"THE TREATMENT OF HYSTERIA"

To the Editor:—There is so much that is good in Capt. J. M. Wolfsohn's contribution concerning the treatment of hysteria (*THE JOURNAL*, Dec. 21, 1918, p. 2057) that it is regrettable that he has added statements which negative a great deal of the positive part of his paper. His statement that "very little has ever been contributed to the treatment but volumes on the classification of hysterical disabilities" sounds peculiar when one remembers the volumes that have been written concerning hypnotism, reeducation and psychoanalysis in the treatment of hysteria. He is modest enough at the beginning to say that the symptom is merely the outward sign, and that we wrongly say we have cured hysteria when we merely cure the hysterical symptom. Yet a little later he states that he cures more than 90 per cent. of the cases within the first twenty-four hours. The old methods of reeducation, combined with the use of mechanical adjuvants, he speaks of with kindly tolerance. He states that he cures in from one-half to one hour the same patients who had electrical treatment for six months by these old methods. Arousing our interest in this way, it comes as a disappointment when we later read that his methods consist also in the use of these mechanical measures coupled with peremptory commands. To quote from one of his cases: "He was finally treated with a mild faradic current and cured after three hours of treatment." To make matters worse, Captain Wolfsohn in his rules of treatment says that every case must be studied fully, and especially psychologically, before treatment is begun, and that this can even take the form of psychoanalysis. How inconsistent this sounds in connection with his remarks in the earlier part of his paper that "he cures and completely cures more than 90 per cent. in twenty-four hours." If Captain Wolfsohn can make a complete psychologic study of every hysterical patient in one day, he is doing more than has heretofore been thought possible. However, I am sure he does not mean this but that he has allowed his enthusiasm to overrun his form of expression. Exaggeration and overvaluation of one's efforts is a hysterical trait, one of those often untouched and untreated manifestations of the hysterical psyche which the Captain speaks about in the earlier part of his paper. The importance of suggestion and the stronger influence that suggestion furnishes under military discipline perhaps explain the remarkable proportion of cure of symptoms he has effected. One gathers from his paper that he is a disciple of Babinski and a believer in the Déjerine isolation treatment but since "very little has ever been contributed to the treatment" he ignores those sources upon whose methods his own are based. In military circles one can do more than in civil life. "When the patient resists the cure," the Captain says, "this must be broken down by strong persuasion or by faradism." In civil practice, when the patient resists a cure the physician must desist because, as a rule, the patient goes to some other physician. This negatives also some short roads to a successful treatment which the Captain offers, such, for instance, as: "Do not discontinue the treatment until the cure is accomplished." And we feel that Rule 25, which says that "the secret of psychotherapy is a mental combat between the physician and the patient," is, to say the least, a rather unscientific and certainly unpsychologic point of view.

EDWARD E. MAYER, M.D., Pittsburgh.

To the Editor:—I have read the article on "The Treatment of Hysteria," by Capt. Julian M. Wolfsohn, with the hope of getting some new and really advanced ideas on the treatment of hysterical cases known on the firing line as shell shock cases. Many of these cases have been treated here in American base hospitals, with the exception that the injuries causing the condition were not received on the firing line; but these cases demonstrated all the points of interest that Captain Wolfsohn set forth in his interesting paper. In his final analysis, however, in laying down the cardinal principles of treatment, he fails, in my opinion, to state at least two, if not three, very important factors:

First, it would seem that at least many of these patients sustained some physical injury in combination with the hysteria, and that in all the cases he reports there was not

one with a purely psychoneurotic disturbance alone. It has been demonstrated beyond a doubt that the patient who originally received a physical injury followed by some form of psychoneurotic symptoms is more easily cured: there is a physical fact from which one may convince the patient that he has recovered; and now that he is physically normal, he must be nervously normal as well.

Second, in the cases reported, all the patients were ill from a few weeks to many months. During that time, while waiting for the physical disturbance to clear up, the patient has been receiving subconscious mental treatment that has not been stimulated from the standpoint of intensive mental treatment by reeducation, which is a real fundamental principle in the treatment of all psychasthenic or psychoneurotic cases. In other words, the patient has undergone a real physical treatment with nervous and mental rest, and when the psychologic time arrives, he is cured almost instantly. This is beautifully illustrated by Wolfsohn's statement that some patients were cured in a few minutes, others in several or many hours, and others after days of anxious observation, waiting for that psychologic moment to arrive. The mental attitude of the patient has everything to do with this so-called spontaneous cure.

I simply make this criticism because the ordinary practitioner will not take into consideration the fact that hysteria without an actual physical cause is not as a rule cured in this way, and as easily as those cases in which some physical violence has been demonstrated or sustained. I could add a number of almost similar results, but it would be a repetition of what has been set forth in this interesting and valuable paper.

