

The History of Anesthesiology

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PROCEEDINGS OF THE ROYAL SOCIETY OF MEDICINE

EDITED BY
JOHN NACHBAR, M.A., M.D.
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SECOND

SESSION 1908-9

PART I

GENERAL REPORTS

SECTION OF ANÆSTHETICS

CLINICAL SECTION

SECTION FOR STUDY OF DISEASE IN CHILDREN

DERMATOLOGICAL SECTION

ELECTRO-THERAPEUTICAL SECTION



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TABLE OF CONTENTS

- I. *Introduction*
Vandam, LD. Anesthesia journals.
- II. *Proceedings of the Royal Society of Medicine. Volume Two. Part I. 1908-1909.*
Gill, Richard. Chloroform action. Pp 1-14.
- III. *American Journal of Surgery. Quarterly Supplement of Anesthesia and Analgesia. Volume XXVIII. 1914.*
Crile, George W. The influence of anesthesia on acidosis. Pp 15-17.
Pike, FH. Some considerations of the nature of surgical shock. Pp 7-12.
- IV. *Current Researches in Anesthesia and Analgesia. #1. August 1922.*
Editorial foreword. P 1.
Bourne, Wesley, and Duncan, James W. Morbidity and mortality in obstetrics as influenced by anesthesia. Pp 27-33, 39.
- V. *Anesthesiology. Volume 1, Number 1. July 1940.*
Haggard, Howard W. The place of the anesthetist in American medicine. Pp 1-12.
- VI. *British Journal of Anaesthesia. Volume I. 1923-1924.*
Foreword. Pp 1-3.
Bourne, Wesley. On the teaching of anaesthesia to medical students. Pp 74-78.

VII. *Anaesthesia. Volume I. 1946.*

Foreword by Sir Alfred Webb Johnson.

Editorial

Featherstone, HW. The Association of Anesthetists of Great Britain and Ireland. Its inception and its purpose. Pp 5-9.

Gillespie, Noel A. The effect of the war on the position of the American anaesthetist. Pp 35-37.

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CONTENTS.

November 6, 1908.

	PAGE
Chloroform Action. By RICHARD GILL, F.R.C.S.	1

December 9, 1908.

Anæsthesia in the Human Subject, with known Percentages of Chloroform Vapour. By N. H. ALCOCK, M.D.	15
--	----

January 20, 1909.

Discussion on Proposed General Anæsthetics Act, 1908	37
---	----

February 5, 1909.

Discussion on the Proposed General Anæsthetics Act, 1908 (<i>continued</i>) ...	47
---	----

March 5, 1909.

Treatment of Shock during Anæsthesia. By DUDLEY W. BUXTON, M.D. ...	55
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**JOHN BALE, SONS AND DANIELSSON, LTD.,
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Section of Anæsthetics.

November 6, 1908.

Dr. LLEWELYN POWELL, Hon. Secretary of the Section, in the Chair.

Chloroform Action.

By RICHARD GILL, F.R.C.S.

THE occurrence of critical states during the administration of chloroform provides the first difficulty embracing the study of its action, for causes occasionally intervene and their several results are intermingled or commingled with those produced by chloroform. Thus the base of the tongue falls backwards and may completely block the glottis, and then you have rapidly produced asphyxia. If the base of the tongue is not at once replaced in its normal position, in a short time death will ensue. Again, in the course of chloroform anæsthesia or narcosis, when the sympathetic nerve is touched, either in the neck or the abdomen, there is a sudden fall of blood-pressure; it occurs during the administration, but it is not causally connected with the action of chloroform. You will observe the characterizing feature of the fall of blood-pressure when it is produced by vasomotor reflex action. It occurs suddenly, but when a fall of blood-pressure is produced by chloroform it is produced rapidly. One of the reasons why chloroform is called upon to explain all these circumstances of arrest of respiration and fall of blood-pressure is the ignoring of another cause acting simultaneously with but independently of it. When there is a fall of blood-pressure the blood passes from the general system into the splanchnic area, and when it gets there, there is a natural tendency for it to return to the main circulation. But it has to pass through the alveolar circulation; the blood passes through the vena cava to the right side of the heart and, after being oxygenated,

through the pulmonary veins to the left ventricle and back into the general circulation. But it may be prevented from getting to the left ventricle; for instance, if the lungs are congested it will be so, and they are congested when chloroform is used with narcotic intensity. The blood, instead of being able to pass freely through the alveolar circulation, as it would if air is present in the alveoli—and air will be there provided the airway is patent—the left side of the heart is prevented from being properly supplied with blood, the action of the heart fails, and in these circumstances death results. These cases of crisis, therefore, do not form proper material on which to found a conception of the physiological action of chloroform, because there is no identity among them. Scarcely any two of them are exactly alike; even if they are alike in kind they present differences in degree; consequently in all cases of complication, which are called critical, the objective of the investigator should be, in my opinion, the investigation of the causation of the fall of blood-pressure or of the arrest of respiration, and not the endeavour to prove that chloroform kills by paralysing the respiratory centre or that it kills by paralysing the heart.

The second difficulty which is provided by the chloroform question is found in the multiple nature of chloroform action. That has not always been considered with the regard it demands. There is a local action of chloroform. I am told by patients that when they breathe chloroform they do not feel any inconvenience in their airway; therefore we will exclude the local action of chloroform on the mucous membrane of the respiratory tract. When chloroform gets into the blood its local action on it, excluding the removal of oxygen, is inappreciable. I shall consider later the relations of pure chloroform and freshly drawn blood to each other, but when we consider the other actions of chloroform—negative, physical, and physiological—we come to differentiate the various sources by which chloroform produces arrest of respiration and failure of the pulse. The negative action of chloroform results from the displacement of air by chloroform in the chloroform atmosphere which the patient breathes. When you remove a certain proportion of air from the atmosphere there is an increase in the frequency of respiration. That occurs in chloroform anæsthesia, and it is very important and I must consider it again later. Secondly, if you increase beyond the normal the quantity of chloroform in the airway you tend to bring about respiratory arrest by charging the respiratory tract with too large a proportion of chloroform. I am afraid that has been done a good many times, but is very easily removed by squeezing the lower part of the chest

whenever it happens. It is, however, generally associated with a rapid displacement of the base of the tongue, and it is difficult in these cases to detect or anticipate, or always be quite ready for the new position of the tongue. The physical action of chloroform may bring about respiratory disability, and I have seen entire absence of movement brought about by the presence of an undue amount of chloroform in the airway. Respiration will be restored not by introducing various drugs subcutaneously but by proceeding, first, to place and keep the base of the tongue in its normal position, and then to squeeze the lower chest so as to expel the heavy vapour which has been allowed to collect in the airway. When the cause is removed the result disappears. Thus it would be very erroneous to say that chloroform, because it produces respiratory arrest in this instance of it, does it by paralysing or depressing the respiratory centre, because in such a case the respiratory centre is not affected at all.

So far as the physiological action of chloroform is concerned, that is the keynote of to-night's address. But I have not yet done with the difficulties surrounding the chloroform problem. We have to keep clear of critical states, and we should, with a view to reaching a rational conclusion, treat each case according to its merits. Whenever we have these various irregular or accidental results happening when chloroform is administered, we have to inquire whether it is produced by a physical or physiological action of chloroform. But when we have got all these things clear, and when we are experimenting with chloroform, we have again to be very careful not to confound normal and abnormal conditions. Much misconception may be produced by experimenting with chloroform upon abnormal subjects—for instance, the anæmic. It is well known that the anæmic resent chloroform, and these require a very small proportion compared with the normal subject. Probably that is one of the grounds for the conclusion which early investigators reached, namely, that chloroform was capricious in its action; that is to say, small quantities produce a very serious effect to-day, and large quantities would hardly produce any effect to-morrow. But there is another explanation of that discrepancy, namely, that chloroform is apt to decompose and become weak, and then a larger quantity has to be used to produce a given effect. Whenever you administer chloroform with a view to determining what it does, you must have normal conditions; and if you have a strong, healthy young man you get normal conditions, a normal quantity and quality of blood, and normal cardiac and respiratory actions; chloroform must not be administered so as to produce any absolute

impairment of the action of the respiratory machine. When you have paved the way by these means, and given chloroform so as not to unduly impair the breathing, you will find the chief phenomena are: First, increased frequency of respiration; secondly, unconsciousness; thirdly, a result which has not been very closely investigated or taken into consideration, namely, muscle atony; fourthly, there is, in the higher degrees of chloroform action, a discoloration of the blood. Those results invariably occur when you give chloroform, and they have to be explained.

We know from our knowledge of philosophy that a single cause or agent can only produce in one of its spheres of action one effect, and we have to find out now what part or tissue chloroform directly affects. If you say that chloroform directly affects the cerebral centres so as to suspend their action, you have to explain how this altered state of the cerebral centres causes acceleration of the respiration and muscle atony, because you get those results invariably accompanying unconsciousness produced by chloroform, and you have to treat them philosophically. One is a direct result. Obviously the cerebral centres will not fit the explanation. Is it the respiratory centre? Does chloroform directly affect that centre? There is respiratory acceleration. Assuming that it does affect that centre, you have to explain how it produces unconsciousness, because now the altered condition of the cerebral centres becomes an indirect result. Then you have also to explain how respiratory acceleration causes muscle atony. There is no direct connexion between the respiratory centre and the cerebral centres, nor a direct relation between the respiratory centre and the muscles. Therefore that notion fails. Then we come to the blood, and that is a very interesting problem. Chloroform is absorbed into the blood and circulates in it, and the question is, How long does it do so? In states of chloroform anæsthesia there is a very slight degree of discoloration, so slight, in fact, that many people would not appreciate it, though that may be from want of refinement; its acquisition requires long experience, so that you may tell slight shades of difference in the colour of the blood and in the colour of a patient's complexion. When you increase the intensity of chloroform action you will find the blood becoming darker, and as you increase the intensity of chloroform narcosis it becomes darker still. I believe there are very few people who have seen the blood so dark in the human body that it is absolutely black, and issuing out of it apparently as drops of tar, and who yet have witnessed the recovery of the patient. In chloroform anæsthesia the colour of the blood is

practically normal; in chloroform narcosis there is dark blood, and the greater the intensity of the narcosis the darker the tint of the blood. We have the same difference also evidenced with regard to respiration. The frequency of respiration in chloroform anæsthesia is 28 to begin with, although it gradually increases in the course of the chloroform action. We find it can only produce these indirect results, namely, acceleration of respiration, suspension of the functions of the cerebral centres, and muscle atony, through the blood. And when you come to consider it you have to ask how it can do so. Does it affect the plasma, or the red cells, or the leucocytes? Take these things seriatim, and commence with oxygen. You assume it takes away the oxygen of the blood. The truth is you have reduced the proportion of oxygen in the blood below a certain level, so as to bring about a suspended condition of the cerebral centres, which we call unconsciousness.

What about the negative action of chloroform? When you give a chloroform-air atmosphere, a certain proportion of air is removed, and we know that less than the normal amount of oxygen is breathed. You tend to bring about, then, respiratory acceleration. But that objection is not valid in chloroform anæsthesia nor yet in chloroform narcosis, because in the former you find that acceleration of respiration reaches 28, but the colour of the complexion is almost normal. What is the proportion of air displaced in the chloroform-air atmosphere? When you give gas, or take away a sufficient proportion of air and replace it by any other inert substance, such as nitrogen, so as to produce a frequency of respiration amounting to 28, you will at the same time have occasioned duskiness. You cannot by this means associate the normal colour of the blood with a 28 frequency of respiration; but when you give chloroform you can produce a frequency of 28 without producing duskiness. That is a very interesting association, but there are others. Though it is not my intention to go through them all, I will mention a few and illustrate the conclusion which I have reached regarding the action of chloroform. Chloroform is absorbed into the blood, and is oxidized by the nascent oxygen of the blood. It opposes considerable resistance to the oxygen of the atmosphere because oxygen loses some of its power when it is diluted; but nascent oxygen, *i.e.*, pure oxygen, is known to be a very powerful agent. You have only to take fluid chloroform and mix it with a solution of hydrogen peroxide and it is destroyed at once by the latter. You may use fresh blood in the place of the peroxide. Take a pint of blood and put in 2 dr. of chloroform and shake them up, and you will not be able to detect the faintest trace of the odour of chloroform;

but the blood also is destroyed. By consequence of the diminished amount of oxygen in the blood, all the indirect results which we have to explain are satisfied. The direct result is on the blood—we may call it a minus-oxygen condition of the blood; that condition is known to be associated with unconsciousness. You know from the physiological laboratory that the respiratory centre is stimulated by diminution of oxygen, or an increase of the quantity of CO_2 assumed to be circulating in arterial blood, or by both of them. Here we have diminution of oxygen and respiratory acceleration; so there are two of the indirect results of chloroform explained. How is muscle atony explained? Atony of the muscles is produced, but not at once. Take chloroform anæsthesia at the beginning, test the muscles, and you find they do not differ much from the normal. Test again in half an hour, and although the same intensity of stimulus is used, there is a difference—the muscle is not so alert. At the end of an hour the experiment becomes interesting, for if you touch a muscle with a knife the resulting contraction will be seen to be limited; at the end of another half-hour the wave of contraction is still further restricted. I have never seen absence of muscle contraction following stimulation, but I have seen it greatly delayed and very limited.

We come now to a comparison of the conditions obtaining in chloroform narcosis with those of chloroform anæsthesia. In chloroform anæsthesia we have acceleration of respiration to 28, with (practically) the normal colour of the blood retained; muscle tone is normal to start with, but it progressively diminishes in the course of chloroform anæsthesia; there is also marked contraction of the pupil. In chloroform narcosis we have the following differences: first, there is a higher acceleration of respiration, the blood is darkened, and the muscle tone is from the first affected. We know that muscles are very sensitive to minus variations in the proportion of oxygen, and if you quickly take oxygen away from a muscle it becomes rigid. That is a curious point. During the course of the narcotic action of chloroform the muscle tone is progressively impaired. The explanation, therefore, of all these phenomena seems to be just. There is no prevailing argument which can be brought forward against the conclusion that chloroform is oxidized by the blood. That theory would reduce the action of the chloroform to a chemical one—chloroform is destroyed, and as a result of its destruction there is a diminished amount of oxygen in the blood. I might refer to the discoloration of the blood. You can show that that is due to the removal of oxygen from the blood. The dilated pupil is a muscular factor, and is explained by atony of the

circular iris muscle. The following is an instructive series of experiments to make. I suppose most people who give chloroform have had the ill-luck, at times, to produce various degrees of chloroform narcosis; the pupil is then seen to have various degrees of sluggishness—the more dilated the pupil, the more sluggish the muscle becomes. I do not like to say how many times I have seen an insensitive pupil. So, on the other hand, by adding oxygen to the blood you can go down the scale, having run up it before by increasing the intensity of chloroform. You have increased the frequency of respiration and the intensity of muscle atony and of sluggishness of the pupil; now give oxygen and maintain the normal patency of the airway, and then the normal colour of the blood will be restored and the respiration will be reduced to 28. At the same time the pupil will again become a pin-head pupil, and the tone of the muscle will be improved. Those occurrences cannot be explained by the negative action of chloroform, but only by the view that chloroform, dissolved in the blood, is decomposed by the oxygen of the blood. We know chloroform is a type of narcotic which acts by removing oxygen from the blood. Thus the conclusion arrived at is that chloroform is oxidized by the blood. Now take freshly drawn ox blood and treat it with chloroform; unless you are very careful the results will be disastrous—you will destroy it. The blood is a delicate tissue, and fluid chloroform possesses a very powerful local action. When I commenced experimenting with chloroform and blood I was almost disheartened. The conclusion seemed so sure, from the reasoning I had done, that when I added chloroform to blood I expected the blood to at once become black, but instead of showing the wished-for colour it was destroyed and became clotted and of a vermilion tint. That was the very opposite to what I expected. I tried in that way for some little time, and once, by chance, I left one of the vessels containing this vermilion-coloured blood on the mantelshelf, and about two hours afterwards I found that the colour of the blood had become black. That gave me hope. I had used blood to destroy chloroform, but in the process the former was also destroyed. So I had to try some means of attacking the blood without destroying it. As a means of producing unconsciousness we use chloroform in the condition of vapour; that vapour is absorbed in the blood and does no harm to it—in fact, you cannot put enough chloroform vapour into the blood to destroy it. By means of two phials, connected by an indiarubber tube, I placed 2 oz. to 3 oz. of blood in one of them and 1 dr. of chloroform in the other, and gradually attacked the blood with chloroform vapour. That is done by

shaking the vessel containing the blood, the warmth of the hand sufficing to volatilize chloroform. You must have a good light, so as to appreciate the various changes of colour. First there is an alteration to a brownish tint, and this gradually deepens in the course of minutes. The change from one tint to another is so gradual that you hardly think any change is occurring. It takes some twenty to twenty-five minutes before you get the blood thin and absolutely black. There is nothing else there besides blood containing oxygen and chloroform. What has happened to the blood? It might not be the oxygen at all which is affected; the chloroform might have an action on the leucocytes or on the plasma. We take that thin black blood and shake it up with air, and then there is a gradual return to the normal colour. By means of oxygen you have reconverted that blood into normal arterial blood. Now you do not give the blood changed by chloroform a rest at all, but you go on and put chloroform to it a second time, and the process takes another twenty-five minutes. With fresh blood you can perform the experiment three times in one afternoon. Now, in order to show you what a wonderfully resistant tissue the blood is, the blood, at the end of ten days, will resist the action of chloroform once—it will change to black blood, and can be reconverted to arterial blood, but if you try a second time to make it thin and black you will fail.

This, gentlemen, is merely the framework of what I have written on the subject, and the conclusions I have been led to by long experience with the action of chloroform. It took me two series of cases of 2,000 each, and I worked on it on two different occasions, namely, 1884-6, and again 1888-90, to determine the frequency of the pulse and of the respiration produced by chloroform. It is necessary to be very careful in all these things, because when I gave you 28 as the usual rate of respiration in chloroform anæsthesia I meant that as the rate during the first five minutes of the anæsthetic action of chloroform. If you take the respiration-rate at the end of every five minutes you will find there is a tendency for the frequency of it to increase, and it increases up to the moment when the chloroform is discontinued. The pulse also shows a tendency to increase in frequency. In the two series of cases referred to, for the male in the first five minutes of chloroform it is 73, and for the female 75; by also taking the pulse-rate every five minutes you will find a tendency for it to increase, but the increase will be observed to appear after the commencement of respiratory acceleration. The view that I have just put before you is, I believe, original. Chloroform is oxidized by arterial blood. This conclusion has, as you have already

perceived, been reached not by deductive, but by inductive investigation. Chloroform can have one, and only one, direct effect in the physiological sphere of its action. But the direct result of chloroform becomes an intermediary cause and produces results of its own. These are the indirect results of chloroform. They have been mistaken for direct chloroform results, and the mistake has led to the construction of false hypotheses regarding chloroform action.

DISCUSSION.

The CHAIRMAN (Dr. Ll. Powell), in the name of the Section, thanked Mr. Gill for his admirable address. The Section could not have been inaugurated under happier auspices.

