After careful adjustment of the telephonic lever and the signal lever of the auscultator, a tracing was taken with all the precautions previously described (the eyes of the auscultator being closed). The result (see fig. 5) was again a surprise, and an interesting physiological observation—which, so far as we know, has not been previously made—for it showed that, under the conditions of the experiment, there is absolutely no “psychical loss.”² The absence of any “psychical loss” under such conditions is a most important practical fact for the purposes of our present research; for it shows that in recording, by means of the apparatus which we have described, the sounds of a rhythmically contracting and slowly acting heart, the personal equation (as regards “psychical loss,” but not, of course, as regards the auscultator’s capability of perceiving cardiac sounds or murmurs) may be left out of account. Within what limits, as regards frequency of action (in other words, frequency of sound), this statement applies we do not at present know; but we hope in the course of a short time to be able to determine.

There are also several other interesting questions connected with this result (not, however, bearing directly upon our present research) which suggest themselves for solution, and which, if time permits, we hope to investigate.

The fact that there is no “psychical loss,” under the conditions we have mentioned, confirms in the most striking and absolute manner the reliability of the previous experiment, which had located the occurrence of the second sound just at the end of the ventricular systole. In fact, the two experiments are mutually confirmatory the one of the other; for the fact that the second sound does occur immediately, or almost immediately, after the ventricular contraction ceases, is, of course, one of the absolutely certain and fixed events in cardiac physiology.

These experiments seem to show that the method is a practical and satisfactory method, and well fitted to serve the purposes for which it is intended.

The explanation of the absence of any “psychical loss” in this experiment is not far to seek. It is obvious that the discharge of the motor centre concerned in the movement of the forefinger, which depresses the signal key, becomes rhythmical; and this rhythmical discharge of the “signal centre,” as it may be termed, takes place synchronously with, or even rather tends, as some of our observations show (unless the attention of the auscultator be very closely concentrated on the sound) to anticipate the recurring sensory sensation (telephonic sound), with which it corresponds in time. It is quite obvious, therefore, that any given sensory impression or telephonic sound cannot regulate the motor discharge, with which it corresponds in time (and on the drum); but that the preceding sensory impression or telephonic sound guides the succeeding motor discharge; or, at all events, that the rhythmical recurrence of the motor discharges is regulated and guided by the rhythmically recurring sensory impressions which precede them.

Such a view explains the fact that the signal marking the occurrence of the second aortic sound, in the case of W. K. (the points at which the letter C. is placed) is not absolutely the same in each succeeding cardiac cycle. It will be seen that in some cycles it slightly precedes, while in others it slightly follows, the termination of the ventricular systole, as indicated by the cardiographic lever. The reason is, that (in consequence, no doubt, of the influence of respiration), the heart does not beat with the regularity of clockwork; that in a normal and apparently perfectly regularly beating heart, some cardiac cycles are longer, while some are shorter than others. We may, therefore, expect that if a short cardiac cycle, or a short ventricular systole, follows a long cycle, that the signal representing the second sound, since its time is regulated not by the second sound, with which it corresponds in time and on the drum, but by the preceding, or several preceding, second sounds (regulating the rhythmical contraction of what we may term the “signal centre”), will slightly succeed; while, in a long cardiac cycle, it will slightly precede the point in the cardiographic tracing at which it ought actually to be placed. Whether this is so or not we have not yet determined by actual exact measurement.

It will nevertheless be noted, that although successive cardiac cycles vary considerably in their duration, the relative position of the second sound, as signalled by the auscultator, to the termination of the ventricular systole, as recorded by the cardiograph, is practically maintained throughout the whole four tracings; in other words, the

slight error in one cycle is not magnified in the next cycle, and that the relative position of these two points (second sound as signalled, and termination of systole as traced) is practically the same, both in the first and in the last cycles of the series; an argument, we think, in favour of the view that the preceding sensory impression (sound) regulates the occurrence of the succeeding motor discharge (signal).

