ment, but I am sorry to see that in an attempt to simplify it an empirical method of attempting to establish nitrogenous equilibrium has been generally adopted. Daily analysis of the urine during the fasting period shows that, coincident with the fall in the sugar excretion, there is a drop in the whole level of metabolism, the total nitrogen sinking until it stands at a steady level of 4 or 5 grams a day. At first there may be a rise in the output of uric acid and creatinine, but as the fasting proceeds they also sink, and the same is true of the amino acids. When protein is added to the diet the excretion of these substances rises again until a level is reached at which the total nitrogen of the urine, plus 1 gram to allow for the loss in the faeces, balances the nitrogen content of the diet. If the protein intake is maintained at about this amount the patient remains in nitrogenous equilibrium, but if more protein is given evidences of a disturbance of nitrogenous metabolism shortly appear, and ultimately sugar is passed in the urine. Merely to rely upon the appearance of glycosuria is therefore far from being an ideal method of regulating the protein intake, and I have found the adoption of a fixed figure governed by the weight equally unsatisfactory in many instances. Estimation of the carbohydrate tolerance presents no great difficulty, especially if analysis of the urine is controlled by determinations of the sugar content of the blood. When treating children it is important to remember, however, that the diet, both as regards protein and carbohydrate, should not be increased at the same rate as for adults, 1 gram of carbohydrate for a child of 10 kilograms, for instance, being about equal to 4 grams to an adult.

Regulation of the fat intake requires more care than is frequently given to it. Fat is generally regarded as a make-weight by which the caloric value of the diet can be adjusted to the theoretical requirement worked out from the patient's weight, but modern research suggests that a carefully adjusted fat supply is as important as a correctly arranged intake of protein and carbohydrate. Bloor has shown that abnormal lipid values in the blood are common in diabetes, apart from obvious lipaemia, especially in severe and chronic cases, and that these lipoids originate from the fat of the food. Increase in the latter involves, therefore, the overloading of an already defective mechanism, whereas fasting, or a reduced fat supply, removes the excess of lipoids and leads to more efficient fat metabolism. There does not appear, however, to be any definite relation between the lipid content of the blood and the appearance of acetone bodies in the urine.

The importance of not commencing the fast in severe cases with acidosis until the patient has been kept for a few days on a fat-free diet, poor in protein, has been referred to, and it is equally important that fat should not be added to the diet subsequently until a considerable carbohydrate and protein tolerance has been established. A creamy blood, an increase in the ketonuria, and the appearance of glycosuria should be controlled by reducing the fat, otherwise it is best to regulate the diet so that a progressive loss in weight is prevented, while a gain, or more correctly an increase in adipose tissue, is avoided.

At regular intervals the whole level of the diet should be materially reduced to avoid overtaxing the defective metabolic functions of the patient. The extent and frequency of the reduction varies with the severity of the case, so that no hard and fast rules should be laid down, but generally one half the usual diet for one day a week is sufficient. In some cases this means that the patient must fast, in others he is limited to low-value green vegetables with two or three eggs, while in others a fair amount of carbohydrate can be taken, provided that the protein and fat are kept low. When carbohydrate tolerance is very defective it is necessary to prescribe one or two vegetable days each week in addition. As the patient improves, more carbohydrate is gradually added to the diet, and the stringency of the fast or semi-fast days is reduced, but progress should be very slow, with sufficient interval between each advance to test thoroughly his powers of dealing with the larger ration. No addition should be made to the protein until all traces of acetone have been absent for some weeks, and there is no evidence of abnormal tissue waste, even then it should be increased very slowly, guided by the nitrogen output in the urine. Most diabetic foods, if not fraudulent as regards their carbohydrate content, consist chiefly of protein, and must

therefore be allowed for in calculating the protein allowance.