Dr. BLUMFELD said he felt very grateful to Mr. Gill for his address. Whether members agreed with his views or not, they were obliged to him for his independent view. Mr. Gill had relied on his own clinical observations, and had disregarded all that had been done in other directions on the subject; that enabled members to take a fresh standpoint. He (Dr. Blumfeld) asked for instruction, as it was difficult to follow all the author's reasoning. Mr. Gill said chloroform action depended on the oxidation of chloroform in the blood, and explained the difference between chloroform anæsthesia and chloroform narcosis, saying that they differed not only in degree but in kind, because in the former the colour of the blood was not altered, and therefore there was no diminution in the oxygen in the blood. Did Mr. Gill maintain that they were two entirely different conditions both produced by chloroform and caused in a different way? If so, how was the condition produced which was not caused by a diminution of oxygen in the blood? If it was produced by diminution of oxygen in the blood, why was not the colour of the blood altered? As far as he followed the arguments of physiologists, he did not agree with Mr. Gill that the influence of chloroform depended entirely on its oxidation in the blood. If the chloroform was oxidized surely physiologists, who had tried so much to solve the question, ought to have found some of the products of the oxidation of the chloroform. But he did not know that they had done so. With regard to the experiments with chloroform and blood in a glass, he appreciated Mr. Gill's courage and originality in the matter, but he could not imagine any two things more different than the juxtaposition of chloroform and blood in a glass vessel and the juxtaposition of chloroform vapour and blood in the living organism. He did not think one could argue from one to the other. He was sure Mr. Gill would realize that his remarks sprang from a desire to follow more closely the reasoning on which the opinions given were based.

Dr. DUDLEY BUXTON said that all the members owed a debt of gratitude to Mr. Gill for the work he had undertaken in his attempts to elucidate the action of chloroform. In the remarks he would make he was actuated by the same motives as Mr. Gill—the desire to get as near to the truth as might be. Mr. Gill and he attacked the problem from different standpoints; Mr. Gill worked from experiments done in *in vitro* and from clinical observation, while his own work, beyond being clinical, had been conducted mainly in the physiological laboratory. The advantage of the physiological method was the following. The many issues of the problem which Mr. Gill had pointed out were difficult to solve owing to intermingling action, but in the physiological laboratory these could be separated, and by the use of test experiments they could be eliminated, so that the action on separate tissues could be determined. But in the entire organism and the blood there was the interaction of the nervous system controlling the lungs and heart, and being itself affected by the heart, lungs, &c. Moreover, in regard to independent action, Mr. Gill had been obliged to resort to experiments on blood removed from its normal physiological environment, and he (the speaker) believed much of Mr. Gill's work was vitiated by having been conducted upon a tissue which was placed in an unphysiological condition. Although he could not accept all of Mr. Gill's conclusions, he thought that the knowledge of all would be enhanced by Mr. Gill's investigations, since they would give a fresh impetus to the work of other observers. If further research should prove Mr. Gill was right he (Dr. Dudley Buxton) would be the first to recognize it, and admit that the sixty years of laboratory work on the subject were useless. At present he believed that work was valuable, and that it would hold its own against merely clinical research and experiment done upon tissues outside their normal environment. He recognized that Mr. Gill had only been able to give a short digest of the important communication which he had made in his book "The CHCl_3 Problem," but he gathered that the main contention of the author was that the action of chloroform depended on the destruction of that agent by oxidation in the blood, and the concomitant deoxidation of the blood and incidentally, he presumed, of the tissues. The reader of the paper argued that the blood had less oxygen than it should have, that the somatic tissues were deprived of their normal amount of oxygen, and that therefore they tended to become rigid; they were unphysiological, and therefore they were asleep. He (Dr. Dudley Buxton) presumed it was contended that the nervous tissues were deprived of oxygen, and therefore they slept. However, as a matter of fact, when dealing with the organism as a whole, if chloroform was given by inhalation with a large excess of oxygen, so that the tissues still remained bright red and the complexion florid, anaesthesia was readily produced and even narcosis; while under like circumstances, *i.e.*, excess of chloroform in an atmosphere of excess of oxygen, tissue death could be brought about by the poisoning of the centres in the medulla oblongata. If that was true, and he had no doubt it was, the deoxidation of the tissues by the destruction of the chloroform

appeared to be altogether unproved. With regard to the experiments on blood *in vitro*, it seemed to him, from experiments he had made and which had been made by others during the work which was done by the special Chloroform Committee of the British Medical Association, that if chloroform vapour were allowed to play on the blood there was a destruction of red blood-corpuscles, hæmoglobin was liberated, and the fluid became cherry-red. If it were allowed to stand, the debris of the corpuscles would sink, and by the ordinary law of physics the lower stratum of the mass would assume a much darker colour. With regard to the oxidation of the chloroform, he was not aware that it had ever been proved that chloroform was oxidized in bulk in the tissues. Certainly, with precautions, one could recover a good deal of chloroform from the blood where the vapour of chloroform had been allowed to enter or liquid chloroform had been shaken with the blood. Further, if a patient inhaled chloroform for a fairly long time, under test conditions, all asphyxial complications being carefully eliminated, the following events were witnessed: at first, if the blood was examined and analysed, there was but little chloroform coming away. Later on rather more chloroform was coming away, while still there was chloroform going in. Later on the inflow was still greater than the outflow. When complete anæsthesia had been established for some time, and the fourth degree of narcosis obtained, there was an increased output of chloroform in the expired air. Mr. Gill appeared to maintain that after a certain point less chloroform entered the blood circulating in the lungs, and so was deported from the blood by the tidal air, without ever entering the blood which traversed the rest of the organism. However, that could be tested and had been tested. If the blood from the vein of an animal in deep narcosis were tested, one could obtain evidence of a larger amount of chloroform in it than in the blood of the afferent artery coming directly from the lung, which would appear to show that the chloroform passed into the organism, travelled with the blood-flow, and left it mainly by means of the lungs, though partly by the kidneys and the other emunctories. If it were true, as Mr. Gill believed it to be, that chloroform produced anæsthesia and narcosis in the same way as morphia produced narcosis—by deoxidation of the tissues—one would expect that the altered conditions of asphyxia would occasion anæsthesia. Many years ago, when he (the speaker) was investigating the physiological action of nitrous oxide, he did a number of test experiments on the effects on the sensory structures of asphyxia, and the conclusion forced upon him was, that asphyxia as such did not, until death was imminent, produce any diminution of sensibility. It occurred only when the higher centres were dead, and they did die just before the vital centres died. With regard to the action of chloroform upon the muscles, no doubt Mr. Gill was aware of the experiments which had been done by Professor Sherrington, in which he showed the action of chloroform on voluntary muscle, intestinal muscle, and the muscle of the heart. This investigator found that the action on the muscle was in direct proportion to the dose of the chloroform given, and appeared to be independent

of other conditions. Professor Sherrington had been good enough in response to a suggestion of the speaker to compare chloroform action upon muscle when free from asphyxia and when complicated by this condition, and his results did not support Mr. Gill's present contention. Experiments on isolated heart and isolated intestine showed that chloroform, when given in a circulating medium, such as saline, rapidly produced an effect on the muscle; it lessened its contractility and rendered it incapable of being stimulated. But if the chloroform was supplied through some competent circulating medium representing the blood, it took a larger proportion of chloroform than was necessary in cases where saline was used; in the latter case there was no question of the deoxidation of the chloroform. Sherrington also found that the tissues were influenced differentially; the myocardium was affected most, the somatic tissues being affected less, and intestinal tissue least. With regard to Mr. Gill's experiments upon muscle, nerve action had to be eliminated. He did not know whether the experiments were made on a frog's sartorius muscle, which was in part free from nerves, or from the ordinary muscle preparation of the frog. Chloroform, whether the vapour or the liquid were used, would have the effect of lessening the electro-motive action, and therefore would lessen the power of the muscle to contract. Sherrington made the test, eliminating the element of nerve action by one of many means available. It seemed to him (Dr. Dudley Buxton) that the experiment where chloroform circulated in saline fluid would eliminate the probability of chloroform exerting its influence wholly and solely by deoxidation of the blood and so acting upon muscle, and therefore upon nerve; and if upon nerve upon other tissues of the organism. That, and the fact that one could produce anaesthesia and narcosis in an organism over-full of oxygen, seemed to him to justify some doubt as to the entire truth of the view advanced by Mr. Gill that chloroform acted solely by becoming oxidized, and so deoxidating the tissues of the body. He ventured to think that the careful work of more than one generation of physiologists had not been shaken by Mr. Gill's experiments done *in vitro*, and he should need more evidence before he admitted that the physiological methods could be superseded by those which Mr. Gill had employed in his research. He thanked Mr. Gill for his enthusiastic work and for the ground which his solitary plough had turned up. He trusted he would continue his investigations and be able to advance more conclusive evidence of his contentions.

Mr. LONGHURST asked whether Mr. Gill's deductions had been made more or less from clinical experience of giving chloroform without oxygen, and whether those deductions would be altered or be compatible with the deductions he would make if the clinical experience of chloroform and oxygen cases were taken. He (Dr. Longhurst) thought that if chloroform acted purely from its deoxidating powers, and if one took a series of cases in which oxygen was freely given throughout the time of chloroform administration, the deductions would be very interesting, especially compared with those already arrived at.

Mr. GILL, in reply, said the criticism of Dr. Blumfeld was a very good one, in reference to the difference in the kind of results attending chloroform anæsthesia and chloroform narcosis. The difference in kind was explained by pulmonic congestion in narcosis. His criticism was also good concerning the removal of oxygen from the blood without discoloration. One could take away a certain proportion of oxygen from the blood without materially altering its colour. He was trying to reduce considerably the time taken in the experiment he alluded to by means of a new apparatus. Blood in a phial could be acted upon by chloroform vapour for a minute before any appreciable change in the colour of the blood occurred. In answer to Dr. Dudley Buxton, the evidence upon which he (Mr. Gill) drew his conclusions was based on the results invariably associated with chloroform action. These results were proved to be chloroform results by varying the intensity of chloroform action, and eventually he concluded that chloroform was oxidized in the blood. This conclusion was then verified according to the laws of inductive investigation. In blood which had been treated by chloroform they could not detect the odour of chloroform, and if they weighed the phial containing chloroform they would find that there was a loss in chloroform, which had been absorbed and oxidized. It must have been a slip on Dr. Buxton's part when he reproached him for experimenting with substances taken out of the body surroundings. That fact did not affect his experiments, for the blood was not at once killed by removing it from the vessels, and it could be preserved in a bottle for ten days and yet exhibit at the end of that interval a considerable resistance towards the action of chloroform. But Dr. Buxton took a fluid which was not blood and put saline into it, and treated with it portions of intestine removed from the body. Was their physiological environment disturbed by that means? (Dr. Buxton: No.) Saline was not normal to the human body. His contention was that as soon as chloroform got into the blood it was oxidized by the blood. Dr. Buxton said it produced certain effects on nerve and muscle structure. If, however, it was oxidized and at once destroyed, the results mistaken for physiological were doubtless produced by the local action of chloroform. He thought Dr. Buxton would have arranged the various effects connected with chloroform in their proper order, as any one cause could only have one direct effect; it was requisite to describe the part of the body directly affected. Chloroform could not paralyse the heart and the respiratory centre and oxidize the blood; it could only do one of these things, and he had proved that its direct effect was on the blood. The other effects—unconsciousness, muscle atony, dilatation of pupil, respiratory acceleration—could be easily explained by the minus-oxygen theory. What Dr. Buxton said about the production of unconsciousness by asphyxial means was quite true, but it should be impossible to kill by that means. Asphyxia could be produced very rapidly. If one took nitrous oxide gas, which was absorbable up to a certain point, breathing might be continuous up to sixty seconds, during which there was progressively increasing diminution in the proportion of oxygen in the blood. If this diminution was not the cause of

unconsciousness, what was? (Dr. Buxton: Oxygen is given with nitrous oxide.) Why should two agents which exercise opposite effects on the blood be given simultaneously? A person could be killed by oxygen. That was his reply also to Mr. Longhurst's criticism. He had heard that in some hospitals abroad a porter carried an oxygen cylinder about with him, and whenever a patient reached the regulation degree of purpleness the oxygen cylinder was brought into use so that the patient might become red again. His reply was: Why produce the purple colour?

Anesthesia Journals

Although certain facts may be derived from archeological studies and the like, the written word remains the most reliable source for historiography. Thus, descriptions of medical treatment may be discerned in Egyptian papyri, including superficial surgical procedures. Many millenia later we find references to narcosis and the agents employed and in the 18th century, reports on the physiology of the circulation and respiration that would eventually lead to the introduction of anesthesia. Several years before the first public demonstration of ether anesthesia in 1846, the failure of several individuals to report on their prior experiences with anesthetics delayed recognition and led to the controversy which ensued. Were it not for the perceptive accounts of the Boston surgeons initially involved, surely the acceptance of anesthesia in the USA would have been further delayed.

How different were the circumstances in England then, with its growing number of hospitals and their appointed staffs recruited from the ranks of general medical practitioners. From this source, John Snow, M.D. would step forth, possessed with the kind of intellectual and scientific attainments that make for progress. Preparation of textbooks and reports became an integral part of this specialization in anaesthesia, a tradition followed by his successors - Richardson, Clover, Buxton and Hewitt - well into the 20th century. In 1893, their common bond culminated in the formation of a London Society of Anaesthetists whose proceedings would eventually be published in the Transactions of the Royal Society of Medicine (1773) in 1908. Thus arose the elements of a new medical specialty later defined by Ralph M. Waters as consisting of men, publications and organizations. This current compilation of original material focuses on those journals which paved the way toward a full-fledged medical discipline.

Leroy D. Vandam, M.D.

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[American Journal of Anesthesia and Analgesia]

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CONTENTS

ORIGINAL ARTICLES

TECHNIC OF SPINAL ANESTHESIA.

W. WAYNE BABCOCK, M.D., F.A.C.S.,
Philadelphia, Pa. 2

SOME CONSIDERATIONS ON THE NATURE OF SURGICAL SHOCK.

PROF. F. H. PIKE, Ph.D.,
New York City 7

PROPHYLAXIS OF POSTANES- THETIC VOMITING.

H. WARREN BUCKLER, M.D.,
Baltimore, Md. 13

THE INFLUENCE OF ANESTHESIA ON ACIDOSIS.

GEORGE W. CRILE, M.D., F.A.C.S.,
Cleveland, O. 15

INSUFFLATION ANESTHESIA.

F. W. NAGLE, M.D.,
Montreal, Canada 17

LOCAL ANESTHESIA AS APPLIED TO THE RADICAL CURE OF HERNIA.

JAMES F. MITCHELL, M.D., F.A.C.S.,
Washington, D. C. 19

DEPARTMENTS

EDITORIALS, 24.

BOOK REVIEWS, 25.

INDEX AND ABSTRACTS, 26

given every four hours. The sugar solution is repeated on the second day and later until the patient is on a diet sufficient to prevent any carbohydrate starvation.

Following out the theory of Waugh that a too restricted diet or excessive purging might be conducive to subsequent acidosis, Dr. Stickney of the Woman's Hospital has rearranged the preliminary routine prior to operation. He gives a purge of castor oil forty-eight hours before operation, and an enema the night before, repeating same in the morning until the fluid is clear. If the patient enters the hospital within twenty-four hours of time for operation the purge is omitted and the bowels emptied only by enemata. Regular diet with plenty of starchy food is maintained until noontime of the day before operation, then a slight supper and albumen and sugar water at midnight. Of course, this régime varies somewhat according to the nature of the operation.

Our results have been most astonishing. We have had no cases of severe vomiting and many with absolutely none at all.

Formerly vomiting was looked upon as to be expected, now it is the exception. Of course, one will say that frequently cases recover without any vomiting. This is perfectly true, but the most striking evidence of the part an acid intoxication plays in post-anesthetic vomiting and the efficacy of prophylactic treatment is seen in those patients who have had several anesthetics, giving a history of severe vomiting with no prophylactic treatment and none at all when properly protected against an acidosis. I believe that nearly everyone develops more or less of an acidosis after a general anesthetic. Whether they will show symptoms of an intoxication or not depends upon the eliminating powers of their kidneys. In a series of cases we examined the urine before and after anesthesia for acetone and found it present in nearly every case after operation. The degree of acetoneuria, however, was of no prognostic value in determining the possibility or probability of an intoxication for the reason just given. There is no doubt that some individuals are more prone to subsequent intoxication than others. Children are very susceptible. Patients with a wasting disease, those with pre-existing vomiting as a symptom of their surgical ailment, are the ones to look out for. I have also been very much impressed with the apparent close association between nervousness and fright and subsequent acidosis.

I regret exceedingly that I have been unable to substantiate my remarks with detailed statistics of

a given number of cases. Scattered as they have been in different hospitals, I can base my conclusions only upon the opinions of patients, nurses and surgeons. These seem so uniformly favorable to the benefits derived from prophylactic treatment of post-anesthetic vomiting on the supposition that it is largely the result of an acid intoxication that I feel justified in calling this society's attention to the subject in the hopes that its efficiency will be tried by others.

THE INFLUENCE OF ANESTHESIA ON ACIDOSIS.*

By GEORGE W. CRILE, M.D., F.A.C.S.,

Professor of Surgery, Western Reserve University, and
Surgeon to Lakeside Hospital,
CLEVELAND, OHIO.

The energy of the body which, in different ways is derived from the environment, is temporarily stored in the several organs and tissues until in response to some adequate stimulus it is transformed into heat or motion. Prominent among the products of this transformation of latent into kinetic energy are acids. We may say that every motion, every emotion, every injury, every physical exertion, every degree of fever, every reaction to infection or to autointoxication, every respiratory movement, every heart beat, produces an acid. Under normal conditions these acids are neutralized into harmless compounds, which are eliminated by the kidney, so that under normal conditions the body tissue and fluids are for the most part slightly alkaline. If every activity of the body produces acidity in a greater or less degree, it is vitally necessary for the body to maintain a large margin of safety against acidosis by the presence of alkaline salts and bases, which are derived from food.

Experiments have shown that for the maintenance of its normal state of slight alkalinity, the body is dependent primarily upon the liver, and secondarily upon the adrenals. When the liver is excised the blood soon loses its slight alkalinity and in a few hours becomes acid. When the adrenals are excised the alkalinity of the blood is maintained for a longer period, perhaps twice as long, but it then becomes acid, and in each case the acidity of the blood is the close precursor of death. The excision of no other organ in the body produces this tendency to immediate acidosis.

We cannot dwell here upon other experimental observations which show the relative activities of the liver and the adrenals in this neutralizing

*Read during the Second Meeting of the American Association of Anesthetists, at Atlantic City, June 22, 1914.

process, nor the evidence that the liver and the adrenals are directly controlled by the brain, which also controls the transformation of energy, which in turn, as we have already stated, always produces acidity; we will merely recapitulate by saying that the harder the body is driven by any stimulus, the more rapidly will latent energy be transformed into kinetic energy. The more rapid the transformation of energy, the greater the production of acid. The greater the production of acid, the greater also the strain upon the power of neutralization possessed by the liver and the adrenals, and the greater the drain upon the body's store of alkalies and bases. When the liver and the adrenals are overtaxed, and the alkalies and bases are exhausted, the state of acidosis is reached.

Clinically it has long been recognized that when a patient is in a state of exhaustion resulting from infection, from injury, from shock, from starvation, from hemorrhage, or from any other cause whatsoever, he may never recover consciousness after the administration of a general anesthetic. In a Hungarian reference, the title of which I do not recollect at the moment, it is shown that starved dogs inevitably die after inhalation anesthesia. Clinicians know well how unsafe it is to give a general anesthetic of any kind to a patient on the verge of acidosis. A patient with chronic vomiting, with or without chronic pyloric obstruction, with an acetone odor of the breath, with peculiarly pink lips and dry tongue and mouth will in all probability never regain consciousness after being anesthetized. The aged not infrequently die after even a short anesthesia.

Why do not these patients recover? If the patient has the power of consciousness before the anesthetic is administered what happened during the anesthesia to make it impossible for the patient to regain consciousness?

We have already referred to the acid-producing power of stimuli. Shall we conclude therefore that the trauma of the operation alone may have pushed beyond the margin of safety the neutralizing powers of the body already taxed by pre-existing conditions; or is the anesthetic itself a factor in producing the fatal result?