These two points, then, should be added to the twenty-seven general rules laid down: First, that the physical condition of the patient must be perfect ere treatment is begun for spontaneous cure, and second, that the long hospital treatment makes it possible to distinguish between a hysterical case with actual physical trauma and a case of purely nervous trauma.

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**ACTION OF DIGITALIS AND ATROPIN, WITH
ESPECIAL REFERENCE TO THE HEART**

To the Editor:—Digitalis affects the heart mainly in two ways: 1. It stimulates the pneumogastric center in the medulla, the resultant inhibition lessening the number of beats of the heart by prolonging the pause in diastole. Relaxation of fibers is increased; added rest is secured. Inhibition of some impulses from auricle to ventricle occurs—hence its value in auricular fibrillation. 2. It affects the heart muscle directly, increasing the strength of the ventricular contractions.

Atropin, in sufficient dose, paralyzes the inhibitory terminations of the pneumogastric in the heart, and stimulation of this nerve therefore causes no change in the pulse after its administration. Drugs have no effect in stimulating the pneumogastric after atropin has been given in sufficient dose to paralyze the inhibitory terminations. What is the sufficient dose? Cushny says 1 mg. (about $\frac{1}{65}$ grain), and that the administration of sufficient atropin to paralyze the pneumogastric involves unpleasant dryness of the throat and difficulty in swallowing. The usual therapeutic dose of from $\frac{1}{150}$ to $\frac{1}{100}$ grain would apparently be insufficient to cause the paralysis. There is some uncertainty about this, however, as the susceptibility of individuals varies greatly.

If we grant, however, that the therapeutic dose of atropin has this effect in causing the paralysis of the inhibitory apparatus of the pneumogastric, are we justified in using it when we are using digitalis? Before answering this question we must consider what we expect or hope to accomplish with atropin. I am thinking of its indications in the extensive and progressive type of bronchopneumonia, lately so common. There is an impression among many clinicians that these patients are often relieved of cyanosis after using atropin. The cyanosis may be due to a moderate narrowing of the bronchioles already filled with mucous secretion, as

asserted by a recent writer. According to Auer in the *American Journal of Physiology*, atropin prevents death in guinea-pigs subjected to anaphylactic shock by preventing the stenosis of the bronchioles. In beginning pulmonary edema it is commonly believed to be of value and is said to have an action on the permeability of the capillaries of the lungs, stopping the outward flow of fluid from the blood. These claims are made for it by several who have had large series of cases during the recent epidemic, including Keeton and Cushman, and Ridge. If, in the doses commonly given, it affects the inhibitory mechanism of the heart, we still have the effect of digitalis on the heart muscle, which effect is not influenced by atropin.

The two drugs are commonly used without paying much attention to these considerations; but there seems to be sufficient reason for keeping them in mind more than is customary. It is quite possible that small doses of atropin may be used in persons not especially susceptible to its effects, without seriously interfering with digitalis therapy; at least that is my conclusion from the foregoing.

WILLIAM E. McCOLLOM, M.D., Brooklyn.
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"THE EFFECT OF 'GROUND GLASS' ON THE GASTRO-INTESTINAL TRACT OF DOGS"

To the Editor:—The article in THE JOURNAL, Dec. 28, 1918, p. 2127, on "The Effect of 'Ground Glass' on the Gastro-Intestinal Tract of Dogs," reminds me of a peculiar case in my practice: About thirty-five years ago a minister brought to me a diaper recently removed from an infant about 30 months old. A small quantity of fecal matter on the cloth showed a number of small pieces of broken glass mixed with it. The child had found the glass on the floor and, as usual, put it into its mouth, and finally swallowed it. There was no blood on the diaper, and, as far as could be determined, no untoward effects resulted.

G. W. H. KEMPER, M.D., Muncie, Ind.

Queries and Minor Notes

ANONYMOUS COMMUNICATIONS and queries on postal cards will not be noticed. Every letter must contain the writer's name and address, but these will be omitted, on request.

INSTANCES OF LOW TEMPERATURE

To the Editor:—The temperature recorded in the following history seems to be about the lowest of which I can obtain any record. Will you tell me what is the lowest authentic temperature of which there is any record? A boy, 8 years old, with a severe pharyngeal diphtheria of eighteen days' duration, was admitted to the Rochester Municipal Hospital. He had received no antitoxin. The admission temperature was 99 F. In the next three days the temperature and pulse declined steadily, although not equally. Seven hours before death the rectal temperature was 91.8 F. Several thermometers were used and all agreed. Fifteen minutes before death the temperature rose to 94.4 F.