Management of the diet is at present our only reliable means for combating diabetes, but it is probable that other methods will be discovered. The recent work of Murlin, Kramer, and others opens a new field in this connexion. Experiment has shown that the mechanism regulating the level of the blood sugar is peculiarly sensitive to disturbances in the acid-base equilibrium of the body, acid production leading to hyperglycaemia, while alkalosis exerts an influence in the opposite direction. Applying this idea, Murlin and Kramer, and more recently Underhill, have reported considerable improvement in cases of glycosuria subjected to careful alkaline therapy. The former used normal sodium carbonate given intraduodenally in 1 per cent. solution. Underhill employed large doses of sodium bicarbonate given by the mouth. Since meats and cereals are in general acid-producing foodstuffs, while vegetables and fruits usually yield an excess of bases, a diet rich in the former will tend to raise the sugar content of the blood, whereas a vegetable diet will lower it. May this not be one of the reasons why the largely vegetable diet given after the fast in the alimentary rest treatment is associated with an increase in carbohydrate tolerance, and partly explain the deleterious effect of more protein than is needed to make good the actual wear and tear of the body?

THE VARIATIONS IN WEIGHT OF THE TUBERCULOUS UNDER SANATORIUM TREATMENT.

By J. HENDERSON SMITH, M.B., CH.B.

THROUGH the courtesy of Dr. S. Vere Pearson I recently had the opportunity of examining the record of weights kept at Mundesley Sanatorium, and an analysis of the six years 1909-14 has given the results here set forth.

The patients at Mundesley are all tuberculous. A few cases are included where the lung symptoms are of subordinate importance, such as laryngitis or peritonitis, but the overwhelming majority are definite pulmonary cases. All stages of the disease are represented and no attempt has been made here to subdivide them into groups. Patients are weighed at regular weekly intervals on getting out of bed, and care is taken that the weight of the clothes does not vary. The weighing is done to a quarter of a pound, the machines are inspected and tested regularly, and there is no reason to doubt that the record is substantially accurate.

In such a place as a sanatorium, where full nourishment is insisted on and where so many of the patients arrive far under their proper weight, we should expect the weekly observations to show on the whole an increase in weight. And this is in fact the case. Each week no doubt some of the patients may lose in weight or remain stationary, but taking all the patients together we find that usually the gains exceed the losses, and there is on the average a net increase per patient per week. In 285 out of 312 weeks there was a gain in weight and in only 27 a loss, the weekly average varying from a gain of over 1.4 lb. per patient to a loss of over 0.4 lb. (See Table I,

TABLE I.—Showing the Average Weekly Gain or Loss per Patient in 312 Consecutive Weeks.

Net Loss.			
In 2 weeks	"	more than .4, not more than .5 lb.	
3	"	.3	.4 lb.
7	"	.2	.3 lb.
5	"	.1	.2 lb.
10	"	0	.1 lb.
Net Gain.			
In 22 weeks	...	more than 0, not more than .1 lb.	
20	"	.1	.2 lb.
26	"	.2	.3 lb.
34	"	.3	.4 lb.
28	"	.4	.5 lb.
34	"	.5	.6 lb.
35	"	.6	.7 lb.
32	"	.7	.8 lb.
17	"	.8	.9 lb.
12	"	.9	1.0 lb.
13	"	1.0	1.1 lb.
4	"	1.1	1.2 lb.
5	"	1.2	1.3 lb.
2	"	1.3	1.4 lb.
1	"	1.4	1.5 lb.

where a summary is given of the average net results during the six years under examination.) The range of variation is considerable, and it is remarkable how some weeks are good weighing weeks and others bad. In some, all, or nearly all, the patients gain weight; in others, no one gains much, and the majority lose. This is sometimes very striking indeed. For example, in the Mundesley figures in July to September, 1909, in the middle of a run of low weeks in which over half the patients failed to gain there came a week in which 96 per cent. did not lose, and there was an average net gain of over 1 lb. per patient. Again, in October of the same year a bad week with an average loss of 0.37 lb. is interposed between weeks with a gain of over 0.7 lb. Variations of this kind where there is a sudden change in the character of the week affecting most of the patients simultaneously may be simply chance happenings, but they at least suggest that some outside factor is affecting the results. The most obvious factor of the kind is meteorological influence, and later on the question is discussed whether there is any evidence for such a supposition.