To answer this question, Dr. Menten in my laboratory made for me observations of the H-ion concentration of the blood under various conditions—the H-ion concentration being an index of the acidity of the blood.

H-ion concentration tests were made after the application of many kinds of stimuli, the results of which confirmed the postulate which we have al-

ready stated, that acidity is the result of the activation of the body by any adequate stimulus. The blood was then tested to determine the H-ion concentration in ether anesthesia, in nitrous oxide anesthesia, and after the administration of alcohol and of morphin. Both ether and nitrous oxide produced a marked increase in the H-ion concentration, that is, both produced acidity in the blood. After coming out from the anesthetic this acidity was neutralized by the animal in about thirty minutes. This result gave us our clue to the tendency to acidosis and to death after anesthesia of weak and emaciated patients. The increased acidity produced by the anesthesia was sufficient to overcome the already narrow margin of safety. That acid intoxication follows the administration of ether and chloroform has been noted by many observers, the acidity being evidenced by the early appearance in the urine of acetone and later diacetic acid. It has also been noted, as one writer states, that the "starvation preceding and following the operation is also a factor of considerable importance."

Our experiments have shown, however, that the increased acidity actually develops during the anesthesia itself, sometimes to a fatal degree, and that a starved condition is not only of "considerable" but of *prime* importance, since it means that the acid-neutralizing power of the liver has been purely impaired, if not possibly lost.

Two more important clues were obtained from the result of the H-ion concentration tests after the administration of morphin and of alcohol. Alcohol caused acidity, the acidity not being so marked, however, as that produced by the anesthetics. The H-ion concentration was not altered by morphin, no matter what the size of the dose. When the administration of morphin preceded the induction of anesthesia then a smaller amount of the anesthetic was required to produce complete anesthesia, and the H-ion concentration test showed that the acidity was markedly less than in anesthetized animals which had not received the preliminary dose. The preliminary dose of morphia not only lessened the degree of acidity produced by the anesthetic, but it in no way interfered with the return of the blood to its normal alkalinity; on the contrary, and the following observation is of great significance, if morphin was given after acidity had been produced by the anesthetic, it postponed the time of neutralization, and if given in large doses prevented the animal from overcoming the acidosis. That is, it would appear that morphin controls the mechanism which governs the neutralization of alkalimization of the blood.

These H-ion concentration or acidity tests of the blood have therefore given us the clue and an invaluable clue to the treatment of patients with mild acidosis or in whom acidosis is threatened. Since in every case the presence of diseased conditions is undoubtedly producing a mild acidosis, needlessly long anesthesia is to be avoided, as the increased acidity produced by the anesthetic will diminish the patient's margin of safety. The degree of acidosis seems to be proportional not only to the length but to the depth of the anesthesia. Therefore the lightest possible anesthesia should be maintained. With starved patients, with patients whose vitality is at a low ebb, in whom acidosis is already markedly present, the inhalation anesthetic may be absolutely contraindicated. If an operation is mandatory it may be performed under local anesthesia, or in the analgesia of twilight anesthesia produced by the gentlest administration of nitrous oxid oxygen.

In cases of acidosis especially nitrous oxid oxygen anesthesia is always the anesthetic of choice, for though our tests have shown that like ether it does produce acidity in the blood, unlike ether it is not a lipoid-solvent; does not impair the immunity of the body, and to some extent conserves the energy in the brain-cells from exhaustion.

Although, as I have shown elsewhere, both the pre-operative and the post-operative use of morphin is of great value in certain cases, in these cases of existing or threatened acidosis its use is contraindicated since it interferes with or prevents the neutralization of acidity in the blood, but bromides per rectum may be safely given to diminish the pre-operative psychic strain. The pre-operative administration of sodium bicarbonate and glucose is of value also.

To recapitulate: The ideal treatment for the class of patients we have been considering, those handicapped by exhaustion, in whom acidosis is present or is threatened, is:

1. The pre-operative administration of sodium bicarbonate and glucose and of bromides per rectum.
2. Ether twilight or a light nitrous oxid oxygen anesthesia.
3. A technic so accurate and so completely associated by the use of local anesthetics and gentle manipulations that but a small amount of the anesthetic is needed.
4. As rapid a technic as is consistent with good work that the period of anesthesia may be as short as possible.

INSUFFLATION ANESTHESIA.*

By F. W. NAGLE, M.D.

Lecturer on Anesthetics and Demonstrator of Pharmacology: McGill University, Montreal. Anesthetist to the Royal Victoria Hospital, Montreal, and to the Montreal Maternity.

To produce anesthesia by the inhalation of volatile anesthetics is a difficult mechanical procedure, and the results of such an anesthesia, both immediate and remote, are often alarming. The mechanical difficulty is due to the obstruction presented by the upper respiratory tract, which is increased by the local action of the anesthetic. The evil and sometimes fatal results are due to overdosage and uneven dosage, chilling of the lungs by cold inspired air, aspiration into the lungs of foreign material, and fatigue from the undue load placed upon the respiratory system.

For these reasons inhalation anesthesia has fallen somewhat into disfavor; and local, spinal, intravenous, and colonic anesthesia have been tried in its stead. It cannot be denied that in many cases these alternative methods are expedient, but the necessity for their use has been greatly lessened by the introduction of an improved method of administering anesthetics by the respiratory route, insufflation anesthesia.

By "insufflation anesthesia" I mean the condition which is produced by blowing into the patient's pharynx or trachea a quantity of air or gas sufficient to supply all his respiratory needs without effort upon his part, containing an evenly distributed, and consequently non-irritating, minimum dose of the anesthetic agent.

It is now three years since I began the use of this method, and to-day, more than ever, I am convinced that, for the majority of cases, intratracheal insufflation is the best method, and pharyngeal insufflation the next best method, for administering anesthetics through the respiratory tract.

ADVANTAGES.

The first advantage of insufflation anesthesia is that it prevents undue exertion of the respiratory system by overcoming the mechanical obstruction to the entrance of air, which exists largely in the patient's mouth.

The second advantage is that it permits of dosimetric administration; that is, all the air delivered to the patient can be measured, and can be supplied with an evenly distributed and measured amount of anesthetic per minute. The local action and reflexes caused by irritation when too concentrated an anesthetic is given, or when it is poorly

*Read during the Second Meeting of the American Association of Anesthetists, at Atlantic City, June 22, 1914.

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CONTENTS

ORIGINAL ARTICLES

- ¶ TECHNIC OF SPINAL ANESTHESIA.
W. WAYNE BABCOCK, M.D., F.A.C.S.,
Philadelphia, Pa. 2

- ¶ SOME CONSIDERATIONS ON THE
NATURE OF SURGICAL SHOCK.
PROF. F. H. PIKE, Ph.D.,
New York City 7

- ¶ PROPHYLAXIS OF POSTANES-
THETIC VOMITING.
H. WARREN BUCKLER, M.D.,
Baltimore, Md. 13

- ¶ THE INFLUENCE OF ANESTHESIA
ON ACIDOSIS.
GEORGE W. CRILE, M.D., F.A.C.S.,
Cleveland, O. 15

- ¶ INSUFFLATION ANESTHESIA.
F. W. NAGLE, M.D.,
Montreal, Canada 17

- ¶ LOCAL ANESTHESIA AS APPLIED
TO THE RADICAL CURE OF
HERNIA.
JAMES F. MITCHELL, M.D., F.A.C.S.,
Washington, D. C. 19

DEPARTMENTS

EDITORIALS, 24.

BOOK REVIEWS, 25.

INDEX AND ABSTRACTS, 26

the dangers of a tracheotomy wound it may be quickly carried out with but few instruments.

AFTER TREATMENT.

No special after treatment is required. In over 6,000 punctures without an occlusive dressing none have shown any signs of infection, so that we consider the sealing or dressing of the point of puncture unnecessary. The head and shoulders should not be elevated until one hour has elapsed from the time of injecting a light solution. Secondary headaches should not occur, and when of the characteristic spinal type, that is when they are increased by raising the head from the pillow, are associated with some stiffness of the posterior muscles of the neck, they indicate the use of contaminated or decomposed solutions. Such solutions may not only cause intense headache, but also abducens palsy, and should be promptly discarded. We do not believe that abducens palsy follows the employment of pure solutions. The anesthetic does not contraindicate the prompt administration of water either during or after the operation, and such food is given as in the particular case seems best without regard to the fact that the patient has been anesthetized. Secondary nausea or vomiting should not occur as a result of spinal anesthesia, and the patient should have less post-operative pain, less headache, less backache and general discomfort than the patient who has received ether. Finally, it is to be emphasized that the novice should not attempt spinal anesthesia without carefully investigating the subject, and should only use the method upon robust patients until he acquires dexterity and familiarity with the technic.

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SOME CONSIDERATIONS ON THE NATURE OF SURGICAL SHOCK.*

By PROF. F. H. PIKE,

Department of Physiology, Columbia University,
NEW YORK CITY.

Before proceeding to a consideration of the nature of surgical shock, it will be well to point out some of the ambiguities and general obscurities of the word shock. I have been working with a very special kind of shock—the kind that comes when the spinal cord is completely cut across, or frozen, or deprived of its blood supply at a given level. The cutting or the freezing or the lack of blood completely blocks the conduction of nerve impulses past the frozen or cut or injured point. The same result may be accomplished through the growth of a sarcoma of the vertebral column.¹ There is a

very definitely localized lesion, and the results are also definite.

If the complete transverse lesion of the cord occurs in the mid-dorsal region, we find a complete and permanent paralysis of all the skeletal muscles whose motor nerves arise below the level of the transection, together with a complete and permanent loss of sensation in all regions the sensory nerves from which enter the spinal cord below the level of the lesion. The blood pressure may be very low, or may closely approach the normal measurements, depending on whether the lesion is high up, at the level of the sixth or seventh cervical segment, or much lower down. The visceral symptoms are severe at first, but improve as time goes on.²

This kind of shock is called spinal shock, but it is a kind of shock with which you, as anesthetists, have very little to do. The kinds of shock with which you deal do not generally involve any destruction of tissues within the central nervous system. It follows from this that there are at least two different kinds of shock—the spinal shock and some other kind. Personally, I believe that there is more than one other kind.

One of the first investigators to recognize clearly this difference and to insist upon a distinction between the two kinds of shock was Professor W. T. Porter,³ and many of the other clear conceptions of processes involved together with the accurate descriptions of conditions found, so far as they deal with the physical aspects of surgical shock, have come from Professor Porter and his pupils. Concerning the chemical phenomena in shock, important though they are, we will have but little to say at this time.

Let me show what I mean by the statement that there is more than one other kind of shock. A patient may have suffered from a severe crushing injury of a limb, but not have suffered any injury to the central nervous system directly, and he may not have suffered much hemorrhage. Yet there is a fall of blood pressure, a rapid pulse, and all the other features of the clinical picture of shock. This is certainly not the kind of shock with which I have been working, since all the ordinary reflexes of the skeletal muscles, or most of them, may be obtained from the various parts of the body. There is no complete blocking of conduction involving the whole area of cross section of any part of the spinal cord.

Again, in some of the specific infectious disease, the patient may suffer collapse toward the close of

*Read during the Second Meeting of the American Association of Anesthetists, at Atlantic City, June 22, 1914.

the disease. No mechanical injury has occurred, yet the picture is that of shock.

We should, then, be careful to tell the circumstances under which shock has arisen. To speak of a particular case as traumatic shock is not sufficiently definite, unless we specify whether or not the central nervous system has been injured directly. If the central nervous system has been injured, we may have the exact homologue of the laboratory condition known as spinal shock. Not all injuries to the central nervous system result in a complete blocking of all the paths below a certain region, so that the description is not sufficiently exact unless the nature and extent of the injury is specified. The man with a crushed leg may die from shock, and the teamster who has had his spinal cord crushed beneath a load of coal may recover, although certain of the manifestations of shock may be more severe in the teamster with the crushed spinal cord.

To the laboratory worker, certain clinical descriptive terms, such as traumatic shock, do not, therefore, convey any very definite idea either of the nature of the injury or of the condition of the patient. I shall not, therefore, attempt, in this paper, to follow the ordinary clinical terminology, and I trust you will pardon me if I speak of conditions rather than names.

Since in practically all cases of acute shock, low blood pressure is one of the physical signs, we may first look into the mechanism for maintaining blood pressure under normal conditions, using this as a type of reflex nervous mechanisms, and then point out some departures from the normal as they occur in shock.

The heart and blood vessels, both of which are under the control of the nervous system, are the principal agents in maintaining blood pressure under ordinary conditions. The vasomotor nerves vary the caliber of the arteries and arterioles, and the mean blood pressure tends to rise or to fall according to whether the vessels constrict or dilate. When the blood pressure tends to fall because of the dilation of the blood vessels, the heart beats faster, because of accelerator impulses sent out from the central system over the sympathetic nerve supply, and forces more blood through the vessels. When the vessels are constricted, the heart beats more slowly, in response to impulses passing out of it over the vagus. The rapid heart rate of a man who has just finished running is familiar to all of you. But the heart will not beat faster when the blood pressure falls, nor become slower when the blood pressure rises unless the sympathetic and

the vagus nerves are uninjured and active. When these nerves are rendered inactive by cutting them across, the rate of the heart is unchanged when the blood pressure rises or falls.⁴ Rabbits can no longer run distances after these cardiac nerves are cut, although there may be no lesion of the heart or of the blood vessels. The thing to remember is that the heart while in the animal body under constant physico-chemical conditions will not change its rate unless acted upon by nerve impulses coming from outside the heart. I insist upon this point at this time because of a slight misconception that may have arisen in the past.⁵ Only after its excision from the body will changes in pressure alone bring about changes in the heart rate, and the rate then increases with the pressure.

It is my belief that there are at least four mechanisms involved in the maintenance of blood pressure, namely, (1) the vasomotor nerves, whose common point or origin lies in the medulla oblongata; (2) the heart and its nerves—intrinsic, perhaps, as well as extrinsic; (3) the skeletal muscles; and (4) some property of the tissues of the vessel walls, possibly independent of the nervous system, in addition to those properties directly under nervous control.

These mechanisms for maintaining blood pressure are, however, dependent for the most part upon the central nervous system for their efficient and coördinated action. And, as Descartes long ago suggested with reference to coördination in general, the central nervous system here serves as the mechanism of coördination between the afferent impulses and the motor response. We must consider, then, the various structural or functional elements interposed between the starting point of the afferent impulses and the muscle or gland cell in which the response occurs.

At the outset of the discussion of the nervous mechanisms involved in the process of coördination, we may point out that there are two systems concerned: (1) the somatic system, sensory and motor, and (2) the visceral system, sensory and motor.

The somatic sensory nerves arise in the organs of general and special sensation, the eye, the ear, the skin, muscles, tendons, and joints. They convey information of the general happenings in the somatic or "body" part of the organism. Most of these impulses at some time or another arise into consciousness, although some of them undoubtedly pass directly through the lower levels of the nervous system without entering into the cerebrum.⁶

The somatic motor system is concerned with the movements of the skeletal or striated muscle.

The visceral sensory nerves arise in glandular structures and in smooth muscles throughout the visceral system. Some of them convey impulses which may enter into consciousness, but others do not. They constitute the afferent portion of the sympathetic or autonomic system.

By the autonomic system, Langley means not only all the sympathetic system but also certain fibers in some of the cranial nerves, such as the vagus, and sacral fibers in the *nervi erigentes*. It includes both afferent and efferent fibers.

The visceral motor nerves, as their name implies, convey afferent impulses from the central nervous system to the various structures comprised in the viscera—the heart and the blood vessels, the gastrointestinal tract, all the glands and other similar structures containing smooth muscle fibers. They belong to the efferent portion of the autonomic or sympathetic nervous system.

In the operation of any mechanism which involves the coördination of afferent and efferent impulses, there is first of all a receptor or nerve ending at the periphery which is sensitive to stimuli. This receptor may be a free ending of the nerve in the tissue or it may be a specialized ending which is particularly sensitive to one particular form of stimulus, such as light, or touch. Sherrington has defined a sense organ as a mechanism for lowering the threshold value of the stimulus. As a rule, the nerve endings are more sensitive to stimuli than the nerve trunks. There are (a) afferent nerves leading from the receptor to the central system; (b) the junction or synapse—perhaps many synapses in series, in the central nervous system itself between the terminations of the afferent neurone and the final efferent neurone; (c) the efferent fiber; (d) the terminations or end plates of the efferent fiber; and (e) the effectors—muscles, gland cells, or whatever else they may be. All of these things enter into the formation of a reflex arc in a higher animal. And as each of them has certain peculiar properties of its own, as evidenced by its reactions to drugs, or to other changes of conditions, we may consider each one separately.

The receptors and the afferent nerves may be anesthetized either partially or wholly, by cold, pressure, cocaine, and similar agents; or some of them may become unusually sensitive through inflammatory processes.

The synapses are affected by strychnine and other drugs, and by changes in the oxygen and carbon-dioxide content of the blood. The synapses also have the faculty of summation in a high degree (Stirling). A single stimulus applied to an afferent

nerve may produce no effect, but if repeated sufficiently often, and for a sufficient time, the nerve impulse set up may become sufficiently intense to pass the synapse and produce an effect. The synapses are the most sensitive portion of the arc to the effects of prolonged lack of oxygen, as is shown by resuscitation experiments.⁷

Strychnine, up to a certain limit, increases the passability, or decreases the resistance, at the synapse. But even in small doses, it may increase the resistance or even block conduction for vasomotor impulses. This paralysis of the vasomotors may occur at a time when the passability of the synapses for reflexes of the skeletal muscles is but little increased,⁸ and the paralysis of a previously injured region of the spinal cord, e.g., during or after recovery from anemia, for responses of the skeletal muscles as well as vasomotors, occurs at a time when the reflex response of the skeletal muscles through uninjured portions of the cord is still increased. The paralytic action of strychnine is manifested earlier after previous lack of oxygen than otherwise.⁹

The efferent nerve cells are excited by an increased concentration of carbon dioxide in the blood and asphyxial convulsions may result. The cells constituting the respiratory center in the medulla oblongata are particularly sensitive to slight changes of oxygen and carbon dioxide tension in the blood. It is the changes in the concentration of the hydrogen ions associated with these slight increases in carbon dioxide or decrease in oxygen which constitute the effectual stimulus for respiratory movements.¹⁰

The relation of the efferent nerves to the effectors, i.e., the muscle or gland cells, is not altogether a simple one. There is good reason for believing that there is a third element intervening between nerve fiber and the muscle or gland cell—the receptor substance of Langley.⁵ The three elements—the end of the nerve fiber, the receptor substance, and the muscle or gland cell—constitute what Elliott has called the myo-neural junction, and, according to Elliott,¹¹ it is upon this myo-neural junction between sympathetic nerve fibers and smooth muscle that adrenalin acts. Adrenalin does not act upon smooth muscle directly, since smooth muscle which is not innervated from the sympathetic system does not respond to its application. But smooth muscle which is innervated by the sympathetic system will respond to the application of adrenalin even after the nerves going to it have been divided and have degenerated.

The myo-neural junction between somatic nerve

and striated muscle is markedly affected by such drugs as curare, by certain toxins whose origin apparently is in the gastro-intestinal tract, by the waste products of metabolism, fatigue products, and other substances of like nature.

The synapses, particularly of certain regions of the central system, and the myo-neural junctions are the weak places in the reflex arc, and the places most commonly acted upon by foreign substances, toxins, or other adverse influences. The nerve fibers are, in general, more resistant than the nerve cells.¹² The Betz cells of the cerebral motor cortex may be inexcitable during ether narcosis, but the fibers of the pyramidal tract in the spinal cord may still be highly excitable. Nor are the synapses between the fibers of the pyramidal tract and the cells in the spinal cord about which they end affected to the same extent as the synapses in other regions, such as the cerebrum.