The practical outcome of this, we fear, somewhat long explanation is, that the exact position—point of commencement—of a cardiac sound or murmur, even granting that it is signalled with absolute precision, cannot by this method be absolutely and accurately determined from the observation of any single cardiac cycle alone; but that, by taking the average, so to speak, of a sufficiently long series of cycles, such location can be determined with very great exactitude, always, of course, granting that the auscultator signals correctly the exact commencement of the sound or murmur. Further, the error which, under such circumstances occurs in any individual cycle, in consequence of the diminished or increased length of that cycle as compared with the previous cycle, is so slight, that for practical purposes, for determining, whether a murmur is systolic, diastolic, or presystolic, it may be left out of account.

It must be remembered that these statements only apply, so far as our present observations enable us to judge, to a regular and rhythmically acting heart. To speak with absolute accuracy, we have only proved that they hold good in timing a sound produced with exact regularity every second; but no one, we presume, will for a single moment be inclined to doubt that this limit, sixty times a minute, may be considerably extended in each direction. By future observation we hope to determine this point.

Further, we hope that by this method we may be able to demonstrate the exact rhythm of the murmur of mitral stenosis, presystolic murmur; and perhaps, after we have perfected our cardiographic apparatus and succeeded in recording on the drum a more ample and satisfactory tracing of the impulse of the heart, that we may be able to indicate the exact points, in the up and down strokes of the tracings, which mark the occurrence of the first and second sounds of the heart.

The apparatus seems also fitted to record other clinical phenomena, as, for example, the exact duration of the delayed sensation which occurs in some cases of nerve disease.

In conclusion, it is perhaps hardly necessary to state that this method of graphically recording the exact time relations of cardiac sounds and murmurs is not intended, nor indeed fitted, for ordinary every day clinical use. The apparatus is far too complicated and delicate for that. But, indeed, the ordinary method of auscultation, aided, if necessary, by the administration of digitalis or strophanthus, with the object of slowing, steadying, and strengthening the cardiac contractions, and so, of making the intensity and rhythm of the murmur more distinct, is, for practical purposes, all that the experienced observer requires. We hope, however, that the method which we have described may enable us to decide some of the questions which are at present in dispute.

Note.—Since this paper was written, Mr. Edison has kindly promised to direct his attention to this subject.

AN ADDRESS

ON

THE THERAPEUTICS OF THE URIC ACID DIATHESIS.

Delivered at the Opening of a Discussion on the Subject in the Section of Pharmacology and Therapeutics at the Annual Meeting of the British Medical Association, held in Dublin, August, 1887.

By I. BURNEY YEO, M.D., F.R.C.P.,

Professor of Clinical Therapeutics in King's College, London; and Physician to King's College Hospital.

I HAVE, in the first place, Mr. President, to thank you for the compliment you have paid me in inviting me to open this discussion.

The therapeutics of the uric acid diathesis is a very wide subject, and one surrounded by conflicting theories and divergent opinions; but it is within our power to limit this discussion to the consideration of certain definite questions of practical interest, in the examination of which we may hope to elicit, from the ripe experience and trained observation of the many distinguished physicians who are present here to-day, facts and suggestions of much value.

² This fact was demonstrated by the authors of the paper to the members of the Edinburgh Pathological Club, on November 30th. Although the demonstration was made at 12 P.M., when the auscultator was exhausted by a hard day's work, and at the end of a meeting of four hours' duration, in which a good deal of tobacco was going the whole time, the result was perfectly satisfactory, the “psychical loss,” under the same conditions, being again nil.

And, in the first place, I would respectfully suggest that we should confine ourselves strictly and closely to the practical aspects of the subject before us. Our constant aim and effort should be to establish our therapeutic processes on sound and rational physiological principles; but, on the other hand, we should also bear in mind that therapeutic investigations are a kind of experimental pathology, and that when pursued in a scientific and discriminating spirit they often illuminate, with a reflected light it may be, many difficult and obscure pathological questions: so that while rational therapeutics must rest on sound pathology, on the other hand, sound pathology must be consistent with established therapeutic facts.

In this discussion we approach the subject of the "uric acid diathesis" from the point of view of practical observation of the human subject, and from that point of view, if this discussion is to be profitable, we, I venture to suggest, must not wander.

The subject of our discussion is the most appropriate treatment of that constitutional state, and the various maladies associated therewith, which depend on the tendency to the accumulation of uric acid, in excess, in the human organism.

