cutting operation of the future, should it be ultimately shown that the uterine incision does not stretch and cause risk in future pregnancies.

Porro's operation should be performed before labour sets in. The advantages of this are great. The operator can make his arrangements and have skilled assistance at a definite hour. The idea that one should wait for labour, so as to have uterine retraction and less hæmorrhage, is an erroneous one, and leads the operator into difficulties. The waiting causes risk to the child's life, undue delay while assistants are being summoned, and should, therefore, not be deliberately chosen. A further point is that the development of the lower uterine segment, abdominal position of the bladder, and formation of the circular vein, force one to make the abdominal incision higher up, interfere with the formation of the pedicle, and may lead to serious hæmorrhage if the circular vein be wounded. There is little risk of hæmorrhage when the operation is performed before labour sets in, provided one operates quickly, uses the elastic tourniquet, has an efficient assistant, and, in fact, regards the whole operation as one analogous to a rapid surgical amputation.

Three points of scientific interest come up specially now, namely, the condition of the uterus and placenta in relation to the third stage of labour, the primitive shape of the child's head, and the condition of the patient's urine so far as any peptonuria was concerned.

On the second and third points I wish to make a few remarks, as I have considered the uterus and placenta in another paper. Before operation the child was found by palpation lying dorso-anterior, with head in the left iliac fossa. A few hours after birth a plaster cast of the upper part of the head was taken. This was found almost perfectly symmetrical, with no shear present.

The urine was tested daily during the puerperium for peptones, and none found. Fehling's solution in the cold was the reagent used.

It is remarkable that this case is, so far as I am aware, the first successful Cæsarean operation of any kind in Scotland.

ON OUR EXAGGERATED ESTIMATE OF THE VALUE OF BEEF-TEA.¹

By THOMAS LAFFAN, M.K.Q.C.P.I.,
Cashel.

It would not be easy to arrive at an estimate of the precise sum expended on beef-tea during a single year in these kingdoms. It probably may be reckoned by hundreds of thousands of pounds. If, however, the money expended be considerable, the responsibilities cast on it and the opportunities lost by it are more considerable still. Fashion is a despot while she reigns, and it is the fashion to prescribe beef-tea in the gravest as well as in the most trifling diseases. Nor does the fashion dominate a section of the medical army only. The bulk of the combatants seem to be obedient to it. From time to time voices indeed are heard, but they are as voices crying in the wilderness, and are as the passing wind which no man heedeth. I would not expect a different result, but that such a gathering as this offers exceptional opportunities for reconsidering important points in medical treatment.

I have headed this paper with the words, "Our Exaggerated Estimate of the Value of Beef-tea." Now, the value of beef-tea is to be viewed from a double standpoint: first, as a nutrient; secondly, as a mere stimulant and flavourer. It is in the first category that it has been placed by so large a number of us. There is too much reason to believe, especially in these days of expectancy, that with the ordering, and possibly with the making, of a certain quantity of this article, many conscientious physicians deem that they discharge most of the service which they conceive themselves able and bound to render to their patients.

With regard to its value as a nutrient, we may fairly take Liebig for our authority. His every interest was in favour of exaggerating its merits, and his eminence as a chemist specially qualified him to offer an opinion. He states that the greatest care is taken to exclude from his extract all fibrine, gelatine, albumen, and fat. He further adds, that its component parts do not give strength where there is none, and that it merely acts on our nerves so that we become conscious of our existing strength. The same authority confesses that its extractives and salts is due all the value it pos-

sesses; that it is to be classed with tea and coffee; and that it neither economises carbon for our temperature, nor nitrogen for the sustenance of our tissues. As to the difference between ordinary beef-tea and his extract of meat, he merely claims for the latter that it contains less water than the former.

Another standard authority, M. Sée, tells us that beef-tea contains very little albumen and very little carbo-hydrates. That the glycogenic matter, the inosite, the sugar, and the lactic acid that belong to fresh meat are altogether altered in beef-tea. As to fats, they are almost eliminated.

We have in it kreatine, kreatinine, carmine, inosite, and other quaternary products, which so entirely resemble, or are so nearly allied to, those found in urine, that their small value hardly admits of question; and it is not matter for surprise that there should be so striking a family resemblance in odour between the two. Patients, to my own knowledge, have oftentimes rejected this alleged "nutrient," on account of the striking olfactory relationship between it and urine. The saline matters alone, *plus* the hot fluid, are therefore left to play the most considerable, if not the only, rôle in the value of beef-tea. Further, M. Sée admits that if with a convalescent the strongest beef-tea should replace milk, the result would be disastrous. Similar views have been advanced in this country, and the names of Hassall, Sibson, Wilberforce Smith, Brunton, Masterman, and others may be cited in support of them.

