show the interesting fact that in the main the valves are first invaded.

Dr. Taylor remarks that in the majority of his cases, the ulcerative or fungating condition was engrafted upon a preceding valvular disease of the heart. Of the fifty-three cases which he describes, eleven were upon the right side, and the remainder on the left, and in all but one case the process was valvular.

Continental observers have devoted attention to determining whether the malignant process begins upon the surface of the valve or within the structure of the valve.

Saenger maintains that in a pathological state the valves may contain vessels, and in these vessels colonies of bacteria have frequently been found.

So there seems to be ground for the position that the process may be primarily intra-valvular.

Indeed Cornil and Babes have found microbes here when none could be discovered on the surface of the valves, and Hanshalter and Lion were able to discover the organisms in the connective tissue of the valves, when the surfaces were free.

On the other hand, in many cases, the disease undoubtedly first appears upon the free surfaces of the valves, and the invasion is most likely to be at that part where the valves strike together when forcibly closed.

The blood within the heart cavities may be free of bacteria while the diseased valves lodge large colonies, as in the case reported by Sidney Martin in the Gulstonian lectures recently delivered. (*Lancet*, April 9, 1892.) Yet in this case there were found infectious emboli in the cerebral and femoral arteries. Here the disease appeared in the left auricle, and the exciting organism was a staphylococcus growing in clusters and not in chains.

The relative frequency with which this form of endocarditis occurs in the right side is a matter worthy of consideration. The proportion occurring in Taylor's series* practically corresponds with the findings of others, namely: eleven out of fifty-three cases.

Lion has made the suggestion that certain bacteria being aerobic, would grow best in the arterial blood of the left side, and that the anaerobic bacteria would flourish best in the carbonized blood of the right heart, and alludes to the frequency of right endocarditis in the fetus.

However this may be, it will scarcely hold good of all instances, as for example the case reported by J. Jackson Clark at the meeting of the London Pathological Society, November 1, in which an excessively ulcerating and fungating inflammation limited to the right heart and lungs was found to depend upon a streptococcus.

In Martin's Gulstonian lectures above mentioned, he particularly showed that many of the changes and symptoms seen in malignant endocarditis were the result of certain albumoses found in the blood, and more particularly in the spleen. Chemically, these were indistinguishable from similar substances found in subjects of diphtheria and anthrax, although it was not so fatal in the same dose when injected into guinea pigs. It was found, however, to be a potent poison; it excited temperature, and in sufficient amount produced death.

Finally, after these reflections, the claim put forward that, clinically, cases of malignant endocarditis

progress with very different symptoms, will not seem unreasonable.

Those who have observed even a few cases, must have remarked this fact. The physical signs may be of the most striking character or sink into insignificance, and the patient may succumb from the systemic poisoning without evidences of heart lesions or embolic disturbances.

The affection may pass for an acute pneumonia when the invasion is in the right heart and septic plugging of the pulmonary vessels follows; and, as is well known, the distant lesions seen in many cases may distract the attention of men of only ordinary carelessness from the actual seat and nature of the disease.

One often inquires, is the disease uniformly fatal? To my mind the answer must depend upon what one regards as malignant endocarditis. I saw a case succeeding a valvular abscess, and accompanied by a septic inflammation of the neck and right hand, which went on to complete recovery.

Instances of puerperal endocarditis resulting in recovery are reported. Personally, I believe that cases recover, and that it is an error and a misnomer to apply the term simple endocarditis so unhesitatingly to all cases that have a favorable termination.

SOME CONSIDERATIONS BEARING ON THE TREATMENT OF PNEUMONIA.

Read before the Section of Practice of Medicine, at the Forty-fourth Annual Meeting of the American Medical Association.

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The conclusion seems forced upon us, by recent investigations as to the comparative mortality in cases of pneumonia, that the death rate from this disease is on the gradual increase, and has been on the increase since 1822.

When we eliminate all elements of uncertainty and unfairness in the comparative statistics of various hospitals and institutions, we are compelled to admit that if the death rate has not actually very materially increased, it has not been reduced by our modern methods of treatment. Those methods of treatment differ very markedly from those in vogue during the first sixty years of this century. The best authorities during that period insisted that in this disease blood letting was "a remedy of indispensable necessity," and they were almost equally unanimous in their use of cathartics, blisters, and occasionally emetics.

Subsequent to 1860 our treatment has been greatly modified, being less active and heroic, and styled expectant. This expectancy consists in doing little or nothing during the first stage of the disease unless the temperature ranges high and the cough becomes harassing, when antipyretics, such as large doses of quinine, veratrum viride, and later the newer antipyretics of the antipyrine order are given with opium to allay cough. Later in the course of the affection, when the heart begins to show evidences of failure, alcoholics have been chiefly relied upon, being strongly recommended in all the recent text books to which I have had access.