It is necessary that I should, in the first place, summarise, and I shall do so with the greatest possible brevity, the chief views which prevail as to the mode of origin of this excess of uric acid in the blood.

First, there is the hypothesis advanced by Murchison, and supported by Charcot and many other observers, that the liver is the organ chiefly at fault in this excessive production of uric acid. It is maintained that, owing to disturbances in the function of this gland, an excessive quantity of uric acid is formed in it, which passes into the blood with the reabsorbed bile.

Dr. Latham also, if I understand him aright, refers the formation of excess of uric acid primarily and chiefly to some fault in the gland cells of the liver. There occurs, he says, in the liver a disturbed or interrupted metabolism of glycocin, which is brought to that organ by the portal vein as one of the results of intestinal digestion. This disturbed hepatic metabolism leads to the formation of hydantoin in the blood, and this in the kidney is converted into ammonium urate; a portion of this fails to be excreted, and "overflows" into the general circulation, where, meeting with the soda in the blood, it is converted into sodium urate. The formation of uric acid, according to Dr. Latham, "is the result primarily of the non-transformation or metabolism of glycocin into urea, whether that glycocin be derived from the bile poured out into the duodenum or found elsewhere in the body."

This view is perhaps not altogether inconsistent with the one which Garrod appears to regard with a certain amount of favour, although he seems careful not to adopt it too absolutely—namely, "that the kidney is the actual producer of uric acid, and its presence in the blood and tissues is explained by resorption from the renal cells—a process which is scarcely appreciable in health, but becomes more and more marked in proportion to the difficulty which the uric acid has in finding its way to the uriniferous tubes." In this view the fault appears to be on the side of the renal cells, rather than in the liver.

Ebstein maintains that, in persons who suffer from uratic arthritis, uric acid is formed in the muscles and in the marrow of the bones; and that its formation in the muscles is due to pathological causes; that it is a disturbance of nutritive evolution. He also asserts that it exists in the fluids of the body in the form of neutral compounds; but when it is deposited in the crystalline form it is as acid compounds, and chiefly as the acid urate of soda. In order that this may happen, a free acid is necessary; and this crystalline deposition is exclusively dependent on local conditions. It occurs in those localities where a free acid is encountered. This free acid is only found in necrosed parts. The formation of a free acid is an effect of necrosis, and it is only in these necrosed parts that a deposition of crystalline urates occurs. The necrosis is caused by the irritant action of acid urates in solution. He further maintains, in opposition to Garrod, that it is by no means necessary that uric acid should be deposited in the tissues in the form of crystalline urates in order to set up gouty inflammations; that such inflammations can be excited, and are constantly excited, without any crystalline deposits, and by the presence of uric acid compounds in solution; and with this latter view I entirely concur.

The view maintained by Frerichs and Wohler that the formation of uric acid is an intermediate step in the transformation of albuminoid substances into urea, and that its accumulation in the blood is an indication of arrested oxidation in the progress of urea formations, has been hotly contested.

Bencke, Voit and Lecorché assert, on the contrary, that the formation of urea and uric acid occurs in the organism quite independently of one another.

Bartels supports the theory of incomplete oxidation as explaining the origin of uric acid, and considers it to be formed in connective tissue and cartilage, because in them the nutritive processes are slow and languid.

I must not omit to allude to the opinion of Professor Bouchard, of Paris. He contends, and I think not without some reason, that "it has by no means been demonstrated that in gout uric acid is the only or even the chief matter contaminating the fluids." He says very pertinently that "we talk much of uric acid in gout, because we can see it without looking for it," but that "we do not talk of what we do not see, and above all of what we do not look for." He admits, of course, that some of the incidents and manifestations of the malady depends on uratic deposits, but that it is not itself a "uricæmia." He maintains that in gout there is "an exaggerated formation or a too slow destruction of the organic acids." This leads to an acid dyscrasia, one of the consequences of which may be the deposition of acid urates in certain tissues. He distinctly rejects as unconvincing the view which throws all the fault on a "torpid liver."

I am glad it is not my business to-day to follow these various observers through the intricate mazes of their pathological chemistry, or to dispute with you the various conflicting doctrines they advance, all proved to their own satisfaction by careful experimental observations and the clearest possible chemical equations!

