

Addison's anæmia²⁰) his object, nevertheless, was a clear one—viz., to prevent, on the one hand, any tampering with Addison's conception, and, on the other hand, so far as possible, to prevent it from being built in and surrounded by the different etiological conception which even in 1883 (see dates in scheme) was rapidly being built up upon Biermer's foundation.

VI.—AUTHOR'S ETIOLOGICAL CONCLUSIONS.

Such, then, was the origin, and such has been the subsequent development up to the present time of the etiology of the condition described by Biermer and entitled "progressive pernicious anæmia." Such was the way in which the anæmia described by Addison as "idiopathic anæmia" came to be regarded as merely one form of a larger group, so-called "pernicious anæmias," and even up to the present time is still so regarded by many observers who have worked at the disease. Such was the way in which the etiological conception of Biermer has from his time to the present assumed more and more imposing proportions, while the conception of Addison is relegated by these observers to a very insignificant position—dismissed in a sentence as of quite subordinate interest etiotologically.²¹

It is little surprising that the great majority of observers since 1871, approaching the etiology of the disease (or diseases) from the side of the progressive pernicious anæmia point of view, have been so impressed with the dimensions and solidity of the structure, that they have hardly noticed—or if they have noticed have regarded as very small and insignificant—the partly hidden stone labeled "Addison's anæmia" "without discoverable cause," in one corner of the foundation. Nor is it surprising that even later English observers approaching it, or desiring to approach it, from the point of view of Addison's anæmia, have also, even while thinking of Addison's anæmia, been unduly impressed with the imposing etiological erection built around it, and have come to speak of "pernicious anæmia" as a condition which may not only be without discoverable cause, but may also be due to many and various causes. (See Scheme.)

The chief etiological conclusion arrived at as the result of my own work on this disease since 1885, now confirmed in every direction by further investigations on 25 cases during the past two and a half years,²² is this: that the anæmia had in view by Addison is not a form of the heterogeneous group described under the name of "progressive pernicious anæmia"; that Addison's "idiopathic anæmia" is not identical with "Biermer's anæmia" producible by many causes. On the contrary, it is, as he thought, a "very remarkable disease"—one, according to my observations, of a definite infective nature, in which both clinical observations and necropsy reveal definite infective and hæmolytic lesions invariably associated with the disease and in which a very definite number of points can be got out of the history, throwing light both on the mode of origin of the disease and the sources of infection.

(To be continued.)

THE MEANING OF URIC ACID AND THE URATES.¹

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No apology need be offered for taking up again such a time-worn subject in view of the fact that if we do not take it, it will take us. We find, in spite of the spread of so-called temperance, that gout, under its modern name of lithæmia, is still easily *dominus morborum et morbus dominorum*—"lord of diseases and disease of lords," at least in that great group of pathological conditions which do not promptly call death to their relief. It might, however, be regarded as distinctly presumptuous to endeavour to add anything to the superb and monumental mass of work which has been done upon

this problem and I will hasten to acquit myself in advance of any intention of doing more than raising the question as to whether some of our hitherto accepted solutions of parts of the problem have stood the test of time and of the advancement of knowledge. If we took as our definition of gout or lithæmia the only one which for various reasons seems to be practicable, disturbance of health associated with the presence of excessive amounts of the urates and uric acid in the urine, obviously the first question upon which it is important to have clear ideas is as to the probable origin of uric acid, and this has absolutely been made the crux of the problem for many years past. We will leave for the present the vital and pregnant question whether in making uric acid the criterion of lithæmia we are dealing with the cause or a symptom, as that can be more appropriately considered later in our discussion.

First of all, what is the source of the uric acid which is present in health? for upon our answer to this will largely depend our view of the cause of its increase in disease. Two answers, or more accurately two groups of answers, have been given to this query. The first, which dominates the popular conception of lithæmia almost exclusively, is that uric acid is a sort of by-product of nitrogenous metabolism, representing the imperfect combustion of proteids—a sort of metabolic "clinker" amongst the urea ashes of the body furnace; the other view is, that it is the normal excretory product of some group of tissues or cells, or parts of cells, in the body. Such a wealth of conscientious and laborious investigation has been done in the light of the first-mentioned view of the origin of uric acid that even to mention the names of the workers would almost exhaust the space at my disposal, so that I must content myself with merely stating the conclusion towards which at present the drift seems very clearly tending, which is that uric acid is in no sense longer to be regarded as a product of the imperfect combustion of proteids into urea, that no mere excess of nitrogenous foods as such is capable of producing it, but it closely depends upon the amount of nucleins, purins, and the alloxur group in the food. Excess of nitrogenous food has been shown by Burian, Schur, Horbaczewski, and Minkowski to result in the increase, not of uric acid and the urates, but of urea, while by the administration of bodies rich in nucleins and purins, such as roe, sweetbread, thyroid extract, &c., the uric acid output could be increased in a marked degree. Finally, by a careful selection of nitrogenous bodies which were especially poor in nucleins and the alloxur group the urea could be largely increased while uric acid was actually diminished in amount. About half our normal output is probably due to this source—the *exogenous* uric acid of Chittenden.

Another hypothesis under this heading which was most alluring from every point of view and which I frankly confess cast its seductive glamour over my mind for many years and was only abandoned with a pang of regret was the so-called "reversion" theory of uric acid formation. Inasmuch as uric acid forms the bulk of the nitrogenous excretion in birds and serpents and in some insects, and bulks much more largely in the urinary solids of the invertebrates generally than in those of vertebrates, it seemed highly plausible to regard it as a more primitive form of nitrogenous metabolism to which, under various disturbances of the general balance of nutrition, the body promptly reverted—in other words, as Garrod put it many years ago, "our gouty patients seem to be a sort of birds." But this pleasant light, I fear, was little better than an *ignis fatuus*, though it took years of somewhat careful observation and numerous tumbles into bog-holes to convince me of that unwelcome fact. Upon closer observation the gravest difficulties are found in the way of such a hypothesis. In the first place, there is absolutely no discoverable relation between either the zoological position, the character of metabolism, of food, temperature, or general bodily habits of animals and the extent to which they excrete uric acid as compared with urea. When we regard the sluggish and cold-blooded serpent alone it seems quite natural to suppose that the slower processes of oxidation in its tissues should result in the formation of the less oxidised product, uric acid, but, on the other hand, the bird, with a temperature of from 4° to 12° higher than the mammal, a creature of most brilliant and excessive motility, living literally at fever pitch, so to speak, still excretes the same sub-oxidised product. Neither can these two families of vertebrates in which this marked excess occurs, birds and snakes, be regarded as in any direct sense in the line of human ancestry, however remote, and as we go more widely

²⁰ Acting consistently on this view he stated with regard to cases showing changes in the red marrow (Pepper and Cohnheim), "these cases must be separated from those of idiopathic anæmia."