The frequency with which the different individual changes occur is given in Table II for over 7,000 consecutive observations, and these plotted at half-pound intervals give a fairly smooth, slightly skew curve. Taken together, there are 4,261 observations of gain in weight with a total of 5,485 lb., 722 without change, and 2,084 observations of loss totalling 1,976½ lb. Over the whole 7,067 observations there is an average net gain of 0.496 lb.—that is, every patient gained on the average about half a pound for each week of his stay in the sanatorium—an excellent result. Here again the individual variations are large—from the three patients who each put on half a stone or more in a single week to the five who lost five or more pounds in the same period. The largest increases are recorded during the first two or three weeks of the patient's stay, as one might expect from the often emaciated condition of new arrivals. The average increase of all patients during their first week's stay was 1.40 lb., during the first four weeks 1.09 lb. per week, during the fifth week 0.75 lb., and during the ninth to the twelfth weeks 0.42 lb. per week.

TABLE II.—Showing the Frequency of the Various Average Weekly Gains and Losses.

A loss of ½ lb.		No. of times a loss occurred.	A gain of ½ lb.		No. of times a gain occurred.
5½	...	1	0½	...	625
5¼	...	1	0¼	...	577
5½	...	1	0½	...	562
5¼	...	2	1	...	536
5½	...	2	1½	...	410
5¼	...	2	1¼	...	338
5½	...	3	1½	...	268
5¼	...	1	2	...	235
5½	...	11	2½	...	156
5¼	...	9	2¼	...	162
5½	...	25	3	...	89
5¼	...	20	3½	...	78
5½	...	41	3¼	...	59
5¼	...	46	3½	...	41
5½	...	81	4	...	29
5¼	...	74	4½	...	27
5½	...	121	4¼	...	19
5¼	...	160	4½	...	13
5½	...	286	5	...	11
5¼	...	308	5½	...	13
5½	...	383	5¼	...	3
5¼	...	505	5½	...	2
5½	...		6	...	1
5¼	...		6½	...	1
5½	...		6¼	...	2
5¼	...		6½	...	0
5½	...		7	...	1
5¼	...		7½	...	1
5½	...		7¼	...	1
5¼	...		7½	...	1
No change occurred 722 times			7,067		

When the observations of the six years are arranged under the months in which they occur, it becomes evident that there is a well-marked seasonal variation in the weight increase. There are six months in which the increase is greater than the general average of 0.49 lb., and these six months run consecutively from May to October (see Table III). Plotted as a graph the figures give a curve rising to a maximum of 0.62 lb. in summer and then falling fairly regularly to a minimum of 0.31 in February. But there is a possible fallacy which we must consider before we can accept this as a correct representation of the facts. We have seen that during the early weeks of a patient's stay he puts on weight more rapidly than in the later weeks. If in the summer months the number of patients coming

TABLE III.—Showing the Average Weekly Gain per Patient for each Month (A) when all Observations are included; (B) after deducting the "Early Observations" of the First Four Weeks of Stay; (C) after bringing the Early Observations to a Constant Proportion.

	A.	B.	C.
January ...	.44	.28	.45
February ...	.31	.16	.36
March ...	.35	.12	.30
April ...	.37	.24	.42
May ...	.58	.29	.46
June ...	.62	.34	.51
July ...	.54	.33	.50
August ...	.53	.35	.51
September ...	.55	.42	.58
October ...	.49	.30	.47
November ...	.46	.22	.41
December ...	.44	.30	.47

to the sanatorium is larger in proportion to the total number of patients than it is in the winter months, the average gain will be greater, and the rise in the curve may be due solely to this cause, and not to any real increase in the weights. And it is the case that there are more arrivals in the summer than in the winter. If we take the first four weeks of stay as "early weeks," there are during May to October more than twice as many observations belonging to the early weeks, as in the November to April period, and the early observations in the summer period form 25 per cent. of all the observations in the period as against 16 per cent. in the winter period. More new patients arrived at the sanatorium in each of the months May, June, July and August than in any other month of the year. This, however, is not the true explanation of the rise in summer. If we deduct from the total observations during each month all the "early" observations, and plot the result, the curve still shows the marked rise of summer; and if we bring the early observations to a constant proportion—namely, 20 per cent. in each month—we get a more accurate representation of the actual weight variations than is obtained from the uncorrected figures. This is shown in Curve 1. The

CURVE 1.—The average weekly increase in weight per patient during each month of the year.

graph rises steadily from March to reach a maximum in September, and then falls again to reach its lowest points in February and March. There is a high level of weight increase all through the summer, culminating in the early autumn, and a lower level throughout all the months of winter and early spring.

