ON
ALCOHOLIC NARCOTISM.*

By W. F. WADE, B.A., M.B.,

Physician to the General Hospital, Birmingham.

I AM unable to admit that the present state of our knowledge allows of any satisfactory theoretical explanation of the distinct and indisputable good effects produced by alcohol in certain cases of acute disease.

The marked and speedy amelioration of certain symptoms is for me a sufficient proof that it does good. But I am not therefore logically compelled, nor am I prepared to admit the truth of any theory hitherto propounded, which professes to explain the facts observed. These facts remain to my mind uncollocated by any theory, and are of a purely empirical character. So much so that increased experience renders me more diffident in pronouncing beforehand that in any given case alcohol will be beneficial, or if beneficial, in what quantity it will have to be administered. Practically this is a less unfortunate or perplexing conclusion than would at first sight appear. For I hold that in acute diseases, unless the treatment employed has a beneficial effect, both obvious and speedy, it is an "anceps remedium."

Now the increased acquaintance which we in this generation possess, with the natural course and progress of disease, must force us to the conclusion that it is a very doubtful philosophy which pronounces, without large qualifications, that "a doubtful remedy is better than none at all."† The more potent the drug, or heroic the treatment, the more is this dogma to be distrusted. The use of alcohol, then, upon empirical grounds, has this great advantage over its use upon theoretical ones; it necessitates a constant observance of the phases of the case; it opens instead of shutting our eyes to the workings of nature; it leaves us free to increase, diminish, or suspend the drug, as may appear to be indicated by the mutation of the symptoms.

This is a most important matter; for, just as surely as I have seen the most advantageous effects from the use of alcohol, so, in the same cases, have I seen the most advantageous results from its discontinuance; and further, just as surely as I have seen it save life in one case, so have I seen it imperil it in another; and this in cases where its free exhibition or continuance were undoubtedly required, either by the food theory or the stimulant one, or by both.

The skill of the physician is shown, not by ordering so many ounces of wine or brandy in a bad case, and when it is worse, so many more, but by selecting the case, without reference to its nosological title, in which alcohol is requisite, by observing the effects of the dose administered, and by increasing, diminishing, or discontinuing it altogether, according to its effects upon those symptoms, for the relief of which it was first ordered, without reference to crude theories or foregone conclusions.

What, as it seems to me, we really know concerning the use of alcohol in acute disease, may be easily stated.

In certain cases of either idiopathic or symptomatic pyrexia, symptoms arise which are often the precursors of others which are the immediate forerunners of death. The administration of alcohol is followed by a diminution of the first set of symptoms,

the second set do not appear, neither does death ensue.

These symptoms occur in various diseases, but in many examples of these diseases they do not occur at all. Their occurrence, or non-occurrence bears no known definite or constant relation to age, sex, previous condition, amount of local disease, intensity of constitutional disturbance, or prevailing type.

We are therefore compelled to assume that the symptoms referred to are only incidental to the disease, and are connected with some condition or conditions of the system at large, or some of its parts, the nature of which we are not at present in a position to state. The idea which the original subject of this paper suggested to mind may be put in the form of a question.

In framing a theory to explain the facts above indicated, by what are we justified in altogether ignoring the undoubted narcotic properties or effects of alcohol? Yet I apprehend few will be found to deny that these powers have hitherto been implicitly, if not explicitly, denied.

It must be admitted that the exciting or stimulating properties of alcohol, like those of opium, are as a rule manifested chiefly after comparatively small doses, its sedative or narcotic properties chiefly after large ones; but in these very cases is it that the use of large doses has been extolled.

Here is another curious consideration. Two of the chief indications for the use of alcohol in acute disease are, excitement of heart—rapid pulse, and excitement of brain—delirium. And the speedy result of its use, in many cases, is the calming of the brain and the retardation of the pulse. But suppose that, in adequate doses, it fail to stay the rapidity of the pulse—what then? Suppose, further, that the pulse become even more frequent than before. Had the condition of the patient improved after the administration of alcohol, we should have attributed the improvement to that administration. And how are we justified in assuming that a deterioration of his condition is not to be judged by the same law?