When we bear in mind these striking differences in method of treatment in connection with the fact that the death rate has increased, or at least has not

decreased, under the newer system, which state of affairs can not be accounted for on the theory of change in the type of the disease, the conclusion seems forced upon us that there is something wrong somewhere.

A recognition of this fact has resulted in the production of a considerable volume of current literature bearing upon the subject of treatment. Those who have thus written, I think, may be divided into three classes: First, those advocating a return to the methods of our fathers. Second, those advocating the free use of such remedies as *veratrum viride*, aconite, tartar emetic, etc., in the early stages of the disease; and third, those advocating expectancy; that is, little or no treatment in the first stages, and alcoholics when evidences of cardiac weakness supervene, and perhaps cold externally for the reduction of excessively high temperature.

If we can believe the advocates of these different methods of treatment, pneumonia is the simplest of diseases to treat; the first two classes claiming to abort nearly all their cases in the first stage, and the third, to conduct nearly all their cases to a favorable issue. Nevertheless, the mortality rates are before us; in view of which, and our individual experience with the disease, we are compelled to accept these statements with a very large grain of salt.

As to venesection, it may be admitted that good may sometimes result in certain cases. The advocate of blood letting has this in his favor, that with the blood withdrawn there escapes a certain amount of toxalbumin; and I apprehend that the amelioration in the symptoms, which it is claimed often follows the operation, is due to this fact rather than to any relief which the right heart may receive by reason of a temporarily reduced total quantity of blood in the vessels; the *modus operandi* of venesection in this case being the same as in uræmic intoxication, where marked temporary relief often follows the operation, by reason of the coincident removal of uræmic poisons with the blood. On this supposition the fact would be explained that venesection is indicated, as its advocates claim, in all stages of the disease.

The *veratrum viride* treatment has always seemed to me to be unscientific and illogical from whatever point of view we regard it, and only capable of doing harm.

The immediate effect of increased body temperature is increase in rapidity of the pulse and respiration. This appears to be directly due to the action of the superheated blood upon the cardiac muscle and upon the respiratory nerve center. And this is the very thing necessary to increase heat dissipation and thus prevent a further rise in temperature. If, by means of such an agent as *veratrum viride*, we succeed in reducing the pulse rate to sixty or less per minute, it may well be doubted whether any good has been accomplished. On the contrary, in view of the impaired action of the heart, which we look for later in the course of the affection, the fact can never be lost sight of that drugs of this nature may add to the dangers under which the patient already suffers, and may be the determining lethal factor in what otherwise might be a favorable case.

We can never treat pneumonia truly scientifically until we are able to strike at the *materies morbi* of the disease, destroying the pneumococcus in the alveoli, or antidoting the pneumotoxin in the blood.

This we are as yet unable to do, and hence our efforts will be palliative only.

We must abandon the idea that the peculiar cardiac weakness in this disease is due to obstruction of the pulmonary capillaries. The evidence all goes to show that such is not the case. Tying off part of a rabbit's lung produces no such effect upon the heart, and in pleurisy, with effusion, where the lung is compressed into the apex of the thoracic cavity, we do not find a heart so embarrassed as in cases of pneumonia. Again, we often see cases of pneumonia with extensive areas of lung involved, and with only comparatively mild systemic symptoms.

A case occurred in my practice last fall in a man forty-seven years of age, in which the lower lobes of both lungs were involved. The maximum temperature was 102.6 degrees, respiration rate 36, and pulse 108, the disease terminating by crisis on the seventh day. If cardiac embarrassment is due to pulmonary engorgement, then it should have been extreme in this case. The ordinary history of cases that terminate favorably is that when crisis occurs there is an immediate fall of temperature and change for the better in the character of the pulse, notwithstanding the fact that the condition of the lung remains for some time unchanged. The widely varying clinical history of this disease in cases where the same amount of lung substance is involved, must have convinced us all that we need pay but little attention to the lung itself, and that the gravity of the case depends more upon the accumulation of pneumotoxin in the blood than upon the local lesion.

All the vital processes are carried on by the agency of the nervous system, the great sympathetic presiding over all the so-called involuntary or vegetative functions. As long as these functions are maintained, so long will life continue. We recognize the *vis medicatrix naturæ*, and this force it is which calls forth those internal responses to external impressions which alone render life possible. When the germs of disease or their products gain an introduction into the human body, if there is not a counteracting internal action set up, then the patient dies. And when we come to study the effects of these germs or their products upon the individual, we find that they gradually reduce or almost abolish nervous sensibility, and that then the vital processes are at their lowest ebb, and if we are unable to reawaken nervous sensibility, death is inevitable. We see this condition of obtunded nervous sensibility very markedly in typhoid fever and pneumonia. In pneumonia the heart becomes very much weakened, and its action inefficient; not from any impairment in its muscular substance, but from impaired innervation, nervous sensibility being diminished by the toxic effects of poisons generated in the system and imperfectly eliminated, and probably in a higher degree by the poison peculiar to the disease—the so-called pneumotoxin.

