A CRITICISM OF THE REPORT OF THE ANÆSTHETICS COMMITTEE OF THE BRITISH MEDICAL ASSOCIATION.

By AUGUSTUS D. WALLER, M.D., F.R.S.,

Lecturer on Physiology, St. Mary's Hospital Medical School.

As the ninth and final utterance of an authoritative committee, formed ten years ago to inquire into a most important question, and that has expended incalculable time and pains upon that question, the report of the Anæsthetics Committee cannot be regarded as other than meagre and unsatisfactory, and even, as we shall presently have to recognise, incorrect in certain essential particulars. Yet if such a report had come to us as the outcome of the industry of a single worker, we should certainly not be disposed to withhold our unstinted admiration from the laborious care that has been devoted to a most unpromising task, and the sobriety of judgment that has evidently dictated its more negative conclusions. For it assuredly is a most difficult task to extract any clear and definite conclusions from the great mass of disparate data that constitute the clinical literature of anæsthetics. And the authors of the statistical report just published have made a valuable contribution to knowledge, if only by having demonstrated that the central point of the whole inquiry—as to the relative dangers of chloroform and ether—cannot possibly be determined from even the most extensive and the best-digested statistics of clinical records.

In the final report of a statistical inquiry of such scope and duration, one desires above all quantitative justification of the conclusions arrived at; and by conclusions one understands conclusions in their strict logical sense as resultants from data, not merely terminal remarks and statements disconnected from or loosely connected with the data presented in the body of a report.

Throughout the thirty-five "conclusions" of the Subcommittee the desideratum is altogether fulfilled in the 1st alone, and partially fulfilled in the 10th, 13th, and 14th. Of the remaining conclusions, some contain qualitative statements of opinion of which it is difficult to find justification in the body of the report, others are frankly negative, to the effect that no conclusion is warrantable from the evidence examined.

As regards guidance towards practice conveyed in these conclusions, two principal points are stated—accompanied, however, by reservations of an exceedingly indefinite scope—in the 2nd and 15th conclusions, to the effect that, excluding nitrous oxide, ether may be accepted as the safest routine agent, and that, in conditions of good health, chloroform is very much more dangerous than other anæsthetics.

A first practical inference may be drawn from this double statement—namely, that ether (or nitrous oxide) and not chloroform, should be employed for all ordinary purposes of minor surgery and dentistry.

But may the numerical estimates of this report be taken as affording any quantitative basis to the greater and smaller danger values of chloroform and ether? This question requires to be examined with some care, since it is upon this series of data that the credit to which the report may be intrinsically entitled is principally dependent.

The first and chief conclusion (relative safety of the various anæsthetics, p. 123) presents succinctly the whole of the numerical values obtained by the Subcommittee from a careful digestion of utilisable clinical evidence (12,517 cases capable of exact classification), and constitutes that portion of the report upon which its real value and the utility of this system of inquiry must stand or fall.

The issue is there very properly presented in its simplified expression as lying between ether and chloroform, representing the ether group and the chloroform group of anæsthetics, and an attempt is very properly made to quantitatively estimate the relative safety of these two groups by a numerical estimate of their "danger-rates." The first thought of any critical reader will therefore be to satisfy himself as to whether the figures expressing the relative "danger-rates" have been properly calculated from the clinical data contained in the body of the report.

Limiting ourselves to the figures given for chloroform and

ether, which alone depend upon any considerable number of cases, we find (p. 123) the danger-rates for chloroform and ether given as 0.582 and 0.065 per 100 respectively, the cases of danger, including death, considered as having been entirely due to the anæsthetic being given as 78 for chloroform and 3 for ether (The total numbers of cases taken are not stated, and we are, even after careful reference to the body of the report, left in doubt as to what these were.)

But apart from this parenthetic defect, we find that the danger-rates as given have been calculated upon a totally illegitimate basis. "Danger-rate" signifies danger or chance of death, of which the numerical measure is the number of deaths occurring in a sufficiently large number of trials or cases. We are not entitled to take cases of reported "serious danger" as an element in the calculation of danger-rates; danger-rates are simply probability figures to indicate chances of death. And on detailed examination of the report we find that it is only in the case of chloroform that any data exist for the calculation of a danger-rate, and that not one single case of death was met with that could be considered as the uncomplicated effect of ether. The danger-rate of 0.065 has been derived from 3 cases (presumably 672, 673, and 674) in which the anæsthetic "threatened the patient's life." The basis of calculation is thus not merely slender, but inadmissible. The facts and figures of the report afford no basis from which to compare the death-rates of ether and chloroform, nor *a fortiori* of any of the anæsthetics referred to in the remainder of the list given in Conclusion I (p. 123). The whole of these figures are illusory.