Afferent impulses over the visceral sensory nerves are not limited in their effects to reflex responses through the visceral motor system. Irritation within the stomach may lead to vomiting, and as will be shown a little later, vomiting involves the action of certain striated muscles. Similarly, afferent impulses over the somatic sensory nerves may bring about a reflex response which will involve the visceral motor system as well as the somatic. The mere sight of a disagreeable object may produce vomiting.

This community of relationship between somatic sensory, somatic motor, visceral sensory, and visceral motor systems is an important one. Various kinds of afferent impulses may lead to the same general motor response. The various kinds of afferent impulses which may lead to vomiting illustrate this point. But no matter over what channels the different afferent impulses which lead to a particular motor reaction may pass, they eventually come to a definite group of cells somewhere in the central system, in which the motor or efferent impulses arise. From this point on, the path is the same, no matter what the nature of the afferent impulse may be. We have, therefore, the principle of the final common path (Sherrington) founded on facts of the general character which are here briefly indicated.

In addition to the reflex elements involved in the maintenance of blood pressure, there is evidence of the existence of an automatic elements, i.e., an element dependent upon the changes of blood pressure or blood constituents within the vasomotor center in the medulla oblongata.¹³ This is analogous, though not so preponderant in its action, to

the well-known automatic element in the respiratory mechanism, dependent upon the "blood-stimulus" for its normal operation. Evidence of such a sensitiveness to the "blood-stimulus" in the motor cells of the spinal cord has been recently adduced by Graham Brown.¹⁴

Such, then, is the nervous, muscular, and glandular mechanism involved in the circulation, and such are its strong and its weak points. Which of these are affected in shock, and how?

It requires but little reflection to see that the sympathetic—the viscerosensory and visceromotor system is the one primarily and most markedly affected in surgical shock. The patient may be fully conscious, and have voluntary control of the movements of the skeletal muscles. It is true that the movements may be sluggish, and that the skeletal muscles may be more flaccid than usual, but it is a question whether this may not be a secondary result of the low blood pressure and other disturbed metabolic conditions of the body as a whole, rather than a primary effect.

Nor would complete relaxation of the skeletal muscles, such as occurs after intravenous injection of curare, account for the great fall of blood pressure observed. The vascular system and the heart must then be responsible. It is commonly observed that the heart beats rapidly in such conditions. The heart itself, independently of its extrinsic nerves, never beats more rapidly when the blood pressure is low. The heart must, therefore, be receiving accelerator impulses over the sympathetic nerves during certain phases of shock.

The reflex mechanism for acceleration of the heart is not exhausted nor depressed, but, on the contrary, is more active than usual.

It has been shown also that many of the peripheral arteries are constricted and not dilated.¹⁵ So far as these vessels go, there is again no exhaustion of the reflex or other mechanism for vaso-constriction, but even an increased activity. The question arises whether all the arteries in the body are similarly constricted, or whether some of them may be widely dilated. It is known, for example, that the peripheral blood vessels, and particularly the arterioles, constrict when the surface of the body is cooled, but the systemic blood pressure does not necessarily arise. Nor does the increased flow of blood to the surface when the external temperature is high necessarily entail a fall of pressure. There is either a compensatory change in the caliber of the deep blood vessels or a change in the heart rate, or both, by which the blood pressure is maintained at a nearly constant level. Porter,¹⁶ how-

ever, has found that stimulation of an afferent nerve apparently affects all parts of the vasomotor system in much the same way, causing constriction of the splanchnic and peripheral vessels alike, or else dilation of all of them. Stewart's¹⁷ studies on blood flow in disease are leading to the acquisition of data on this point, and clearly indicate that in certain disease processes at least, there is a difference in the distribution of the blood to various parts of the body, probably due to local changes in the caliber of the arteries.

It is known also that active muscles and glands receive more blood than when in the resting state. For the present, I believe that the assumption that all the arteries are constricted in surgical shock must remain purely an assumption until further evidence is adduced, either for or against it.

But if all the arteries are constricted, and the heart is beating rapidly, indicating no exhaustion of these mechanisms, some other part of the vasomotor system must be at fault. It has been suggested that the veins, particularly in the splanchnic region, are dilated and that the blood is in them. One fact seems to militate against these two suppositions, i.e., that all the arteries are constricted, and that the blood is largely accumulated in the veins; it has been found that stimulation of an afferent nerve will produce a rise of blood pressure when the animal is in a condition of surgical shock.¹⁸ Such a rise of blood pressure usually comes about through constriction of the arteries rather than through constriction of the veins, and it would appear that there were some arteries at least which were still capable of further constriction. Such a view would necessitate the assumption that all parts of the arterial system were not affected alike by the vaso-constrictor nerves during shock, but that some arteries, possibly those in the splanchnic region, were somewhat dilated.

Whatever the immediate cause of the low blood pressures may be, it may well become part of a vicious circle. Even though the blood be well oxygenated by artificial respiration, and the heart be beating regularly, a previously damaged portion of the central nervous system, e.g., the brain, after subjection to anemia, does not recover as long as the systemic blood pressure remains low.¹⁹ Nor does this fact need surprise us when we remember that, among the other relatively constant conditions of the mammalian body, a blood pressure varying but a few millimeters under the various conditions of activity from day to day or year to year, is an important item. Any considerable variation from this usual level is strong presumptive evidence of

abnormal processes involving other mechanisms than that for circulation.

If the law of mass action holds for the reactions of the animal organism, we would expect any very wide departures of any one condition—blood pressure, oxygen or carbon dioxide content of the blood, or whatever it may be—from the usual standard to bring about associated changes in the other conditions coexisting with it.

With regard to the causes of shock, we may consider briefly a few of the current theories or hypotheses.

Crile has assumed as the basis for his explanations a stimulation of the nociceptors by the operative procedures. He emphasizes the fact that the afferent nerves are still excitable, and that they are transmitting impulses to the central mechanisms. It is true that in surgical anesthesia with ether, the efferent or motor nerves are still excitable, and probably the afferent nerves are also excitable. But it is a familiar fact, known to every one who has done even the routine mammalian experiments of the student laboratory, that no reflexes of the skeletal muscles can be obtained by stimulation of afferent nerves in an anesthetized dog. But the vasomotor responses, the effects on the heart beat and upon the respiratory movements can still be obtained in response to stimulation of afferent nerves when the animal is in surgical anesthesia. Furthermore, these effects on the smooth muscles and upon the respiratory movements may be obtained in a decerebrated dog or cat. In other words, the only reflexes which can be obtained in anesthetized animals may be obtained equally well in decerebrated animals, and there is no particular reason to suppose that the only reflex path open in anesthesia involves any part of the cerebrum. They are all visceral reflexes through the lower levels of the encephalon.

Dr. Crile, however, says that he finds chromatolysis in the nerve cells occurring in the cerebrum, and attributes this to the effect of afferent impulses. When we consider that, in addition to the fact that no reflexes involving the skeletal muscles, and hence none that could involve the cells of the cerebrum in any way, can be obtained in anesthetized animals, the cerebral cortex is itself inexcitable in such animals, one wonders just how much damage these afferent impulses are doing to the cells of the cerebrum. And, furthermore, while it would not perhaps be so surprising to see some chromatolysis in the general sensory area of the left cerebral cortex after amputation of the right leg, it is passing strange that we find chromatolysis in the visual and

auditory cortical areas of the other hemisphere of the cerebrum.

Similar considerations apply to the cerebellum. There is inexcitability of the cortex, and a complete absence of all the usual reflex phenomena attributable to the cerebellum, such as tone of the extensor muscles of the limbs. Yet, chromatolysis occurs here also.

It is one of the well-established facts of neurology that afferent fibers from a given region of the body terminate in definite regions of the cerebral cortex. And it is likewise a fact that the cells of origin of the motor fibers to the muscles of any given region of the body lie in a definite area of the cerebral cortex. There is a definite localization of the projection fibers, afferent and efferent, in the cerebrum. We should accordingly expect to find chromatolysis in those regions of the cerebral cortex in which the afferent fibers from the injured part end, or in which the cells of origin of the motor nerves to its muscles and glands lie. Such, however, is not the case. Dr. Crile admits that there is no specificity of chromatolysis, but that it occurs in all regions of the cerebrum and in the cerebellum.

It has been shown also that chromatolysis occurs in animals which are not in the condition of surgical shock, but which manifest rather different physical signs.²⁰

Finally, Nissl himself pointed out some years ago that chromatolysis was not a lesion indicative of any specific injurious influence, but that it might arise in response to many injurious agencies.

To sum up the situation with reference to, the rôle of the cerebrum in, and the relation of chromatolysis to, the cause of shock, the various lines of evidence adduced: (1) that, when an animal is in a condition of surgical anesthesia, no afferent impulses which are normally operative are producing any noticeable reflex response on the skeletal muscles; (2) that, in general the cortical cells are inexcitable to electrical stimuli at such a period; (3) the fact that chromatolysis occurs not only in the particular regions of the brain in which they might be expected to occur on any known basis of localization, but also in other widely removed and scattered regions of the brain; (4) that animals may show a considerable degree of chromatolysis and not manifest any of the physical signs of surgical shock as they are ordinarily understood; and (5) that chromatolysis may occur in response to the action of many diverse injurious agents, such as the anesthetic itself, force one to conclude that the relation of chromatolysis to the onset of shock cannot be very important or very definite.

An animal may be completely decerebrated without showing any particular signs of surgical shock, although the spinal shock may be profound. After a time the reflexes of the skeletal muscles return and the symptoms of spinal shock gradually abate in severity. But it is now possible so to treat the animal as to induce the condition known as surgical shock. The blood pressure falls, the pulse becomes rapid and feeble, and the respiration shallow, or even periodic. Surgical shock may be induced in the absence of the cerebrum, and whatever afferent impulses may be involved in its onset certainly do not pass through the cerebrum. Nor are efferent impulses from the cerebrum demonstrable here.

These facts acquire a peculiar significance with reference to the onset of shock when considered in the light of the relation of the medulla oblongata to the visceral system. It is in the medulla oblongata that we find the first extensive connection between the nerves bearing afferent impulses which are capable of affecting the viscera and the efferent visceromotor fibers.²¹

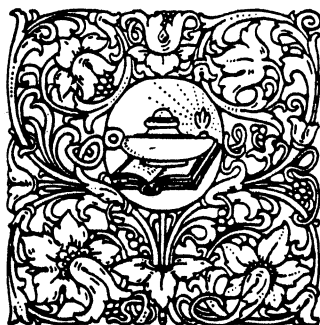
In addition to the rapid heart rate and the constriction of the arteries already mentioned, there are certain other effects due to the sympathetic or autonomic system that are worthy of some attention. Space does not permit their consideration at this time. Nor can the discussion of the sources of the afferent impulses be taken up in detail. Both of these questions must be left for future discussion.

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CURRENT RESEARCHES IN
ANESTHESIA
& *ANALGESIA*

F. H. MCMECHAN, A. M., M. D., EDITOR



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Contents of the August, 1922, Issue

EDITORIALS

Foreword	
Coming Meetings	1
Continuous Nitrous Oxid-Oxygen Analgesia and Anesthesia With Rebreathing in Obstetrics	3
<i>A. E. Rives, Ph. G., M. D., East St. Louis, Ill.</i>	
Nitrous Oxid-Oxygen Analgesia in Combination With Local Anesthesia in Cesarean Section	8
<i>Wm. F. Hewitt, M. D., F. A. C. S., Chicago, Ill.</i>	
Experimental and Clinical Observations on Cotton Process Ether	10
<i>Paul Cassidy, D. D. S., Cincinnati, O.</i>	
Anesthesia as a Phenomenon of Oxygen Want	13
<i>Charles W. Greene, Ph. D., Columbia, Mo.</i>	
Noted Anesthetist Dies a Martyr to His Skill	18
Methods of Etherization	19
<i>Albert H. Miller, M. D., Providence, R. I.</i>	
Quantitative Urinary Determinations With Reference to Toxemias and Anesthetic Risk	23
<i>Clifford Mitchell, M. D., Chicago, Ill.</i>	
Morbidity and Mortality in Obstetrics as Influenced by Anesthesia	27
<i>Wesley Bourne, M. D., and James W. Duncan, M. D., Montreal, Canada</i>	
Butyn, A New Synthetic Local Anesthetic	34
<i>Albert E. Bulson, Jr., M. D., Fort Wayne, Ind.</i>	

ABSTRACTS

Ether. Experiences With a New	39
Spinal Analgesia, How Apothecin Compares With Others Agents Used in	41
Blood, the Buffering of the	43
Feeding Before Anesthesia, Importance of	43

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Editorial Foreword



WITH this issue the former Bulletin becomes a regular Journal to carry on the organization and educational campaign of the National Anesthesia Research Society. As in the past, the Journal will print papers from the Transactions of the several Associations of Anesthetists, special and selected articles of pertinent interest, as well as abstracts from the current literature of anesthesia and analgesia. It is hoped that the innovation of the advertising sections will be favorably received as this additional support will enable the officers and editor of the Society all the more rapidly to achieve its purposes. The make-up of the Journal is such that for permanent binding the advertising pages may be withdrawn without discarding any of the context. Your continued cooperation and the securing of as many new members as possible, will enable the Journal to rapidly enlarge its scope and utility and eventually to cover and exhaust the international field in Current Researches in Anesthesia and Analgesia.

Coming Meetings



PLANS for the Congress of Anesthetists, at the Hotel Deshler, Columbus, Ohio, October 30 to November 1, are rapidly maturing and a very interesting meeting and program are assured. Aside from the two leading conferences on the Teaching of Anesthesia and the Development of Hospital Anesthetic Service, there will be Special Sessions devoted to Anesthesia in Oral Surgery and Dentistry, as well as in Relation to Cardiology, Pressure and Re-breathing. It is also expected that some noted foreign anesthetists will be in attendance. The Interstate and N. A. R. S. join in urging your attendance at this Congress so that its influence may materially aid international progress in the specialty. Those wishing to present papers are requested to get in touch with the Editor at once; and those desiring space for commercial exhibits

Morbidity and Mortality in Obstetrics as Influenced by Anesthesia*

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THIS IS NOT our intention, to dwell on the history of anesthesia. Only, it is worthy of mention that not long after the discovery of nitrous oxid, Humphrey Davy, the first superintendent of the Pneumatic Institute of Clifton, England, while working on the then known gases, noticed that the inhalation of nitrous oxid relieved pain. His friend Southey, the poet, used to visit the laboratory frequently and inhaled some. So impressed was he that the following eulogy came from his pen to his brother, "*Oh Tom! Such a gas has Davy discovered! Oh Tom! I have had some, it makes me strong, and so happy! So gloriously happy! Oh excellent gas bag! Tom, I am sure the air of heaven must be this wonder working gas of delight.*" The anesthetist of today often hears this paraphrased, especially by the parturient women. This was written in 1798 and yet it was not until forty-six years later that this elastic fluid was used as an anesthetic, and then chiefly in dentistry for another twenty-four years, until Joseph Clover, surgeon and anesthetist, adopted nitrous oxid for general use in 1868.

Following the suggestion of Paul Bert, Klikowitsch of Petrograd, in 1880, used laughing gas in twenty-five cases of labor and reported satisfactory results. Then, rapidly followed the reports of a host of others with results not nearly so satisfactory, so that its use was entirely dropped, not to be revived until 1910 by Guedel, Webster and Davis. This revival was made possible by three factors. *First*, the purification of nitrous oxid by what is known as the acid wash system.

Secondly, by its admixture with oxygen (Marshall), and *thirdly*, by the perfection of apparatus for the control of pressure. So perfected are these factors now that there has been an unfortunate tendency for many to think that an automatic apparatus solves all problems of technic. This, of course, is a mistake and recently there has been begun a movement to create scientific interest in anesthesia by improving the courses and clinical instruction, not only for the undergraduate medical student but also for the post-graduate, to enable those who so desire to fit themselves as competent anesthetists, (McMechan).

*Read during the Joint Meeting of the Canadian, New York and Interstate Anesthetists with the Ontario Medical Association, Hotel Clifton, Niagara Falls, Canada, June 1-3, 1921.

Anesthesia and Analgesia — August, 1922

Physiological Considerations

OF THE known anesthetics, nitrous oxid is the least harmful in its immediate and remote effects on metabolism, recovery after its use being almost instantaneous and usually without post-operative complications. The research work of Buxton, Chadbourne, Casto and others on human and animal subjects, tends to prove that the percentage of hemoglobin is always lowered in the anesthetics of chloroform, ether and nitrous oxid oxygen. This reduction of hemoglobin is *most marked* with chloroform and the anemia is severe, recovery not taking place until after the seventh day; with ether, the maximum reduction is at the end of twenty-four hours, after which there is a gradual return to normal in about one hundred hours. With nitrous oxid-oxygen, the reduction is not *only slight* but in about two hours there is a return to normal. The practical application of this is that any case with a color index of hemoglobin below sixty percent is a hazardous risk with ether or chloroform. Nitrous oxid-oxygen anesthesia is strongly indicated in anemias when operations must be done.

With all anesthetic agents there is a reduction in the number of red blood cells. This reduction is slightly less with nitrous oxid, averaging about sixteen per cent. However, with nitrous oxid, *the coagulation time is considerably shortened*, which is never the case with chloroform, and it is only slightly shortened with ether. Again, there is a leukocytosis in anesthetics of all agents, more marked in nitrous oxid-oxygen, under which the average gain is

sixteen per cent, affecting chiefly the small lymphocytes and the transitional forms. All these blood changes seem to be due to an increase in the H-ion and we must remember that this is directly influenced by oxygenation, as increased carbon dioxid tension means increased H-ion content, and this is the real constant in interpreting acidosis. The watchword, therefore, is to avoid cyanosis and practically, this means the maintaining of a good color in the capillary blood, so easily seen at the patient's ears. In shock or acidosis, whether obstetrical or otherwise, where analgesia or anesthesia is advisable, nitrous oxid-oxygen is the agent of choice by common consent. This was particularly emphasized by Geoffrey Marshall, of London, England and by W. B. Cannon, of Boston, in the surgery of the late war.

A few words about circulatory disturbances would seem apropos and these are somewhat intimately connected with blood pressures. We find that none of the agents used for anesthesia are accompanied by normal readings. Chloroform always causing a very early and very abrupt fall in all readings with a marked reduction in pulse pressure. There may be a fall of blood pressure with ether, but it is not nearly so marked if the administration is properly carried out, whereas with nitrous oxid-oxygen, the change is an initial slight rise in blood pressure readings, returning to almost normal as soon as the first stratum of third stage anesthesia (Guedel) is established, and so remaining for several hours, unless some extraneous factor intervenes. On some occasions, when

Anesthesia and Analgesia — August, 1922

too large percentages of nitrous oxid are given at the expense of good oxygenation, there will be a fall in blood pressures but they return to normal as soon as the anesthetic is discontinued or oxygen is added in proper percentage.

In interpreting our blood pressure findings in terms of circulatory depression, we classify, with Moots and McKesson, as follows:—

First degree circulatory depression is that in which there is a fifteen per cent increase in pulse rate without change in blood pressure or a ten per cent decrease in blood pressure without a decrease in pulse rate.

Second degree is that in which there is an increase of twenty-five per cent in the pulse rate, along with a ten to twenty-five per cent decrease in blood pressure.

Third degree, which may be known as *shock*, definitely is that in which the pulse rate is a hundred or more and ascending, accompanied by progressively falling blood pressures reaching eighty millimetres of mercury systolic and twenty pulse pressure or less.