In connection with the discovery of Mühlheim, that peptones, when injected into the veins, produce a soporific effect, I may mention that cold beef-tea, when prepared so as to contain a percentage of albumen, has been found by me, when administered to sleepless patients, to produce a distinct narcotic effect. I do not aver that this result has been attained in every instance, but the effect has in some cases been too pronounced to escape note. Dr. Brunton found that the urine of a healthy man contained all, or nearly all, the albumen that he had ingested but a few minutes before. He added that the albumen in the case was simply absorbed by the stomach and intestines, that it passed without change through the portal system, and was then eliminated by the kidneys, just as sugar would be under like circumstances. He then goes on to raise the question whether, on this account, beef-tea may not be frequently injurious, or even actually poisonous.

Dr. Hassall long ago showed that 14½ lbs. of beef would be required to yield beef-tea enough to supply the nitrogenous daily waste of one individual, calculating that such waste amounts to 512 grains of urea and 21 of uric acid daily.

Experiments made on dogs have brought out the remarkable fact that they die sooner when fed exclusively on Liebig's extract than when left unfed.

Three leading methods may be set down for manufacturing the article under discussion, and the names of Liebig, Leared, Letheby, Hassall, and others, may be bracketed therewith. The essential differences between them are as follow: Under one method, the heat is kept below the coagulating point of albumen. The tea here is a mere infusion, and is consequently what its name purports it to be. Under the second method, the heat is carried above the coagulating point of albumen, and we have consequently to deal with a decoction and not with an infusion. Sometimes the makers of these two forms enclose their meat in a vessel distinct from the water; but in the majority of instances they do not resort to this refinement. Under the third method, the meat is merely macerated, and no heat at all is applied. This extracts a larger percentage of juice from the meat than is attainable by the application of heat. When treated by cold water, the meat yields up, according to Liebig, 60 parts per thousand, and, under boiling water, only 30 parts per thousand.

It is a curious fact, in my experience, that patients, as a rule, tire very soon of the best beef-tea, and that, no matter how well made it may be, they dislike or downright detest it after a few days. Observe that this applies to beef-tea given in quantities of a pint or two in twenty-four hours.

I now come to the method introduced by Mr. Jessop, and which seems to me to give promise of special utility. It proceeds on a different basis, the resultant of which is not properly beef-tea at all, but consists of crushed beef boiled and suspended in water. He lays down the proposition that the essence of the meat resides in its fibre, and not in its salts; and that this fibre must be thoroughly pulped and suspended in a large proportionate quantity of water, so as to suit the requirements of a patient who is incapable of mastication, and who can only swallow fluid food. He allows only one pound (or one-fourth the standard quantity) to

¹ Presented to the Section of Medicine at the Annual Meeting of the British Medical Association, held at Glasgow, August, 1888.

four pints of water, and simmers it for five or six hours. He then strains the entire; but if any portion be not small enough to pass through the colander, it is further broken up until it is enabled to do so. He makes his beef-tea from rump beef. This article is infinitely richer than ordinary beef-tea, but where solid food is inadmissible its administration seems to me to require further investigation. I have obtained from Sir C. Cameron the following analysis of a sample of this preparation:

Water	...	...	...	...	94.65
Albuminates and crystalline bodies, kreatine, etc.	...	...	...	...	4.25
Fats	...	...	...	...	0.20
Salts	...	...	...	...	0.90

Sir C. Cameron observes that the foregoing beef-tea was thick, containing the greater portion of its contents in a state of mechanical suspension. It will be seen how much richer it is in albumen, fats, and salts than another sample extracted from the same part, under one of the old systems.

The following is an analysis of the other sample; I was not able to procure a separate estimate of the albuminates:

Water	...	...	...	...	98.48
Albuminates and crystalline bodies, kreatine, etc.	...	...	...	...	0.90
Fats	...	...	...	...	0.07
Salts	...	...	...	...	0.55

This was made from beef the same in quality, but 2 lbs. meat to 2 pints, macerating it for four hours, and subsequently simmering for six. No strainer was used, but the meat *débris* was excluded. The resultant was clear.

We have it on the authority of Kühne that, while the intestinal juice would dissolve raw albumen and fibrin, it would not at all act on boiled albumen and fibrin, unless the latter were first subjected to the action of pancreatic juice. When the latter, however, was the case, the dissolving process took place quicker than even when the meat was raw.

Now, taking the analysis of Hassall, who found only 22.10 grains of nitrogen in each pint of beef-tea (1 lb. to the pint) made under the boiling, and 41.10 grains made under the infusing process, and the further opinions of Sée and Liebig, it is quite plain that we have in beef-tea, properly prepared, no value whatever commensurate with the cost, and an insignificant nutrient for the sustenance of the sick.

If beef-tea, then, afford us such small aid, let us see what aids we can summon for the sustenance of the patient who is temporarily incapacitated from using the ordinary solid meat.

Here nice questions suggest themselves: What does this incompetence arise from? Does it really extend more to nitrogenous than to other foods? Ought the cravings of, say, a convalescing fever patient be entirely ignored as tests of the capacity of the system to use meat; and ought the present ones of tongue, etc., to continue to be exclusively employed? And might we not set down the sudden deaths which every now and then do occur after a full meal of meat during convalescence from, say, fever, not to the meat, but to the largeness of the quantity, and to its possible insufficiently triturated state?