One disease in which empiricism justifies the occasional use of alcohol is pneumonia. And here its anodyne and sedative powers are often most marked, sometimes in the relief of the sensation of dyspnoea and the frequency of the respiration, sometimes, though not so frequently, in the relief of pain. So, in some cases of spasmodic asthma, it stops the paroxysm.

So, in certain cases of delirium tremens, it has been found that large doses of alcohol produce a narcosis which opium alone has failed to procure. I refer to this mode of treating delirium tremens, without any desire to commend it.

Individuals often discover for themselves the anodyne powers of alcohol. I know of four persons, three males and one female, who have been induced to take it largely for the relief of pain. This is a most unfortunate discovery for anyone to make, so likely is it to become a permanent habit even when the immediate necessity has ceased. At the same time, persons who have become drunkards in this way are more easy to reclaim than others.

The question must be repeated: In theorising upon the use of alcohol in acute disease, by what scientific knowledge or clinical observation are we justified in excluding the fact that alcohol has distinct narcotic properties? The instances hitherto referred to have all been taken from the class where empirical experience sanctions the use of the drug. And the particular instances might easily be multiplied.

But there are other cases where its use ought to be most advantageous, were it altogether and solely either stimulant or nutritive. But in these, my own

* Concluded from page 690 of last number.

† This was written before the delivery of Dr. Sieveking's Croonian Lectures, in which he expresses a similar opinion.

experience leads me to expect no benefit, to express no stronger opinion.

In old cases of Bright's disease, and in more recent ones where the eliminative powers of the kidney are seriously impaired, we not unfrequently find inflammation of a not very active character, with brown tongue, quick pulse and delirium; a combination which, of any with which I am acquainted, most nearly supplies a reliable guide to the exhibition of alcohol. Does clinical experience justify us in ordering alcohol freely in such cases, with the same confidence of success that we should feel in administering it were the condition of the kidney different? My personal experience has taught me a very different lesson.

In cases of malignant small-pox or scarlatina, what results do we obtain from the early use of large doses of alcohol? From the first doses, *perhaps*, some benefit; but from a continuance nothing that would justify any confidence in such a practice, were we guided by empirical rather than by theoretical (or, to speak more correctly, hypothetical) considerations. Such at least is my experience.

I do not, however, mean to assert that even in these cases the unknown condition may not arise for which I, for one, admit that alcohol is the true remedy. Were alcohol a pure stimulant, it would be difficult for empiricism to lead anyone to these conclusions. The fact that it has also redoubtable narcotic properties, offers at least a possible explanation, though, after all, it may not be the true one.

These remarks will probably appear to many crude. It is true that they really are so, and I have intentionally abstained from attempting to disguise their crudity. I have no desire to start a "narcotic theory of alcoholism", with a view to supersede the "food" or the "stimulant" theory.

Persons who desire rather that their opinions should be fashionable than that they should be correct, may find it difficult to acknowledge any respect for empiricism or to admit it avowedly into their practice. The discovery and employment of medical "arms of precision," which must ever make the nineteenth century an epoch in medicine, and the enormous strides towards exact knowledge made through them, have engendered a natural leaning to exactitude. This feeling is a healthy one up to a certain point. So far as it leads to greater precision in observation and caution in drawing inferences, it is an unmixed good. And indeed it exercises as beneficial an influence upon empirical as upon more scientific medicine. But when it induces a disregard for those rules of art which stop the gaps left in a science as yet far too imperfect, and when it renders odious the practice of those arts, it takes away our means of defence and offence, rude and unartistic as they may be, without giving us arms of precision in their place. So far as scientific medicine does this, it forges arms against itself. Nothing has so far retarded the development of medicine into a science, as the repeated advancement of unsubstantial hypotheses put forward by their inventors as theories. The healthy desire for exactitude is subject to this unfortunate degeneration. It ought not to be overlooked, that the light which has flooded diagnosis during the last sixty years has paled before reaching therapeutics. It is true that therapeutics have improved, but the improvement has been in a great measure negative. Redundancies, superfluities, and excesses of treatment have been curtailed and repressed to the credit and great advantage of scientific medicine. But these excesses have been committed, not by the empiric sitting at the feet of nature, but by the theorist distorting nature to support his views. This needs no historical proof. Excesses are still committed.

