THE PHYSIOLOGICAL AND CHEMICAL ACTIONS OF ALCOHOL.

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A KNOWLEDGE of the physiological and chemical actions of medicines is especially essential to every medical practitioner. To administer drugs on a mere routine, or even from the apparent successes of a long experience, is to reduce medicine to the rank of an art. In such practice, the success or failure must depend entirely on the memory and training of the practitioner. A young man must simply flounder as best he can through a series of blunders, learning from experience, whereas the aged and well versed in medical lore must often need to tread in hazy and uncertain tracks.

The administration of alcohol by medical men, as a medicine or as an article of diet, is a matter of policy which is being severely commented upon by a large number of the public. To defend our position, it is necessary to obtain a clear conception of the action of alcohol on the human frame. The study of the question is a difficult one, and needs to be approached with great caution. It is the more difficult, because medical men are not agreed as to the nature of its action. The one school, following in the steps of Baron Liebig, assert that alcohol is entirely burnt off in the body as carbonic acid and water or acetic acid and water. Another school asserts that such is not the case, but that the whole, or nearly the whole, of the alcohol passes off from the body unchanged. Dr. Dupré has recently conducted a careful series of experiments, in which he appears satisfactorily to prove that only a small percentage of the alcohol taken could be again collected from the various excreta. Dr. Subbotin, on the other hand, gave a large quantity to some rabbits and re-collected a large percentage from the breath and urine. Doubtless, both gentlemen were correct in their experiments, but the two series were not equal. Dr. Dupré gave small quantities to men; Dr. Subbotin large quantities to rabbits. Alcohol, to be burnt off from the body, must unite with oxygen, which chemical change could not take place, unless as rapidly as oxygen was available. There is a vast difference between the respiratory viscera of a man and of a rabbit, consequently a much larger quantity could be burnt off in the body of the former than in that of the latter. The quantity given by Dr. Subbotin to the rabbits was excessive; hence he re-collected some of the alcohol, simply because it would be passed off with the excreta mechanically. Remaining unconsumed in the general circulation, owing to a lack of oxygen sufficient to burn it off rapidly, it would pass off mechanically, just as any of the watery part of the blood might; only, as proved, at about one-fifth the ratio that water does. Thus, in a rabbit weighing 3 lbs., and so containing four or five ounces of blood, if I were to inject one ounce of alcohol, the blood would contain 20 per cent. of alcohol mechanically held in solution. Thus, in one hundred pounds or minims of urine excreted, I should expect to find one-fifth of 20 per cent., equal to four minims, of alcohol. As the alcohol began to oxidise, the amount found in the excreta would decrease, simply because burnt off in the system and forming a gradual decreasing percentage of the blood.

Dr. Edward Smith endeavoured to show that alcohol passed off unchanged; but, as Dr. Dupré pointed out, there is an acid in the urine which acts to the chromic acid-test in a similar manner to alcohol; hence it is necessary, to secure accuracy, in the first place to make check experiments. Dr. Smith does not seem to have noticed this. A few considerations appear to me clearly to prove the correctness of Dr. Dupré's experiments, and the conclusions at which he arrives.

1. The experiments were performed with great care and verified by check experiments.

2. If alcohol passed off mechanically unchanged, it would not interfere with chemical combustion, whereas it does, and, as Dr. Smith's own experiments show, materially alters the ratio of carbonic acid and watery vapour in the expired air. It appropriates all or the greater part of the available oxygen, so that the fats and sugars obtained from the foods and the wastes of the body, and which naturally ought to be burnt off, cannot be consumed in its presence, and so are laid up in the tissues or got rid of in other ways. The fat landlord and the bloated drinker are both examples of this. The quantity of milk available in mothers taking large quantities of alcohol cannot be accounted for on any other hypothesis. In a case which, a short time ago, came under my notice, a woman unaccustomed to alcohol, during pregnancy, became an inveterate drinker. The infant, when born, was literally a mass of fat. In a *post mortem* examination of a gin-drinker who died from apoplexy, I found that the heart and liver were permeated with

fat, and could be broken down under very slight pressure; the coats of the stomach and intestines were separated and thickened with fat. Fat was stored up in every available position. Such phenomena as the above could not be accounted for, if alcohol did not decompose in the system, and thus interfere with the natural combustion.