Dangers of Chloroform

A GAIN, Goodman Levy, of London, England, has clearly shown that death from ventricular fibrillations in chloroform anesthesia may occur during induction, during operation and after operation: (1) During induction in the struggling and excitement stage, in the removal of the chloroform and in the abrupt administration after removal or a sudden increase during a period of light anesthesia. Also, by a combination of these three. (2) Dur-

ing operation by a strong sensory stimuli under light anesthesia and (3) after operation on removal of this drug, especially in short cases. I may here mention that Fleming's Coroners' statistics, presented to the Anesthetic Section of the Royal Society of Medicine, show rather conclusively that in the hands of the general practitioner, chloroform involves prohibitive immediate and remote mortality, more particularly in the presence of co-existing pathological states, systemic diseases and obstetrical complications. In the year 1911, in England and Wales alone from the Registrar General's reports, we find that there were two deaths from chloroform every three days. There is a growing tendency to believe, and we think correctly so, that injurious effects of any anesthetic persists for longer periods than was thought possible in the past, so that the anesthetic may be an important factor in some of the more remote post-operative complications. This makes one realize that the vulgar term *anesthetic death* means nothing.

Effects of Anesthetics on the Liver

I T HAS been found, experimentally and clinically, that there never have been any changes in the mother or child in this organ with nitrous oxid-oxygen anesthesia. Nitrous oxid *alone*, when pushed too far, produces a liver picture exactly like that of ordinary asphyxiation; whereas chloroform is found, both experimentally and clinically, to cause swelling of the liver cells with fatty infiltration and necrosis in both mother and child, but more particularly in the child. Ether

Anesthesia and Analgesia — August, 1922

does not cause necrosis but there is a mild form of parenchymatous degeneration and tissue swelling. The work of C. H. Davis, of Milwaukee, in this connection is very recent and thorough and his experiments disprove Gwathmey's belief that chloroform vapor is made entirely safe when administered with pure oxygen.

Mortality and Hospital Days

AT THIS point a few remarks on mortality seem advisable. That of nitrous oxid-oxygen anesthesia is about one in one million for short operations, and one in five hundred thousand for long ones (Zemp). Whereas for ether it is one in sixteen thousand; for ethyl chlorid one in six thousand and for chloroform one in four thousand. Even this is not a fair comparison because nitrous oxid-oxygen is now selected as the anesthetic of choice when all others are contra-indicated, often when the patient is practically moribund. If this anaesthetic is selected in the very worst cases it is surprising that more deaths have not been reported recently through its use.

Meeting Certain Objections

YOU WILL hear some outstanding objections to the use of nitrous oxid-oxygen made by many, even of our best men. One is that it requires an *experienced anesthetist*—but should this not be the case for any anesthetic. That anyone can give an anesthetic is an idea evidently unscientific. Undoubtedly, many deaths have been caused by ignorance. Another is that its cost is considerable. This is true at first sight but on second thought

you will realize that the after-effects being so much less than with any other agent, your patient is saved so many days in the hospital at so much per day. Personally, we prefer to take one chance in one million as against one in four thousand in spite of increased cost.

To combat the so frequently expressed argument that the gases are inimical to the mother and foetus in the toxemic type, in which cyanosis is always present, let us emphasize the fact that cyanosis produced in a normal case is a sign of poor administration. If it is possible to maintain good color in normal cases it is equally possible to maintain the same relative balance of color in the toxic cases. Indeed our experience has been that, at least during the period of administration, we have been able, temporarily, to dispel the toxic cyanosis. This argument against the gases in this class of cases is pernicious and unjustifiable.

Persistent headache has often been quoted as a sequel to a prolonged nitrous oxid-oxygen administration. In our relatively large number of cases this complaint has not been brought to our attention and to those who have had to combat it, we would strongly advise the avoidance of cyanosis.

Technique of Administration

IN OUR application of this method, the technic for normal labor has been as follows:—

The work is divided between the obstetrician and the anesthetist. When the former believes labor to be definitely established he administers $\frac{1}{12}$ to $\frac{1}{8}$ grain of heroin,

Anesthesia and Analgesia — August, 1922

or $\frac{1}{2}$ grain of morphin. This allays the pain of the first stage to the point of almost complete dilatation of the cervix, the nitrous oxid-oxygen being more particularly reserved for the second or expulsive stage. It has been our experience that the too early use of nitrous oxid-oxygen is an unnecessarily prolonged and expensive procedure.

At or near the completion of the first stage, the anesthetist is called, who, after becoming acquainted with the outstanding details of the case, applies a blood pressure apparatus to the left arm along with a bracelet stethoscope, all tube connections being of sufficient length to avoid any inconvenience in the taking of readings. These are recorded on a chart for this purpose, together with pulse and respiratory rate. The recording of these is repeated at least every five minutes, in order that curves may be made. As explained before, from these curves, the slightest depression of circulation can be detected. Recently, we have decided to chart the foetal heart rate. The charts used are those of the National-Anesthesia Research Society.

The uterine contraction is allowed to start before the gases are administered because no pain is ordinarily felt until contraction is fully established, and because of the rapidity of the action of nitrous oxid, *analgesia*, in our experience, is quickly produced, particularly in the presence of heroin or morphin. The patient is conscious of her environment so that she may carry out our instructions and yet be free from pain. She is, therefore, told to take four or five deep inhalations. It is impossible to de-

termine the percentage of gases for every case. It is safe to begin with nitrous oxid 80 per cent and oxygen 20 per cent, the main object being to produce analgesia and obtain good oxygenation. The varying of these figures must suit each individual case, always however, maintaining a good color. Let us remember that cyanosis is never a sign of analgesia or anesthesia. It is rather a sign of poorly conducted narcosis. This analgesic condition is produced intermittently for each pain until the head is about to be delivered over the perineum, at which point, *anesthesia* takes the place of analgesia. Here again, we deliberately avoid cyanosis. With the delivery of the child, 100 per cent pure oxygen is given under slight pressure until infantile respiration is fully established. We find it unnecessary to slap our babies and it is interesting to see the lobster pink color without effort. Throughout the entire procedure, constant maternal and foetal supervision is rigidly carried out in the observing of maternal blood pressures, pulse, respiratory rate, and color; and also of the foetal heart.

Operative Obstetrics

IN THE toxemias of pregnancy, the modern obstetrician and anesthetist are, we believe, fully convinced that continuous and even moderately prolonged anesthesia under either chloroform or ether, is detrimental to the safety of both mother and child. In many of the toxemias, in which the minor operations of obstetrics, such as bougie and bag induction, are indicated, the technic may be carried out with little or no anesthetic.

Anesthesia and Analgesia — August, 1922

Nitrous oxid-oxygen, in this particular group of cases, is the recognized method. On the other hand, when major surgery is to be performed, the choice must be between the three agents, *namely* chloroform, ether and nitrous oxid-oxygen or a combination. There is no question, from the experiments so far made, but that chloroform has a detrimental effect on both liver and kidneys in anesthetics of over five minutes duration. For the sake of its ready application and powers of relaxation, the usefulness of chloroform anesthesia must be admitted. To the case or surgeon demanding its administration for five, but certainly not ten minutes, chloroform must be considered permissible but not without, in our opinion, jeopardizing the patient and her child unnecessarily, simply for the sake of convenience and personal equation.

Locally, nitrous oxid-oxygen has not, as yet, made a way for itself deeply enough into the confidence of all obstetricians, so that we have not a very large number of cases of this class to detail. From the results which we have obtained, we feel that nitrous oxid-oxygen is worthy of sincere consideration, not only as a primary and complete anesthetic but as one secondary to the delivery of the child following primary chloroform administration in operative work.

Results and Statistical Data

IN OUR series of cases, the majority at the Montreal Maternity Hospital, we have had to deal with vaginal hysterotomy, abdominal hysterotomy, bougie and bag insertion with forceps or version as secondary to ether. The

conditions being those of toxic vomiting, eclampsia, chronic nephritis and diabetes. In our series of cases, we have consistently relied upon nitrous oxid-oxygen as the anesthetic of choice. In a few isolated cases of abdominal Césarean section, we have had to add a small amount of ether to obtain the necessary relaxation.

Our total number of cases has been 202; 132 were primiparas, the remaining 70 multiparas.

Of the total, 145 were spontaneous and 57 artificial labors. Of the artificial labors the conditions were:

Pyelitis	4
Renal Stone	3
Hypo-thyroidism	1
Concealed Hemorrhage	1
Cardiac Disease	4
Breech Presentations	10
Premature	5
Toxemias of Pregnancy....	26
Placenta Previa	1
Multiple Pregnancy	3
Post-operative (App.)	1
	<hr/>
	57

The interferences for these were as follows:

Low Forceps	29
Mid-Forceps	9
Version	2
Accouchment Force	2
Vaginal Hysterotomy	2
Bougie Induction	4
Bag Induction	4
Abdominal Cesarean Section	14

The average length of time for nitrous oxid-oxygen analgesia and anesthesia was two and one-half hours per case in normal labor or in labor leading to forceps; the longest being ten hours and the shortest, one-half hour. There were no maternal deaths. In the

Anesthesia and Analgesia — August, 1922

uncomplicated cases, there were no deaths of children of the period of viability. In the lot, there were two foetal deaths, one in a case of threatened eclampsia, ten hours after birth, seven months terms, and one from hemophilia, twelve hours after birth with a direct history of this condition on the father's side. Here gas was given for two and one-half hours. The mother was an albuminuric.

In contra-distinction we may offer Hospital Statistics for the years 1908 to 1912 inclusive, in which chloroform was practically the sole anesthetic, and it should be stated that these supply a far greater number than we have at our disposal, and in which, no doubt, many of the patients were in a very bad condition. However, a comparison is interesting.

1. Albuminuria of Pregnancy

Maternal Mortality	Nil
Foetal Mortality	13 per cent
2. Nephritis

Maternal Mortality	8 per cent
Foetal Mortality	38 per cent
3. Eclampsia

Maternal Mortality	11 per cent
Foetal Mortality	29 per cent
4. Vomiting of Pregnancy Post-Operative

Maternal Mortality	5 per cent
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Summary

We make the following conclusions:

1. That nitrous oxid-oxygen is

the most acceptable anesthetic to the patient.

2. That it is the most difficult to administer properly.

3. That it is the least harmful of all known anesthetic agents to the blood, liver and kidneys of the mother and child.

4. That with it the uterine contractions are decidedly stimulated.

5. That under it the freedom from pain permits the parturient woman to use her contractions to best advantage.

6. Instrumentation is reduced from high-, and mid-, to more frequent low-forceps.

7. We have in this method at our disposal, a remarkable means of indirectly oxygenating the child after birth.

8. As against the frequent distressing collapse of the mother in post-partum administration of chloroform, we fail to find such experiences under nitrous oxid-oxygen.

9. It has been established that the uterus in its systole absolutely blanches itself, and as these gases are administered only during the rise and acme of the systole, no nitrous oxid can be conveyed to the foetus during that period. In a well conducted case, the oxygen is commenced at the moment of declination of the systole and continued throughout the diastole until the uterus is at rest. We would emphasize particularly the necessity of this type of administration because by it alone the foetus escapes all possibility of nitrous oxid influence or asphyxia.

10. In no case in our series of normal labors or labors leading up

(Continued on page 39)

Anesthesia and Analgesia — August, 1922

4. According to our experience to date, butyn in the quantity required is less toxic than cocaine.

5. It produces no drying effect on tissues.

6. It produces no change in the size of the pupil.

7. It has no ischemic effect and, therefore, causes no shrinking of tissues.

8. It can be boiled without impairing its anesthetic efficiency.

Respectfully submitted,

ALBERT E. BULSON, JR.,
Fort Wayne, Chairman.

WILLIAM ZENTMAYER,
Philadelphia.

EDGAR S. THOMSON,
New York City.

H. MAXWELL LANGDON,
Philadelphia.

HARRY S. GRADLE,
Chicago.

Quantitative Urinary Determinations

(Continued from page 26)

5. In normal urines there should be no yellow or red color to the lead acetate precipitate (uroerythrin).

6. In normal urines there should be no green fluorescence noticed, when zinc acetate solution is added to the urine with filtering (urobilin).

7. Individuals in whose urine there are found to be disturbances of these balances should be investigated as to diet and mode of life. If the regulation of both fails to restore the balances, these individuals should hardly be regarded as good anesthetic risks until proved in all other respects to be fit.

Morbidity and Mortality in Obstetrics

(Continued from page 33)

to forceps or breech extraction, have we been able to find any condition detrimental of mother or child attributable to the use of nitrous oxid-oxygen.

11. In all the toxemias, the local statistics of the cases in which chloroform and ether have been used, show results to mother and child that have not been as favorable as those which we have been able to record.

12. Above all, constant and momentary supervision is particularly imperative, in nitrous oxid-oxygen analgesia and anesthesia for this is not the work of the lazy obstetrician or anesthetist. This is also in keeping with the motto of the Canadian Society of Anesthetists, "*We watch closely those who sleep.*"

Finally, we may say, in concluding, that every obstetrician should have a working knowledge of anesthetics, and that every anesthetist should know a good deal about obstetrics. Were this the case, there would not only be less blaming of one another but better co-operation and more efficiency.

34 ST. MARK ST.

Ether, Experiences with a New.

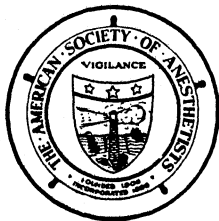
WM. WEBSTER, M. D., C. M.,
Winnipeg, Canada. *American Journal of Surgery (Anesthesia Supplement)*, July, 1922.

ETHANESAL is the name given to the new ether recently produced by Wallis and Hewer. Wallis had unusual opportunity, some years ago when

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Volume 1

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Number 1

THE PLACE OF THE ANESTHETIST IN AMERICAN MEDICINE *

HOWARD W. HAGGARD, M.D.

Director, Laboratory of Applied Physiology, Yale University

WHAT I have to say here regarding the place of the anesthetist in American medicine is not an encomium either of the men in this field of medicine or of their contributions. I offer no praise of the anesthetist as a scientist or as an humanitarian, nor do I glorify the relief from suffering afforded by his skill and knowledge. If then, I depart, as my negations must signify, from the easy, ingratiating words customarily spoken on occasions of this kind and under a title such as I have chosen, it is with a purpose.

That purpose is not to define the calling of the anesthetist in terms of what has been done and what can be done in the laboratory or at the operating table or at the bedside. It is not the contributions of the anesthetist with which I deal, but instead, the public regard in which these contributions are held. And I shall emphasize the fact that it is this public regard which determines the place of the anesthetist in American medicine.

There are some here among you, perhaps, who feel that your duties as anesthetists are complete when to the individual patient you have given the best anesthesia that modern knowledge affords. Admitting the primary importance of good anesthesia, this view is, nevertheless, to my mind, a limited and a narrow one.

It contributes little to the real advancement of anesthesia. It is not enough that good anesthesia can be given and that it is given to a fortunate few. What is of real importance is that all anesthesia shall be the best that modern knowledge affords. And this desirable end can be reached only when the public recognizes the need and the importance of good anesthesia; and recognizes most of all that the administration of an anesthetic is a major therapeutic operation. It is only with such

* Read at a meeting of the American Society of Anesthetists, Inc., New York World's Fair, New York City, October 12, 1939.

recognition and with such understanding that the anesthetist will receive that public regard and public support which are essential to the fullest development of his calling. And to attain this necessary public recognition, the anesthetist must not only give good anesthesia; he must also shape public opinion.

It is thus a social rather than a medical matter with which I deal. And if, in dealing with it, I skirmish rather than strike to the center, if my views seem philosophical rather than practical, and if I speak in analogy, it is because no one can give direct and simple solutions to social problems. I do not need to call your attention to conditions here and elsewhere to emphasize the fact that our knowledge toward the solution of social problems in any walk of life is meager. You and I cannot solve the social problems of today. But we can recognize some of them and we can try to analyze some of them. And from this analysis there are certain inferences which we may draw at least regarding the social forces which operate in medical progress. Some of these inferences will form the theme of my discussion.

It is obvious beyond question that true progress is achieved in medicine only when two conditions have been fulfilled. The first of these is medical discovery; the medical research which establishes the means by which disease and suffering can be prevented or alleviated. But discovery alone prevents no disease and it alleviates no suffering. Medical discovery without the fulfillment of the second condition is of academic interest only. And this second condition is more of a social than a medical matter. It is application. Application, utilization, in turn, are determined by public regard, public opinion. Such application comes only when public opinion is shaped. The shaping of public opinion is a social matter.

It further has been an obvious fact in medical history, but never more obvious than at the present, that the benefit that could be conferred by any measure of medicine and the extent of the need for it are no direct indications of the regard in which it is held by the public or the extent to which it is accepted and applied. Regard and application come when public view is shaped to an appreciation. No beneficial measure of medicine ever reaches public acceptance and support on the basis alone of laboratory experimentation or clinical investigation. It receives the recognition and support only when the public view is shaped to an appreciation.

And finally, medical regard and public regard go hand in hand. Public opinion is the doctor's opinion. He is a member of the public. Public demand and regard shape the education in our medical schools.

The only common denominator in all these inferences is the shaping of public opinion.

The three inferences are my propositions. Let me now expand and illustrate them. I have in view particularly the situation of anesthesia but my illustrations may take me far afield.

My first proposition is, I repeat: The benefits that could be conferred by any measure of medicine and the extent of the needs for it are no direct indications of the regard in which it is held by the public or the extent to which it is accepted and supported. Regard and application come only when public view is shaped to an appreciation.

More than 300 years ago Paracelsus laid the foundations of chemotherapy. The new branch of therapy obtained public regard through the unfortunate method so characteristic of the efforts of Paracelsus—that of contention. The members of the medical profession and the public as well took sides in violent controversy as the herbalist and mineralist; the followers of Galen and the followers of Paracelsus. The public attention was there but the difficulty lay in the fact that the scientific basis was inadequate. The basis of controversy is too much public opinion and too little fact. Such a situation in time arouses ridicule which forms its own public opinion—such ridicule as that which was once directed at a controversy of this sort with the statement that the patients of the herbalists died of the disease and those of the mineralists of the remedy. As a matter of fact, except for steel in anemia and sulphur for scabies, little benefit to the patient was obtained from the minerals. True, mercury given just short of therapeutic mayhem hastened the disappearance of the secondary manifestations of syphilis, but it had little effect on the tertiary.

And then, three centuries after the time of Paracelsus, Ehrlich introduced salvarsan. This time it was the laboratory and clinical side which was fully developed. This time it was the social side which lagged. Syphilis continued to exist and to exist plentifully in the presence of what was offered against few diseases—a positive method of diagnosis and a specific remedy.

This anomalous situation, in which there was a prevalence of a disease on the one hand, and, on the other, certain means of control, might have continued indefinitely had not, within the last few years, a deliberate drive, with which you are all familiar, been made to break down the barriers. The breakdown was not accomplished by developing better therapeutic methods, or by clinical demonstrations. It was broken down by radio, newspapers, magazines and books and by word of mouth which led to open discussion. It was an effort made in what should be the most cherished privilege of any profession, that of shaping the folkways of our people. We once called it education; we now call it propaganda.