I think, however, we may conclude as a basis for our therapeutic discussion, that there is an agreement to this extent amongst all observers—viz., that the "uric acid diathesis" is associated with imperfect nutritive metabolism and imperfect excretion of the results of retrograde metamorphosis, especially of albuminous substances, and I am disposed to claim as a good working definition of gout one I have given elsewhere—viz., that it is mainly a "disturbed retrograde metamorphosis."

An appeal to practical therapeutic results may, after all, be the best test of the probability of any theory put forward in explanation of the formation and pathological significance of uric acid.

The uric acid diathesis is admittedly the parent of a great number of morbid phenomena for the relief of which numerous remedies have been proposed.

PRINCIPAL MORBID CONDITIONS DEPENDENT ON OR ASSOCIATED WITH THE URIC ACID DIATHESIS.

Arthritic Affections:

Acute uratic arthritis (acute gout).	{	Anchyloses
Chronic " "		Deformities
		Uraté deposits (tophi).

Gastro-Intestinal Affections:

Dysphagia from œsophageal spasm	{	Cardialgia, or
Dyspepsias		Gastrodynia (heartburn)
		Pyrosis
		Enterodynia.

Circulatory Affections:

- Palpitation of the heart and large vessels
- Irregular cardiac action
- Angina
- Chronic arteritis (aneurysm)
- Phlebitis (embolism).

Respiratory Affections:

- Laryngo-pharyngeal catarrh
- Bronchitis -
- Asthma
- Pleuritis (dry).

Renal and Urinary Affections:

- Chronic interstitial nephritis
- "Primary renal gout" (uratic deposits in kidneys, and absence of arthritis)
- Uraté gravel and calculi (renal and vesical), oxalate of lime
- Cystitis and irritable bladder
- Prostatitis.

Nerve Affections:

Neuralgias	{	Of various nerves
		Lumbago
		Sciatica
Headaches (hemicrania)		
Cramps of muscles		
Hysteria		
Hypochondriasis		
(Epilepsy)?		

Organs of Sense:

Ophthalmia
Conjunctivitis ("hot eye")
Iritis, etc.
Otitis (deafness).

Cutaneous Affections:

Eczema
Psoriasis
Prurigo (anal and vulvar)
Acne.

PRINCIPAL PROPOSED REMEDIES FOR AFFECTIONS CONNECTED
WITH THE URIC ACID DIATHESIS.

*Diet and Regimen:**Drugs.*

Colchicum
Salicylates
Benzoates
Guaiacum
Iodide of potassium.
Arsenic
Sulphur.

Alkalies and Alkaline Earths:

Salts of potassium, sodium, lithium, ammonium, calcium,
and magnesium

Various Local Applications to Affected Joints:

<i>Mineral Waters
and Baths</i>	a. Alkaline (bicarb. of soda), for example,
Vichy
	b. Alkaline and aperient (carb. and sulphate of soda), Carlsbad
	c. Chloride of sodium, Wiesbaden, Homburg
	d. Lime (earthy) waters, Contrexéville,
Bath
	e. Indifferent (thermal), Buxton, Gastein
f. Sulphur, Harrogate, Aix.

Water hot (and cold)

The treatment of the diathesis, rather than of its several morbid manifestations, must, in this discussion, chiefly occupy our attention.

In this, as in all other therapeutic questions, I would point out that we should always bear in mind that we have to deal with three elements or factors:—1, the pathogenic element; 2, the constitutional element; and, 3, the remedial element; and I would beg you to notice that the third is the only possible constant element.

The pathogenic factor may be present in all degrees of varying intensity; the constitutional element is, I need not say, extremely variable. The reactions of these two on one another may also vary greatly, leading frequently to the development of morbid conditions as different from one another as, for example, primary uratic nephritis and primary uratic arthritis.

The reactions of the third element, on the other two must also, of necessity, vary enormously.

Now, these considerations, although elementary, are frequently overlooked. Since these are the conditions of nearly every therapeutic problem, why should we look for constant uniformity in results? and why should we be discontented, or sceptical with regard to remedies because they do not always produce the same desired effect?

