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.

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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.

as a remedial measure for many years. Nor can he, I think, claim to have proved that the method under consideration is the *only* one at our disposal. It is obvious that the epiglottis may, in many cases, be raised by the finger; Dr. Foulis has actually used the handle of a spoon for the purpose; and Mr. Jennings has devised forceps to secure the same end.

I would, in the next place, make a few remarks concerning the clinical and practical aspects of Dr. Howard's paper. As Dr. Howard himself points out, the subject with which he deals has an important bearing upon apnoea arising during the administration of anaesthetics. Now there are many important points to be borne in mind in recommending or attempting to apply complete extension of the head and neck when breathing has become arrested under an anaesthetic. In the first place (1) I shall attempt to show that there are cases of apnoea in which complete head and neck extension will be found to be utterly impracticable; and in the second place (2) I shall refer to cases in which the extension, although practicable, will fail to secure the restoration of breathing when other means will succeed.

(1.) It does not follow, because complete extension of the head and neck can be produced in the relaxed and dissected subject, that the procedure can be effected in the apnoeic patient. Many conditions may be present in the latter case which would be absent in the former. When Dr. Howard's paper was under discussion I referred to one of the most important causes which may prevent complete extension of the head and neck; and if I may be permitted I will again mention it. With general anaesthetics, and more especially with ether and nitrous oxide, much tonic spasm of the muscles of the jaws, neck, floor of mouth, and other parts may accompany, and in some cases actually be the cause of, the arrest of breathing; and although in minor and transient cases of this nature tilting back the head may be accomplished, in many other instances it will be found perfectly impossible to perform this "utmost manual extension." In short-necked, full-blooded persons—more especially if ether be employed—respiration may become so impeded that, if certain precautions (to which I shall presently refer) be not observed, a condition of apnoea may readily arise, and, despite all attempts at throwing the head back, may persist unless other means be adopted for its relief. I have known the neck muscles become so rigid that extension of the head was, for this reason alone, impossible. This condition of tonic spasm is rarely met with under chloroform, for reasons which I cannot enter upon here.

(2.) Without wishing to deny the general utility of the procedure under consideration, it will, I think, be found that in many cases extension of the head and neck, although practicable, will fail to secure the admission of air to the lungs. In the first place, the nasal passages may be partially or completely obstructed by pre-existing morbid conditions. In the next place, such tumefaction of the nasal mucous membrane may arise, if ether be employed, that more or less complete occlusion of the nasal air-way will result. Moreover, there may be—and I am again here referring to ether—such an abundant secretion of thick frothy mucus that the nasal passages will, when thus obstructed, become inadequate for purposes of respiration. Lastly, granting that the epiglottis is the cause of the apnoea, the extension of the head and neck may fail to dislodge it. I would again most strongly urge that it is a mistake to compare the relaxed *post-mortem* subject with the patient in whom respiration has become suspended under an anaesthetic. In the latter case the epiglottis, the aryteno-epiglottidean folds, and other adjacent parts may be, and usually are, engorged and increased in size, and may thus prevent the entrance of air to the larynx, even though extension of the head and neck is practised. The effect of etherisation upon the size of the tongue is often very marked, and may be taken to illustrate what I mean. But there is, I believe, another factor in the arrest of breathing at the superior aperture of the larynx which may possibly help to explain the failure of head-and-neck extension in some cases. Granting that the epiglottis is closely applied to the larynx, or that the base of the tongue is in contact with the pharynx, or that these conditions coexist, all automatic (or reflex) respiratory efforts would have the effect of increasing the difficulty, for the epiglottis and tongue would be sucked more tightly towards the larynx, and extension of the head and neck might not, in obstinate cases, bring sufficient force to bear upon the tongue and epiglottis to overcome the obstruction. Dr. Jameson, in the *JOURNAL* of last week, describes a case in which head-and-neck extension failed to restore breathing, although tongue-traction and artificial respiration proved successful. Possibly nasal obstruction existed, but, in

the absence of this cause for the failure of the remedial measure, I would submit the above explanation for consideration. Utmost extension of the head and neck in the *post-mortem* room doubtless causes the epiglottis and tongue to recede from the larynx, and will also be found to do so in certain cases of apnoea; but there is, I submit, no proof that this extension will have a like effect when the conditions to which I have alluded are present.