The doctor often looks down upon this shaping of public opinion; he treats it with indifference, with aloofness, and that in spite of the fact that for him, and for the public, it is equally as important as medical discovery. The doctor, I fear, forgets that his calling is a social calling. At times, although he may complain of fees, he seems to disregard the fact that his calling involves not only personal but also broad and fundamental problems of economics—the direct or indirect pur-

chase of his skill at a level comparable with the service he renders. The important feature is the value placed on this service by the public. The value placed by the public is not based on definite and tangible value received; it is determined by the public's opinion of value received. The patient of today too often takes anesthesia for granted as an accepted accessory to surgical operations for which he must pay as he does for the rent of the operating room. In contrast I ask you what would a patient, of say a hundred years ago, faced with an inevitable surgical operation, have paid for the certainty of painlessness? Is it actually any less important to the patient of today who accepts his anesthesia as a commonplace? The often repeated statement that it isn't lack of appreciation but of economic necessity that leaves the doctor's bill unpaid is a sophistry. No one yet has talked of subsidizing the automobile manufacturers because the public appreciates but cannot pay for automobiles. The fact of the matter is that the quite opposite attitude toward the medical fee is a cultivated one—cultivated now to a point when our citizens are beginning to think that medical service for everyone poor and rich alike is to be regarded as a civic contribution like the paved roads. The city pays for the roads but the citizen still pays for his own automobile. It is all, gentlemen, a matter of cultivated public opinion; the establishment of value by the public. And in these matters the modern physician has not influenced public opinion but instead has been influenced by public opinion. He has assumed something of the self-protective attitude of the cloistered research worker, of the austere institutional clinician. He has deliberately assisted the public in cultivating this regard of him. And it is to his detriment and I think to the detriment of public interest.

Public opinion, for good or for bad, is shaped by propaganda. People use one toothpaste, or another, not wholly because of the proven merits of the product, but because of propaganda. Have you ever stopped to think why the public, on the one hand, learns so quickly of any discovery in dietetics, and, on the other hand, so slowly of some medical measure, say the striking benefits of modern scientific anesthesia? The food discoveries are popularized by food manufacturers for commercial interests. Commercial interests know the value of good propaganda. Among them it does not have this euphonism; it is known as advertising. Discover a new vitamin today and tomorrow it will be in beer and bread and the day after the technical terminology of the nutritionist will roll glibly off the tongues of 100,000,000 people. The same 100,000,000 people still regard chloroform as the major anesthetic agent in use and look upon the administration of an anesthetic as something requiring only slightly more skill and professional knowledge than the giving of a dose of castor oil. The members of the medical profession are inclined to believe—at least so their actions would indicate—that the world will pause and eagerly, seriously and intelligently weigh and ponder the best in therapy and will then in sol-

emphatic decision accept and use it. There is no greater fallacy. The public does not ponder and weigh; in medical matters it has not the knowledge to do so—only the emotions. It takes its opinions fully formed and accepts those which are forced upon its attention. This direction of the public in the shaping of folkways is education, propaganda, advertising—take whichever term suits your taste. Each can have dignity; and each can be a public service.

Before my digression on publicity I had in a few sentences recapitulated the progress made in one branch of chemotherapy to essential completion—from discovery to application. Let me outline now some steps in the progress of anesthesia.

First there was the great discovery of principles. Here are the stories that are familiar to you and even becoming familiar to the public. They are those of nitrous oxide and ether and chloroform—Davy, Long, Morton, Wells and Simpson. The sum total of the propaganda value of these stories is that anesthesia is available; that there are drugs which give a blessed relief from pain. The sum total of public opinion formed is that it was unpleasant to have an operation before the days of anesthesia. This was very useful propaganda 90 years ago when the principle of anesthesia was under criticism. Today it is about as useful as is propaganda in favor of the principle of utilizing medications or surgery or having hospitals or trained nurses. Propaganda which deals only with principles now accepted, obscures the one feature of anesthesia which is of importance to the public today—that is the administration of anesthesia.

The second era in the progress of anesthesia was the search for new anesthetics. This was led, as you know, by Simpson with the discovery of chloroform. It is true that Simpson did a yeoman's service in shaping public opinion in his forceful pamphlets justifying the use of the principle of anesthesia. But chloroform was a long time ago. Since then we have seen the new anesthetics multiply in number and in professional usefulness. But these things are wholly the technical equipment of the anesthetist. They permit him to give better anesthesia but they mean little in creating public opinion. It is not the anesthetic agent which should be held up for public interest, but the administration of the anesthetic.

We have lived through the period of apparatus. We have seen the towel on a wire mask give way to a complicated respiratory apparatus with delicate controls. But again this means little to the public except that in the increasing number of moving pictures showing surgical operations, it is evident that the patient is still alive when the rubber bag fills and empties rhythmically and the valves jingle. In such pictures which express public views and public regard the anesthetist is a handsome nurse, or, lacking this appeal, he is obscured beyond the frame of the picture so that the surgeon may have the center of the stage.

And gentlemen, it was not the invention of a new antiseptic, of a new scalpel, or a new operating table that gave the surgeon—I speak collectively—the prestige and pre-eminence which he enjoys in public regard. This prestige gives surgery a prominent position in the curriculum of the medical school. It draws many of the better students into this field.

Today—to my mind at least—where the anesthetist, and again I speak collectively, is weakest, is in this very social aspect; this matter of prestige. The analysis of prestige and its importance brings me to the last of the propositions which I postulated: Medical regard and public regard go hand in hand. Public opinion is the doctor's opinion. He is a member of the public. Public demand and regard shape the education in our medical schools. And I may add that the public regard in which a branch of medicine is held is no direct measure of the benefit derived from that branch of medicine. It is a measure of the shaping of public opinion.

In broadest terms what I am saying is that throughout the ages the respect in which the doctor was held, the veneration bestowed upon his calling, and the support given to him have had absolutely nothing to do with the benefits the doctor and his calling have conferred upon the public. The regard given him is a cultivated regard; it is a fortunate coincidence when he deserves this regard.

Let me support my point with generalities. Look back, if you will, at the medicine man of uncivilized people; from our point of view all that he possessed was a bag of tricks in psychotherapy, and a few empirical methods of drug therapy, and yet he was held by his people in a veneration that amounted to actual awe. He was the great leader of his people.

Look next at the physician of the late Middle Ages and the Renaissance. He had far more to offer than the savage, but nevertheless he was regarded with no veneration. He was a menial. The surgeon was a barber.

And then look at the physician of the late 18th century, particularly in this country. That was a period in which men's minds were turned to serious matters. Devotion to principle was characteristic of the day. Public-minded, socially-minded men devoted their services to the needs of their fellow men with an almost religious enthusiasm; they were in medicine and they were in public affairs. They followed medicine as a duty of service to their fellow men; they signed the Declaration of Independence. They may—and often did—commit therapeutic outrages on their patients. You will recall Rush's famous 10 and 10; 10 grains of calomel and 10 grains of jalap at a single dose, often with copious bleeding. But he and his brethren were regarded with the highest respect. Their position in the public mind was far higher than that of the physician of today, in spite of the fact that what they had to offer as science was negligible.

You remember that famous remark of Dr. Benjamin Rush when he expressed his regard of science. He said "Medicine is my wife and science my mistress." You may remember, too, the waspish comment a half century later of Oliver Wendell Holmes when he said: "Medicine may have been his wife and science his mistress, but this breach of the seventh commandment cannot be shown to have been of any advantage to the legitimate recipient of his affections."

I bring in this anecdote because it shows more clearly than any words of mine the tendencies of the times. Holmes, present on that day when anesthesia was first publicly demonstrated, came in the era when science was beginning to dominate medicine—when the whole problem of the ills of mankind was to be solved by science and science alone. The laboratory and the clinic rather than the public place were to become—and then did become—the retreats of medicine. The door closed on the doctor. He was engaged, it is true, in a fundamental feature of his profession—the accumulation of knowledge. But it was to the exclusion of an equally fundamental feature—the shaping of public opinion to the full application of that knowledge.

In consequence of his sequestration a mode of thought was created. It was one which put the premium on medical discovery and not on medical application. In the last hundred years, with the introduction of the exact sciences into medicine, medical research has yielded some of the most beneficial knowledge that the human race has ever acquired. Enthusiasm has grown high and the mode of thought crystallized. The beginning and the end of medicine seemed to be research; the finding of new and better ways. And that, unfortunately, in the disregard of the fact that discovery without application is only of academic interest. It was a miserly method; the accumulation of valuables without putting them into circulation. The social side, the equally dignified propaganda side of medicine, was largely ignored, or, if not ignored, handled so badly or with such indifference as to fail in its purpose. It was treated with contempt as below the dignity of the doctor.

Now I speak feelingly on this subject and for a personal reason. I am, by sheer chance, a research worker; a laboratory man. But ten years ago I held much the same idea that I hold today regarding the need of propaganda although my views have changed considerably on how it should be done. At that time I was offered radio facilities to talk on such subjects. To the possible disadvantage of my professional career, I rather unwisely accepted.

For a little over a year I continued to talk with considerable criticism from some of my scientific confreres. The criticism did not come because of the amateurishness of my talks—and they were amateurish—but because such work was not consistent with a scientific career. So pressing was the criticism that I felt it then advisable to stop. That was ten years ago. In the intervening years conditions have changed. Medical radio propaganda of a similar sort—mostly rather bad in exe-

cution and misdirected I fear—has now become reasonably respectable. Time on the radio is eagerly sought by many medical societies. This year, to my amusement, the talks I gave with only a meager knowledge of the principles of propaganda ten years ago are now being repeated nearly verbatim by six different medical groups which have been given radio time. The certain conclusion that one must draw is that, while the principle of propaganda has been given some respectability, the method of carrying it out has not correspondingly improved.

In these rather personal digressions I have wandered from the point I was trying to make that public opinion is shaped by social endeavor and not by laboratory and clinical discovery. I have spoken in generalities. Let me next trace out along a somewhat different line a specific example of the development of prestige and the importance of prestige to the advancement of any branch of medicine. I turn to the surgeon. Incidentally I shall speak only of the advantages of prestige and deal with none of the disadvantages, the most obvious of which is the scramble of the mediocre toward the specialty which at the moment enjoys prestige.

As you are all aware, the surgeon did not always have prestige with the public or even the reasonable respect of his medical associates. In this regard there is a most pertinent and illuminating line in that ancient ritual which we call the Oath of Hippocrates but which in reality was the more ancient oath of the medical priests in the Temples of Aesculapius. It says, in effect: We, as respectable physicians, swear not to cut for the stone but to leave this to men who do such things. This stricture was not directed at the urologist, but at the surgeon. The surgeons were the men beyond the pale of professional respectability who did such things. Such was the attitude toward the specialist in surgery in the classical period of medicine. It was not one of great prestige. And it sank distinctly lower during the subsequent Arabic period of medical supremacy. Surgery was menial work; the professional standing of the surgeon was something comparable to that of the hospital orderly of today.

This regard of the surgeon carried over into Europe. No clearer indication of the prestige, of the recognition, or the reverse, of the importance of any field of medicine by the public, is to be found than in the curriculum of the medical school. In the early great medical schools of Europe no surgery whatever was taught. You will remember that Ambroise Paré, the 16th century Father of French surgery, was a barber. Only royal insistence obtained for him a grudging recognition by the medical men of the period. You may have read his *Surgery* and you may have read one of the standard medical textbooks of that day. If you have, you will see that Paré with his surgery had far more to offer than the internist with his comparatively greater prestige. All the physician had, as contrasted with the surgeon, was a more scholarly social distinction in the eyes of the public. Yet this public opinion was

sufficient to literally abolish intelligent surgery. By the time of Louis XIV, Paré's surgical knowledge had actually been so far forgotten that when the king developed a fistula in ano none of his attending medical men knew how to perform the necessary operation. It required six months study and preparation in a veritable medico-surgical comedy before the operation was performed on the royal posterior. This incident brought royal favor to surgery. Royal favor brought public regard with a revival of surgery. Of such things, gentlemen, is prestige created.

And I may add, parenthetically, that this was not the only specialty of medicine which profited by royal example. Obstetrics made its first stride toward social acceptance when Louis XIV had a male midwife for the confinement of Lovalliere. You all recall the public opinion created in favor of anesthesia by Queen Victoria's acceptance of chloroform at the birth of Leopold.

A sounder but no more effective prestige than that of royal favor was given to surgery by John Hunter when he introduced surgical pathology. This step made the surgeon something more than a technician who clipped off legs or arms and tied up aneurisms. It brought in surgical diagnosis. Surgical diagnosis required something more than manual dexterity. It required intelligence and education. A surgeon of the times, which was only about 150 years ago, said that John Hunter had made the surgeons gentlemen. This surgeon, in his statement, did not mean that the men of his calling had gained prestige with the whole public, but only with that part of it represented by the physicians. It meant that after the long struggle the surgeon might stand on a level with the physician and that the curriculum of the medical school would, in consequence, include a fair proportion of surgery. The public prestige of surgery followed. Inseparable to its development were the discovery of anesthesia, antisepsis and asepsis, the founding of trained nursing and the rise of the modern hospital. Anesthesia and antisepsis were taken as adjuncts to surgery; trained nursing originated and grew as surgical nursing; and the modern hospital was built to house surgery. The public saw these things, the public recognized the spectacular success of surgery; and surgery became, in public regard, the prodigy of medicine. Public prestige meant large fees; large fees, plus prestige, meant a flood of students seeking surgical training; this demand further influenced the curriculum of the schools. And as a final advantage, surgery was personal. As a branch of medicine it has done far less toward modern health and longevity than has sanitation, but sanitation lacks prestige because it is impersonal. From the World War there came a score of books of surgical reminiscence for public consumption. There was not more than one that I know of on sanitary reminiscences. And yet the benefits conferred by sanitation were profoundly greater than those of modern surgery.

As I understand it, the sound, enduring establishment of any specialty of medicine is predicated upon three major points:

1. It must be an intellectual as well as a manual occupation.
2. It must receive respect and prestige from the other members of the medical profession. This respect may come primarily from the doctor as it did in surgery, or it may follow public opinion.
3. It must have public comprehension and must receive public respect and prestige. In short, it must appeal to the public.

Most sketchily I have traced out these steps in surgery. Let me apply them in turn to anesthesia. Anesthesia is new and anesthesia got off to a bad start from its very beginning. There was for years in anesthesia no intellectual or scholarly basis; it was a technical procedure carried out by rule of thumb by men or women who had no special knowledge of the respiratory and circulatory physiology and of the pharmacology that today are the primary requirements of the professional anesthetist. Anesthesia was the giving of a dose of medicine. The dose could be administered by the nurse or interne and, in an emergency, by a layman. A good many patients survived this sort of anesthesia. For that matter, a good many patients survived surgery before the days of Lister. Moreover, anesthesia was rarely, if ever, administered without accompanying surgical procedures and those were the days of rapid traumatic surgery. It was difficult to separate the risk of one from the risk of the other. Surgery was recognized and accepted as a hazard. But less well recognized was the fact that bad anesthesia often added immeasurably to the hazard of surgery—not the risk on the operating table of death from anesthesia, but that of subsequent failure hours or days after the operation was completed. In the maze of variables the part played by bad anesthesia was obscured.

I speak feelingly of bad anesthesia of the not very remote past—and there is still much—for I was once, for a short time, an anesthetist and a very bad anesthetist. During my internship I was trained by a nurse. I was given a cone, a can of ether and a few empirical tricks. The memory of those days had a salutary effect on me. In later years when it came time for me to undergo an operation—a tonsillectomy—my first thought was to obtain the services of the best anesthetist I could find. The second, and very secondary, was to find a throat specialist. Any good operator—and there were dozens at hand—could do a safe and competent tonsillectomy. But anesthesia, possibly because of my early experience, possibly because I was a respiratory physiologist, was a serious matter. It was a major therapeutic procedure without regard for the significance or insignificance of the operation. That is a point that the public does not appreciate nor, for that matter, some members of the medical profession.

If the members of the medical profession at large held such convictions, then the teaching of the principles of anesthesia would not, in some otherwise good schools, be crowded into physiology and pharma-

cology and its clinical aspects dismissed with a few demonstrations. It would be taught, as it should be taught, and I hope soon will be taught in every school, so that every medical student would leave the school with a beneficial and practical knowledge of respiratory and circulatory physiology which most do not get; and with an appreciation of the vast fund of knowledge and of the judgment that the good anesthetist must possess. If he did obtain these things which seem so essential a feature of a medical education he would carry with him a high regard for the specialty of anesthesia. If the members of the general public shared such views regarding anesthesia—and they are perfectly willing to share them if they are told of them—the anesthetist would come into his rightful position.

Thus, gentlemen, it seems to me that the future position of the anesthetist in American medicine is largely a matter of social change. The anesthetist will not establish his position by laboratory and clinical research alone, or by the development of new anesthetics and new apparatus. He will establish it only when he deals with the important but often neglected social feature. Even in spite of this neglect by most anesthetists the fact remains that the anesthetists have, during the last decade, made more progress toward establishing their specialty than has any other group in the profession. So far the progress has been mainly from within. It has been organization, the founding of journals and sections and the insistence on better teaching of anesthesia. And I do not need to tell you here that for this progress you owe a great debt to one of the most socially-minded and certainly one of the bravest men I have ever met—Dr. F. H. McMechan.

And now, in conclusion of this rather discursive and rambling talk, I am going to assume that you agree, at least in part, with my views. And in so assuming, I am going to presume so far as to offer some practical suggestions.

The first of these is that propaganda does not mean of necessity great radio programs, magazine articles and books. They are vastly helpful. But the first step is an earnest conviction on the part of the anesthetist of the importance of his calling and with this conviction an enthusiasm to tell of it by word of mouth to those with whom he comes in contact. Word of mouth may be slow, but it is the soundest propaganda there is. If the efforts toward shaping ideas go on further to public talks and magazine articles, then do not fall into the common fault of dramatizing discovery. Dramatization is emotional and sound social points cannot be put over in a setting of emotions, only conflict and fear. There is no need of arousing interest; the public already has an avid interest in all medical matters. Discovery alone so often emphasized in medical propaganda is not the feature to bring pointedly before the public. Discovery is medical research; it belongs to the doctor. The public plays no part in it except possibly to provide funds. The point to emphasize is application. What good does it do anyone to

announce over the radio, and in the papers, that a new anesthetic has been discovered? The announcement provides only table talk for the members of the public. It leaves no impression of the importance, the skill, the knowledge, of the man who must administer the anesthetic.

Today the public, by and large, believes that the important decision in anesthesia is what anesthetic they will be given, or possibly what method will be used. When, by propaganda, you have changed this view to one in which the important decision is what man shall give the anesthetic, then the problem of the place of the anesthetist in American medicine will be solved.

A Section on Anesthesiology was established in the American Medical Association during its Ninety-First Annual Session, which was held in New York City, June 10-14, 1940.

CONTENTS

The Place of the Anesthetist in American Medicine. HOWARD W. HAGGARD	1
Cyclopropane Anesthesia. ARTHUR E. GUEDEL	13
Abdominal Relaxation with Combined Regional and Cyclopropane Anesthesia. CHARLES F. MCCUSKEY	26
Cyclopropane-Air-Oxygen Anesthesia. A Preliminary Report. H. B. HAAS, HENRY J. HIBSHMAN and FLOYD T. ROMBERGER	31
The Role of the Liver and the Kidneys from the Standpoint of the Anesthetist. LLOYD H. MOUSEL and JOHN S. LUNDY	40
Convulsions During Anesthesia. An Experimental Analysis of the Role of Hyperthermia and Respiratory Acidosis. W. H. CASSELS, T. J. BECKER and M. H. SEEVERS	56
The Local Anesthetic Properties of Ephedrine Hydrochloride. FRED H. SCHULTZ	69
The Prevention and Treatment of Postoperative Pulmonary Complications with Special Reference to Suction Bronchoscopy. URBAN H. EVERSOLE	72
Changes in the Velocity of the Blood Flow During Spinal Anesthesia. ERNEST A. DOUD and E. A. ROVENSTINE	82
Studies on the Early Postoperative Reduction of Prothrombin in the Jaundiced and Biliary Fistula Patient with Special Reference to Anesthesia. J. GARROTT ALLEN and HUBERTA LIVINGSTONE	89
The Treatment of Aphonia by the Intravenous Administration of Pentothal Sodium. LOUIS H. GOLD and MARIO L. GAROFALO ...	94
Editorial	95
Book Reviews	97
Abstracts	101

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TABLE OF CONTENTS.