P. A. B., Rochester, N. Y.

ANSWER.—The lowest temperature on record, so far as we can discover, is that observed by J. J. Reincke (*Deutsch. Arch. f. klin. Med.* 16:12, 1875) in a man who had spent part of the night outdoors, lying in a drunken stupor, exposed to a temperature of 32 F. At 8 a. m. the man's temperature by rectum was 75.2 F., and not until 1 o'clock in the afternoon of the following day did it return to normal. The man lived. Reincke also records the case of a man who jumped overboard from a transport, striking his head on a rock. He was picked up apparently dead, but vigorous attempts at artificial respiration proved successful. His rectal temperature was 91.7 F. Magnan (*Virchows Arch. f. path. Anat.* 53:27, 1871) records the case of a drunken man whose rectal temperature was 79.8 F. Musser (*General Diagnosis*, 1900, p. 206) refers to a case of intermittent fever, with jaundice, in which the lowest recorded temperature was 91 F. He does not state whether the patient survived. It will be noted that these references are far from being recent. Perhaps some of the readers of THE JOURNAL may know of more recent references.

BENEDICT TEST—HILUM OF LUNG—KRÖNIG'S ISTHMUS

To the Editor:—1. In an article on diabetes which I read in THE JOURNAL, mention is made of the Benedict test. 2. and 3. In another article, reference is made to the lung hilum and to the isthmus of Krönig. As I have been unable to find in my books the meaning of these terms, I should appreciate your explaining them to me.

LUIS LAZO AMIAGA, M.D., San Salvador.

ANSWER.—1. The mixture for the Benedict test for sugar in the urine is composed of:

	Gm. or C.c.
Copper sulphate (pure crystallized)	17.3
Sodium or potassium citrate	173.0
Sodium carbonate (crystallized)	200.0
Distilled water	to make 1,000.0

The citrate and carbonate are dissolved together (with the aid of heat) in about 700 c.c. of water. The mixture is then poured (through a filter if necessary) into a larger beaker or casserole. The copper sulphate (which should be dissolved separately in about 100 c.c. of water) is then poured slowly into the first solution, with constant stirring. The mixture is then cooled and diluted to 1 liter. For the detection of glucose in the urine, about 5 c.c. of the reagent is placed in a test tube and from 8 to 10 drops (not more) of the urine to be examined are added. The mixture is then heated to vigorous boiling, kept at this temperature for one or two minutes, and allowed to cool spontaneously. In the presence of glucose the entire body of the solution will be filled with a precipitate which may be red, yellow or greenish in tint. If the quantity of glucose is low (under 0.3 per cent.), the precipitate forms only on cooling. If no sugar is present, the solution either remains perfectly clear or shows a faint turbidity which is blue and consists of precipitated urates. The test is said to be ten times as sensitive to sugar in urine as is Fehling's or Haines' solution.

2. The hilum of the lung or hilum pulmonum is the depression on the mediastinal surface of the lung where the bronchus, blood vessels and nerves enter, and the reference in the article is to this region.

3. Krönig's isthmus refers to the apexes of the lungs behind and above the clavicles or in the area bounded by the clavicle and the borders of the trapezia, to the auscultation and percussion of which Krönig has called attention in the diagnosis of tuberculosis of the lungs.

DR. EDWARDS' OLIVE TABLETS

To the Editor:—Please advise me as to the composition of Dr. Edwards' Olive Tablets in the next copy of THE JOURNAL, but do not mention my name.

M.D., Chicago.

ANSWER.—These are purgative pills, and the claims under which they have been sold are either directly or inferentially misleading. The public has been given the impression that the pills owe their alleged virtue to the presence of olive oil. The American Medical Association's Chemical Laboratory made some cursory tests of the pills in 1913, and reported that they were essentially an aloes pill. These findings were substantiated by a more complete analysis made by the Connecticut Agricultural Experiment Station, which, in its report for 1914, declared that the pills contained dextrose, starch, aloes and ginger. A brief article on this product appears in the pamphlet, "Miscellaneous Nostrums," price 15 cents.

GERMICIDE IN THERMOMETER CASE

To the Editor:—There is a thermometer case that holds a small quantity of fluid to disinfect the thermometer while in the case. The use of alcohol in the case is recommended as the disinfecting fluid. Is alcohol an efficient disinfectant for this purpose? If not, what do you recommend?

C. R. PICKERING, B.S., M.D., Muscoda, Wis.

ANSWER.—Various disinfectants have been recommended for use in the thermometer carrying case, the chief among them being formaldehyd and alcohol solutions. Formaldehyd is a slowly acting germicide and is irritant to some persons. Alcohol is probably as effective for the purpose as anything. When alcohol is employed, care should be taken that its strength be maintained between 50 and 80 per cent., otherwise its germicidal value is lessened; also the thermometer should be cleaned of all secretions before it is returned to the "aseptic" case. To be disinfected, the thermometer should remain in the alcohol solution at least thirty minutes, although a longer time is preferable. The case should be recharged frequently.