By whom, but by the theorist, as they always have been?

Empiricism is not the highest conceivable medical philosophy, but it is still in many circumstances the highest medical philosophy possible.

It seems to me that the use of alcohol in acute disease ought to be empirical; at all events, I have no hesitation in avowing that I so use it myself. Indeed, it is the hope that this view may become more general, that has been my inducement to write this paper. No one can be more alive than I am to the importance of devising a theory which may rightly supersede this empiricism, but the time for that has not, in my opinion, yet arrived.

My desire is to restrain the undue and promiscuous administration of alcohol, not to disparage its obvious and paramount utility. It is because I esteem alcohol as a therapeutic agent, not because I despise it, that I write. I fear lest its abuse should lead, as all abuses do, to a reaction which may, as all reactions do, unduly limit its use. Although I should deeply deplore a recurrence to the fashion of bleeding as it used to be practised, yet I cannot conceal my belief that blood-letting has been condemned without regard to its merits. I believe that it is a most powerful agent of which we might not improbably beneficially avail ourselves, had not the excesses of its votaries placed it without the pale. I grieve when I see an advocate of the employment of alcohol as a therapeutical agent say that, because all the medical officers of a large hospital administer it in certain quantities, therefore those quantities cannot be excessive.

I blush when I think at what the medical critic a hundred years hence will estimate the medical logic of our age. Substitute blood-letting for alcohol, and it might have been written when venesection was as much the fashion as stimulation is now in some quarters. The practice may be right or it may be wrong, but what sort of argument is this to use in its defence? This is not the strain in which the "grand old masters" who have advanced our art, have either thought or spoken. It is not the way to advance it now.

My present object is accomplished if I can succeed in obtaining the assent of my professional brethren to views which may be summed up in the following recapitulation; and when I say the assent, I do not merely mean a passive acquiescence. I desire that, each time they direct the therapeutical administration of alcohol in acute cases, they should have these considerations present to their minds, and they should exercise an influence upon their decision, whatever that may be.

1. We ought to use alcohol empirically in acute disease, because there is at present no adequate theory of its action.

2. When alcohol is beneficial, its good effects are speedily manifested.

3. It is to be given for the relief of certain conditions of the system, and not for certain diseases.

4. The conditions for which it is requisite may disappear as speedily as they may appear, and then the alcohol should be discontinued.

5. We ought, therefore, to watch and judge by results, and disregard theories.

6. In employing alcohol in large doses, we should never forget that it possesses narcotic properties as well as stimulant ones.

7. It is not proved that acute disease neutralises or even antagonises these properties.

8. It is not proved that acute disease does not render the system even more subject to these effects.

9. There are some grounds for believing that its

narcotic properties are beneficial in certain conditions arising in acute disease.

10. No remedy which is potent for good can be impotent for evil.

CASES OF DUODENAL PERFORATION.*

By ANDREW CLARK, M.D., F.R.C.P.,

Physician to and Lecturer on Medicine at the London Hospital.

CASE V. Of this case I possess only imperfect notes; but the main facts were these. A girl, about 10 years old, was brought to the London Hospital for an extensive scald. She did well till about the fourteenth day after admission, when she began to complain of pain in the right side of the belly, shooting through to the back. This continued for nearly three days, sometimes better, sometimes worse. At the close of the third day, peritonitis set in, and ended fatally within twenty-four hours. After death, it was discovered that the peritonitis had arisen in consequence of the occurrence of perforation in one of Curling's ulcers of the duodenum near the pyloric extremity of the stomach.

CASE VI. W. T., 43 years old, a short, stout, but pale and flabby looking man, was admitted into the London Hospital as an extra case, about the middle of July, complaining of intense pain, increased by pressure, on the right side of abdomen, occasionally shooting through to the back, of sickness, and of a sense of extreme prostration.

The patient had been for some months liable to pain in the right hypochondrium, coming on about two hours after food. The pain had never before been severe, and never before had either arisen or subsided suddenly. There was no history of jaundice, hæmaturia, or gravel. The patient's present attack came on quite suddenly, shortly after a hearty meal taken four hours before admission.