3. The passage of unchanged alcohol would set up irritation and inflammation in all the excreting organs. We should have pneumonia, laryngitis, nephritis, and other diseases. Coldness of the skin would be a sequent, for the evaporation of the alcohol from the skin would cause an increased radiation of heat, and thus leave the parts chilled or cold. I have lately performed experiments proving the correctness of this assertion. I took a batch of rabbits one month old. To one I gave twenty-five drops of rectified spirits of wine in a little water; it immediately dropped down dead. I gave to a second seventeen drops in the same way. It fell on its side, but recovered. It never, however, seemed to regain its natural vigour, and died in nine days. The *post mortem* examination showed congestion of the dura mater, and very severe congestion of the liver, which was almost black. The folds of the bowel in contiguity to this viscus were also congested. To a third I gave 193 drops of spirits of wine within the hour, but warded off immediate death by means of strychnine. The animal died in six weeks. On making the *post mortem* examination, I found slight congestion of the dura mater, but severe congestion of the liver, which was black and literally rotten. The intestine was also severely congested, doubtless in a great measure from the action of the strychnine. In this case, a large amount of alcohol was kept in the blood of the rabbit by artificial means (equal to seven to eight pints in an ordinary-sized man). The result was, that there was not oxygen available to the extent required for its rapid combustion and elimination; hence the severe mischief set up by its presence.

4. The double action of alcohol as a stimulant and narcotic is against reason. It can only be accounted for in another manner, as I hope presently to prove.

Having shown that alcohol is burnt off in the human economy, excepting such as passes off mechanically, the next point for consideration is its action on the frame. Dr. Anstie considers it a stimulant in small doses and a narcotic in large. Dr. Edmunds, in a paper read before the British Medical Association last year, considered it a narcotic. He accounted for the stimulant or excitor symptoms as merely "a play of the coarser faculties let loose by the narcotisation of those finer sympathies which are designed to govern and regulate the others". This is certainly an ingenious method of meeting a difficulty. Because Disraeli and Gladstone pour out effusions of eloquence and become excited in the effort, are those outbursts simply the play of "coarser faculties let loose" by means of some narcotic acting on the *finer sympathies* of their nature? Because a man's appetite is keen, does it but show a falsehood, and implore nothing but the abstinence from food? According to Dr. Edmunds's theory, all narcotics narcotise, but all stimulants merely cause a play of deceptive phenomena.

To adopt Dr. Anstie's theory, and to say that alcohol is a stimulant in small doses and a narcotic in large, is to assert an apparent contradiction. It is only natural to infer that, if a small dose stimulate, a larger dose must stimulate to a still greater extent. If, then, this large dose narcotise, it must be the result of prior stimulation to paralysis of the nerve-centres upon which the alcohol acts. This reasoning must involve us in a sea of difficulties. In the first place, we get paralysis of the brain, or portions of it; such paralysis, if occurring naturally in a patient, is looked upon as a most serious malady, and generally entails a bad prognosis; but, when occurring artificially or at the bidding of the practitioner using this line of argument, it creates no alarm. In the second place, allowing such nerves or nerve-centres to be paralysed, by what means or nerves is a contra-action set up, and by what means do the paralysed nerves receive life again?

What, then, is the action of alcohol? I maintain that it is a *pure stimulant*. It flushes the face, reddens the ears, stimulates the trigeminal nerve, warms the body generally, and stimulates the animal passions, even beyond the control of the will. If given in large enough dose, it stimulates to death. Thus, when a person takes a large amount, e.g., a quart of brandy or rum at a draught, the probability is, that he will fall down dead, not from the shock, as commonly supposed, but stimulated to death, all the nerve-power being at once stimulated or called into play. Thus the person dies from the exhaustion of nerve-force.