A second practical inference from the report of the Committee is that chloroform, in spite of its admitted greater danger of administration, is an indispensable anæsthetic. That being so it is desirable that its price in lives should be as far as possible ascertained in relation to the various methods of its administration. This would appear to be an eminently clinical question answerable in part at least by clinical statistics.

We turn to the report, and under the heading "The Best Methods of Administration" find the following four conclusions (Report, p. 124):

(iii) No method of administration is free from danger, but an examination of the complicated cases appears to show that the occurrence of danger depends largely upon the administrator who employs any particular method.

(iv) No conclusion from the evidence before the Committee as to the best method of administration of ether and "gas and ether" is possible.

(v) The data warrant the conclusion that the A.C.E. mixture should not be given from a closed inhaler (for example, Clover's). This conclusion applies to all mixtures containing chloroform (vide *Anæsthetic*, No. 5, p. 94).

(vi) As regards the rate of use of the various anæsthetics, no conclusions other than those in the section on "Quantity of Anæsthetic Used," p. 94, can be offered.

All these are indefinite qualitative statements of opinion. "No method of administration of chloroform is free from danger," but we know that, and what we want to know is how dangerous the various methods are. We are anxious above all to know whether a method of administration elaborated during the last twenty-five years from a crude device due to Junker, and crudely applied to animals in every physiological laboratory, is or is not what it has been repeatedly claimed to be on the strength of data published in the *BRITISH MEDICAL JOURNAL*, *Lancet*, and elsewhere over the names of experienced medical practitioners, namely, a volumetric method for the relatively "safe" administration of anæsthetics.

Altogether inadequate if not misleading allusion to this most important point is made by the Committee (or Subcommittee) on Anæsthetics, on page 60, in connection with Case 719, on pages 90 to 92, and on page 95; on page 92 one death is considered as attributable to this method, inasmuch as it happened in a case tabulated under "combined methods," where a Junker apparatus was in use (presumably Case 719 on page 60), and death is attributed to the two factors—anæsthetic and state of patient.

On turning back to Table L on p. 91, we find that it credits the "Junker" method with 411 uncomplicated as against 11 complicated cases, none of which were seriously complicated classes B₇ or B₈ (the rubric employed by the report to denote danger of death and actual death).

Indeed, it is rather surprising that this Table L on page 91

should not have been referred to in connection with Conclusion III that no method of administration of chloroform is free from danger, for at least a tentative order of merit of methods, and the cases upon which such tentative order of merit is based. The order would have come out thus:

—	Total Number of Cases.	Cases of Serious Danger or Death. (By + Bb.)	Rate per Minute at which CHCl_3 was used.
"Junker" ...	411	0	5.58 minims.
"Open Method" ...	326	0	Not given.
Lint ...	3,793	23	8.25
Skinner or mask ...	2,375	26	8.86
Towel ...	4,441	65	10.6

And from the data given in Table LXVI on page 95, the rate at which the anæsthetic was used would have come out as in the right-hand column above.

I hold no brief for any method; my opinions to-day are those which I have deliberately formulated in the BRITISH MEDICAL JOURNAL upon two previous occasions—in introduction of the discussion on Anæsthetics at Montreal in 1897 with reference to the relative power of ether and chloroform; and in a communication to the Society of Anæsthetists in the following year with reference to the paramount necessity of dosage in the administration of chloroform. The principal conclusion of the first communication was that chloroform is seven times as powerful as ether. The principal conclusion of the second communication was that successful chloroform anæsthesia requires the regular respiration of air in which the chloroform vapour is maintained between the limits of 1 and 2 per 100.

The opinion expressed by the Committee that no method of administration is free from danger is probably correct, but I am convinced that the dangers of anæsthesia, as shown by the annual death-rate returned by the Registrar-General as occurring under anæsthetics, can and will be considerably diminished.

But clinical inquiry, as exemplified in the ninth and final report of the Anæsthetics Committee of the British Medical Association, has said its say after ten years of careful deliberation, and it is evident that further clinical inquiry on these lines can serve no useful purpose.