²¹ Ehrlich and Lazarus: Die Anæmie, Part 2, 1900.

²² See Clinical Journal, 1902.

¹ A paper read before the Washington State Medical Society, Tacoma, S.A., June, 1902.

afield and further back down the scale of animal life we find that uric acid is by no means the exclusive, although, on the whole, the chief, product of primitive renal secretion. Some invertebrates excrete almost as large a proportion of urea as of uric acid; in others, guanin seems to take its place (mollusca); in others, hippuric acid, so that I am afraid that we can hardly claim that uric acid is much older ancestrally than urea in vertebrates, although more common in most invertebrates. The solution simply seems to be that both urea and uric acid being satisfactory and economical end-forms of nitrogenous metabolism in the animal body, some classes and orders have found it convenient to form one and others the other. So that uric acid formation in birds and reptiles is analogous with *urea formation* in our own bodies and not with uric acid at all. Finally, the more efforts that were made to investigate the clinical side of the question and to study gout and gouty processes in lower animals, the more unsatisfactory the situation became, for not only did new cases fail to discover themselves but one began to entertain the gravest doubts as to the accuracy of the reports of such cases as had been hitherto reported. For instance, the so-called gouty concretions in the toes and feet of birds were found to be exceedingly rare and when they did occur they were of tuberculous, rather than of gouty, origin, although containing urates which are easily deposited in avian tissues, and gout in the bird, in spite of the enormous excess of uric acid and its antecedents which must be constantly present in its tissues, was found to be a decidedly rare disease. Almost the only animal in which there appears to be reasonable proof of even the moderately frequent appearance of gout was the pig, and even here specimens were few and hard to come across and not particularly convincing when found. Calculi of all sorts were, of course, frequent enough, both vesical and intestinal, but that is another story which I hope to be able to take up on some future occasion. So that as far as the question of the influence of diet upon gout is concerned we can get little testimony of any value from any of the lower animals.

This "imperfect combustion" theory, however, is quite inadequate to account for the presence of uric acid in health for two reasons; firstly, that it could hardly be imagined that a body should, with such extraordinary and constant regularity, fail completely to oxidise about from 2 to 3 per cent. of its proteid output; and, secondly, that the experiments above mentioned, while they have shown that the output of uric acid can be most markedly increased and diminished by feeding with a food rich or poor in nucleins, have also demonstrated that there is a certain level, and that about half the normal half-gramme which is excreted daily in health, constantly present in the urine no matter how completely the food is free not only from nuclein but even from nitrogenous material of any sort; indeed, upon an absolutely nitrogen-free diet.

This, then, brings us to the second theory of the origin of uric acid, and that is that it is the normal end-product of the excretion or of the breaking down of some particular tissue or cell. Some little light has been given us towards the determining of this question by the feeding experiments already referred to, inasmuch as it has been shown conclusively that from nuclein to adenin, from adenin to xanthin, from xanthin to hypoxanthin, and from hypoxanthin to uric acid is merely a series of steps in oxidative and catabolic processes of the simplest order. In fact, the last three mentioned— $C_5H_4N_4O$ = hypoxanthin; $C_5H_4N_4O_2$ = xanthin; $C_5H_4N_4O_3$ = uric acid—differ from one another only by the successive addition of one atom of oxygen to the molecule. Moreover, uric acid can be formed from nuclein by simple oxidation in the presence of air, so that we may be comparatively safe in concluding that whatever cell group is concerned in the production of uric acid in health is a group which is rich in nuclein.

Two possibilities are thus open to us—one that the uric acid is the product of some group of cells which are exceedingly rich in nuclein and which normally undergo a considerable amount of disintegration in the day's work, or that it simply represents the wear and tear of the nuclei of the body cells wherever situated. The latter proposition seems hardly tenable as both the earlier and later views of the nucleus make us regard it as emphatically a creature, and if the figure may be pardoned, a "boarder" of the cell, taking its nourishment from the cell protoplasm and returning its waste product to the same medium, so that the only way in which the direct product of its decomposition could get into the

circulation would be by the death of the cell. Of the tissues particularly rich in nuclein we have, of course, several which might be considered as possibilities—the lymph nodes which consist largely of nuclei within thin cell walls, the spleen, and the thyroid. In none of these, however, does it seem likely that a sufficiently rapid rate of disintegration is going on to supply dead nuclei enough for the production of even the small normal amount of uric acid. There is, however, one tissue in the body, and that none the less a tissue because it is not tied together or anchored in any way—that is, the great brotherhood of the leucocytes—which fulfils both the required conditions of the abundance of nuclein, in their large nuclei, and a sufficiently rapid rate of constant disintegration in the process of neutralising infectious toxins introduced into the body. As the starvation experiments referred to have clearly shown that nearly half the normal uric acid secreted is due to the destructive metabolism of the body tissues, the *endogenous* uric acid of Chittenden, and in this destruction the leucocytes certainly bulk largely and are the most readily ascertainable index of the change, I now propose briefly to examine the clinical evidence which is just as real and convincing as chemical or pathological data with reference to its bearing upon this theory of origin.

