

urine. Frequency becomes acute and may ultimately reach virtual incontinence. The ureters are affected by back pressure arising from the contracted bladder or fibrosis and stricture of their lower ends, and hydronephrosis and hydro-nephrosis result. Renal impairment becomes progressive, with ultimate death from uraemia.

The only way to save a patient so affected is to divert the urinary stream from the bladder to the colon or skin, and this should be done before hydronephrosis is established. Bilateral renal involvement with tuberculosis or the presence of a solitary diseased kidney is not necessarily a contra-indication. The urinary diversion may not save life, but it will certainly prolong it and ensure some measure of comfort. Transplantation of the ureter or ureters into the colon is now a well-tried procedure and gives the minimum of discomfort and after-trouble, but in a very occasional case it may be preferable to bring the end of the ureter to the skin surface. The difficulty here is the collection of the exuding urine without leakage with all its discomforts. In such a case the formation of a skin-covered spout, or nipple ureterostomy as described by Band (1950), makes leakage unlikely and may add greatly to the patient's comfort and well-being.

Epididymectomy

Tuberculosis affecting the epididymides, the seminal vesicles, and the prostate can result from a direct blood spread, but often it follows infection of the prostate direct from the prostatic urethra. Although this paper concerns urinary tuberculosis, it must be remembered that at least 50% of cases of genital tuberculosis result from a renal source by direct spread through the ureter, bladder, posterior urethra, prostate, and seminal vesicles to the epididymides. Hence it follows that in any case of chronic epididymitis a complete investigation of the urinary tract is necessary. When a focus is found in the kidney, nephrectomy, and possibly ureterectomy, followed by sanatorium treatment, may be enough to arrest the disease, but in some cases epididymectomy will be required in addition.

Conclusion

Might I summarize by again repeating that tuberculosis of the urinary tract is a focal manifestation of a generalized infection, and thus investigation and treatment must not be confined to one system. The principles of treatment are by rest in suitable environment, adequate nutrition, and the aiding of the body's defences by suitable antibiotics and chemotherapeutic measures. When the local lesion is demonstrable general measures are never enough and surgical extirpation is essential, but it must be undertaken only when the patient has reached the optimum condition likely to be achieved by a sanatorium régime, and it must be followed by the prolonged rest and treatment required to ensure against recurrence of active disease. Finally, it cannot too often be reiterated that surgery alone is not enough; it can only be regarded as one of the weapons in the attack on this major killing disease.

I desire to express my thanks to my friends and colleagues, Mr. D. Band, Mr. W. M. Borthwick, and Mr. A. Jacobs, who have been pioneers in the modern investigation and treatment of genito-urinary tuberculosis, for their willing help and permission to quote from their experience.

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CLINICAL PSYCHOLOGY AND SOME FORGOTTEN EPISODES*

BY

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We can all talk about psychology now, though when I qualified 50 years ago one rarely heard the word. Since then I have been carried along on a stream of events related to clinical psychology and with a curious repetitive pattern. These I propose to recapitulate, with the intention of drawing a moral from them.

The Hysterical Symptom in Wartime

The stream caught me when in November, 1914, I joined the R.A.M.C. and became the surgical member of a board in London for sick and wounded officers. At that time the war neuroses had not been recognized and shell-shock was not yet invented. Many of the officers we boarded suffered from functional troubles, chiefly hysteria, and brought to us diagnoses of physical disorders that for me did not exist. I was taken aback, for I could not believe I was a better diagnostician than the élite of Harley Street. I am less diffident now, but I did not realize then that to make a correct diagnosis—in one case or many—would set up the insoluble problem of what to do next. Nobody likes insoluble problems, and my attempts to draw official notice to what was happening did not meet with encouragement. In the new year I was transferred to the Alexandra Hospital, near Portsmouth, as a surgical specialist, but before that, having seen how frost-bite (later called trench-foot) was carrying a heavy load of hysteria, I joined my old colleague E. G. Fearnside in examining a batch of men in hospital with that diagnosis, and we published a paper in 1915 declaring that many of them showed hysterical symptoms. So far as I know this was the first mention of war neuroses in our literature.

