and then through a Berkefeld candle. A nearly clear brown filtrate was obtained.

This filtrate produced the acute intoxication described by Besson when injected into guinea-pigs, the fatal dose for an animal weighing about 300 grams varying from 3 to 5 c.cm. with different preparations. It was at once evident, however, that its toxic constituent had none of the characteristics of a true bacterial toxin. The relatively large volume of the filtrate required for the production of general symptoms and the almost immediate onset of the symptoms, when the dose was sufficient to produce them at all, suggested rather the non-specific action of some product of putrefactive digestion, and the necessity of using solid meat as a culture medium was suggestive in the same direction. The suggestion was confirmed by the observation that the toxicity of the product was but little affected by brief boiling. Dr. Walpole filtered the fluid under pressure through a collodion film, which was impervious to diphtheria toxin, and the filtrate showed the full toxicity of the preparation.

The symptoms produced are both local and general. Locally a large, oedematous, haemorrhagic infiltration is produced in the subcutaneous tissue and underlying muscle, in a considerable area surrounding the site of injection. With a non-fatal dose this local effect may be the only one, except for a vague malaise. To the general intoxication small guinea-pigs are much more susceptible than larger ones. Thus 3 c.cm. may cause the death of a guinea-pig weighing 200 grams in 10 to 15 minutes, while 6 c.cm. of the same preparation may cause little more than a local infiltration in a guinea-pig weighing 400 grams. The general symptoms comprise tremors, weakness, and excitability, rapidly passing into extensor convulsions, which usually terminate in death within fifteen minutes of the injection. *Post mortem* the lungs are found to be oedematous, with areas of haemorrhagic consolidation, and the trachea contains a blood-stained froth.

Though the toxic principle resisted brief boiling, it was rapidly lost if boiling was prolonged, and no concentration of the activity could be effected by evaporation, even at a low temperature and under diminished pressure. The toxic substance was evidently very unstable, or highly volatile. Omitting experimental details, we may say at once that we found that the toxic substance could be distilled readily from the fluid if the latter was made alkaline, and could be collected in acid; but that it was held back if the fluid was made acid to Congo red with sulphuric acid. We were dealing, therefore, with a volatile base. We accordingly made a quantity of the "toxin" alkaline with excess of potash and distilled into normal sulphuric acid. The quantity of volatile bases obtained was surprisingly large. From 150 c.cm. of collodion-filtered "toxin" we obtained volatile bases neutralizing 113.2 c.cm. of normal sulphuric acid. The mixed sulphates, obtained by evaporation of the distillate, were exhausted with absolute alcohol, in which ammonium sulphate is even less soluble than ammonium chloride. From the part insoluble in alcohol, consisting of ammonium sulphate, we obtained ammonia equivalent to 96 c.cm. of normal H_2SO_4 ; the organic bases, with sulphates soluble in alcohol, were therefore equivalent to 17.2 c.cm. of normal acid. The "toxin" was therefore 0.64 N. in respect of ammonia, and 0.115 N. in respect of other volatile bases. The alkaline residue left after distillation contained large amounts of soaps, and from these, by acidifying and distilling with steam, we obtained volatile fatty acids in quantity corresponding to a normality of 0.75 in the original fluid. This accords with the observation that the "toxin" has but a feebly alkaline reaction.

The question of practical interest was the identification, among the volatile basic constituents, of the toxic principle.