First of all, is there any parallelism between those disease conditions in which excessive leucocytosis and corresponding tissue destruction are known to occur and the excretion of uric acid? Upon what we might term the positive side of the problem there is a parallelism and a striking one, as Levison has pointed out, since nearly all the acute febrile conditions in which leucocytosis occurs—such as pneumonia, rheumatism, appendicitis, and septicæmia—are attended by a large increase in the output of the urates. On the other hand, those diseases in which there is a marked absence of leucocytosis or in which, if it occurs, it is slight in extent—such as pulmonary tuberculosis (before secondary infections), certain forms of rheumatism, typhoid fever, and cholera—are seldom attended by an excess of urates in the urine. Furthermore, there is a very curious group of diseases which have been a stumbling-block in the way of the believers in the uric acid theory of gout for many years past, and that is the leukæmias and chlorosis, also various forms of nephritis, dyspnoeas of both cardiac and pulmonary origin, in any of which, though not all, a large amount of uric acid will be excreted and yet not a single symptom of gout or lithæmia be produced. This is also shown in childhood when positive showers of uric acid may occur in the urine in conjunction with a very high leucocytic count but without a single gouty or lithæmic symptom. Especially striking is this parallelism during the first few days of life when the napkins may be stained red with crystals of uric acid just at the time when the entire stock of nucleated, red blood cells is being broken down and replaced by new erythrocytes of the adult type.

The exceptions are numerous, and I am fully aware that there are numerous conditions in which a marked leucocytosis may occur without any corresponding increase in the output of uric acid, and on the other hand we do not as yet positively know that in the disease itself which we are considering the number of leucocytes is always increased. It should further be stated that we are still quite in the dark as to what is precisely the meaning of the symptom, leucocytosis. Cabot and other leading authorities on hæmatology are inclined to regard it as largely due in many cases merely to an increase of the number of the leucocytes within the walls of the blood-vessels brought about by their migration from the intercellular spaces of the tissues, so that there may be no actual increase in their total in the body. Further, it by no means follows that the presence or even formation of leucocytes in excess will be attended by a corresponding destruction of these cells, which is required in the production of uric acid upon this hypothesis. One, at least, of the most prominently alleged discrepancies in the leucocytic origin of uric acid, that the normal digestive leucocytosis is frequently not accompanied by a rise in the hourly uric acid output, is in my judgment not conclusive or even valid, inasmuch as this digestive leucocytosis is merely a swarming of the leucocytes into the circulation to feed upon the readily soluble portions of the newly introduced food, not of course, be it well understood, with intention and intelligent forethought on their part, but in response to chemotropism. Our knowledge of the actual occurrence of leucocytosis in gout itself is as yet incomplete and unsatisfactory, in which respect it differs but little from our knowledge of the

chemistry and pathology of this puzzling disease. In frank gout of the classic type, with throbbing toe and distended arterioles, it is I believe usually present, as it would be in any other acute inflammatory condition. In the so-called latent or lithæmic form it has been found by Croftan to be present in some and absent in others, so that until a much wider and fuller study shall have been made of its relations to this condition we cannot pronounce positively in regard to it. The researches of Croftan, however, do indicate very distinctly that leucocytosis was present in a considerable majority of the cases examined and that in nearly all of them the occurrence of polynuclear basophilic leucocytes was a marked and striking feature. In any case, however, it must be remembered that it could only be during or just before the acute attack, when alone the increase of uric acid is distinctly marked, that we could expect to find an increase of leucocytes.

This, then, brings us fairly to the question, Is uric acid, with its congeners or precedents, the cause of gout and the lithæmic state or merely a symptom? On the one hand, we find the bulk of English and American observers, and on the other the mass of the continental, headed by Levison and Ebstein. On the face of it the former theory appears far the more probable, for we have apparently naked-eye proof of its correctness, in the form of visible deposits of the urates of sodium, forming the tophi and acting as apparent irritants in the inflamed joints; while the acute attack is usually accompanied by a great increase in the excretion of urates and its decline by a corresponding diminution. Garrod and many other observers declare that the amount of uric acid in the blood itself of gouty patients is markedly increased, while Haig furnishes us with a graphic picture of solid crystals of uric acid, crystallising out of the blood stream in the sheaths of the nerves, in the parenchyma of the kidney, in the mucous membrane of the nose and throat, the tissue of the tonsils, and every other region which furnishes an ache in gout, giving rise to the chronic neuralgia, nephritis, rhinitis, and tonsillitis of gouty subjects. But the value of this evidence has been greatly undermined of late years. In the first place, very serious doubt has been thrown upon the power of uric acid as such, or even of urates, to give rise to serious irritation in the tissues. It is alleged to be almost absolutely non-irritating, as much so as its relative urea, in support of which might be advanced the facts already alluded to, of its occurrence in large amounts in some of the leukæmic conditions without giving rise to any irritation whatever. The same is true in children, where it will be present in perfect showers in the urine without giving rise to any symptoms except the renal or vesical irritation due to its mechanical effects. It has also been injected in large amounts into the bodies of animals, as well as administered in their food, with no toxic results whatever or more than purely local irritation.

From the comparative point of view it has been shown that those forms, such as the birds, in which it is normally present in enormous amounts are not in the slightest degree more disposed to gouty formation than are mammals. Indeed, so far as my own necropsies and investigations go, they would appear to be distinctly less so. The oft-cited experiments of Minkowski, in which by ligating the ureters of birds he succeeded in producing an accumulation of urates in the tissues resembling tophi, gave rise to no other gouty or lithæmic symptom, not even to local reactions around the deposits, and only such toxic symptoms as were due to the retention of all urinary excreta. In my necropsies at the London Zoological Gardens I repeatedly found small masses of urates just under the surface of the pericardium and the peritoneum, and even upon the valves of the heart itself, in birds and reptiles without giving rise to any symptoms of local irritation. More serious yet, the actual occurrence of uric acid as such in the blood or living tissues, in any circumstances, is most gravely doubted, and even the urates seem incapable of accumulating in appreciable amounts in other than fibroid or necrotic tissue. The results of the earlier observers are now regarded as due to faulty technique and the impossibility of distinguishing between the innumerable forms of proteid extractives. Indeed, since the experiments of Gaucher in 1884, the majority of chemists and pathologists have pretty well abandoned the theory of the toxic action of uric acid and the urates in gout and have fallen back upon the earlier and less oxidised members of the alloxur group, xanthin, hypoxanthin, and paraxanthin. Among them Kolisch, Rachford,

and Croftan have made out an exceedingly good case in favour of these being the actual toxic elements in gout and lithæmia.