It is absolutely impossible, from the necessary conditions of the therapeutic problem, that they should do so. For example, in certain constitutions, a peculiar quality of the tissues may cause the uric acid deposit to cling to them with peculiar tenacity, while in others, for a like reason, they may part with it with unusual readiness. This obvious consideration must tend necessarily to disturb all experimental inquiries into the action of the various remedies proposed as eliminators of uric acid.

And now, without further preface, I will pass to the consideration of our remedies.

Diet and Regimen.—It is universally admitted that the treatment of the uric acid diathesis must take largely into account questions of diet and mode of life. Apart from the influence of heredity, which is the chief etiological factor in this diathesis, no other cause is so influential in its production as habitual excesses or errors in eating and drinking; and even when the hereditary tendency to this diathesis is strongly pronounced, very much may be accomplished by careful attention to dietetic rules, so as to keep in abeyance its more serious manifestations. It would, however, be a grave error to conclude that

all the victims of this diathesis merit the reproach of intemperance, or that they should all be submitted to the routine of a rigid abstinence. One of the greatest differences of opinion at present existing with regard to the most appropriate food for the gouty is that which has arisen in connection with Ebstein's views on this subject. He maintains that one of the most compromising conditions that can affect a person predisposed to gout is obesity. He says: "The gouty who have grown old in spite of their disease are almost always those who have been able to keep obesity at arm's length;" and he considers the treatment of obesity an essential element in the treatment of gout. His method consists in allowing sufficient albuminous food for the physiological wants of the body—from seven to nine ounces of roast or boiled meat per diem. But, instead of avoiding fat, he encourages his gouty patients to take it, for he contends that the addition of a suitable amount of fat to the food is the best means of combating obesity. So far, he says, from increasing their fat, under this regimen they become thin, while their physical and intellectual faculties are improved. He explains this by the rapid manner in which fats allay the appetite or craving for food, and so prevents its excessive consumption. It also diminishes thirst and lessens the tendency to drink large quantities of fluid (which in German, means, beer!). Good fresh butter he considers the best form of fat. He allows from two to three and a half ounces a day. He forbids starchy matters, or limits their consumption to two and a half to three and a half ounces a day. He forbids sugar, farinaceous foods, and potatoes. He allows a moderate quantity of fruit and vegetables—peas, lentils, and beans in the form of *purées* (passed through a sieve), spinach, cauliflower, red cabbage, but forbids turnips.

It has been clearly established that in the uric acid diathesis no good results from abstinence from a moderate amount of animal food, and harm has no doubt occasionally been done by too severe a limitation in this respect, in the case of feeble persons. In opposition to this view, many object to a diet in which there is much fat or saccharine and starchy substances, on the grounds that, by affording the system material for combustion, these substances tend to check the metabolism of nitrogenous compounds and prevent their complete elimination. Senator says there should be a minimum of fat. He objects also particularly to yolk of egg, on account of the amount of fat and *lecithin* (an abundant source of phosphoric acid) it contains. Tea and coffee he forbids.

In these matters it has seemed to me that each patient requires a separate study, especially with regard to digestive peculiarities; and that our object should be, in accordance with and in subordination to certain generally admitted truths, to construct a diet which shall be readily digested, and which does not tend to excite acidity and undue fermentation in the alimentary tract; and that diet will differ with different persons. Whatever may have been the case with our predecessors who originated the gouty constitution, I am quite satisfied that in the present day, especially in the neurotic manifestations of this diathesis, we often encounter them in persons who are delicate feeders with small appetites, and who consume a minimum rather than a maximum amount of food.

A most important question in connection with the management of the gouty diathesis is the use of alcoholic drinks; in some persons, and especially in women, I am satisfied it is best to avoid them altogether; in others I have not found their moderate and discriminating use injurious. Of all alcoholic beverages, malt liquors are, I consider, most prejudicial in this diathesis, and bad quality wines. With regard to wines, I have had occasion to form a very decided opinion that it is not the name but the quality of the wine which is important. And no greater error has ever crept into medical practice than that exceedingly common one of advising everybody to drink light claret. A more injurious beverage than bad claret, or imperfectly matured claret—and speaking generally, all but the more expensive or most carefully selected clarets are bad—was never drunk. Cheap clarets consist of admixtures of the worst and most valueless wines that are grown on the surface of the globe. I repeat, it is the quality, not the name or even the quantity, of the wine taken, which is of chief importance to the subjects of this diathesis. Half a glass of bad sherry or claret or other wine will produce a decided disturbance of health, when considerable quantities of fine quality wines will pass through the system without injury.

