the remedy is resorted to, the longer will it be needful to administer it in order to secure its full effect.

5. In cases in which no mercury is given, the disease often rises to exceptional severity. This to some extent is only the converse of the preceding statements. I mention it, however, in order to remark that all the cases of secondary syphilis of unusual severity which I have recently seen were cases in which, for one reason or other, no treatment had been resorted to in the early stage. I refer to cases in which the eruption came out very copiously, or the throat was badly affected, or the eyes suffered not only from iritis, but from inflammation of the vitreous and retina. Some of the worst cases of this kind which I can remember were in persons in whom the chancre was not venereal, and consequently passed through its stages without suspicion and without treatment.

6. Another argument in favour of the belief that the relationship between the syphilitic virus and mercury is really that of a poison and its antidote, may be taken from the circumstance that the drug invariably produces some influence. It may not cure, but it always changes. From defective mode of administration or other causes it may, and often does, fail to rid the patient altogether of his symptoms, but it never leaves him wholly unhelped. No one ever saw a case of well-marked primary or secondary syphilis in which, after a course of mercury, the symptoms remained as they were before. Complications may ensue and necessitate suspension of the remedy, but it always, on fair trial, proves its specific power.

Thus, then, I allege that, as regards the early stages of syphilis—the primary sore and the exanthem—there is abundant evidence in favour of the belief that the remedy kills the virus, since we find that the results of the latter decline under its administration, and that their normal evolution is arrested, and often altogether prevented. Whilst asserting that the evidence of antidotal value is almost invariably in ratio with the efficiency of its administration, it is well that I should advert to two objections which may possibly be alleged. It may be asserted by some that, after all, a course of mercury rather delays than prevents the evolution of syphilis, that sooner or later the exanthem will show itself, and that it is possibly better to have it over early. In reply to this, I readily admit the fact of delay in many cases; indeed, to the power of mercury in this way I attribute the prevalent opinion that syphilis is a disease the stages of which are of very variable duration and irregular development. If we never gave any antidote we should find it a much more regular malady. If a short course of mercury be given for the cure of a chancre, in many cases the exanthem, which ought to have shown itself two months after the date of the contagion, may be delayed for six or eight months. In these instances I believe it will usually be found that the symptoms which come thus late are comparatively mild in character, and, as already hinted, instead of admitting that they prove that mercury is not an antidote, I should regard them as instances of too early disuse of the remedy. The delayed exanthem seldom or never shows itself until the mercury has been laid aside for a considerable time. In a somewhat similar manner I should suggest that we ought to deal with the undoubted fact that now and then syphilis will appear to flourish although the patient is profusely salivated. This is a rare event, but it does now and then happen. Usually, I think, the cases are examples of premature salivation, and in which, by too rapid use of the remedy, the mouth has been made very sore, and the mercury then suspended. The secret of success is to avoid any interruption of this kind. Although pytalism is for the most part a reliable proof of the constitutional influence of mercury, yet its value probably varies in different cases, and not improbably, in some, the stomatitis may persist after the mineral has ceased to be present in the blood.

If we allege that it is not difficult to form a confident opinion as to the power of mercury in reference to the primary and secondary stages, we must admit that the question becomes much more difficult when we have to deal with the more remote manifestations. What are called tertiary symptoms do not constitute a necessary stage, and are rather to be regarded in the light of sequelæ, which may or may not show themselves. From this uncertainty as to their occurrence we become unable to infer in any given case that any special treatment was the cause of immunity from them. Then, too, the interval is often so long that it is but seldom that