But even these findings are now sharply challenged by other investigators, Solkowski and Frederick Müller in particular, upon the ground that from faults of technique the estimates of the amounts present were altogether too high, and that there is no adequate evidence that the alloxur bases are, in any way, increased in the blood or tissues in gout. And although Rachford and Croftan, in a most interesting series of experiments, have shown that their prolonged injection is capable of giving rise to a variety of toxic symptoms, arterio-sclerosis, and degenerative changes in the kidney, yet they have utterly failed to produce the characteristic arthritic changes and fall far short of a complete picture of gout. So that it seems to me, from consideration of the evidence, that we can scarcely do otherwise than enter against the charge that uric acid or its precedents are toxic elements in gout the Scotch verdict of "not proven." In short, the appearance and behaviour of uric acid in gout are precisely like those in any other acute intoxication such as plumbism, phosphorus poisoning, and acute yellow atrophy of the liver, in all of which it is excreted in abundance. As Magnus Levy has shown it is *only one of the various products of destructive tissue metabolism* excreted at this time, as it is always accompanied by large amounts of other nitrogenous extractives in the urine. Not only so, but the same observer has shown a marked increase in nitrogen retention in the tissues after the attack and corresponding diminution in the urine, just as in convalescence from any fever or other acute toxic attack. Last, and most significant of all to the cytologist, is the marked increase of phosphoric acid in the urine during a gouty attack. As has been shown by Fletcher (Osler) the curve of uric acid output runs in a striking parallel with that of phosphoric acid.

What does this mean? Simply, that as all nucleins are composed of a phosphoric acid element, nucleic acid $C_{10}H_{59}N_{14}O_{22}P_2O_5$ united with a purin or adenin base, we have here the *other moiety of the nuclei of the cells which have been destroyed in the toxic process*. We might as justly blame phosphoric acid for the attack as uric acid. Both are symptoms.

What light does our dietetic and medicinal treatment of gout and the lithæmic state throw upon the question of causation? Certainly nothing in favour of the uric acid theory. The two substances which are clinically recognised as being by far the most active and constant elements in the production of gout and goutiness are not even nitrogenous bodies, but pure carbohydrates, alcohol and the fruit-sugars. While there appears to be a certain amount of parallelism between the amounts of nuclein and purin bases in certain classes of food, and their tendency to produce gout—namely, the red meats, caviare, the osmazomes, and the extractives generally—yet on the other hand it must be remembered that all of these may be consumed in enormous amounts without the production of a single gouty symptom. Trappers, hunters, Arctic explorers, rancheros, living for months on an exclusively meat diet, seldom or never develop symptoms of gout or even lithæmia. Among birds and animals gout is as rare in flesh-eaters as in grain-eaters, and the only species in which it occurs to an appreciable extent are the parrot (a fruit-eater) and the pig (a mixed feeder). An excess of starches or sugars in conjunction with a moderate amount of nitrogenous elements in the diet will promptly produce gout in susceptible subjects, and last, but not least, the action of the purin group of extractives in red meats would appear to lie more in the fact that they are the principal flavouring elements of the class of foods which are most fascinatingly attractive to the palate and hence most likely to be indulged in to excess; so that their tabooing will usually practically result in a marked diminution of the actual amount of food consumed. Lastly, it must be remembered that although the amounts of uric acid and the urates in the urine can be enormously increased in both men and animals by a diet rich in nucleins this is by no means gout, nor is it usually accompanied by lithæmic symptoms, except in those who have already been subject to the disease.

What, then, would appear to be at present the most rationally tenable idea as to the nature and causation of gout? In my judgment we have included under the head of gout and lithæmia an enormous number of often utterly unrelated metabolic disturbances of varied origin, whose sole

bond of union has been that they were accompanied by an excess of urates in the urine and, in well-developed cases, by deposits in the tissues. In other words, *the unity of the so-called lithæmic group of disturbances depends solely upon uric acid*, which is purely a symptom or index of certain changes taking place in the body metabolism. From this point of view gout would be defined as a toxæmia of varying causation, usually of gastro-intestinal origin, accompanied by the formation of an excess of urates, this excess of urates being due to the breaking down of the leucocytes and fixed cells in the attempt to neutralise the poison—in other words, being the measure of the resisting power of the body tissues. The formation and introduction of the toxins, be it well understood, are by no means confined to the gouty; it is only *the nature of the resistance* of the body to them that gives the character of gout.

From this point of view our patients may be roughly divided into three great groups, first, those in which leucocytosis (used as an index of, and large factor in, the general resisting-power) is merely deficient; second, those in whom it is moderate but from one cause or another not quite adequate; and third, those in whom the leucocytic scavenging power of the body is abundant for all emergencies. The second of these two classes will furnish us gouty and lithæmic patients. The first group, if exposed to any poison either in food, air, or otherwise, will be apt to succumb promptly to the invasion with the development of acute toxæmic, choleraic, or other symptoms, never rising to the dignity, so to speak, of a lithæmic attack. The diseases to which they are more subject are tuberculosis, typhoid fever, and certain forms of chronic rheumatism, in all of which the leucocytic action is now known to be below the normal and leucocytosis both rare and difficult to produce. This tentative definition would also coincide with the alleged, and as I believe genuine, antagonism which appears to exist between lithæmia and tuberculosis. We seldom find a development of tuberculosis in our lithæmic patients—if it does occur it is usually of the so-called chronic or fibroid type—and the occurrence of a well-marked deposit of urates in a tuberculous patient, in any stage short of cavity-formation, has always proved a most hopeful sign in my experience. I am aware that accurate data in any considerable mass in regard to this alleged antagonism are yet lacking, but nearly every practitioner of experience seems to be imbued with the belief and has seen cases which illustrate it, and such systematic work as has been done has strongly corroborated it. For instance, Croftan has investigated the history of 100 consecutive arthritic patients in the Philadelphia hospitals and found that only one out of the number had ever developed pulmonary tuberculosis. In 200 consecutive cases of advanced tuberculosis he found only three with arthritic manifestations. In a similar number of patients suffering from pulmonary tuberculosis in St. Joseph's Hospital Reilly found a history of arthritic and other lithæmic complications, apart, of course, from the well-known tuberculous neuritis and synovitis, in only six cases, and of these three presented a slow "fibroid" type of the disease with a strong tendency to healing by scar formation and a fourth improved rapidly and soon left the hospital. Bernheim has confirmed Croftan's researches and cites several French authorities in support of the alleged antagonism. Anders's recent statistics bear out the favourableness of the prognosis in tuberculous cases which give a history of rheumatism. The second group, when the toxins or other poisonous bodies are introduced into the system, will promptly attempt their neutralisation by means of leucocytic action, but the resistance being inadequate the toxins still produce an effect, although a chronic and much modified one, and the leucocytes and fixed cells which are destroyed in the struggle appear in the urine in the form of urates. That leucocytosis and cell resistance are deficient in the gouty is strongly suggested by the fact that the actual uric acid output, between the attacks, has been found to be *below the normal* in many of them. In the third group the poison, if introduced, is promptly neutralised by the leucocytes without any unusual amount of fatalities in their part, hence the system is protected from the attack with little or no increase in the amount of urates excreted.