There are, however, other phenomena resulting from the administration of alcohol. These are the narcotic effects: stupor, sleepiness, drunkenness. They are not the result of the direct action of the alcohol, but are induced by the accumulating carbonic acid and impurities in the blood of the drinker, consequent on the decomposition of the alcohol. The same symptoms occur if a person descend into a sewer

or deep well loaded with carbonic acid. Animals thrown into the carbonic acid lake in the Island of Java exhibit the same phenomena, and persons killed by the choke-damp in colliery-explosions are affected in a similar manner. No stimulant action will induce drunkenness. I stimulated a cat to death with ammonia. The phenomena exhibited were essentially those of increased vitality; inflammation of the eyeballs, pneumonia, inflammation of the brain-membranes, and inflammation of the skin to such a degree that it peeled off. Such were the results of the stimulus. The pneumogastric nerve was stimulated to such a degree, as to contract the muscular fibres of the heart, so that the chambers were kept permanently empty; the result was death. Stimulant action always draws out latent nerve-power, provided such be present. Thus the phenomena exhibited are those of strength, whereas narcotic effects are precisely the opposite. In proof of the doctrine, that alcohol stimulates in large enough dose beyond the control of the will, I will briefly detail two cases which lately came under my notice. A short time ago, a patient consulted me for secondary syphilis. He was strictly moral; but, being at an evening party, he partook rather freely of wine. He left, intending to get a cab, but suddenly lost all recollection. Next morning, he found himself with a fallen one in a house of ill-fame. His mortification was so great, that I feared he would commit suicide. The syphilis was clear proof that the passions had been stimulated beyond the control of the will. The excited, rapid conversation of drinkers, and the extraordinary crimes committed under its influence, are further proofs of stimulation beyond the control of the will. A woman, who is still living, makes it a practice to drink above a pint of neat gin before breakfast; she seems to require such a stimulus to enable her to digest her breakfast, which is unusually large. Those who have seen her say she never seems to get drunk. I need scarcely say that she suffers from severe attacks of gravel.

In habitual drinkers, the continued stimulation exhausts the nerve-power. A few weeks ago, I was called in to see a man who was a habitual drinker. His daily diet was a pint of milk, which he washed down with five ounces of whiskey. The nerves were kept stimulated, and their latent power called into play, by alcohol. They thus became exhausted. Nature endeavoured to repair the mischief, the result of which was, that he slept for weeks. Before his death, he slept for nearly a month. He appeared too weak to stir across the room. It was too much trouble for him to speak. He died *utterly exhausted*.

I think it will be allowed that I have shown that the apparent contrary effects produced, on the administration of alcohol, result from different causes. The stimulant is produced by the alcohol; the narcotic by the carbonic acid. These two series of phenomena are going on at one and the same time, in a varying ratio, according to the amount of decomposed or undecomposed alcohol. These different actions are consequent on the operation of two series of nerves, the cerebro-spinal being essentially excitator and the sympathetic essentially depressor. All medicines which stimulate or call into play vital actions act by means of the cerebro-spinal nerves, whereas all medicines which narcotise or depress, as aconite, opium, chloroform, etc., act by means of the sympathetic nerves. To counteract a severe stimulus to the one series of nerves, it is necessary to stimulate to a higher degree the other series. Thus, when a gall-stone is passing through the ductus communis choledochus, a person may take as much opium as would ordinarily poison before he gets relief. The result of the experiments on antagonistic medicines related by Dr. Hughes Bennett of Edinburgh, at the late meeting of the Association, is easily explained on this doctrine. Looked at in this light, narcotism or drunkenness is the safety-valve of the drinker. Were it not for such, every habitual drinker would sooner or later be stimulated to death.

In studying the action of alcohol, I have observed another law, which accounts for several of Dr. Bennett's experiments—viz., that medicines which act upon different portions of the same series of nerves counteract and modify each other. Thus atropine and morphia, alcohol and strychnine, modify each other. Alcohol will instantaneously relax the spasms in cases of strychnine poisoning. Great care is necessary in this experiment not to give an overdose of the alcohol, lest the animal die. After stimulating a cat to a high degree with alcohol and strychnine (three grains), I failed to cause it much inconvenience by strong ammonia. It merely carelessly moved its head aside from the stimulant, and leisurely walked to another part of the room.