correct details are to be obtained respecting the whole of the case. However well, apparently, a patient may have been cured, and however long may have been the interval, we can never feel sure that tertiaries may not occur. The question to be answered may be put thus:—Is a patient who has been cured by mercury in the early stages more or less likely to suffer from tertiaries than one in whom the disease has been allowed to develop itself without interference? It seems to me hopeless to try to find an answer to it by appealing to statistics, for the fallacies are such that it would be easy to make them tell either way. I shall content myself then by appealing first to theory, and secondly to individual experience. The theory of the relationship between the secondary stage and the tertiary symptoms I take to be simply this:—The secondary stage is equivalent to the period of outbreak or rash in any of the specific fevers (small-pox or the like), differing only in its greatly extended duration. This prolongation allows time for the solid tissues to be largely modified in their structure by the nutrient material which they derive from a poisoned blood, and this modification is often permanent. With the cessation of the secondary or exanthem stage the disease in the blood ceases, but the tissues which have been built up from syphilitised material are liable to exhibit, long after, peculiar types of inflammation. Thus it may be that years afterwards some local exciting cause is brought to bear, or the patient's general tone fails, or senile degeneration takes place, and as a result tertiary symptoms show themselves. The secondary symptoms were general and symmetrical, being due to blood; the tertiary are local and unsymmetrical, being due to tissue-change. If this argument be correct, we are led to the inference that the risk of tertiaries must be in some sort of ratio with the prolonged duration of the humoral stage. The longer the poison is allowed to flourish in the blood, the greater the probability that the tissues fed by it will become permanently tainted. If, then, mercury tend to cut short the secondary stage, it must necessarily tend to reduce the risk of tertiary symptoms. My own impression is strong that this result is observed in practice. I have for long been in the habit, in cases of tertiary syphilis, and in those in which the existence of taint was proved by the occurrence of it in offspring, of trying to get a history of the original treatment, and certainly in many of the worst the omission of mercurials comes out strongly. Many of them have of course taken mercury to a greater or less extent, but it is exceptional to obtain evidence of a really prolonged and efficient antidotal course. I am aware that converse cases—those, namely, in which syphilis has been cured by mercury, and no tertiary symptoms have followed—are of but little value, still they have a certain weight, and it is difficult to avoid receiving some impression from such statements. An elderly member of our own profession, since dead, once said to me during a consultation conference, "I hope, Mr. Hutchinson, that you believe in mercury. I do, for it cured me. When a young fellow I had syphilis badly, was covered with it. They wouldn't give me mercury, till, getting very bad, I consulted Sir Astley Cooper, who made me keep my room, and be salivated. I was well very quickly, and have been so ever since, and here I am at near seventy without ache or pain." I believe that the number of those who could, if they liked, give similar testimony to the permanent good effects of the drug is very large.

(To be concluded.)

BEEF-TEA.

By ARTHUR LEARED, M.D., M.R.I.A.,
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It is now generally admitted that attention to nutrition is a most important part of treatment. Beef-tea has long held its place in the sick room. But beef-tea as ordinarily made is a very unsatisfactory preparation. The process is wasteful because the meat is imperfectly extracted, while the decoction, always of uncertain strength, is generally too weak to support the patient unless in quantities which his stomach cannot receive. It is impossible by the ordinary method to obtain a concentrated decoction except by a

tedious and injurious evaporation. Prepared extracts of meat, of which Liebig's is the best known, are not efficient substitutes; they are neither so palatable nor so digestible as fresh extracts or decoctions. The medical attendant sometimes endeavours to meet the difficulty by directing an extract of meat to be made by the long exposure of meat, with the addition of a small quantity of water, in a covered jar, to the heat of an oven, or else by plunging the jar, having its mouth closed with a piece of bladder, in water which is kept boiling for some hours. Both methods are troublesome and uncertain. In the one case the meat is apt to be charred, in the other it is very difficult to prevent the external water from entering the jar owing to leakage caused by expansion of contained air.

These considerations have led me to devise an apparatus by means of which an extract of meat or beef-tea of any required strength can be quickly and economically made. It consists of a receiver, closed by an air-tight screw cover provided with a safety-valve, into which the meat is put, and a boiler into which water is put. The receiver is then placed in the boiler, and care must be taken that the latter is filled with water. In the woodcut A is the boiler, B the receiver, C its screw cover, D the safety-valve.

If, as often happens, it is necessary to prepare beef-tea quickly, add salt to the water in the boiler in the proportion of one pound to two pints of water; the high boiling point of the mixture will cause the heat in the receiver to reach 230° F., and the pressure of steam to be that of about one atmosphere and a half. Under these conditions extraction of the meat will be completed much more rapidly.

By means of this apparatus, which can be varied in size according to requirement, it is for the first time possible for the medical man to order a fresh extract or decoction of definite strength. It has been ascertained by repeated experiments that if a pound of good beef, free from fat, bone, and gristle, is put into the receiver, with the addition of four ounces of water, the product will be nine ounces of beef-tea; thus the water will be increased by the addition of five ounces of meat-juice. If eight ounces of water, or one-half the weight of the meat is added, the product will be thirteen ounces. This proportion is recommended for ordinary use in the sick room. It would scarcely be supposed that so much fluid could be extracted from meat, and it is probable that further investigations as to the relative yield of juices, as tested by this apparatus, will prove a valuable means of comparing the nutritive values of different meats, as well as of different samples of the same kind of meat. The aroma of the meat, much of which is dissipated by the common method of making beef-tea, is by the present plan entirely preserved.