The objects, then, to be accomplished are the increase of nervous sensibility and the elimination of the specific poisons of the disease and the products of retrograde metamorphosis. We have seen that the treatment almost universally recommended under these conditions of obtunded nervous sensibility has been alcoholics, freely and often administered, in order to stimulate the nervous system to renewed activity.

Alcohol has always enjoyed the reputation of

being a stimulant, but I am firmly convinced that such is not the case, and fully believe that this view of the question will ultimately prevail. Symptoms of this are becoming more and more numerous every year. The most commonly observed effects of alcohol are all sedative. Samuel Wilks enumerates these effects at some length: the dilated capillaries from vaso-motor paralysis, the semi-stupor sought by those who "drown their sorrows in the flowing bowl," the forgetfulness of evil done or wrongs sustained, the insensibility to cold and other unpleasant external impressions.

Cosgrove, reviewing the experimental work done by Ridge, Lauder, Brunton, Parkes, B. W. Richardson, W. A. Hammond, Prout, Fife, Vierordt, Smith, Perrin, Lehman and others, says that, contrary to what has been and is supposed, they found that small doses of alcohol produce from the first a narcotic rather than a stimulant effect. That all these observers, with the exception of Smith, also found that alcohol in small doses diminished the amount of carbon dioxide exhaled.

H. C. Wood, before the International Medical Congress in 1890, said that his doubts as to the stimulating effects of alcohol on the heart during anæsthesia had grown stronger and stronger for the past ten years, and that his own experiments showed that alcohol does not increase the size of the pulse or arterial pressure, but rather appears to increase the rapidity of the fall of arterial pressure, and thus hastens death.

N. S. Davis summarizes the physiologic effects of alcohol as determined by very many and elaborate experiments by many experimenters, including himself, as follows:

In doses of any size, from the least to the greatest, it is an anæsthetic, lessening nervous sensibility to all external impressions—heat, cold, weariness, despondency, weakness, pain; it also diminishes the oxygen carrying power of the red blood cells, thus materially interfering with the processes of Katabolism, thus impeding nature rather than aiding her in the elimination, not only of the ordinary products of retrograde metamorphosis, but also of those foreign disturbing elements which constitute the poisons of disease.

Experiments with alcohol on digestion are no more favorable to its use in cases of pneumonia than those already mentioned. Blumenau says that as a result of a long series of experiments with alcohol on digestion, he has proven that the functional activity of the gastric juice, its general acidity as well as amount of hydrochloric acid present and its corresponding digestive powers are diminished, and that this diminution of power is relatively greater in persons not accustomed to the use of the drug; also that the motor power of the stomach and its capacity for absorption are diminished in direct proportion to the strength of the alcoholic solution.

If these conclusions as to the physiologic action of alcohol are warranted by the facts, and I do not see how they can be doubted, we are justified in the conclusion that its use in cases of pneumonia is illogical; that it is utterly incapable of doing good; and that it is largely responsible for the high mortality rates in this disease.

We ought therefore to abandon the use of alcohols in the condition in question, and extend our observations in other directions, in order to ascer-

tain whether we are in possession of a therapeutic agent from whose physiologic action we have any reason to expect aid in the accomplishment of the objects to be attained, namely, the increase of nervous sensibility and the elimination of the specific poison of the disease and the products of retrograde metamorphosis. I think we have such an agent in strychnia.

There appears to be no difference of opinion as to the action of this drug. By its administration the sensibility of the nervous system is heightened in every part, not only the cerebro-spinal system but also the sympathetic. It acts as a stimulant on the respiratory nerve center, upon the cardiac ganglia, and increases the sensibility of the nerves of special sense, touch, sight, hearing, and also increases the activity of the olfactory sense. Its stimulant action on the involuntary muscular system is witnessed in its increase of intestinal peristalsis and in increased force of uterine contractions when the drug is administered in the progress of parturition. It increases the mechanical movements of the stomach as well as the amount and acidity of the gastric juice, thus assisting the process of digestion, and at the same time there is an increased action of the kidneys, witnessed by an increase in the quantity of urine eliminated.

We have, then, in strychnia, a drug which in its action is diametrically opposed to alcohol and one which is above all others, by reason of its physiologic action, indicated in the treatment of pneumonia at the stage which we are considering, but which, so far as I am able to judge from text books and current literature, has as yet received but little recognition; and it is the purpose of this paper to urge the abandonment of alcohol as a stimulant in this disease and offer strychnia as one substitute therefore.

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TO BLEED OR NOT TO BLEED IN PNEUMONIA?

Read in the Section of Practice of Medicine, at the Forty-fourth Annual Meeting of the American Medical Association.

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Fifty years ago when pneumonia was recognized as a pure and simple local inflammation of the lungs this question would have been answered in the affirmative, and no reputable physician would dare raise his voice against it. But in this, the last decade of