The accompanying diagram will illustrate my meaning. The action of alcohol is to stimulate the origin of the pneumogastric nerve, E, and the adjacent parts of the brain. If pushed to excess, it stimulates the nerve-currents so severely towards that point, as to subtract them from the spinal cord from below upwards. This is doubtless another cause of drunkenness—the absence of nerve-currents passing down the spine to keep the muscles of the lower extremities contracted and contracting; hence the creature first loses power in its hind legs, then in

its fore legs. (If the stimulus be pushed, the diaphragm, G, only acts spasmodically; and this, together with stimulus of the glosso-pharyngeal nerve, accounts for the various degrees of chronic alcoholism.) When drunkenness is established, if the nerve-currents be stimulated to the

A. Cerebrum. a. Cerebellum. B C. Spinal Cord. F. Phrenic Nerve. G. Diaphragm. H. Heart.

lower portion of the spinal cord by the power of the will, if strong enough, or artificially by strychnine, the muscles will again act, and the drunkenness pass off. Thus we see that there is a three-fold cause for drunkenness: 1. The stimulus to the brain, exhausting its substance; 2. The withdrawal of nerve-currents from the lower part of the spinal cord; and 3. The accumulation of the carbonic acid and other impurities, which act more speedily and effectually because of the exhausted and inactive nerves.

It is urged that alcohol prevents waste; but is such theory correct? Waste is caused by motion, voluntary or involuntary. The administration of alcohol prevents the burning off of fats and sugars, whether derived from the food or the consequent of wasting tissue. Hence there is less carbonic acid in the breath and less urea in the urine. There is less waste eliminated, but such is not proof that there is less waste held in suspension in the blood. If there be less waste, it shows lessened activity, and is, doubtless, consequent on the depressor action of carbonic acid. To give alcohol for this purpose in active disease cannot be wise. The deposition of fats and sugars in the tissues and fluids of the body, consequent on the chemical combustion of alcohol, lays the foundation of numerous and chronic diseases. It will thus be seen that, as a rule, alcohol, in large and continued doses, is not a medicine on which we can rely. If, in disease, we would administer a stimulant, such cannot be the remedy, because of its depressor action. If, on the other hand, we wish to give a depressor remedy or antipyretic, alcohol cannot be the remedy, because of its stimulant action. Its stimulant action is not continuous, and the double action is exhaustive. If alcohol be useful at all, it seems to me to be in extreme weakness after long exhaustive disease. In such cases, a light unadulterated wine, diluted with water, might gently aid to tone the exhausted nerve-power; but the administration should be carefully watched and skilfully managed, even as in the case of any other medicine, lest exhaustion, on the one hand, or too great depression, on the other, be produced. If by small doses the depressed nerve-powers be restored, without being materially lowered by the small amount of carbonic acid generated, then alcohol does good. Like ammonia, it relieves in syncope. It is also evident that alcohol would act beneficially where the double action is required. Such cases are the exception, the only one presenting itself to my mind being in prolonged labour-cases. Here the stimulant action would tell on the fibres of the fundus of the uterus, and the sedative on those of the cervix and os uteri. In this case, however, ergot, on the one hand, and opium and chloroform, on the other, subserve the same purpose and without the danger of exhausting the nerve-power. Medical men should not trust the administration of alcohol in the hands of patients. It is a severe irritant, and, by reason of its decomposition, narcotic poison. To prescribe unlimited quantities and unknown potencies for any and every disease is unprofessional. If we think alcohol necessary, we are justified in prescribing it; but we ought to

use the spirit of wine. To give brandy or port, is to give we know not what. What would be thought of a practitioner ordering his patients to take a few tumblers of sea-water whenever he wished to give the iodide of potassium? or ordering them to take mineral waters whenever he wished to administer iron? Yet such is the practice with regard to alcohol. We give it for all diseases, and in unknown strengths. Such prescribing is not worthy the sages of a learned profession. Our medicines are so numerous, that it is unnecessary for objectors ever to use alcohol, while the moral and religious aspects of the question require the practitioner to exercise the utmost caution in administering such a powerful and destructive drug.