So marked a seasonal variation suggests climatic or meteorological influence, and it became a matter of interest to see whether the weights varied either directly or inversely as the elements of the weather, or any one of them in particular. For this purpose the available data for the whole eastern district of England, and also for the single neighbouring district of Cromer, were extracted from the records of the Meteorological Office for these six years; and use was also made of the published figures giving a twenty to thirty-five years' average. A close correspondence was perhaps not to be expected; certainly none was found.

The amount of sunshine in this area is least in December, and increases steadily to reach a maximum in May, falling fairly steadily to December again. The temperature curve rises to a maximum in July and August, and falls smoothly to a minimum in January. The rainfall is least in February and increases steadily, with a break in September, to a maximum in October; January, February, and March being the months with least precipitation. The rainfall for 1909-1914 departs from this, which is the general rule, by rising to a very high maximum in December owing to exceptionally heavy precipitation at the end of the year in four out of the six years. December, 1909, and December, 1910, had both exceptionally high rainfalls, over 3.75 in., with the very small weight

averages of 0.18 and 0.0 lb., and December, 1914, with 5.75 in., and August, 1912, with over 7 in. the unusually high weights of 0.60 and 0.69 lb. The relative humidity curve is very different from the weight curve, and the absolute water-content of the air shows a curve which agrees closely with the curve of temperature. No special influence of the south-west or other wind or of any other element could be discovered.

There is, then, no close agreement with any of the meteorological curves, whether these be plotted from a twenty to thirty-five years' average or only for the six years 1909-1914, and whether the figures be taken for the whole eastern district of England or for the neighbouring district of Cromer only. There is undeniably a general agreement. The six months from May to October, which are the months of greatest weight, are also the months of greatest water-content, most sunshine and highest temperature, as well as of greatest rainfall on the thirty-five years' average. But one cannot demonstrate a closer relationship than this, and what there is can scarcely be held to establish a direct causal connexion.

It still seemed possible that the sudden weekly variations to which reference was made above might be traced to weather conditions, and there is some theoretical ground for imagining that this might be the case. Exhaled air is approximately saturated with water vapour at the body temperature—that is, it holds about 19 grains of water per 1 cubic foot. At rest a man may be taken as breathing 1 cubic foot every four minutes, or over 6,500 grains in twenty-four hours, and during periods of exercise the rate may increase to as much as 1 cubic foot every thirty to sixty seconds.¹ Under ordinary conditions external air does not contain nearly so much water as 19 grains, and the difference is supplied from the body. Thus at 60° F. the air, even if saturated, contains only about 5.7 grains per cubic foot, and the body must find 13 grains every four minutes, or over 4,500 grains of water every twenty-four hours. The less the air approaches saturation the larger the quantity of water exhaled that has to be found—for example, with a temperature of 60° F. dry and 55° F. wet bulb the body must supply about 15 grains to each cubic foot, and over 5,000 grains every twenty-four hours. With an external temperature of 37° F. and a saturation of 90 per cent., such as is not uncommon in January, the air contains only about 2.25 grains per cubic foot, and 6,000 grains (0.85 lb.) must be added to saturate at body temperature the air exhaled in twenty-four hours.