I have made an observation, of the value of which I am more and more convinced: it is that the wines which act freely as diuretics are the wines which agree best with all persons. In some this will be champagne, in others claret, in others hock, and so on; but they are always, or nearly always, wines of good and often very fine quality. A very eminent physician of New York told me only a few weeks ago that he found champagne and port—carefully selected, I need not say

—the best remedies for his gout. I have certainly known many gouty persons who find champagne the wine that suits them best. For those who find a certain amount of wine a necessity, it is advisable that they should add to the wine they drink a small quantity of an alkaline water.

Some years ago I took to recommending *still* Moselle mixed with Apollinaris water for thirsty persons who required a "big drink." I find from the German wine merchants that these *still* wines of the Moselle have been so largely recommended of late years to gouty persons that their price has advanced more than 25 per cent.

A great deal has been said about the importance of exercise for gouty persons, and no doubt a sufficiency of exercise is important to all persons. But gout is a disease which prevails amongst a class of persons who take a vast amount of physical exercise. Lord Beaconsfield's definition of the aristocracy as "People who live much in the open air and don't read," is, so far as the first point is concerned, a true one, and the comparatively slight incidence of gout in women as compared with men, who lead much more active lives, points in the same direction—viz., that want of exercise is not such an important feature in the etiology of gout as has been imagined.

As to climate, I am convinced that a climate that is warm, dry, and equable is most advantageous to the subjects of this diathesis, and that climatic conditions which interfere with the free action of the skin are most prejudicial. It is a matter of common observation with patients themselves that if the action of the skin gets disturbed by the onset of a cold east wind they soon begin to feel "gouty."

Water.—I am also convinced that no remedy is more valuable and important in the treatment of this diathesis than the regular consumption of a considerable quantity of pure water, and preferably hot water.

As a diluent and solvent of renal excrementitious substances, it is most important, while in its rapid passage through the system, it must tend to dissolve and carry away waste matters from the blood and tissues. When drunk at a higher temperature than that of the blood the effect of this hot water flowing through the hepatic portal circulation must be to stimulate the functions of the liver cells and promote biliary excretion.

(To be continued.)

CLINICAL MEMORANDA.

HORN ON THE HEAD OF A WOMAN.

THERE is (or was) a horn, similar to that described by Dr. V. Bejan as occurring on the head of a Roumanian woman, in the Pathological Museum of St. George's Hospital, which I removed from the face of an old woman in Yorkshire some years ago. I forget its dimensions, but I think it was somewhat smaller than the one referred to in your note, and its shape was twisted in the form of an antelope's horn. It appeared to be developed from a mole, and there were two or three rudimentary horns on other parts of the face of similar origin. The wound healed well, and the patient lived several years after the operation, but I do not know whether the smaller growths increased in size.

C. ROBERTS, F.R.C.S.

Curzon Street, W.

NATAL SORE.

I HAVE recently seen cases of "Natal sore" from Maritzburg, and I have no hesitation in stating that their appearances and histories correspond with the disease known in India as "Delhi boil." The "Natal sore" has been long ago mentioned by African travellers, including Livingstone, as very common amongst the native tribes; and, quite recently, the medical officer of one of the Castle mail packets, trading between Mauritius, Natal, and the Cape, informed me that he is often called on to treat passengers suffering from this troublesome skin disease.

The late Dr. Tilbury Fox proposed that the name "Oriental sore" should be applied to this disease, as it was then thought to be confined to the East; but it is now known to have a very wide distribution. I saw, many years ago, a case from Jamaica.

J. FLEMING, M.D., F.R.C.S.E., Surgeon-Major M.S.

Mauritius.

UNUSUAL CAUSE OF URETHRAL HÆMORRHAGE.