	PAGE
Foreword - - - - -	I
Early Anæsthesia. By Sir D'Arcy Power, K.B.E., F.R.C.S. - - - - -	4
Painless Childbirth by Synergistic Methods. A Preliminary Report. By E. P. Donovan, M.D., and James T. Gwathmey, M.D. - - - - -	8
Sacral or Epidural Analgesia. By J. Blomfield, O.B.E., M.D. - - - - -	22
New Anæsthetics. By C. Langton Hewer, M.B., B.S. - - - - -	27
Remarks on Broncho-pulmonary Complications following Operations under Anæsthesia. By W. J. McCardie, M.B., B.C. (Cantab.) - - -	30
Joseph Thomas Clover, a Pioneer in Anæsthesia. By Dudley Wilmot Buxton, M.D., B.S., M.R.C.P. - - - - -	55
Painless Childbirth by Synergistic Methods. By John O'Reagan, Leland R. Cowan, M.D., and James T. Gwathmey, M.D. - - - - -	62
Some Special Methods of Administering Anæ- sthetics. By H. P. Fairlie, M.D. - - - - -	68
On the Teaching of Anæsthesia to Medical Students. By Wesley Bourne, M.D., C.M. - - - - -	74
The Value of the Reflex Bases in the Interpretation of Phenomena during Anæsthesia. By S. R. Wilson, M.B., M.Sc., F.R.C.S.E. - - - - -	79
An Improved Open Ether Mask. By S. R. Wilson	86
National Anæsthesia Research Society of America. By E. J. McKesson, M.D. - - - - -	87
John Snow and the Advent of Chloroform. By Dudley Wilmot Buxton, M.D., B.S., M.R.C.P.	101
Laboratory and Clinical Experiments with Ethylene, and other Hydro-carbon Gases. By James T. Gwathmey, M.D. - - - - -	108

	PAGE
The Endo-Tracheal Administration of Nitrous Oxide-Oxygen-Ethanesal as the Routine Anæsthetic of choice for Major Operations. By C. Langton Hewer, M.B., B.S. (Lond.) - - -	113
The Advantages of Warm Ether Vapour. By Francis E. Shipway, M.D. - - -	123
Snow's Work on Chloroform. By Dudley Wilmot Buxton, M.D., B.S., M.R.C.P. - - -	149
Modern Developments in Anæsthesia. By S. R. Wilson, M.B., B.S., F.R.C.S.E. - - -	159
Some Results of Operations upon the Circulation : Their Significance and Treatment. By E. I. McKesson, M.D., Toledo, Ohio - - -	170
Cardiac Massage. By A. G. Levy, M.D., M.R.C.P.,	179
Abstracts of Current Anæsthetic Literature - -	41, 89, 135, 187
Bibliography of Current Anæsthetic Literature -	47, 100, 144, 197
Book Reviews - - - - -	147, 199
Editorial - - - - -	128, 184
General Items and Notes - - - - -	141
Latest Books on Anæsthesia - - - - -	148
Society Reports - - - - -	50, 51, 52, 194

British Journal of Anæsthesia

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JULY, 1923.

FOREWORD.

WITH this Number, the "British Journal of Anæsthesia" makes its debut as a publication devoted entirely to the interests of Anæsthesia and its practitioners. That there is need for a Journal of its character is amply demonstrated by the hearty support received in many quarters, and it thus, optimistically, takes its place amongst those special Medical periodicals which are representative of a particular branch of Medicine or Surgery.

As regards the future it should be stated that it is hoped to be able to keep abreast of the times in all that appertains to Anæsthesia, and to reflect the progress our Speciality is making everywhere.

In a word, the "British Journal of Anæsthesia" anticipates acting not only as the mouthpiece for those who desire to give public expression to the results of their research and experience, but to place before its readers an account of what is being done generally in the anæsthetic world.

Thanks to the confidence vouchsafed by the Guarantors, and to the encouraging support of our advertisers, the financial arrangements for the first year are assured. The Journal is thus happily launched upon its journey with hopefulness and good cheer.

To make the Journal of the greatest value to its readers, each Number should contain a summary of contemporary thought and achievement. To accomplish this end it has been decided to include, in addition to short Editorials, as broad a variety of original articles as possible.

In this Number we are especially fortunate in having the Historical Aspect of Anæsthesia discussed by Sir D'Arcy Power. It is obviously desirable to learn the views of those across the Atlantic, and we are privileged to include as a contributor Dr. James T. Gwathmey, of New York, the Founder and First President of the American Association of Anæsthetists, which now has a membership of 1,200. The Bibliography of Current Anæsthetic Literature and Programmes of Foreign Societies are intended to place before our readers the trend of the latest research and thought of the day.

* * * * *

The question of organization for the general body of Anæsthetists must sooner or later present itself in a concrete form. There is always the possibility of finding ourselves in a position where it becomes necessary to exert an influence commensurate with our numbers, and to attempt to do so, except as a united entity, would more than likely meet with failure.

If there is strength in unity there is surely futility in the reverse.

At a recent meeting of the Anæsthetic Section of the Royal Society of Medicine, the matter of Coroners' Inquests in relation to all anæsthetic fatalities was discussed. There was manifest a marked difference of opinion as to the necessity of these public examinations following every ordinary case, and it was made evident that there is ample room for reform.

Questions of this nature, and others, could be influenced to a great extent by a united organization. Placing aside purely legal matters, the scientific, educational and economic aspects of the Practice of Anæsthesia demand discussions which are only possible in assemblies of those especially interested in that branch of Surgery.

With the exceptions of the Section in London and the Society in Scotland, the great majority of the something like 500 Practitioners of Anæsthesia have no affiliations with similar bodies.

A suggestion to be considered in this connection is the formation of a Midland Society to comprise the Anæsthetists of Birmingham, Bristol, Derby, Leicester and Cardiff; a

North of England Society to consist of Manchester, Liverpool, Leeds and Sheffield; and others according to geographical divisions.

These, with London and Scotland, and, ultimately, the Dominions, Colonies and Dependencies, should form the basis of a general organization, viz., the British Association of Anæsthetists.

This may, no doubt, be regarded as a somewhat ambitious programme, but, in truth, it only parallels similar activities elsewhere.

Such organisations as the Ophthalmological Society, the Pathological Society, the Psycho-Neurological Society, and the Oto-Laryngological Society are examples of successful, country-wide associations of specialists; yet probably not one of these has a greater field to draw from than the grand total of anæsthetists in the same area.

It is a self-evident fact that anæsthetists united in one large, virile body can do much to advance the science and practice of anæsthesia, and it behoves those of us who are alive to the *possibilities* of unity to give the matter attention and thought.

THE EDITOR.

ON THE TEACHING OF ANÆSTHESIA TO MEDICAL STUDENTS.

By WESLEY BOURNE, M.D., C.M.,

Lecturer in Pharmacology, McGill University.

TO advance anæsthesia in medicine we believe that those interested in the subject must do more research work and better teaching. The quality of the latter is affected by the former. Realising these, we have, during the past three years, made some improvement at McGill University.

In research the anæsthetists work individually, but in teaching collectively for the purpose of establishing unanimity of doctrine. The student is given his first instruction in anæsthesia in the fourth year during the course in pharmacology. Here, after the theory of anæsthesia has been discussed and the behaviour of certain narcotics has been studied in the laboratory, Professor Barbour allows the anæsthetists to conduct the activities of one week in which there are two lectures of one hour each, one laboratory period of two hours, one demonstration, and, lastly, one conference of one hour. The two lectures are didactic, and deal with the details of the pharmacological actions of the general anæsthetic agents commonly used.

At the laboratory period the students, in groups of four, do the work themselves under the supervision of demonstrators. Dogs are used, half of which have had no preliminary medication, the other half being under the influence of morphine (10 mg. per kg.). It has been customary to induce anæsthesia with ether given by a cone and continued by a tracheotomy tube, a branch of which was connected to a tambour for tracings. Carotid blood pressure was also recorded on the kymograph. Now that the Department of Pharmacology has moved to the new biological building, the procedure for this present session is planned in which the observations will all be made by bloodless methods and mortality reduced to a minimum. The students will be able to maintain anæsthesia by intra-pharyngeal insufflation, and

will follow the circulatory changes by auscultation, as well as by tracings on the kymograph, using a modified Kolls blood pressure cuff recently devised by Dr. Joseph Kaufmann and the author.

With the aid of a synopsis the students are allowed to study the pulse and respiration of normal or morphinized dogs, to induce anaesthesia with ether, using a wooden cabinet which has one side of glass and two openings. One admits air under pressure which carries with it anaesthetic vapour, and the other acts as an exit. Here they note the behaviour of the animal, the reflexes, the movements and position of the eye-balls and movements of the larynx, they mark the stages of anaesthesia and locate the strata of the third stage, and observe the time required for complete anaesthesia.

The animal is now removed from the cabinet and tied on a heated table and anaesthesia maintained by pharyngeal insufflation by passing the tube, which was attached to the cabinet, through a wooden mouth gag to the pharynx. A pneumograph is now adjusted for respiration tracings, and the blood pressure cuff placed on a hind leg, so connected that tracings may be recorded. And now detailed observations may be made under the following conditions:—Light ether anaesthesia, insufficient aeration, a change from light ether anaesthesia to chloroform, deep ether anaesthesia, insufficient aeration again, a change from deep ether anaesthesia to chloroform, after which insufficient aeration once more and then deep chloroform anaesthesia. We avoid the usual instructions of chloroform poisoning. However, should respirations cease, methods of resuscitation are done, such as stretching of the anal sphincter, artificial respiration, cardiac massage, intravenous injection either of epinephrine, 0.1 mg. per kilo mixed with 50 cc. normal saline, or caffeine sodio-benzoate, 50 mgms. per kilo.

The students are called upon to tabulate the observations made under the various conditions enumerated, and to compare as well the differences between the morphinized dogs and those without morphine. Further emphasis is laid upon a comparison of ether with chloroform.

At the demonstration they are shown how anaesthesia may be smoothly induced and conducted in a dog even without

preliminary medication. The animal is placed in a large glass case which has two openings. One permits of connection to an insufflation apparatus, the other acts as an outlet. Air is pumped in to which ether vapour is gradually added. The student becomes impressed with the quietude of this induction, for the animal becomes anæsthetized without struggle and with comparatively little increase in muscular movement in from two to four minutes. It is then quickly removed and placed on a heated table, and with the aid of a Chevalier Jackson laryngoscope intubation is done and anæsthesia maintained by continuous intra-tracheal insufflation under low pressure. Blood pressure is observed, as mentioned above. Tracings of respiration are made by a T-tube connection to the intra-tracheal tube. This demonstration affords a further opportunity for explanations of many important details of anæsthesia.

The conference consists in a review of the week's work, when the students are asked and allowed to ask questions.

The practical applications of anæsthesia are taken up in the following fifth year during the course in Clinical Pharmacology. Two lectures are devoted to anæsthesia and such matters are discussed as the choice of anæsthetic agents and methods, the preparation of the patient, the details of conduction of the anæsthetic period and after care. Lantern slides are used to illustrate Guedel's sign chart, some typical anæsthesia charts and several of the more important anæsthetic implements and machines. One gas-oxygen machine is demonstrated in detail.

Practical work is exacted of the students during the final year. Each student must administer anæsthesia himself at least four times, and make in each case a full report to his instructors on what is known as the McGill anæsthetic record here illustrated. Again, in each large hospital an anæsthetic clinic is given once during the session when several short operations are performed and as nearly as possible all of the various anæsthetic agents and methods demonstrated, the surgeons allowing the anæsthetists to hold the floor. The students are encouraged to participate further in the giving of anæsthetics as opportunity offers, and many graduate with fifty or more anæsthesias to their credit.

MCGILL ANÆSTHETIC RECORD

Hospital

NAME *M.*
SURGEON Dr.AGE *32* SEX
DATE *23-8-22* WARDOPERATION *Hysterectomy + appendectomy*

SPECIAL CONDITIONS PRESENT

Fibroid

INDUCTION SMOOTH, DIFFICULT, STRUGGLING

HOLDING BREATH, SPASMODIC OBSTRUCTION

CYANOSIS, COUGH, TREMOR

ANÆSTHETIC

N₂O + O.

METHOD

Closed

APPARATUS

MED. MORPH. *1/6* ATROP. *1/150* SCOP. *0.*GIVEN AT *8 O'CLOCK* *am*DURATION ANÆS *1 hr. + 10 mins.*

DURATION OPERATION

same

POSITION

Trendelenburg's

STIM

*none*INFUSION *0* INTRAVENOUS*none*

SUBCUTANEOUS

none

CONDITION AT CLOSE

Good

RECOVERY IMMED. EARLY, DELAYED,

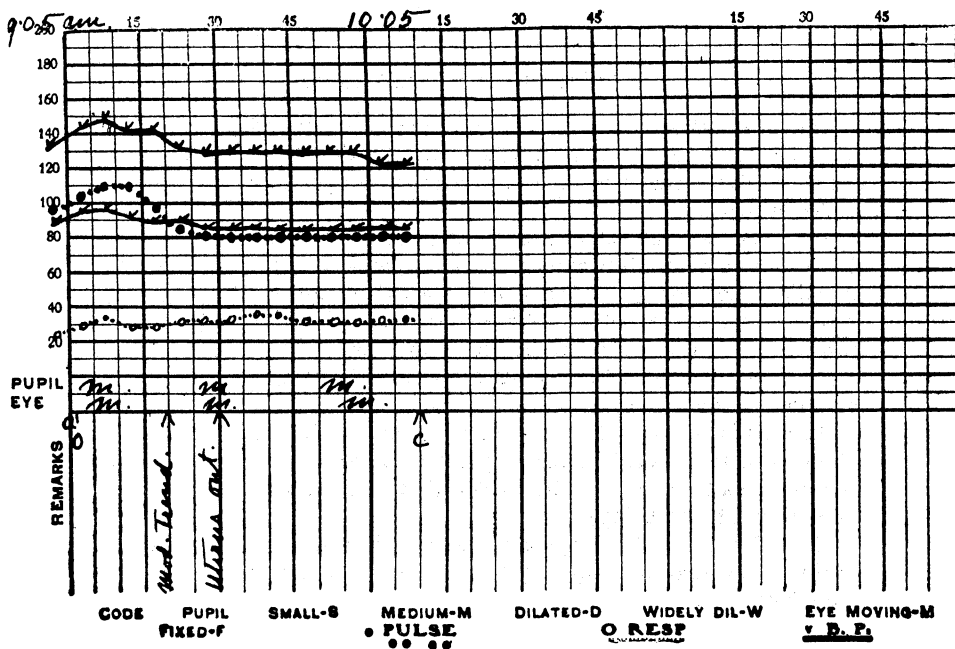
AFTER EFFECTS

NAUSEA, VOMITING, NONE, SLIGHT, MOD. SEVERE,

COUGH, *once immediately*

REMARKS

Anæsthetist.



For the routine administration of ether we use in all of our hospitals the McGill modification of the Ferguson mask, and for which Dr. W. B. Howell deserves most of the credit. For reference text-books we recommend Solmann's "Pharmacology," Cushney's "Pharmacology," and works on anæsthetics by Buxton, Gwathmey, Flagg, Hewitt and Silk. In this connection, in the near future, we hope to have prepared a small book on anæsthesia to be used as a practical guide by students and to be known as the McGill Handbook of Anæsthesia.

To us it is particularly encouraging to note the increasing interest shown by the majority of students, some of whom are now asking to be admitted as house anæsthetists at graduation. This should offer a real solution to the difficulty in obtaining qualified graduates in medicine to administer anæsthetics.

A Diploma in Anæsthesia.

With the steady advance of our speciality the time is rapidly approaching when its status, after the completion of a satisfactory course of higher instruction, would warrant the granting of a Diploma in Anæsthesia.

For those who desire to prepare themselves properly to undertake the uncommon and intricate procedures necessary in what may well be called the higher grades of the practice of anæsthesia, a special course of instruction of from three to six months in a well equipped hospital and university would be more than worth the time and expense, especially if the reward of close application proved the attainment of a diploma.

The first three months could be spent at certain specified hospitals where special demonstrations might be arranged, but the final three months should probably be confined to one such centre as the University of Cambridge.

Candidates for the diploma would, of course, hold a medical qualification, and be required to do a certain amount of practical anæsthetic work.

Anæsthesia

JOURNAL OF
THE ASSOCIATION OF ANÆSTHETISTS
OF GREAT BRITAIN & IRELAND



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CONTENTS

	Page
FOREWORD by Sir Alfred Webb Johnson	3
EDITORIAL by C. Langton Hewer	4
THE ASSOCIATION OF ANÆSTHETISTS OF GREAT BRITAIN AND IRELAND by H. W. Featherstone	5
CENTENARY OF ANÆSTHESIA IN GREAT BRITAIN by A. D. Marston	9
CURARISATION COMPARED WITH OTHER METHODS OF SECURING RELAXATION IN ANÆSTHESIA by F. Barnett Mallinson	17
LOCAL ANALGESIA FOR ABDOMINAL OPERATIONS by A. R. Hunter	22
THE ANÆSTHETIST AND CARE OF THE SURGICAL CASE by John Beard	25
THE EFFECT OF THE WAR ON THE POSITION OF THE AMERICAN ANÆSTHETIST ... by Noel A. Gillespie	35
A RARE CARDIAC PROGNOSTIC SIGN by J. U. Human	37
REVIEW 	38
ABSTRACTS 	39
ASSOCIATION NEWS 	41

Extract from CONSTITUTION OF THE ASSOCIATION

Rule 3.

The objects of the Association shall be:—

- (a) To promote the development and study of anaesthetics and their administration and the recognition of the administration of anaesthetics as a specialized branch of medicine.
- (b) To co-ordinate the activities of Anaesthetists.
- (c) To represent Anaesthetists and protect their interests.
- (d) To promote the establishment of diplomas and degrees in anaesthetics.
- (e) To encourage and promote co-operation and friendship between Anaesthetists.

And to do all such lawful things as may be incidental or conducive to the attainment of such objects.

MEMBERSHIP

A candidate for Membership of the Association must:—

- (a) Be a registered medical practitioner.
- (b) Satisfy the Council that his professional interests are predominately in anaesthetics or be an anaesthetic specialist or graded specialist in anaesthetics in the Royal Navy, Army, Royal Air Force, Emergency Medical or Municipal Services.
- (c) Be nominated by two Members, or Ordinary or Senior Fellows who shall vouch that the candidate fulfils the conditions of Membership.

FOREWORD

By

SIR ALFRED WEBB JOHNSON, K.C.V.O., C.B.E., D.S.O., T.D.

President of the Royal College of Surgeons of England

I AM grateful for the invitation to write a Foreword to this first number of the Quarterly Journal "Anæsthesia," for it not only gives me the opportunity to wish the Journal every success but also to say how pleased I am that the Association of Anæsthetists of Great Britain and Ireland has decided to launch a periodical publication on this important subject.

The year of the celebration of the centenary of the first operation under general anæsthesia in this country is a most opportune time for the institution of a Journal which will make British teaching and records of discovery and achievement available for the medical profession throughout the world. It is very fitting that the country which made such valuable contributions in the early days of anæsthesia by the pioneer work of Hickman, Simpson, Snow and Clover should be represented in the medical literature of the subject.