No doubt the loss of water by exhalation is ordinarily compensated for by diminished loss through skin or kidneys, and by increased intake of fluid; but the patients are weighed on getting out of bed, and we might expect a very dry night, or succession of nights, to be followed by an appreciable reduction in weight. The sudden changes in the weight average might be due to a marked change in the water-content of the external atmosphere. There does not, however, appear to be any support for this in the actual facts. In the six years there were eleven weeks of exceptional gain in weight, averaging over 1 lb. per patient, and thirteen weeks of exceptional loss, averaging over 0.25 lb. per patient. Yet the average water-content of the air in the losing weeks was 4 grains per cubic foot, as compared with 2.65 grains in the weeks of gain, and in no week of pronounced gain in weight was the water-content so high as the average for the losing weeks. The average rainfall was practically the same for the weeks of gain as for the weeks of loss. The ten weeks of highest rainfall, averaging 1.83 in., and the sixteen weeks of lowest rainfall, averaging 0.005 in., show the same average weight increase, and in dry weeks with 0.01 in. rain or less the weight average varied from 0.10 to 1.07 lb. gain. The hours of sunshine were practically the same for the weeks of gain as for the weeks of loss; while the air temperature and the accumulated temperature were higher in the weeks of loss than in the weeks of gain.

There is, then, no obvious connexion between the weather and the weekly variations in weight, and it is probable that the sudden interposition of good or bad weeks is merely a chance happening. We have seen that out of 7,067 observations there were 2,084 of loss and 4,983 of gain or no change—that is, the losses and not-losses were approximately as 2 is to 5. With such a proportion as the general rule the probabilities against obtaining, say, 29 not-losses in a week of 30 observations are only about 2,000 to 1, and in repeated samples of 30 the chances against are

correspondingly reduced. In the Mundesley figures the chances against obtaining the actually observed results are not greater in any instance than 150 to 1, not enough to make a dominant interfering factor a necessary hypothesis.

In the literature I am acquainted with only one paper, a very interesting work by Strandgaard,² which deals with weight variations in sanatoriums in a manner similar to the foregoing, and his results show a striking agreement with the Mundesley results. Strandgaard, who writes from Boserup sanatorium in Denmark, has collected the figures of eight Danish sanatoriums over a period of several years, and this has given him a material of about 800 observations a week—enormously larger of course than the Mundesley data. Plotting the monthly averages he obtains a curve very similar in its main features to the Mundesley curve. From January to May the weight gain remains fairly constant about a minimum of 230 gr., then the curve rises steadily to reach in September a maximum of 380 gr. (0.8 lb.), falls again in October and November and reaches the minimum once more in December. The spring and winter months give the lowest values, the summer and autumn the highest, and Strandgaard gives reasons for thinking that in this the weight curve corresponds with the general well-being of the patients, and that the tuberculous in Denmark do better in summer, and especially in autumn, than in spring or winter.

Strandgaard considers that the three main factors determining the weight curve are the relative humidity of the atmosphere, the average temperature and the amount of sunshine. He supports this conclusion by a certain agreement between the curves of weight and those of rainfall, relative humidity, sunshine and temperature, as well as by various minor arguments. We have seen that the English figures scarcely admit of so definite a conclusion, and we may remark that the weight curve can hardly be expected to follow the curve of relative humidity, as Strandgaard suggests. The amount of water supplied from the body varies not with the relative humidity but with the absolute water-content, and the seasonal curves of relative humidity and weight in the English figures are as a matter of fact wholly divergent.

There seems no reason to suppose that the six years selected for examination were exceptional years, or to doubt that the summer-autumn increase in weight is the general rule at Mundesley. The remarkable agreement with the Danish figures, moreover, suggests that this would be found to hold good at other sanatoriums in England, and probably in other countries—at least, of Western Europe; and it would be of interest to learn whether similar results were met with in the high districts of Switzerland, in the numerous institutions in America, in South Africa, and in other parts of the world. The results would be of value, whether they agreed or disagreed with our finding, and might have some bearing on the treatment of consumptives and the regions to which they may best be sent at different seasons of the year. It is not probable, of course, that this seasonal variation is peculiar to the tuberculous. It is much more likely that there is a general increase in the weight of the population in summer and early autumn, though I have not met with any satisfactory data bearing on the point. And it is a large assumption that even in the tuberculous the season of greatest weight is the season when they thrive best. Strandgaard believes that it is so, but his evidence is scarcely conclusive. It is difficult in so variable a disease as pulmonary tuberculosis to obtain clear and objective data of improvement and recovery, and the Mundesley figures are not large enough to allow of definite conclusions. But the fact remains that increase in weight is undoubtedly a sign of improvement in the tuberculous, and, unless there is evidence to the contrary, the presumption is that the season of greatest weight is the season of their best improvement in health.