Veal, mutton, fowls, &c., are of course equally applicable for extraction as beef. It is hardly necessary to say that if the bones and sinewy parts of meat are used, the extract, being gelatinous, will consolidate on cooling. And although gelatine is comparatively unimportant in nutrition, it is popularly supposed that this consolidation constitutes the "strength" of beef-tea.

To make a small quantity of beef-tea, cut a pound of beef, free from fat, bone, and gristle, into very small pieces, and put them in the receiver of the apparatus. Add eight ounces of water; screw the cover of the receiver tightly

on, and place it in the boiler, previously filled with water. Place the apparatus on the fire or over a gas flame (a Bunsen's burner is best), and boil for three hours, or, if with the addition of salt to the boiler, for one hour; then remove it from the fire or gas, and lift the receiver out of the boiler. After the receiver has sufficiently cooled, unscrew the cover. The meat will now be reduced to a tasteless mass, while, by the aid of squeezing, thirteen ounces of rich beef-tea will be obtained. This will be at least three times stronger than average beef-tea prepared by the old method. If a more concentrated preparation is required, it will only be necessary to add less water to the same weight of meat. Messrs. Maw, Son, and Thompson, of Aldersgate-street, have made an extractor for me. I am sanguine that on a large scale the apparatus will be found valuable for hospital use.

Old Burlington-street.

PRACTICAL REMARKS

ON AN

OVERLOOKED SOURCE OF BLOOD-SUPPLY FOR TRANSFUSION IN POST-PARTUM HÆMORRHAGE,

SUGGESTED BY A RECENT FATAL CASE.

By WILLIAM HIGHMORE, M.D., M.R.C.S.,

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THE practice of transfusion, although known for so many years and so valuable in itself, has but seldom been carried out because of the impossibility of doing so in most cases. Medical men who have been many years in midwifery practice must have seen a good many cases of uterine hæmorrhage, sometimes coming two or three together in a fortnight, and sometimes only an isolated case for years; the same patient often being the subject of hæmorrhage in successive labours, causing the greatest alarm to her friends and the doctor also. In Dr. Aveling's successful case the coachman was called in, and consented to be the blood-donor; and in Dr. Barnes's lately reported case the gardener offered his arm for the purpose. But it is not every patient who has a coachman or gardener at command; and in the excitement in the family, when the mother is dying, it is impossible to procure the blood for transfusion in time, even if you had the apparatus in your pocket. I speak particularly of country practice, where you may be miles from any other doctor or efficient assistance.

I have been led to these remarks by a case I was called into in consultation about a fortnight ago by a brother practitioner.

Mrs. —, the wife of a highly respectable tradesman, and mother of several children. On my arrival I found the doctor endeavouring to staunch the flow of a severe post-partum hæmorrhage, with his extended hands applied over the fundus uteri. In a few minutes I had applied a firm binder and thick compress, and the hæmorrhage ceased; but, to make doubly sure, the solution of perchloride of iron was injected also. I myself saw several pounds of blood in a vessel and in the bed. The patient was able to swallow and took large quantities of brandy and thin gruel; but it was all of no avail, and she died in an hour after my arrival. The only chance of saving her life would have been by transfusion; but where was the blood-donor to be found in this case? The husband was wandering about the room in the greatest alarm, pale as ashes, no man could have got a drop of blood from him; his sister was running up and down stairs for one thing or another every moment; then there was the old monthly nurse looking after the new-born baby, or fanning the patient, &c.

It has occurred to me since that if I had at once collected the hæmorrhagic blood of the woman, and defibrinated and warmed it to the proper temperature by my clinical thermometer over hot water, and injected it with my Higginson's syringe and transfusion-pipe, it would have all been the work of a few minutes, and the only chance of life the poor woman had would have been afforded her.

I commend this plan to the notice of the profession, and have resolved to use it myself in the first case of hæmorrhage that may occur in my practice.

Sherborne.