At the Alexandra Hospital, if I made the diagnosis of hysteria I had to face for myself the problem of what to do next. Thus when I insisted that a man in my ward, supposed to have tuberculous disease of the tarsus, really presented a hysterical clubfoot, for which I could do nothing, I was assured that the War Office would not accept the diagnosis of hysteria as an invaliding disability and I must board him out as a sufferer from tuberculous disease. I cannot detail the manœuvres that led to his being invalided out with the correct diagnosis, but within a few weeks he had re-enlisted and his new M.O. wrote to us asking what had been his disability. Soon after this, when a similar case appeared before an invaliding board as talipes varus following osteoarthritis, and I demurred at putting my name to that diagnosis, the man was sent to my ward to be dealt with as best I could. After a time the M.O.s of neighbouring units sent their tricky cases "for surgical opinion and advice." I found myself carrying on an out-patient clinic and sometimes obtained satisfactory results by common-sense methods—methods made up of persuasion and exhortation aided

*This paper is based upon a B.M.A. Lecture given to the Norfolk Branch of the British Medical Association on April 9.

by a certainty of surgical diagnosis and the confidence born of psychological ignorance. In 1916 I published an article on practical hints on functional disorders, and the following case, there described, shows how I had reached a dangerous facility in removing the presenting symptom of a severe psychoneurosis:

An officer was wounded in the right arm; the musculo-spiral nerve was said to have been sutured, and he was treated by massage and electricity for seven months (for this I was partly responsible). His hand and forearm were then as if carved from wood, without movement and anaesthetic. He was to appear before a medical board in a fortnight, and feared being invalided out of the Service. His condition was at last recognized, a straight talk followed, and the board passed him as fit for general service, his musculo-spiral nerve being found to function perfectly.

He wrote me a grateful letter from France, but I read the rest of his story in a daily paper. He was invalided home bedridden from paralysis, and, after a visit from a specialist, made his way to another room, found his revolver, and shot himself. A retrospect brought up some traits—overscrupulousness, hesitancy of speech, peculiar tics—suggesting pathological doubts and fears that finally defeated him.

Shell-shock

Service in France kept me busied with gas gangrene and such-like, but I became acquainted with shell-shock and was able to note the change-over of opinion about it. How many of you know that its victims appeared in the casualty lists as "wounded, shell-shock," and wore decorative wound-stripes to indicate that they had suffered by enemy action? It was assumed that the trouble was due to physical disturbance of the central nervous system, and the literature of the period gives us theories about separated synapses, dissociated cerebral centres, carbon monoxide poisoning, and what not; and on the German side a similar assumption was made. But here was a mass epidemic that, unlike the individual cases of hysteria, could not be ignored; the war might have come to an untimely end if, on both sides of no-man's-land, the soldier who had reached his limit of emotional endurance could be hurried home and greeted as a wounded hero. So the official use of the phrase shell-shock was prohibited, and instead of the wounded-hero tune we heard, "Shell-shock? I'd soon cure shell-shock. I'd shoot every man that had it." This met the need for the speaker to express moral indignation about what he couldn't understand.

Towards the end of 1917 I came home on leave, and whilst airing my views met the late Colonel Aldren Turner, then in charge of what we should call Army psychiatry. Taking me seriously, he said the Army had more surgeons than it needed but he could not find men to look after his people. Would I drop surgery for a time and help him? If so, he would send me to Maghull for three months' training, for the war had revealed some lamentable defects in our medical education, and perhaps mine was not perfect. Curiosity sent me to Maghull, and I soon had to admit the existence of much clinical material about which I had been taught nothing. This was apart from modern theory, which cleared up some of my problems and offered methods of treatment conforming to the basic dogma that two and two must make four.

D.A.H.