One other fact deserves a brief mention. If we arrange the data in periods of four weeks instead of by months, so that we have thirteen periods in the year instead of twelve, a marked break in the curve of weights appears, which is obscured in the monthly arrangement. As a whole, the curve remains much the same, but for the four weeks covering the last fortnight in July and the first fortnight of August the weight average drops sharply, and this is true for five out of the six years. A similar drop at this time apparently occurs also in Strandgaard's figures,

He plots nine curves at weekly intervals and gives detailed figures for three more, also at weekly intervals, and, taking the twelve together, there is evident a marked fall in the average values, a break in the curve, in the July-August period. So far as published meteorological data enable one to judge, this is not due to change in the weather conditions. It may be noted that in the curve published by Kroon,³ showing the seasonal variation in the frequency of human conception, as calculated from the birth curve, there is a similar break in August.

SUMMARY.

In tuberculous patients undergoing sanatorium treatment there is on the average a steady increase in weight. This increase shows a marked seasonal variation, being greatest in the six months from May to October with the maximum in September, and smallest in the six months November to April with the minimum in March.

The six months of greatest weight are also the months of most sunshine, highest temperature, largest rainfall, and greatest atmospheric water-content. This agreement suggests that the weight variation is due to meteorological conditions, but, if it be so, the climatic influence can only be indirect, and no single factor has predominant effect. Sudden changes from week to week occur in the average weight, and there is nothing in the available records to show that these changes are due to weather conditions.

I have pleasure in expressing my thanks to Dr. S. Vere Pearson for permission to use the Mundesley records, and also to Sir Napier Shaw, of the Meteorological Office, for the loan of records and other kind assistance.

REFERENCES.

- ¹ See, for example, BRITISH MEDICAL JOURNAL, 1916, April 15th, p. 541. ² Strandgaard, *Beitr. zur Klin. d. Tuberk.*, 1914, Bd. 32, H. 2, p. 179. ³ Kroon, *Nederland. Tijdschr. v. Geneesk.*, 1914, December 19th, p. 2010.

A METHOD OF ANAESTHETIZING SOLDIERS.

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I HAVE long noticed that when coughing occurred during the early stages of etherization a change for a few minutes to chloroform stopped the cough and much lessened mucous secretion. The small quantity of chloroform inhaled exercised apparently a prolonged mitigating effect on the irritating quality of the ether. It seemed to me, therefore, that if the short interval of chloroformization had such a prolonged effect it might be worth while to try the addition of a small quantity of chloroform to the ether in Clover's inhaler, with the object not only of lessening irritation but also of obtaining an anaesthesia more nearly approaching the ideal—that is, a sleep-like one. Since chloroform acts as a depressant, but ether as a stimulant, it should be possible to obtain by a mixed vapour of these two drugs an anaesthesia in which, while there is a proper muscular relaxation, the respiratory and circulatory functions are neither depressed nor stimulated but approximate to their condition during sleep. Such a mixture would probably differ in its proportion according as to whether the open or closed method were used, and also according to the type of patient, if the best results were to be obtained. Perhaps the best all-round mixture would be one in which the respiration and circulation are very slightly stimulated by the addition of a little more ether, thus ensuring safety and counteracting any shock arising from the operation. The idea is to take ether as the standard of safety and so to dilute or modify its action as to obtain some of the advantages of chloroform anaesthesia. Ether thus modified by chloroform, as suggested later, possesses practically the safety of pure ether. When we use such mixtures as E_2C_1 , E_3C_1 , we regard chloroform as the main drug and modify its action by the addition of ether. It has been shown of late years that the mixing of chloroform with ether enhances the anaesthetic power of each drug; indeed it has been asserted that a mixture of six or seven parts of ether with one part of chloroform increases the potency fully 30 per cent. without increasing the toxicity.