When first seen by me, the man lay upon his back, with his knees drawn up and slightly apart. The face was dusky-pallid and anxious; the breathing hurried and costal; the skin bedewed at parts with a cold sweat; the manner restless and dejected. The belly was flat; and no pressure could be borne in the region of pain, which extended from below the right hypochondrium to near the crest of the ilium, and was felt at intervals under the lower angle of the right scapula. The tongue, yellow-furred at base, was red and eroded at tip and edges. There was occasional vomiting of greenish frothy mucus streaked with blood. The introduction of water into the stomach produced no increase of pain. The bowels had been freely and naturally relieved the day before. The urine was normal. No disease could be detected in the heart, lungs, or brain. The pulse was about 100, small and somewhat sharp.

Plainly enough, this was no case of intestinal colic. Was it, then, one of hepatic or of renal neuralgia due to the presence of a calculus, or arising independently of one? Here was an assemblage and order of symptoms not uncommonly attendant upon the passage of a gall-stone into the bowels. Why, in the present instance, then, were not these symptoms ascribed to this cause? In the first place, because the chief seat of pain was not over the liver, but somewhat below it, and because there was neither swelling nor tenderness over the gall-bladder. You could percuss the liver and press its free margin,

without adding to the patient's sufferings; but, when the hand was laid upon a spot about an inch below that and a little to the right of the umbilicus, the pain, throughout severe, became at once insufferable; moreover, it had arisen and remained in that spot. In the second place, though the absence of jaundice was no disproof of the presence or passage of a gall-stone; the absence of bile from the urine, and its presence in the faeces, showed that the biliary passages were free. In the third place, a number of minor circumstances concurred in disavowing the notion of a gall-stone as the cause of the patient's symptoms. Among these were the history of painful digestion with an eroded tongue, the well defined position and fixity of the abdominal pain, the quick small pulse, and the partial collapse.

In the light of these considerations, it seemed impossible to ascribe the patient's symptoms to any disease of the liver. Did the fault, then, lie in the kidney, and was the cause a renal calculus? I concluded not. There was no pain, and there had been none, in the region of the kidney; no retraction of the testicle; and no numbness or pain in the right thigh. The urine was healthy and abundant; and there was no history of hæmaturia or gravel. Having put the kidney out of the question, I came to the conclusion, that I was dealing with a case of ulceration of the duodenum in which a slight leakage had occurred. Acting upon this conviction, the man was ordered to be kept scrupulously quiet on his back in bed; and to take no food or drink by the stomach for forty-eight hours. A small pebble was kept in the mouth to allay thirst; the skin was sponged with tepid water; and nutritive injections were administered by the bowels. He was put freely under the influence of opium; and hot flannels sprinkled with turpentine were kept constantly applied to the abdomen. For the first fourteen hours, there was no improvement in the patient's condition; but, after that time, he began and continued to amend. At the end of thirty-six hours, I was obliged to forego the rigid abstinence which had been practised up to that time; ounce doses of milk were allowed to be taken every hour. On the third day, the patient was much better; the pain had subsided; the countenance had become more natural, the skin warm, the breathing deeper, the pulse fuller; vomiting had ceased; and the weight of the hand could be borne on the abdomen. Milk and beef-tea were now allowed more freely in small quantities at short intervals. In a week, the patient had become so much better as to be able to sit up. In a fortnight, he expressed himself as well, though confined to a sloppy and almost exclusively milk diet. There was still, however, uneasiness after food; and pressure could not be borne along the former seat of pain. The patient now took to nursing; and, after a few weeks' careful dieting upon pultaceous food, abstinence from stimuli, counterirritation to the epigastrium, and the use of bismuth, left the hospital, as I understand, quite well.*

Whilst the last-mentioned case was under treatment, a woman was admitted into Redman Ward suffering from somewhat analogous symptoms, but arising from quite a different cause, and, for the sake of comparison therefore, worthy of brief narration in this place. She was 42 years old, had a fat flabby body, a sallow anxious countenance, and a slight icteric tinge in both conjunctivæ. Tossing restlessly about in bed, and uttering loud and frequent groans, she complained of agonising pain in the pit of the stomach, shooting through to the back and upwards

* Concluded from page 689 of last number.

* Another case, similar in all essential points, was admitted into my wards at the hospital in the beginning of May, and has just been discharged cured.