Of course it must be remembered that these classes are by no means hard and fast, or constant, in their boundaries, but that an individual who normally would be in the third class of abundant, adequate leucocytosis and general resisting power might, from an ingestion of an excessive amount

of poisonous material, or during depression of the general vitality or disturbance of the general health, succumb to the attack and fall into the second, even the first class. This view would also account for the marked increase of urates which will occasionally occur in perfectly healthy individuals after the ingestion of unusual amounts of food or exposure to other toxic influences without the production of any gouty or lithæmic symptoms. In these cases the poison has been introduced, a number of leucocytes have been destroyed, but there is still a sufficient margin of resisting power to neutralise and to expel the poison.

For the sake of convenience we will designate these three classes as the *acytic*, in whom cellular resistance and leucocyte-formation are markedly deficient; the *hemicytic* or *gouty* class, in whom the cellular resisting power is fair but not adequate to all emergencies; and the *hypercytic*, in whom the resisting power is adequate to all emergencies. I should regard the division between these classes as much more rigid in, so to speak, the upward direction than in the downward, that is to say, that while the hypercytic or thoroughly normal individual may readily, in circumstances of great stress or an unusually severe invasion of toxic material, drop into the hemicytic or much more seldom into the acytic class, and while the hemicytic individual may become the acytic under certain conditions, yet the acytic individual will much less readily become hemicytic or the hemicytic hypercytic. In other words, patients who with any degree of frequency reach the hemicytic or lithæmic level will rarely be found to become tuberculous, and once gouty always lithæmic.

What are the considerations which seem to point towards the view that "gout" is a group of direct intoxications of widely-varied origin and entirely independent of uric acid? First of all, there is the most significant fact that, as has been known from time immemorial, all the lesions of gout, arterio-sclerosis, degeneration of the kidney, neuritis, neuralgia, muscular pains, and perfectly characteristic tophi, can be produced by a poison which is not only non-nitrogenous but not even organic; that is, lead. According to Garrod 30 per cent. of hospital cases of gout give a history of chronic plumbism. Here, I think, there can be little or no reasonable question of any specific influence on the part of the urates. To have a so-called auto-intoxication of endogenous origin reproduced with absolute fidelity in every symptom and result by a metallic poison is, in my mind, a most serious and significant flaw in the theory. Secondly, we have a most notorious factor, over which probably the most furious and acrimonious controversy has raged in the whole wrathful question of gout, and that is alcohol. There is probably no question upon which it is more unsafe to be dogmatic than this. On the one hand, wider acquaintance with the lithæmic state shows us that not merely its characteristic symptoms but even attacks of true gout itself occur—and, indeed, in my experience the former are frequent—in total abstainers and in prohibition communities. The familiar myth, for instance, of the absence of gout or lithæmia in America is rapidly fading away in the light of broader knowledge and more careful study, and this with no marked increase in the consumption of alcohol. We must remember that alcohol in some form is as regular an article of diet and staff of life in every part of the world—except a few of our puritanical western communities—as bread, and that the mere fact that the gouty individual uses alcohol in some form has no necessary connexion with his disease. The great majority of moderate drinkers escape entirely. Thirdly, there is abundant clinical evidence that certain readily-fermentable sugars—such as those contained in the richer fruits, preserves, pies, &c., and even starches, such as those of potatoes or pastry—are almost as capable of producing an attack in gouty subjects as wine or beer. A prominent London clinician once assured me that a slice of gooseberry-tart would produce an attack in some of his gouty patients as promptly as a glass of port. Yet, when all deductions have been made, while we may fully recognise the fact that the liberal use of alcohol, particularly in the form of spirits, may have no injurious effect whatever, even upon gouty individuals, yet there can be little question that there is an overwhelming preponderance of the use, and in many cases the excessive use, of either malt or vinous liquors in those who are the subjects of gout.

In the light of the toxic origin of the disease it would seem highly probable that we were actually dealing with a direct intoxication, either on the part of alcohol itself or, as

seems more likely, some of the other readily fermentable carbohydrates, either of the ether, ester, or acetone group, in which the sweeter, more fruity wines, which are so infinitely more effective in the production of gout than any other form of alcohol, are particularly rich. So that we appear to have two instances in which toxic substances, produced entirely outside the body, are capable of producing the disease—not only so, but are probably responsible for its production in something like 50 per cent. of all cases.

As we study more carefully each individual case of the disease which confronts us I think we can hardly help being more and more inclined to the conclusion that we have to deal either with some form of more or less direct external intoxication or some impairment of the normal digestive and assimilative processes of the body, which results either in abnormal poisonous products being formed in the alimentary canal or in excessive absorption of the toxic substances which are normally present in the process of digestion. I believe that in the future our study of the diet of a gouty patient will be more with reference to the detection of substances which, either in themselves or in the conditions of nutrition obtaining in the cells of his intestinal wall or possibly of his liver, are capable of producing intestinal putrefaction and consequent auto-intoxication than with reference to either their nitrogen- or nuclein-content. Nowhere is it more necessary to remember, as Roberts and Osler have with special emphasis reminded us, that, in the witty paradox of Moxon, "it is quite as important to know what kind of a patient the disease has got as to know what sort of a disease the patient has got." Time and again have we found that the question of our ability to relieve our patients from the torture of their toxæmia has depended upon our ability to correct some vice of bodily habit. It may be in exercise, in bathing, in sleep, in mental stress, in hurry after meals, even in errors of refraction, that the crux of the lithæmic problem will be found to lie. Any agency that will improve the general nutrition and vigour and raise the resisting power of our patients may cure lithæmia.