Having at the 1st Southern General Hospital, Edgbaston, the opportunity of anaesthetizing soldiers—that is, patients of much the same age and type, and trained, fed, and living under the same conditions—I tried the effect of different mixtures of chloroform and ether administered by the closed method from a Hewitt's wide-bore modification of Clover's inhaler. To soldiers a routine method seems particularly applicable. They are mostly strong and fit men, well fed, much accustomed to tobacco and consequently with irritable throats; nearly all have coughs due to exposure to wet and cold indoors and out; their nervous system is on a much higher plane of tension than that of the normal individual during ordinary times, so that altogether as a type they are much more difficult patients in whom to induce anaesthesia than are the general run of those met with in civil practice. On the other hand, when once induced, anaesthesia is maintained in them more safely and easily than in the ordinary non-military patient, because, generally speaking, of the healthy conditions in which the former have been living. In fact, soldiers more nearly approach the normal healthy human being than does any other class of patients.

At the end of October, 1915, I began to use various mixtures of chloroform and ether in Hewitt's wide-bore Clover's inhaler, beginning with mixtures of E_4C_1 , E_7C_1 , E_8C_1 , $E_{16}C_1$, $E_{18}C_1$, $E_{20}C_1$, $E_{32}C_1$, by volume.

In the first three of these mixtures the chloroform factor predominated too much for safety, causing blueness and too quiet respiration, whilst in the last one the ether component predominated too much, and the effect of the chloroform was practically negligible. Finally, in $E_{16}C_1$ I found a mixture which suited admirably my purpose, and have used it in more than 1,200 cases. Since the above-mentioned date it has been my routine method for the induction and often for the maintenance of anaesthesia. The $E_{18}C_1$ and $E_{20}C_1$ mixtures I found very useful for debilitated patients.

In 1916 I administered $E_{16}C_1$ to 732 out of 843 cases at the 1st Southern General Hospital—that is, to practically 7 out of 8 patients.

I look upon it as and call it "mitigated ether," and administer it exactly as I should ether by the closed method, the only difference being that during induction I very often allow one or two inspirations of air.

The respiration and circulation are slightly stimulated, the patient is of a good colour, respiration being about half as deep as that during pure etherization. There is much less irritation of the respiratory mucous membranes than with ether alone, but if much coughing ensues during induction, as so often happens in soldiers, I immediately change down to a mixture of chloroform and ether, or more commonly to chloroform alone given by the open method. In fact, in the last few months, in nearly all operations which last longer than a few minutes, save rectal ones, after induction of anaesthesia has been safely brought about and some degree of stimulation of the vital functions established by $E_{16}C_1$, I change down to chloroform and find it best suited to maintain anaesthesia.

The contraindications to the addition of the small amount of chloroform to the ether are cases in which the fullest stimulation by ether is needed, as in patients in whom there is great shock, loss of blood, or toxæmia, or where rapid dilatation of a sphincter or stricture is necessary.

As compared with pure ether, administration of $E_{16}C_1$ by Clover's inhaler causes less irritation, less muscular spasm, less mucous secretion, less excitation, and less obnoxious smell. The induction of anaesthesia is quicker and quieter and muscular relaxation appears more quickly and is more complete. The addition of this small amount of chloroform seems to make a very great and advantageous difference. The $E_{16}C_1$ may easily be given without any preliminary administration of nitrous oxide or ethyl chloride.

All soldiers have a preliminary intramuscular injection of morphine one-sixth of a grain, and atropine one-hundredth of a grain, as nearly as possible half an hour before the time of operation, so that the maximum effect of these drugs is obtained. This injection is essential for quiet induction and stoppage of secretion. The effect of the morphine helps greatly to quieten induction and to maintain and steady the anaesthesia. It also enables a lighter form of anaesthesia to be used than is possible without it. The disadvantage of the morphine is that