ON PLEURITIC EFFUSION.

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(Continued from p. 580 of last number.)

THE above reported cases present many features of great pathological and practical interest; and they prove, as before remarked, that pleuritic effusion, even when in considerable quantity, is not to be looked upon with such dread as it was formerly contemplated. In the great majority of instances, it is amenable to treatment, and in many cases patients now make a full recovery, who would, if they had been subjected to the inert means formerly adopted, unquestionably have perished. Of the foregoing eighteen examples, twelve were males and six females. That men are more prone to pleuritic inflammation than women, most writers do not fail to point out; and this tendency of the former may doubtless be accounted for by their greater exposure to vicissitudes of temperature, and being more accustomed to indulging in alcoholic and fermented liquors. With regard to age, three were under 10 years, three from 10 to 20, five from 20 to 30, three from 30 to 40, three were 40, and one 65; the greatest number thus being in the third decade. Twelve made a full recovery, four were greatly relieved, and only two died. In one of those fatal cases (6) the gravest symptoms had shown themselves when he came into the hospital; there was distressing dyspnoea, the heart was thrust out of its position, the legs had become cedematous, and a general dropsical tendency was apparent. The introduction of the trocar, which in two places was attempted, did not reach the fluid, because, as inspection afterwards revealed, the lung was broadly adherent to the thoracic wall. If this operation had been performed at an earlier date, it would doubtless have conferred great relief to the distress under which he suffered, though it might not, perhaps, have fully arrested the progressive phenomena of his phthisical condition. The other patient who died was a pale, debilitated woman, whose vital energies had, by previous illness, been much lowered, and she sank by asthenia rather than from any effects of the shock produced by the operation. In six, the effusion was in the right, in twelve, in the left side. All authors agree that this event is of most frequent occurrence in the last named half of the thorax. In thirty-six fatal cases, given by Hasse, nine were double (hydrothorax?), ten in the right, and sixteen in the left, side. Of fifty-six examples by Mohr, nineteen were in the right, and thirty-seven in left. Copland, Hughes, and Hamilton Roe, notice the same fact of predilection for that side. MM. Rilliet and Barthez assert that, in children, pleurisy, in the uncomplicated state, is more frequent in the right than in the left side; whilst, in complicated cases, it is most frequent in the left. Again, it has been maintained that paracentesis is not equally favourable in both sides of the chest. In a work published by Aran, some years ago, he affirmed that, in right effusions, thoracentesis produces only a temporary benefit, that the fluid reaccumulates, or tuberculosis follows. Hippocrates believed more recoveries resulted from opening the left than the right side. Trousseau says he cured a large number of pleurisies with effusion in the right chest. This physician also believed that right pleurisy is very generally of tuberculous origin. Bowditch's experience in this particular is opposed to the opinion of Trousseau. The first-named writer found that, in twenty-five cases, fourteen were of the right, and only eleven of the left side. Of the former number, only one had tubercles, and in that the pleurisy was cured. The result of tapping in Bowditch's cases was, that twice as many recovered from tapping of the right as of the left side. With regard to what I have said relative to the frequency of effusion in the left thorax, it may, perhaps, mainly be explained by the fact, commonly acknowledged, that the left pulmonary apex is more prone to tubercular deposits than any other part of the lungs; and, as we know that secondary pleurisies often are followed by serous, sero-purulent, and purulent collections, thus it is that these products are most commonly found in this cavity.