Now followed a long spell in neurological hospitals. There my men told me about their faintings and palpitations and shortness of breath, and I noted their blue hands and pulses up to 150 or so. I rarely examined a heart, but would tell a man his nervousness was making his heart go all floppy, and when we had dealt with the nervousness the

heart would come all right. I did not say there was nothing the matter with the heart, and so far as my memory goes the men accepted my explanation. One evening, in an idle moment, I picked up an article on disordered action of the heart and received another shock. This was what half my men suffered from and I hadn't known it. Only as time went on could I believe that it was pure chance whether a man was sent to my hospital as a neurotic (I have to use that word, though I dislike its mixed flavouring of mythopathology and moral judgment) or to a heart hospital as suffering from D.A.H.

Of this latter condition Sir James Mackenzie (1925) wrote: "Hospitals for the investigation of the heart were set apart; groups of doctors with special knowledge were detailed for this investigation, and every conceivable device was employed. For several years such investigations were pursued with the greatest industry, and if to-day we seek for the increased knowledge of the heart that has accrued it will not be possible to detect the slightest gain."

After some experience on pension boards I wrote an article (1920b) that ended: "But the majority of cases found among pensioners and diagnosed D.A.H. are entirely of psychogenetic origin. If this fact could be generally recognized it would aid in the restoration to social usefulness of some of the men who now regard themselves as permanent invalids."

In the recent war D.A.H. was bound to recur. There was general agreement to call it "effort syndrome," and after some rearguard skirmishing it was made a responsibility of the psychiatrist. Its numerical incidence was very low by comparison with that in the earlier war.

Railway Spine and Traumatic Neurasthenia

So far shell-shock and D.A.H. stood by themselves as mass diagnostic errors, but after I published (1920a) my M.D. thesis on the war neuroses I received a letter from Herbert Page, written with a noticeable tremor, saying he had read my book, that all the symptoms of shell-shock would be found in his book, *Railway Injuries* (Page, 1891), but the lesson he had tried to teach had been forgotten. His book had to do with Erichsen's "railway spine," a condition still mentioned in my student days but attributed to traumatic neurasthenia. Erichsen (1877) had described at great length symptoms that I have seen only in a ward full of titubating shell-shockers, and he invented theories of concussion of the spine, meningeal irritation, ascending or descending degeneration, meningomyelitis, and such-like. His position made his views authoritative, and it took years of controversy to destroy this fancied pathology, which was, however, buried so obscurely that but for Herbert Page I should never have known about it. I had the privilege of meeting Page, and in our talk about railway spine I mentioned psychology. He shook his head. "I don't know anything about psychology," said he; "I only knew it was something the matter with the man himself."

Traumatic neurasthenia was a diagnostic stand-by for years after railway spine was blown upon, but Sir Farquhar Buzzard (1923) attacked the idea that "an injury of any degree of severity and inflicted at any site may produce a physical condition of the nervous system, with characteristic signs, but without any morbid anatomy, to which the name traumatic neurasthenia is fairly applicable." I once examined in a Midland town 10 miners receiving compensation for the disorder, and wrote up the experience (Culpin, 1931), for each case was a museum specimen. One man, for example, received an injury to his back in 1925, and I was shown a sheaf of reports about his back and his nerves. At the interview with me he complained that he was not feeling well. "My back gives way when I walk. I burst into a sweat if I slip. I get pain in the head on bending. The strength goes out of my legs. I can't stand the noise of picture shows. I get a pain in the privates." I asked, "Do you feel anything else the matter?" and pressed the

question without eliciting further symptoms. His wife was there, and I asked her, "How does he sleep?" She told of how she wakes him up because he makes all sorts of noises. The man, asked about these noises, now said: "I dream of the accident, and the wife wakes me up," then admitted that he was restless and afraid in a room by himself (claustrophobia), was frightened in the dark. "If I meet a cat or dog in the night I burst out all of a sweat. I'm nervous in a bus. On the pavement I keep thinking the buses are running into me." Asked whether he could go down the pit if his back were all right he said, shamefacedly but sincerely, "It's not a nice place to go when you are frightened." A recognition of this man's disability might have saved years of waiting for him to go down the pit again.