What has the ordinary administrator of anæsthetics learned from this clinical inquiry? That chloroform is more dangerous than ether (Concl. I), but that as regards methods of administration (III and IV), rate of use (VI), methods of restoration (VII), clinical evidence has not warranted any conclusion. That special circumstances may render the use of some anæsthetic or combination of anæsthetics both safer and easier than that of ether or nitrous oxide (II), but that no indications are derivable from the present inquiry as to the nature of the anæsthetic or combination of anæsthetics that it may be advisable to employ (XXXIV) with the exception of the A.C.E. mixture, which holds an intermediate position between chloroform and ether (XXXI) but is more dangerous to males than females (VIII, XIII). And the most important clinical conclusion is the last (XXXV), that by far the most important factor in the safe administration of anæsthetics is the experience of the administrator, and that in many cases the anæsthetisation is of such importance and gravity that it is absolutely essential that an anæsthetist of large experience should conduct the administration.

This last, and evidently most desirable, condition is not always easy to fulfil, and we must above all look to increased diffusion of precise knowledge for diminution of the death-rate by chloroform. For knowledge to be diffused it must first be acquired. To this end certain definite steps should be taken:

1. An experimental examination of the statement that by proper application of a volumetric method anæsthesia can be certainly effected of any required degree.
2. An experimental comparison of the relative power of various anæsthetics.

3. The determination of the best method of quantitative estimation of anæsthetics in the various fluids and tissues of the body.

4. A careful redetermination of the statements made by Snow, Gréhant, Paul Bert, and Dubois with reference to the percentage of chloroform required for various degrees of anæsthesia, and the percentage of chloroform in the fluids and tissues of animals variously anæsthetised.

A PRELIMINARY NOTE ON THE ETIOLOGICAL AGENT IN VACCINIA AND VARIOLA.

By DR. M. FUNCK

Chef du Laboratoire de Bactériologie of the University of Brussels;
Director of the Institut Sérothérapique.

[Translated from the French MS.]

THE scientific history of vaccinia dates from 1798, the year in which the memorable discovery of Jenner was published. Since then the practice of vaccination has been fairly rapidly extended to most civilised countries; but, in spite of the continuous researches of a great number of experimenters, we have not yet attained to any certain knowledge of the etiological agent of the affection.

A series of investigations carried on for two years enables me to give here for the first time an experimental demonstration of the cause of vaccinia and of variola. These two diseases are caused by a similar micro-organism, a protozoon of large size, easily observed under a small microscopical magnification.

A detailed report of my experiments with explanatory figures will appear very shortly, but I desire permission to give a summary of work the importance of which will not be overlooked, work which may have a considerable influence on the study of the etiology of a whole group of dreaded diseases—the eruptive fevers.

I propose to prove experimentally the following facts:

1. Vaccinia is not a microbial disease.
2. It is caused by a protozoon easily found in all vaccine pustules and in all active vaccine.
3. The inoculation of this protozoon in a sterile emulsion reproduces in susceptible animals all the classical symptoms of vaccinia.
4. This inoculation renders the animals refractory to subsequent inoculation with vaccine.
5. The variolous pustule contains a protozoon morphologically similar to that in the vaccine.
6. It follows from these experiments that variola and vaccinia are two identical affections, that vaccinia is nothing more than an attenuated form of variola, and that consequently the immunity to small-pox which vaccination confers does not form an exception to the general laws of specific immunity.

I. *Vaccinia is not a Microbic Disease.*

I have compared the vaccines issued by the principal European vaccine institutes. It has long been known that freshly-prepared vaccine, that is to say, the glycerine emulsion of the contents of the vaccinal pustule, contains a very large number of bacteria. It could hardly be otherwise, considering that the vaccine lymph is made by scraping the pustules, and evidently that operation cannot be conducted antiseptically. The fact that vaccine undergoes "autopurification" is acknowledged by most bacteriologists, and we know that in three months most of the microbes originally contained in the lymph have disappeared. I have examined from this point of view 20 specimens of vaccine from different sources, and after being kept in sealed tubes in the dark for three months all were sterile, although they still produced absolutely characteristic pustules. I have proved that the contents of the pustules produced by the inoculation of this purified lymph are themselves sterile, and present the same morphological appearance as the fluid of the ordinary pustules of vaccinia.

It follows, therefore, that vaccine freed from microbes (aërobic and anaërobic) produces the specific pustules.

II. *The Etiological Cause of Vaccinia is a Protozoon.*

When a microscopical preparation of vaccine is made in the ordinary way, that is to say, if the vaccine is spread out on a cover glass fixed after desiccation and stained in cold or hot solutions, it is found that the preparation contains a large