This can be done most effectively by a Journal which is devoted entirely to this special branch of medicine. I feel sure that the Journal "Anæsthesia" will fill a long felt want and will be greatly appreciated by anæsthetists of all countries.

EDITORIAL

SOME three thousand years ago, the greatest living sage recorded that "of making many books there is no end." Few at the present time would question the truth of this statement so that there should be some good and definite purpose for starting a new journal.

What then are the reasons for launching "Anæsthesia"? In the first place it has become obvious for some time that the rapid advance in all types of anæsthetic and analgesic technique requires fuller and quicker expression than can be provided in the overloaded columns of the general medical press. The progress of the specialty during the past hundred years is briefly sketched in the review by the President of The Association of Anæsthetists on another page and the tempo of this progress is continually quickening. The full and accurate presentation of observations, theories, new methods, etc., is essential if a true advance is to be maintained and if unprofitable detours are to be avoided. Original contributions dealing with any aspects of the subject will be welcomed, the sole criterion for acceptance being their merit in the opinion of the Editorial Board, subject to space being available for reasonably quick publication. It is hoped that our colleagues abroad will also give us the benefit of their observations and views.

It will be clear, therefore, that the Journal will primarily be a scientific one, but it is also the official organ of The Association of Anæsthetists of Great Britain and Ireland. The activities of the Association are many and increasing as will be realized by perusing its history kindly summarized for this issue by its first President. It is doubtful whether most of the Fellows and Members have any conception of the amount of work which the Council, and especially the executive officers, transact in the course of a year. Many of the results obtained may seem relatively unimportant and certainly do not justify the heavy expense of circularisation. They can only be referred to very briefly, if at all, in the Annual Report. A quarterly journal, on the other hand, should be able to disseminate any news of general interest without undue delay. The prospect in the immediate future gives some urgency to this matter. Whatever view may be taken of the merits or demerits of a National Medical Service, there can be little doubt that the implementation of the Health Bill will profoundly affect all anæsthetists. The latest information on the Specialist service and the results of negotiations now proceeding between representatives of the Association and various official bodies will be published as soon as they are available.

In conclusion we cannot do better than to quote the last sentence of a recent leading article in "The Times." "One of the greatest contributions to human well-being in the last hundred years has been the great advance in the field of anæsthesia." To facilitate this advance by all available means is the not ignoble aim of this journal.

**THE ASSOCIATION OF ANÆSTHETISTS
OF GREAT BRITAIN AND IRELAND**

Its Inception and its Purpose

By H. W. FEATHERSTONE, M.D., D.A., O.B.E.

FIRST PRESIDENT, ASSOCIATION OF ANÆSTHETISTS.

DURING Surgical Anæsthesia's century of existence not only have valuable agents and sound technique multiplied and the benefits extended vastly, but the capacity of administrators has undergone important developments. Training and skill, bred of experience and of knowledge constantly nourished by research and observations in many branches of science, have enabled the uncertain, even dangerous, ministrations of earlier days to be replaced by improved methods wisely selected and skilfully employed by specialists. "

In this country the scientific development of anæsthetics under clinical conditions has progressed largely under the ægis of the Society of Anæsthetists and of its successor the Section of Anæsthetics of the Royal Society of Medicine. But when, forty years ago, the Society of Anæsthetists was thus merged into the re-constituted Royal Society of Medicine, the Anæsthetic Section's activities were limited by the regulations entirely to the scientific aspect of the subject. The position in the year 1930 was this: the science and art of anæsthetics and also the bulk of anæsthetic practice had extended enormously—the latter being in part a result of the recent Poor Law Reform and the consequent growth of general hospitals under county council and municipal authorities—but the practitioners of this branch of medicine had no means of organising their department nor of representing the needs of the anæsthetic service. No standard of training had been laid down, and there was little means of distinguishing trained workers from unskilled but optimistic novices. This was unsatisfactory for patients, for surgeons, and for hospital authorities, while skilled anæsthetists were inclined to feel that devotion to this branch of the Profession had exposed them to an unstable career where 'goodwill' could not be established, the problems of routine work could not be voiced, and there was no official body representative of anæsthetists, to whom medical or lay authorities could turn for guidance.

It fell to the lot of the writer, who was the retiring President of the Section and who had recently visited Canada and the U.S.A. with the British Medical Association, to have the need for action impressed upon him by many colleagues. Accordingly, invitations were issued to a hundred anæsthetists on the staffs of the main teaching hospitals to attend a meeting for the purpose of forming an association which would meet the needs. Very remarkable enthusiasm was shown, and a provisional Council was appointed which included the senior anæsthetists from each of a dozen medical

schools. This council drew up a list of objectives and formulated rules which were based on those of the Association of Surgeons and of similar bodies. The rather lengthy name—the Association of Anæsthetists of Great Britain and Ireland—was selected after careful reflection. To the writer, at first, the title ‘ Guild ’ appealed, for this emphasised the technical skill based upon training which specialists in anæsthetics must possess ; but such a title would have been too stylistic. There was a general desire that colleagues in Eire should be able to join the Association and a number have done so.

During the ensuing years the Association made smooth and effective progress. Encouraged by the enthusiastic support of the members, which was demonstrated at general meetings and in correspondence, the Council worked in an atmosphere of co-operation. A short synopsis of some of the earlier activities of the Association may be of interest and perhaps helpful in framing future policy.

The Association has been careful to refrain from encroaching on the sphere of the Anæsthetic Section of the Royal Society of Medicine ; each year the President of the Section has been included in the Council of the Association. The Association’s scientific contributions have been either in answer to outside enquiries or for the purpose of impressing on other bodies that certain action by them was advisable. Thus the need for improvement in methods of analgesia for midwifery, emphasised by Lady Baldwin and her co-workers, was regarded by the Association as a matter for investigation in which it should take an active part ; and a general meeting followed at which Dr. Minnitt and Dr. Mennell respectively first demonstrated the Minnitt gas-air machine and the Mennell chloroform bottle, appliances which they had devised through their interest having been aroused at discussions of the Council. Representations made by the Association to the Home Office and to nitrous oxide and oxygen manufacturers led to the introduction of light steel cylinders into anæsthetics. Following his work on the use of coal-gas as a fuel for motor cars and its storage in ‘ vibrac ’ steel cylinders, Dr. C. M. Walter, consulting gas engineer to the Birmingham Corporation, at the writer’s suggestion, described to a meeting of the Association the properties of the cylinders and their suitability for the storage of anæsthetic gases. At first the manufacturers were not prepared to charge these thin-walled, though tough, ‘ bottles,’ but they co-operated very helpfully when they had received a special assurance from the Head of the Cylinder Department at the Home Office after his attention had been drawn to the matter by Dr. Walter and by the Association.

The Association’s most far-reaching achievement has been the establishment of the Diploma, and it was the Association’s first undertaking. The cardinal points of the Council’s plan were :

1. The Diploma should be the ‘ hall-mark ’ of a trained anæ-

thetist. The examination should not be competitive, but it should be so far as possible a test of sound practical knowledge and skill.

2. If possible, there should be only one Diploma. Multiplicity of diplomas would prevent the establishment of a definite standard.

3. Experience of the long delay between the establishment of the Diploma in Public Health in 1885 and its general recognition fifty years later as the essential test for doctors who wished to take up state medicine, and, by contrast, the rapid establishment of the diplomas offered by the Royal College of Obstetricians and Gynæcologists which awarded, without examination, diplomas of suitable grades to recognised gynæcologists, convinced the Association that it was important that the Diploma in Anæsthetics at the outset should be held by the senior teachers of anæsthetics.

The Councils of the Royal Colleges and the Examining Board in England showed immediate interest in the matter and, following very helpful interviews which they gave to representatives of the Association, each of the three desiderata were fulfilled. Few will deny that the Diploma has been a most successful institution. During the War it was of special value in selecting specialist and graded anæsthetists in the Services, and undoubtedly the equality in rank and grading with other specialists, which worked so smoothly, was granted because the specialty was thus well established and its members clearly denoted. Moreover, the supply of resident anæsthetists in general hospitals is encouraged by the regulation that training for the Diploma requires tenure of such posts, and thus the Diploma has helped to raise both the standard of performance and the status of hospital anæsthetists.

In 1935 the Home Secretary appointed a departmental committee under the chairmanship of Lord Wright to take evidence and to make recommendations concerning the duties of coroners. The Association was permitted to give evidence on the problems connected with deaths under anæsthesia. A booklet of evidence was submitted to the Committee, and at the subsequent hearing the three representatives were well received, their views were approved, and in due course the Committee's recommendations embodied the points which they had raised. Unfortunately, on some subjects, not however relating to anæsthetics, the Committee was not unanimous, legislation was likely to be contentious, and the international situation was over-riding domestic issues; therefore the recommendations have not as yet been taken up by the Government. Following these recommendations, however, it may be that coroners have been encouraged to use a wider discretion as to when inquests should be held in cases of death under anæsthesia.

Since its inception the Association has encouraged its members in each medical centre to use it as their representative body. A proportion of the Council is selected from the provincial schools, while each medical school not directly represented on the Council has an elected representative who should bring together fellows and

members in the locality and thus maintain liaison with the Council. The principal countries of the British Commonwealth of Nations have representative members. A variety of problems of local or of personal interest have been submitted to the Council, and advice or action by the Council or by the Association in general meeting has followed, as might be suitable. Problems connected with the amounts and the charging of fees caused the Council to approach the Council of the British Medical Association. A sympathetic hearing was given and subsequently a satisfactory statement of policy was issued. However it was our experience in pre-war days that individual hospitals and medical schools were not willing to pay very serious attention to advice on matters of salary, of fees, of status (e.g., membership of hospital medical boards), or of conditions of service. Public health departments of the Government and of other public authorities on several occasions consulted the Association concerning their anæsthetic services. Among the activities in association with other bodies it should be recalled that the Central Medical War Committee accepted the Association's nominee, Dr. Mennell, and he also was previously a member of the committee set up by the Royal College of Obstetricians and Gynaecologists to appraise the value of the various methods of analgesia for midwifery.

The trend of policy has appeared to some members to be in the direction of providing a service of trained anæsthetists, working under good conditions at satisfactory rates of pay, rather than to concentrate on the provision of numbers of specialists in independent practice. The War, the development of large anæsthetic departments in the Services, the ever-growing proportion of hospital work—private as well as public—and now the provision of anæsthetists under the coming National Health Service have emphasised the former aspect. Indeed, during the War it became evident that many specialists in anæsthetics in future years would not necessarily be members of teaching hospitals and that the Association, if it were to be fully representative in future planning, must comprise among its members many who were not eligible under the original rules. After full consideration, the membership rules were altered to extend the permitted maximum number of members (hitherto fixed at one hundred and fifty) and to provide for fellows as well as members. This has been an acceptable development.

The finances of the Association, with careful husbanding by Dr. Mennell, have grown steadily, with the result that when the Royal College of Surgeons offered accommodation and secretarial facilities in Lincoln's Inn Fields the Association was able to take advantage of the plan. This valuable step was due largely to the understanding interest shown by the President of the Royal College of Surgeons, Sir Alfred Webb-Johnson, and the far-seeing leadership of Brigadier Ashley Daly, at that time President of the Association.

The very successful Annual Dinner and the instructive discussions

and demonstrations associated with the Annual Meeting last October made it clear that the Association is in a very healthy state, and a special tribute must be paid to Dr. Marston for the enthusiasm and wisdom which he has displayed as Honorary Secretary and as President.

With the inauguration of its own journal, *Anaesthesia*, now added to the amenities of the Association under the experienced editorship of Dr. Langton Hewer, anæsthetists may have confidence that they possess the means of discussion and representation, a diploma to ensure a sound standard of work, and a home which will enable the specialty to progress alongside the other departments of medicine on the best lines in the new circumstances which the advent of the National Health Services will reveal.

CENTENARY OF ANÆSTHESIA IN GREAT BRITAIN

By A. D. MARSTON, M.R.C.S., D.A.

PRESIDENT, ASSOCIATION OF ANÆSTHETISTS OF GREAT BRITAIN AND IRELAND.

THE observance of anniversaries is an ancient and honoured tradition in our custom-loving land, and it is probable that the centenary of anæsthesia will be associated with much gratitude and genuine thanksgiving. In gratitude we shall honour the memory of pioneers who made freedom from operative pain the common heritage of mankind.

In these days of increasing co-operation and understanding with our American cousins it is pleasing to know that most of the original scientific research, and certainly its successful application to clinical medicine, was carried out by citizens of the English speaking race on both sides of the Atlantic.

We must give honour where honour is due, and here it is well to remember that the whole credit for the introduction of anæsthesia cannot fairly be given to any one man, for historical investigation shows that this epoch-making discovery was due to the uncoordinated efforts of a number of workers.

Much time has been wasted and recrimination caused by attempts to assess the precise credit due to individuals for priority in this great work—a task which even baffled such an august and astute legislative body as the Congress of the United States of America—and in this centenary year we feel that it is sufficient to remember these pioneers in common gratitude for their successful efforts.

There was certainly no element of priority in the first administration of ether in this country in those late December days of 1846, as Crawford Long had been successfully using this drug since March 30th, 1842, in his practice in Jefferson in the State of Georgia, U.S.A.

But Crawford Long had not given any public demonstrations of his discovery, neither did he describe his technique in the current medical literature of his day, and so this valuable work aroused only local interest and some opposition.

the delayed gastric emptying which accompanies apprehension in some patients—more especially children. Impatience and the pressure of work have resulted in catastrophe in many outpatient departments. When it is essential to proceed, the stomach should be emptied—in children by an emetic and in adults by a tube—before anæsthesia. In cases of obstruction to the alimentary tract or of gastric or intestinal dilatation, where vomiting may be expected, to withhold gastric lavage and aspiration involves taking a risk, normally unjustifiable, at the patient's expense. Patients too ill for this procedure are possibly moribund for lack of it, and certainly too ill for general or spinal anæsthesia. Unorthodox techniques such as pentothal and curare may serve in specially skilled hands, but, in general, few exceptions can be made with any safety.

(To be concluded)

THE EFFECT OF THE WAR ON THE POSITION OF THE AMERICAN ANÆSTHETIST

BY NOEL A. GILLESPIE, D.M., D.A.

ASSISTANT PROFESSOR OF ANÆSTHESIA,
UNIVERSITY OF WISCONSIN.

TRAINED as he is to regard the administration of anæsthetic agents as a duty which, in the present state of British law, it is too hazardous to entrust to any person not duly qualified and registered, the British anæsthetist has little concept of the position in the United States. In the majority of States, although it is felony to practise medicine without a licence, both public opinion and the decisions of courts condone the practice whereby a medical man may "cover with his responsibility" an unlicensed person in the discharge of duties essentially professional. The neglect of the art of anæsthesia by the medical profession was common to both countries in the middle of the last century, except for the legal difference already mentioned. The sordid reasons for which it became customary for anæsthesia to be administered by technicians in America need not be stated here. It is sufficient to point out that in 1940 probably from 75-80 per cent. of all administrations in this country were conducted by technicians; that in only a dozen or so medical schools was there any real teaching of the art to medical students; that in a number more the "teaching" was performed by pharmacologists or physiologists with no experience of clinical anæsthesia; and that several of the more famous medical schools still countenanced the maintenance of courses for the training of technicians in anæsthesia. In 1940 the membership of the American Society of Anæsthetists was a mere 800: and this figure included men in general practice with only a casual interest in anæsthesia.

It had long since been realised that technicians could not be eliminated until sufficient competent medical men were available to replace them. The mobilisation of the country naturally intensified

the demand for trained anæsthetists, especially since the army soon evolved an appointment known as "Chief of operating and anæsthesia section." The incumbent of such a post had indeed to be versatile ! His was the responsibility, not only for the administration of anæsthetics, but for the posting and arrangement of the operating list, and for all therapy by fluids and blood, as well as for all the minor details of equipment and personnel in the operating rooms. As a rule, he took charge of all therapy by inhalation.

When such an office was first created, the army realised that very few of its surgeons had ever worked with a professional anæsthetist, or had any knowledge of how to make use of his skill. The Surgeon-General wrote to one of the senior members of his advisory sub-committee on anæsthesia, and asked if a statement could be prepared on the skills and functions of an anæsthetist. This would enable him to distribute a memorandum on the subject to all chiefs of surgical services.

The prestige of an anæsthetist, therefore, depended on his ability to demonstrate his professional worth. So well have anæsthetists in the armed services done this that their efforts during the war are already bearing fruit as the men concerned return to civilian life. Many surgeons, having learnt during the war what it means to the safety and success of operation to depend upon a reliable medical opinion as to the condition of the patient, are now unwilling to give up that advantage. Having returned to normal work they are seeking for trained medical men to replace the technicians on whom they used to depend.

Anæsthesia owes a great debt to the men who discharged their professional duties so well in the most trying circumstances as to achieve a conversion of so many surgeons. It is no easy matter to keep meticulous records when working day and night under fire. Considerable restraint is needed to endure daily the apparent hostility and professional insult which is born of a surgeon's ignorance of the functions of an anæsthetist. The men who went to war have proved that they could not only endure these hardships, but in spite of them could work with such competence as to show surgeons that they are necessary members of a surgical team. The farewell interview of a young anæsthetist, who had just received orders to move to another unit in the Middle East, with his colonel, is fairly typical of the general experience. The Colonel said, "When you first came I didn't know what to do with you. Now I don't know how I am to do without you."

In England and Australia the law makes the anæsthetist answerable for the life of the unconscious patient. Though this liability has sometimes irked British anæsthetists*, their American colleagues give thanks for an example which, during the war, has helped to enlighten American surgeons. In various parts of the Empire they have seen for the first time what are the responsibilities of the medical anæsthetist. They have been amazed by the responsibility borne by

*See *Brit. Jour. Anæst.*, 1936, XIII 89 and 1936, XIV 17.

him in law ; having learnt from British Coroners that, in some cases, the anæsthetist may become the scapegoat for the surgeon's sins.

The future of anæsthesia in this country now depends on the extent to which the demand for capable anæsthetists can be satisfied in a reasonable time. The number available is woefully small, and the facilities for training more men are inadequate in quantity.

Thanks to the devotion with which medical anæsthetists have proved their worth in the services, the specialty has attained its majority in this country. British and American anæsthetists have learnt to work together in war, and have mutually exchanged much that is of value. If we are not again to "lose the peace," we anæsthetists, who are admirably placed to do so, must set a high example of international friendship, understanding, and unselfish mutual assistance.

A RARE CARDIAC PROGNOSTIC SIGN

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SOME phenomena in anæsthesia occur so rarely that no single anæsthetist is ever likely to encounter any one of them more than four or five times, and one hesitates to publish conclusions drawn from so small a record. However, if all such observations are published by all anæsthetists it will in time be possible to form a correct assessment of the value of any sign, however rare its occurrence.

I have on four occasions found extra systoles occurring in a patient for the first time during nitrous oxide—oxygen—ether anæsthesia, and in each of the four cases the patient died of sudden cardiac failure within the first five days after the operation.

Cases I and II.—Both of these were cases of long standing frontal sinus infection which had become acute. Both patients were obviously suffering from toxæmia and one of them was semi-conscious before the operation. Both recovered consciousness after the operation and appeared to be improving but one died suddenly 24 hours after operation and the other died unexpectedly on the third day. These two patients had atropine gr. 1/100 only half an hour before operation as premedication.

Case III.—A bloated and unhealthy looking alcoholic of about 50 years of age who was brought into hospital as an air raid casualty one night during the "blitz" on London. His injury was a depressed fracture of the vertex of the skull. He was not unconscious on admission and after operation he appeared to be doing very well when he died suddenly on the fifth day. His premedication, too, consisted of atropine gr. 1/100 half an hour before operation.

Case IV.—A man of seventy who was regarded as a poor risk