A., aged 34, sent for me complaining of somewhat severe hæmorrhage from the urethra. On examination the entire left half of the scrotum and penis, and the whole of the perineum to within two inches of the anus were seen to present one mass of varicose veins. There was an enormously dilated venous pouch where the vein passed out of sight

near the anus. The condition was congenital, and had remained apparently unchanged ever since he could remember. There was no other vessel lesion to be made out. The hæmorrhage was arrested by passing a catheter into the bladder, and bandaging the penis on to it, the hæmorrhage coming apparently from a ruptured vein about three inches up the penile urethra.

F. R. HUMPHREYS, M.R.C.S.E., L.R.C.P.Lond., etc.
79, Queen's Crescent, Haverstock Hill, N.W.

THERAPEUTIC MEMORANDA.

OLEATE OF ZINC AND IODOFORM IN GYNÆCOLOGY.

I SHOULD be glad to record my experience of the value of iodoform and oleate of zinc when mixed together, either in equal portions or with two parts of iodoform to one of the oleate, according to the effect desired.

The dry treatment in gynæcology having attained such success, I particularly wish to notify the beneficial effect of this powder on ulcerated surfaces of the uterus, or to aid granulation after the application of strong caustics. It may be applied by insufflation, or on the tampon, according to the nature of the case. It is especially useful in cancerous ulceration, in which the pain and discharge may be greatly reduced by its use, the foster corrected, and a clean granulating surface often left. It diminishes hyperæmia and exudation, and may be used with advantage to the inflamed endometrium as a dusting powder; it will heal up most ulcerations with great rapidity.

The oleate of zinc seems to me to be preferable to bismuth alum of the oxide of zinc, and when used in combination with iodoform, it will be found to be soothing, astringent, antiseptic, and healing.

W. D. HASLAM, M.D., M.R.C.S.

19, Mecklenburg Square.

A PLEASANT ANÆSTHETIC MIXTURE.

MUCH discussion has lately appeared in the JOURNAL on the subject of anæsthetics. The A.C.E. mixture appears to hold great favour with many practitioners, and undoubtedly very justly so, it being apparently a much safer anæsthetic than either chloroform or ether.

I should like to place before your readers another mixture, which I think will be found to give even greater satisfaction, especially for all minor operations—namely, a mixture of equal parts of alcohol and chloroform. Its anæsthetic effects do not come on quite so rapidly, but it is much less unpleasant to the patient, and I believe even safer than the usual mixture of A.C.E. I have administered it (using a Skinner's inhaler) in minor surgical operations and in dental cases, and have found no unpleasant after-effects whatever, except in one case in which there was slight excitement, which very soon passed off.

A more pleasant mixture can be made with equal parts of the best eau de Cologne and chloroform, and in addition the inhaler should first be sprinkled with a few drops of pure eau de Cologne, so as to render the first two or three inhalations still more pleasant and comforting to the patient. In dental operations I have found this plan most satisfactory for nervous ladies, who are prejudiced against any form of anæsthetic other than gas.

WILLIAM J. STEPHENS,
Anæsthetist to the Brighton Dental Hospital.

OPHTHALMOLOGICAL MEMORANDA.

SPASMODIC ENTROPION TREATED BY STRETCHING THE ORBICULARIS.

Mrs. W., aged 69, consulted me at the Bolton Eye Hospital, on November 9th, last. She had been ill nearly three months; there was mucopurulent conjunctivitis of both eyes, ulceration of both corneæ, very extensive in one eye, double iritis with numerous posterior synechiæ, and spasmodic entropion of the lower lids. Here I am concerned merely with the entropion, for which I stretched the orbicularis by an extreme separation (maintained for about a minute of the eyelids, using for this purpose the eyelid retractor. This single manipulation accomplished its object. When the lids were, before its performance, placed in their normal, they immediately resumed their morbid position, notwithstanding the utmost efforts of the patient. She reports that at no time since the stretching (now six weeks ago) have the lids been turned in except momentarily at the time of making local applications.

The treatment by suture, to which I called attention in the JOURNAL of May 28th last, and of the efficacy of which I have since had additional evidence was contra-indicated by the discharge.

Manchester.

DAVID McKEOWN, M.D.