I recorded in 1943 how the failure to value properly the importance of neurotic symptoms in relation to coal-mining sent three "Bevin boys" to prison. In one case the secretary of a miners' lodge said the lad was pit-nervous and it was unsafe for him to go down the pit. "Pit-nervous" is good.

Traumatic neurasthenia has faded away. I regret that anxiety neurosis, which once had a meaning, has so far taken its place that my evening paper lately reported Mr. Justice X as causing loud laughter in court when he said, "The typical cause of anxiety neurosis is the anxiety of the plaintiff who wishes to recover damages."

Night-blindness as an Epidemic Artifact

I have described (Culpin, 1933) hysterical night-blindness in connexion with miners' nystagmus, and as an extensive epidemic in the Continental armies during the 1914-18 war. Alarmed at possibilities here, I wrote (Culpin, 1940) a warning that if we talked enough about vitamins and night-blindness we might start an epidemic. "Then," said I, "psychopathologists will declare the nature of the disorder and will be derided at first; but when the epidemic is well established and the cases are thoroughly ripened they will be called upon to treat it."

It all came true. In 1941 our newspapers carried pictorial advertisements showing how the nocturnal streets looked to the victims of night-blindness, and telling them what to cure it with. Radio comedians made gags about it, vitamin fans ate carrots till their skin turned yellow, and at least one scientific-looking article, full of graphs and what-not, treated the disorder as if it were a result of eating food deficient in vitamins. Then Dr. E. Wittkower and his team (1941) in Glasgow, examining some 50-odd soldiers with this epidemic night-blindness, found it was only an excrescence upon deeper troubles and that "most of them, quite apart from their night-blindness, were unfit for military service because of their psychological abnormalities and disorders." Dr. F. M. R. Walshe (1941), taking his share in the fulfilment of prophecy, was delightfully derisive of this work. Then an official circular was sent to Army ophthalmologists telling them that for practical purposes night-blindness did not exist in the absence of gross ocular disease and any difficult cases should be referred to a psychiatrist. To my prophecy I should have added that the affair would be forgotten as soon as possible and some people would deny that it had ever occurred.

Unconsciousness and Deep-diving

Such episodes as these do not arise through lack of knowledge. Was not Erichsen a president of the Royal College of Surgeons? They are due to a wrong but rarely questioned philosophic outlook which at this time of writing I can only declare to exist. Behind it is a long history, which I have set forth (Culpin, 1945).

This outlook is manifest in almost any issue of any of our professional journals, but an attempt to discuss a specific example might appear as a personal attack upon a writer. In one example, however, in which I was concerned, the

same problem was tackled by opposite methods, dependent upon what may, for the time being, be called respectively the animistic and the mechanistic outlook, and both methods are on record with their results. The animistic outlook is still so unfamiliar that I must give the whole record from the original publication (Culpin, 1933) (permission to quote this has kindly been granted by the honorary editors of the *Proceedings of the Royal Society of Medicine*):

"To increase the depth at which divers work, Sir Leonard Hill has invented a decompression chamber which the man enters on his way to the surface,* his helmet is removed and he receives refreshment, whilst the chamber is sealed and hoisted on deck, where decompression can be carried out at leisure, thus avoiding the danger of caisson disease. By this means a depth of 300 ft. can be reached with safety. But unexpected things happened; some men carried out the deep diving successfully, others sent up erratic signals by telephone or lifeline and on return to the surface claimed to have been unconscious. One man to whom that had happened demanded to be pulled up on another occasion before he reached his depth, and declared that he had seen his own face looking at him through the window of the helmet.

"Physiological theory could not account for these happenings and the Medical Research Council, when approached about the problem, suggested a psychological attack upon it. I was asked to undertake this and, with the assistance of Surgeon Lieutenant-Commander Phillips, I examined half a dozen divers, of whom two had suffered this unconsciousness.