Finally, what data bearing upon the other than uric-acid intoxication theory are to be found in our experience of diet and drugs? First, as regards the remedies which have been found effective, we may frankly premise that, long as they have been known and effective as many of them have proven themselves, we are still utterly in the dark as to in what manner they influence either the production or elimination of uric acid. In the twentieth century at least it is scarcely necessary to more than mention the collapse of all the theories which have been held as to the effect in this disease of any class of remedial agents, especially the alkalies, based upon their either diminishing the acidity of the blood or increasing the solubility of uric acid, since it was years ago demonstrated to the satisfaction of all but a few observers: first, that acidity of the blood is entirely unknown and, indeed, impossible during life, all findings indicating this condition being now known to be due to errors in technique; secondly, that there is no adequate proof that either uric acid or the urates as such are to be found in the blood during life, either in gouty or normal individuals; and thirdly, that it is impossible to administer drugs in anything like adequate doses to alkalise and to neutralise all the fluids and tissues of the body.

The classic remedies cover an enormously extended range, chemical, botanical, and therapeutic, ranging from sulphate of soda to piperazin, and from colchicum to the salicylates. They are all mysterious and uncertain in their action—in fact, they have only two things in common: one that they relieve gout and the other that they *check intestinal putrefaction* or diminish the absorption of its products. This effect may be produced, of course, in a variety of ways, first, and most potently, as by the saline cathartics, notably sulphate and phosphate of soda and the mineral waters generally, by sweeping out of the alimentary canal the offending material before it has time to form the toxic products or before these can be absorbed; next in order of effectiveness, by direct inhibitive effect upon fermentative changes in the alimentary canal, like the salicylates, guaiacum, phenacetin, menthol, and all the aromatic group; thirdly, by reducing the acids of gastric digestion and possibly, to a certain extent, by directly neutralising the toxins, which are usually acid in reaction, and also by producing by the use of mildly laxative effects

as the simple alkalies of all sorts, while colchicum, iodide of potassium, acetate of potash, and the mineral waters are powerful stimulants of renal excretion. In short, almost every remedy which clinical experience has proved to be of value in gout and the gouty state will be found to prevent the formation or absorption of intestinal toxins or to promote their elimination from the system.

Last of all we reach the proverbially difficult question of diet. Here every one of us is an expert and so innumerable are the varieties of our views that I must again be pardoned for giving merely the briefest and most sketchy outline of what appear to me to be the salient facts. First of all I think we will promptly agree, from bitter personal experience, that any attempt to rely in our treatment of gout upon the exclusion of any food or class of food from the dietary, regardless of the circumstances of the case and the other aspects of the problem, will end in disaster. While many of our cases are somewhat improved by excluding the red meats from their dietary and others are benefited by the absolute prohibition of all animal food, except milk and eggs, yet in the majority of cases it will be found that most of our lithæmic patients do better upon a mixed diet, somewhat limited, perhaps, in respect of the red meats and certainly of the sweeter wines, coupled with a marked restriction in the amount of certain starches and sugars, especially potatoes, pastry, and preserves, and the richer fruits, and an abundance of water, mineral or otherwise, than they do upon any exclusive diet. Personally, I am free to say that I find it practically much more frequently necessary to limit my gouty patients in regard to potatoes, pastry, and preserves than I do in respect to meat of any kind. Some of the worst cases of gouty dyspepsia that I have ever seen, attended by an abundant deposit of urates, occurred in women who lived almost exclusively on bread-and-butter, potatoes, sweets, and tea. In a very considerable percentage of cases, in fact, *the fault does not lie in the diet at all*, except in so far as this is deficient in nutritive value, on account of diminished appetite, as not unfrequently happens, or excessive, relatively to the actual needs and combustion-powers of the body. In fact, it seems to me that diet in our gouty and lithæmic patients is chiefly of interest according to the extent to which it favours intestinal fermentation and putrefaction, and as some of the most acutely and frequently toxic products of disordered intestinal digestion are non-nitrogenous, such as the acetone, oxalic, and butyric acid groups, they may be derived as readily from an excess or disproportion of carbohydrate as from an excess of nitrogenous foods.

It may perhaps be advisable to refer briefly to the well-known views and experiments of Alexander Haig. First of all, it might be pointed out that nearly every physiologist or expert chemist who has expressed himself in regard to these views has either utterly repudiated them or expressed the gravest doubts as to their correctness. Primarily, because they are absolutely and entirely based upon the position, now known to be untenable, that uric acid represents a waste or imperfect product of urea metabolism, and that the percentage or relation between the output of uric acid and of urea is a factor in the problem. Every physiologist and chemist of eminence who has written upon the question within the last three years has declared positively that urea and uric acid are the end-products of a totally different series of metabolic changes. Practically they are as distinct from each other as the urine from the fæces. This, of course, fundamentally upsets his entire series of conclusions. Secondly, the overwhelming consensus of opinion among physiologists is that the process by which his theory would have us believe that uric acid is deposited in the tissues when the blood is acid—which it never is—and dissolved out into the blood stream again whenever this becomes alkaline, is practically impossible in the living body, or at all events there is no shadow of evidence that it actually occurs, and that even if it were possible its effects upon the nervous system, in the production of neuralgia, headache, vaso-motor rhinitis, and the like, would be utterly incapable of explanation unless it were much more highly toxic and more readily capable of passing through the cell wall than it is now known to be. Thirdly, that the method upon which he relies for the determination both of urea and uric acid and the detection of the presence of the latter in the blood are antiquated, untrustworthy, and, in the language of Edsall, "utterly unsuited for serious investigation."