The first and chief resource seems to me to be milk, the richness of which in albumen, fat, sugar, and salts marks it out as pre-eminently suited for the supply of the necessary nutriment during what we shall call the interregnum. The digestive weakness which hinders the use of solid meat is a mere approximation or return to that of the infantile state. We know that the wants of this are fully supplied by milk; we know also that this is the only nutriment with which its powers are able to cope.

The next substitute for the system of alimentation by beef-tea is that with which Trousseau's name is associated. It is that of minced raw meat. When we find this successfully employed in conditions of the system in which superficial ulceration of the intestinal mucous membrane constitutes the essential feature, and where that membrane is still more acutely inflamed, as in the infantile cholera of summer, it is hard to set limits to the cases to which its employment may not be applicable. This prospect of extension becomes clearer still when we bear in mind the possibilities opened up by Kühne's investigations before referred to, and which have been fortified by a growing professional verdict in favour of artificial digestive ferments. Lastly, we have rectal alimentation to supplement any deficiencies of mere fluid ingesta. The late Dr. Peaslee, of New York, for five and twenty years employed this system in many cases of acute inflammation, where solid food was inadmissible by the mouth. At first he employed mere beef-tea, but, losing all confidence in it, he soon abandoned it for Leub's pancreatic emulsion, which he administered in quantities

of 3 or 4 ounces each, every three or four hours. It is surprising for how long a period life has been sustained by this method alone. The late Dr. Flint quoted several cases, in a paper which he read before the New York Academy of Medicine. In one of these cases life was maintained for fifteen months, and in another for five years, almost entirely on it.

In the preceding suggestions, I indicate no preference in favour of one substitute more than another; each will be found to have a sphere of its own. Milk will be that which will be applicable to the great majority of cases, while combinations of one or more may find their utility in a percentage of cases.

In conclusion, I would observe that I have approached the subject in no partisan spirit; that I am entirely without predilection, unless so far that I confess I am not entirely free from prepossessions for established practice. I have not sought after new theories of my own, where so much light has been shed on the matter already by others. I have simply endeavoured to place, in a collected and concentrated form, views which are scattered up and down through a wide circle of literature, and which, I venture to think, refer to a matter of importance sufficient to make a claim on your attention here.

ON RAISING THE EPIGLOTTIS.

By FREDERIC HEWITT, M.A., M.D. CANTAB.,

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ALTHOUGH I was of the opinion, when Dr. Howard brought his communication before the Medical Society of London, that the title which he had selected for his paper was, to say the least, inappropriate, I confess I hardly liked to express such a view when the subject came on for discussion. Instead of adopting this course, I thought the matter over in all its aspects, and decided to write to you and to state the convictions which I held. But I again shrank from appearing in the unpleasant capacity of a fault-finder. We all know how easy it is to take exception to the writings and conclusions of others, and it was this thought which caused my hesitation. Dr. Bowles's paper on raising the epiglottis, in your issue of January 12th, has, however, finally convinced me that there is need for further criticism of Dr. Howard's assertions; and I would, therefore, ask you to be kind enough to allow me to add some remarks to those which I made at the adjourned discussion at the Medical Society.

In the first place, a few words concerning the claims which Dr. Howard advances. In plain unvarnished terms, Dr. Bowles has given us a summary of his own observations concerning the epiglottis and the share it takes in apnoic states; and I most cordially sympathise in the feeling which prompted him to assert his position. Dr. Bowles has done much practical and original work in this particular field, and I have read, with the greatest pleasure and profit, not only his article now published, but many other of his contributions. I think there can be no doubt that Dr. Bowles is correct when he says that "Dr. Howard claims too much in asserting that his method is the new and only way of raising the epiglottis, or that it is, as he expressed it in his paper, 'my discovery.'" Every one who is interested in, and conversant with, the subject will, I believe, recognise that the work done by Dr. Howard is essentially corroborative in its nature. As I shall presently attempt to show, Dr. Howard has neither described a new way of raising the epiglottis, nor has he demonstrated that the method he advocates is the only one. But in addition to the double claim which he makes, he speaks of his "discovery of the failure of traction of the tongue to raise the epiglottis." Many years ago Sir Joseph Lister,¹ experimentally observing the effect of tongue-traction in his own throat, wrote: "the epiglottis was not moved forwards in the slightest degree." Dr. Howard simply corroborates what Sir Joseph Lister pointed out. But, although Dr. Howard's work is, to a great extent, corroborative, it is none the less valuable. He has given us fresh and striking anatomical proof of the manner in which head-and-neck extension raises the epiglottis in the dead body and in certain forms of arrested breathing. I say in certain forms, because, as I mentioned at the conclusion of Dr. Howard's paper, and shall presently show, there are many cases in which complete extension of the head and neck will be found to be impracticable or ineffectual. Dr. Howard has not, I venture to think, described a new way of raising the epiglottis, because more or less complete extension of the head and neck has been known

¹ *System of Surgery*, 2nd edition, 1870, vol. v, p. 492.