"From the first of these two, a well-built healthy Petty Officer, I elicited the following symptoms: Always nervous of doing the wrong thing, even when being dressed for diving. 'I don't like to attract attention in the street.' Would not go into a strange tea-shop alone, would feel that everyone is watching him. Would rather go hungry. Never afraid of the dark, but of closed spaces—feels sealed in. The terror (his own word) came on first when skylarking, when other chaps fell upon him.

"Plays football and hockey. Useless in the water. 'I don't like it.' Would bathe in the sea but not go out of his depth. Yet has passed his swimming tests.

"Did quite well at ordinary diving; fear of being closed in came on badly once but passed away. 'It came on that time just before I went off on the bottom. That stirred it up and I've had it off and on since.'

"Describing his dive, he said: 'I felt dizzy at 40 (fathoms); at 45 felt in a nightmare. Felt like going under ether—that made me think of going unconscious. I had the feeling of closed in, and went off. When I came to daylight I came to my senses,—i.e., about 100 ft.

"Then he described his second attempt, when he experienced various hallucinations and had to be pulled up. I now put him on a couch and directed him to go over the first descent and talk of it as if it were happening now. With some urging on my part he repeated the performance and recalled with hallucinatory vividness what seemed to be the whole of the period for which he had claimed unconsciousness. He cried out, 'Pull me up, for God's sake pull me up'; 'I feel as if I'd never go up'; 'I'm tied on the bottom. My mouthpiece is caught under my nose. It's getting lighter now. I can see the chamber.'

"The case is clear. The man was a claustrophobe and his 'unconsciousness' was a state of *Angst*.

"The second man, an engine-room artificer, provided less striking symptoms. He has the feeling of being watched, but has got over the tea-shop fear. He takes measurements two or three times at his work; says, 'There's always the possibility of trouble.' Was afraid of the dark but has got over it. Not keen on being in a crowd (a mild phobia). Has good days and bad days. 'If things have been going wrong, I wonder what's going wrong next.'

"He described one descent: 'All right till I touched fifty fathoms. Thought I was all right. Did my job. Didn't feel very bright. Was proud of myself. Was about to look round at the light and knew nothing more. They say I gave correct signals.'

"Like the other man, on a second attempt he demanded to be pulled up.

"I postponed the attack upon his amnesia till I secured the presence of my colleague, Dr. A. E. Phillips, when I asked him

*Out of respect to the memory of Sir Leonard Hill I must record that he reproved me for carelessly attributing this invention to him instead of, if I remember aright, to Sir Robert Davis, of Siebe, Gorman and Co.

to take down everything I said and everything the man said.* Using the same technique as before, I induced a hypnoidal state in which he recapitulated the whole episode with minute detail and appropriate actions but little emotion. The verbatim account occupies 1½ foolscap pages, and I only spoke 41 words after he began. He reproduced the conversation over the telephone, and, by an extraordinary piece of luck, Dr. Phillips had been at the other end of the telephone during the actual dive and was able to affirm the verbal accuracy of the reproduction. I think that makes the experiment unique.

"So far, then, we could assure the authorities that the manifestations were not due to any defect in their physiological theory or practice—an important matter to them. That physiologists welcomed the psychological explanation is perhaps also unique.

"It was obvious, moreover, that the presence of a minor psychosis could have been recognized beforehand in these two men. This was being wise after the event, but in the following year Dr. Phillips was still in medical charge of men selected for training in deep-diving, and decided to put his psychological experience into practice. He 'vetted' five men and asked me to vet them independently. We both agreed that one of them might readily fall a victim to the occupational neurosis, but for various reasons decided to let him carry on. His most important symptom in my view was a phobia of horses. The result of the dive to 300 ft. is described by Dr. Phillips in a paper given to the United Services Section [18]. The man demanded to be brought up, and emerged from the chamber in a state of collapse. Dr. Phillips decided to make the man abreact the experience and the dramatic result is described in his account:

"With sweat like a stream of water running down a face the colour of chalk, dilated pupils and rolling eyes, he went through all the emotions of the dive, sobbing and tearing at his clothes under the impression that he was again in his diving-suit, and clawing at his face to pull off the imaginary face-glass. It was a picture of stark mad terror and the impression it left is very difficult to describe. No earthly power could then have got him near a diving-suit.