Although his interesting diet experiments have shown the

possibility of increasing the amount of uric acid excreted by the administration of certain articles of food, this is subject to the grave deduction as to whether uric acid is in any sense the active cause of gout; and, secondly, he has never shown, in reasonably healthy individuals, that any other symptoms of gout, except uric acid, can be produced by such modifications of diet. In short, his premisses, methods, and conclusions are unconvincing to the last degree, and I frankly confess that I can hardly repress my astonishment when I see how widely his views have been accepted. It may, however, be objected, with much greater weight, if animal foods have no connexion with gouty disturbances, why has it been so universally the habit of clinicians either to forbid or to limit markedly their use in gout? This fact, I think, is susceptible of explanation, at least in part, upon two grounds, first, upon the fallacious principle of reasoning that inasmuch as uric acid is a nitrogenous body it must be derived from nitrogenous elements in the food, and consequently the use of these is to be discouraged as much as possible. Second, and what in my judgment really underlies the practical benefit which is derived from prohibiting or strictly limiting this class of foods, is that they are by far the most attractive and toothsome element in the diet. The one thing that the average man who is financially able to indulge his gastronomic tastes is most likely to run to excess in, both in amount and in frequency, is meat of one sort or another. Take away beef, mutton, venison, game, oysters, lobsters, and all the animal proteid group from the average male dietary and you will have deprived it at once of two-thirds of its attractiveness. Comparatively few men would waste much money in over-indulgence in sweets of any sort, with the exception of certain wines and liquors. In the great majority of cases the practical effect of strictly limiting the red meats, beef, mutton, game, &c., in the dietary is that you actually diminish its amount from 20 to 40 per cent. Few men, especially of sedentary habits, have any very active or aggressive appetite for vegetables and starches, eating them more as a sort of diluents or accompaniments of their meat foods. If deprived of meat they will, in their attempt to obtain an adequate amount of nutriment, eat even larger weights, and certainly much greater bulk of total diet, than when upon a mixed food, yet I believe that in the great majority of even such cases the actual nutritive value of the food taken will be found to be diminished, often markedly so. By cutting off beef, mutton, game, and soups at one end, and wines, fruit, and preserves at the other, you have simply destroyed two-thirds of the pleasures of the table. A diet such as suggested by the enemies of nitrogen, consisting of bread, milk, apples, potatoes, cheese, and green vegetables, would be attractive to no one but a Belgian hare. But, as already suggested, even these processes of partial starvation will prove utterly ineffective in a considerable number of our gouty cases. Instead of diminishing the food value of the diet or restricting any element in it, what is really needed is an increase of combustion power on the part of the body, and this may be brought about first of all and most favourably by exercise, by allowing plenty of time for digestion, by the powerful stimulus of the cold plunge or shower-bath in the morning, and by correcting any deviation from the proper bodily balance which may exist. We are, I believe, quite frequently confronted with the fact in our sedentary patients that their nerve tissues have, so to speak, an appetite for more food than their liver and muscles are able to oxidise. They demand and really require large amounts of food of high nutritive value, any deficiency in which is promptly felt in a lessened working power, but which seem to throw in some ways too great a strain upon their secondary digestion in liver and muscles. In this the cure is not less food, but more exercise, and less mental and more outdoor work.

Finally, it may be stated that the one element which, whether we call it a food or a medicine, has been found to be of the most overwhelming importance and value in the treatment of gout and lithæmia, water, would act most admirably upon a toxic condition from any source: first, by sweeping out both the alimentary canal primarily and the liver, kidneys, and skin secondarily; and secondly, by supplying to the body cells that abundant salt-water bath in which alone they can live and discharge their functions. 99 per cent. of our body cells, we too often forget, are still aquatic organisms and marine at that. What salts are dissolved in it is purely a matter of taste. The one active agent in all the much-vaunted mineral waters is the H_2O . Their alleged solvent

effects are now known to be pure moonshine and the wonderful effect of mineral waters of all sorts in this disease is, in my judgment, almost solely due to the one thing which they all have in common—plain H_2O , plus suggestion—not to say humbug—aided, of course, by the pure air of the springs and the excellent hygienic rules of the “Kur” or “Bad.”

The mineral water delusion is one of the strangest survivals in medicine from the times of the “trembling of the waters” in the Pool of Bethesda. It originated most unmistakably in the good old demon-theory days, the potency of the water being rated according to the amount of heat and effervescence from contained gas and, best of all, sulphurous smell and abominable taste, all of which to the primitive mind were clear and convincing proofs that it issued directly from the infernal regions, was possessed by spirits, and hence was peculiarly suitable for the casting out of devils by Beelzebub. Either sparkle, heat, or brimstone taste is still the popular requisite for the success of a mineral water. If it has all three it inspires a confidence little short of that of Montaigne in the waters of Corsena, which he declares “powerful enough to break stones” (Osler).

But the apparently most serious objection to the other than uric-acid intoxication theory of gout is yet to be considered, and that is the actual occurrence of a deposit of urates in the inflamed joints and tissues. Surely here is a physical demonstration of their causative action. Here, as nowhere else so keenly in this discussion, are the necessary limitations of both space and patience irksome to the last degree, but, I fear, necessary nevertheless. A whole chapter should, and I hope some day will, be written on this fascinating aspect of the problem. For the present the barest suggestive outline must suffice of the reasons which lead me to regard tophi and their congeners as purely symptomatic in their character. First, the overwhelming balance of the injection experiments referred to shows clearly that the urates are either non-toxic or so feebly so as to be utterly incapable of accounting for the furious outburst of frank gout; secondly, that their deposit in the tissues by no means coincides with the acute attacks, but, on the contrary, is more apt to occur during the intervals; thirdly, that their formation and deposit in large masses and amounts may, and frequently do, occur without giving rise to any symptoms whatever, or even attracting the attention of the patient; and, fourthly, that they can be produced in abundance by the prolonged ingestion of a purely mineral poison—lead. In short, that they behave precisely like the reaction products of fibrous tissues stimulated by any slowly acting toxic substance.

But the questions still remain, Why are such deposits confined to gout and why do they attack so exclusively the joints and bones? If, however, we regard “gout” as simply a term applied to the reaction to a wide variety of poisons in patients of a certain middle grade of resisting power the first objection loses most of its force. Tophi form in the gouty in response to any intoxication because the cells have sufficient resisting power to, so to speak, die in opposing its action and form urates. They do not in the acytic because the cellular resistance is absent—the body submits without a struggle, as it were—nor in the hypercytic, because the resisting forces are sufficiently abundant to neutralise both the poison and if necessary to dispose of their own casualties as well. Besides, this deposit is not confined to true gout but occurs in chronic plumbism, which is, in my judgment, not gout at all.