In conclusion, therefore, I am inclined to regard extension of the head and neck as serviceable only in particular cases, and I think it would be wrong to rely too much upon it. Apnoea occurring during the administration of an anaesthetic may or may not be due to some anatomical obstruction; and, were we to rely upon head-and-neck extension in non-obstructive (paralytic) apnoea, we should be committing a serious error, and losing most valuable time. I do not say that the advocates of this procedure recommend its employment in such cases, except as an adjuvant to artificial respiration, but my contention is that, as matters at present stand, there is much probability of head-and-neck extension being regarded as infallible in re-establishing respiration. Putting aside embarrassed and temporarily suspended breathing, there can, I think, be no doubt that the more permanent and serious state of apnoea, should, in the first instance, be met by chest-compression, by which a threefold purpose may, and usually will, be served. By this practice not only will the administrator be at once able to decide whether any obstruction to breathing is present or not, but, should the epiglottis obstruct the air-way, it may be forced from its dangerous position by the escaping pulmonary gases; and, should no further impediment exist, air will be drawn in by the ensuing inspiration, a point of paramount importance. If air enters the lungs by this chest-compression, artificial respiration may be proceeded with; but if it does not, the nature of the obstruction must be ascertained, and the case dealt with accordingly. Given that the arrest of breathing is obstructive, and is not remedied by chest-compression, head-and-neck extension may be of use if the mouth is not available. But what can be said of the remaining cases, those in which chest-compression and head-and-neck extension have failed? Seeing that such cases of obstructed breathing will yield, if the mouth be open, to measures applied through the oral cavity (hooking forward the tongue or epiglottis with the finger, tilting forwards the base of the tongue and hyoid bone with the handle of a spoon, sponging away mucus, and possibly intubation of the larynx), I cannot avoid the conclusion that the safety of the anaethesised patient will be best consulted by providing for, or producing when need be, an oral as opposed to a post-oral air-way. Such an air-way may be provided for by placing a small dental gag between the teeth or gums prior to administration, and, even though the mouth may not be used (the patient breathing through the nose), the air-way is always available. In short-necked, plethoric patients, more especially if they possess good and accurately fitting teeth, much embarrassment to breathing may arise under ether, and I know of no better way of preventing this from becoming a formidable danger than by the preliminary insertion of a small dental gag. If the teeth are deficient, the mouth may at any moment be opened by the finger or a mouth-gag. This is, however, hardly the place to enter more fully into the causes and nature of arrested breathing during anaesthesia; I hope shortly to publish the observations which I have made on the subject. I have here introduced only what seemed to me to bear most directly upon the questions raised in Dr. Howard's paper, and apologise for having occupied so much of your valuable space.

A FATAL MISTAKE.—M. Jaubert, late Sous-Préfet of Baranquette, has fallen a victim to an ill-written prescription dispensed by a female assistant. We gather from the accounts given that M. Jaubert was ill, and consulted his brother, a doctor of medicine, who wrote a prescription for a dose of antipyrin hurriedly in pencil, and sent it to a neighbour, Dr. Richaud, to be dispensed. That gentleman was away, and, in his absence, his sister read the prescription as an order for atropine. At first she refused to supply the poison to the messenger, but yielding at length to his importunities, she gave the atropine—we are not informed as to the dose—which was administered to the patient, whose death quickly followed. The lady dispenser appears to have known that atropine was dangerous, but to have yielded to a fatal desire to do an act of kindness. This case illustrates the importance of keeping alkaloids and other deadly poisons in "poison bottles," and under lock and key.