"The interview lasted until about 10.30 (from 9.0), and when he had "come to" again there was an immense improvement in his condition. The deep sigh which heralded in the return to external consciousness was indicative of the dam which had been loosened."

"Such abreaction I regard as essential for the prevention of further symptoms after terrifying experiences. I have described a case [19] in which the effects of a fright during compressed-air work were ascribed to caisson disease and a severe psychoneurosis was allowed to develop which, I believe, a timely abreaction would have prevented."

The other record (Donald, 1947) tells us that "the investigations of oxygen poisoning described here were started in April, 1942, owing to the occurrence of several cases of unconsciousness in oxygen-breathing apparatus at depths and in times that were then considered to be safe." This was a restatement of the problem that had presented itself in 1930, the introduction of oxygen-breathing having made no difference to what happened. A painstaking study of the physical effects of oxygen under all sorts of conditions (time, pressure, exercise, and so on) was carried out. The symptoms, reported at length, included vertigo, nausea, and syncopal and convulsive attacks, a long paragraph being headed, "Toxic Effects of Oxygen on Brain Metabolism." No conclusion, however, was reached about why several cases of unconsciousness occurred at depths and in times then considered safe. I wrote (Culpin, 1947) suggesting that Dr. Kenneth Donald and I had been concerned in the same problem, but no discussion followed.

*Without a witness my account might have been discredited, for this procedure was then under a cloud. Now it is correct and praiseworthy, though as a propitiatory ritual the physician should put on a white coat and inject some thiopentone. Let me explain that the first diver reproduced a story I had heard so often from shell-shockers: "Then I became unconscious and I came to myself when I reached the dressing station"—or the C.C.S., or the base hospital, or wherever there was freedom from fear. This "unconsciousness" had been regarded as due to concussion and became the starting-point of the shell-shock theories, but was really the blotting out of a period of terror the memory of which could be revived by a simple technique. The diver said, "I had the feeling of closed in and went off. When I came to daylight I came to my senses." This gave me the diagnosis, which I confirmed by using the same technique as that used for the shell-shockers.

Conclusion

Common to these episodes is a particular kind of diagnostic failure. Their recurrence through the years suggests that this failure is a constant factor in medicine, and recent discussions about the prescribing of placebos show a lamentable acquiescence in it.

When I lectured to students about the psychoneuroses I found I could get them interested in psychological theory, but I assured them that, as a clinician, my aim was to enable them to elicit a patient's symptoms and recognize their significance. I had to warn them, however, that my stuff was no use for examination purposes and they had better leave it behind when they entered the examination-room.

We shall have made some progress towards remedying our failure when at a qualifying examination it becomes possible for an examiner to expect from a candidate an intelligent response to the request, "Will you please find out what neurotic symptoms that man has and tell me what you make of them?"

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SIGNIFICANCE OF TESTICULAR EXFOLIATION IN MALE INFECUNDITY

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Infecund semen often contains numerous exfoliated testicular cells (Lane-Roberts *et al.*, 1939, 1948; Baker, 1939; Moench, 1940; Joël, 1942; Staemmler, 1942-3; Belonoschkin, 1949; and others). Incidental observations had suggested to us that exfoliation was of diagnostic significance, and special attention was therefore paid during the past six years to such cases. The present communication reviews some data and conclusions which require but simple methods for their verification and which may be of interest to the clinician.

Recognition of Exfoliated Cells

By comparing the cells in semen with biopsy and post-mortem sections most testicular cells can be readily recognized as such, provided stained semen films are examined—for example, wet films stained by the osmic-acid-Giemsa method (Lane-Roberts *et al.*, 1948). The most common cells have a single unfragmented nucleus but differ greatly in size. One type has a cell diameter of 10-15 μ , with a nuclear diameter of 8-12 μ ; the chromatin may be concentrated in a skein, the cell having preserved the appearance of the spermatocytes familiar from cross-sections through the seminiferous