In fourteen out of the eighteen cases, the average time under treatment was 59.9 days. The time, however, elapsing before recovery eventuates, is necessarily, most various. This will depend upon many circumstances, such as the nature of the disease by which the effusion is produced; the kind of liquid which is poured out; the age; the amount of vital energies inherent in the patient, and, consequently, the degree of absorptional power in the system; the mode of treatment which has been selected; the period which has passed before medical agents have, or instrumental aid has, been brought to bear on the case; and the thickness, extent, and tenacity of the false membranes which have been formed. The restoration to health is doubtless far more speedy when the effusion is merely serous than when it is sero-purulent, and much more especially so when it is purulent. When thoracentesis is performed in empyema, the cure is often very protracted, because this kind of sequel implies a secondary or chronic form of inflammation in the serous membrane. In some cases, in which paracentesis was performed without delay—before time was allowed for false membranes to form, and bind down the lung and lessen the power of absorption—I have known recovery to be remarkably rapid. The modern practice is to tap early, and not to leave it until the powers are weakened and as a last resort. Since Trousseau advocated early interference, there has been much more boldness and decision. In one gentleman, in whose case I was consulted many years ago, a fistulous opening continued for between six and seven years; the sinus then gradually closed, and for twelve years he has been in robust health.

As already remarked, pleuritic effusion may be unilateral or bilateral. It may, however, be observed as a rule for our guidance, that liquid in both cavities, in nearly all cases, implies hydrothorax, when there is a different clinical history, and there are different subjective and objective symptoms; and hydrothorax is signally the episode of organic and constitutional disease. It is a sequel not having foregoing inflammation, nor is it accompanied by notable febrile phenomena; it is generally, as I have remarked, traceable to cardiac or renal changes; the heart is not displaced, and there is often pericardial dropsy; paroxysms of dyspnoea or threatened orthopnoea come on; and it is generally that condition which, after long and gradual alteration of structure, supervenes as the herald to dissolution. In one-sided effusion there has, in the majority of examples, been cognisable inflammation. Not unfrequently the inflammatory symptoms have been but indistinctly pronounced, and sometimes they have been scarcely pronounced at all. But careful inquiry generally elicits the acknowledgment of there having, at an earlier date, been sharp pains, or some condition which directed the patient's mind to his chest. He will say, that about such a time he had stitch in his side, accompanied with cough; that it was regarded as being rheumatism; that he could lie less easily on one side than on the other; that he was at some particular date unwell, after which the more disturbing symptoms passed away. Or, what is far more commonly the fact, there has been veritable pleurisy in the primary or the secondary form. In seventeen out of the eighteen cases above recorded, pleuritic inflammation had preceded. Primary pleurisy may eminently be regarded as unilateral; and double effusion as the sequel of pleurisy, is as before asserted, quite exceptional. When occurring in the left side, fluid in the pericardium may, by extension of the inflammation to the cavity of that organ, be the accompaniment; or pericarditis with effusion, as in case No. 9, may be the forerunner of left-sided pleurisy. Instances of double pleurisy do sometimes present themselves, when they are of the secondary form and the consequents of adynamic, malignant, or exanthematous fevers. When such examples are met with, there is a debased condition of the circulatory fluids, the exudative product is aplastic, and unlike those exudates which characterise the fibrogenic materials which in the sthenic types prevail.

It is not an uncommon thing for the presence of fluid in the pleural sac to be overlooked or unsuspected, and more especially in latent examples of pleurisy. In all instances in which there is an account of foregoing acute thoracic disease, with embarrassment of respiration on increased exertion remaining, or a feeling of uncomfortableness on lying on one particular side, the chest to the base should be thoroughly examined. For the want of such caution, I have known the complaint to have been quite passed over, and grave consequences ensue. When the presence of fluid is not detected, but long remains, the chances of cure become far less speedy and much more uncertain. The mechanical disturbance which a large quantity of liquid in the chest confers to the heart and lungs, is one of the most prominent of the symptoms. In no less than fifteen out of the eighteen cases now given, dyspnoea in greater or less degree was proclaimed. In some cases, a very small amount of effusion—even a very few ounces—will cause oppression, and, as the fluid increases, the breathing will become easier. In these exceptional examples, it would seem that the exudation, at the first, acts as an irritant, and that, in the course of time, the cavity becomes tolerant of its