In the question why these toxic substances wreak their heaviest vengeance upon the joints and their surroundings we are simply facing that great metabolic problem, Why do so many other poisons show the same preference? What of the extraordinary susceptibility of the joints to the poisons of rheumatism, of gonorrhœa, of tuberculosis, of pyæmia, of syphilis? This problem calls for a complete monograph and a most interesting and important one, and even then we could not yet answer the question. We know certainly that the bones and joints are points of poorest blood-supply, of slowest circulation, and of feeblest metabolism in the entire body. Bony tissues, in spite of their physical hardness, are the class of lowest vitality in the cell state, most easily injured, slowest in repair, and slowest in growth. Almost every inflammation if it lasts long enough—tubercle, syphilis, septicæmia—wreaks its heaviest vengeance upon, and is last to be dislodged from, bone. And we are beginning to have a suspicion, slowly mounting to a belief from a comparative point of view, that our bones are merely the half-dead

calcified cores of our limbs and metameric segments, probably excretory in their origin, like the shell of the oyster and the chitine armour of insects and crustaceans, and that the localised deposition in the tissues of large amounts of nitrogenous waste products is one of the necessary precedents and accompaniments of all calcification processes, whether the result be bone, calculi, or the plates of atheroma, all of which occur in gout. Tophi, if they persist long enough, come to contain both phosphate and carbonate of lime, and the gouty nodosities about a joint ultimately form true bone with surprising frequency.

In conclusion, I would submit the following propositions which seem to me in the light of our present knowledge to outline the most rationally tenable view of gout and lithæmia.

1. There is no connexion whatever between the production of urea and of uric acid, hence interdiction and marked limitation of animal or of nitrogenous foods, as such, in gout is irrational.
2. The uric acid produced in health comes exclusively from two sources, the larger moiety, or *exogenous* uric acid of Chittenden, from the nucleins and purin bases of the food, the smaller, or *endogenous* moiety, from the destructive metabolism of the nucleins of the body tissues.
3. It is the *endogenous moiety alone* which is increased in gout and lithæmia.
4. Gout and lithæmia are mere symptom names for a miscellaneous group of chronic toxæmic processes of widely varied origin, characterised by the production of uric acid and the urates.
5. By "gouty diathesis" we mean the possession of a sufficient degree of resisting power on the part of the protective cells of the body to oppose the entrance of any poison, whatever its character or source, with consequent destructive metabolism and production of uric acid, but not adequate to neutralise or successfully to prevent its absorption.
6. The uric acid of gout, like the phosphoric acid which invariably accompanies it, is merely a result and measure of the destructive metabolism of the nucleins of the body cells, chiefly probably of the leucocytes, in response to the invasion of poisons or toxins, either organic or inorganic (lead, phosphorus, alcohol, acetone).
7. Hence the use of lithia or other "solvent" agents is irrational and any benefits resulting are to be explained on other grounds.
8. As most of the toxins setting up this destructive metabolism and consequent uric acid production are of intestinal origin or entry *diet in gout should be regulated solely with regard to the diminution of intestinal fermentation and putrefaction.*
9. As animal foods from their much more appetising and attractive flavours are more apt to be indulged in in excess of the oxidative powers of the body, their limitation may be found to be more frequently necessary than that of vegetable foods, but sugars and starches are also very often at fault.
10. As uric acid and the alloxur group are not toxic, or at best feebly so, and are not the cause of gout the prohibition of even foods rich in nucleins and purin bases, such as red meats, roe and sweetbreads, has no rational basis and is clinically of doubtful utility except by diminishing the attractiveness of the dietary.
11. The rôle of the liver in gout is a negative one, being inability to perform its chief normal function as a "poison filter," and to absorb or to transform into harmless excretory substances the excess of toxins brought to it by the portal vein.
12. The drugs found of value in gout owe their efficacy chiefly to their power of checking intestinal putrefaction or of preventing the absorption or promoting the elimination of its products.

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THE SURGICAL TREATMENT OF GASTRIC AND DUODENAL ULCERS.¹

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In the following paper I propose to tabulate and to make comment upon a series of cases of gastric and duodenal ulcer which I have had under my care. Operative treatment may be required in perforation of a gastric or duodenal ulcer, in hæmorrhage manifest either as hæmatemesis or melaena, and in chronic ulcer of the stomach or duodenum

producing stenosis in the body of the stomach or near the pylorus, dilatation of the organ, or persisting and intractable dyspepsia. The indications for the treatment of the first of these complications, "perforation," have been so carefully studied and so definitely set forth by various recent writers that I shall be content with a mere statement of my experience. Of the two latter forms, hæmorrhage and chronic ulceration, producing dilatation or inveterate dyspepsia, but little has been written by the surgeon and I therefore propose to deal with these at greater length and with fuller detail.

1. *Perforation of gastric or duodenal ulcers.*—Perforation may be acute, subacute, or chronic: acute when the ulcer gives way suddenly and completely and the stomach contents are free to escape at once into the general peritoneal cavity; subacute when the ulcer perforates rapidly but the stomach is empty, the opening small or quickly closed by adhesions or a plug of omentum; and chronic when, owing to limiting adhesions to the anterior abdominal wall, the liver, the pancreas, or any neighbouring structure, the escape of contents is slight and confined by barriers of lymph to a circumscribed area, with the result that an abscess slowly forms and becomes manifest at a later period. In acute and subacute perforation an operation should be advised as soon as the diagnosis is assured. In the very great majority of cases the diagnosis is made with confidence, but unless the patient is seen within the first few hours the initial shock may have passed off, especially when morphia has been administered, and it is then difficult to convince oneself of the extreme urgency of the case. The severity of the symptoms depends upon several conditions: the previous state of health, the size and number of the perforations, the condition of the stomach whether full or almost empty, the bacterial virulence of its contents, and the occurrence of vomiting. The clinical picture presented by an acute perforating ulcer will depend in some measure on the site of the ulcer. If in the fundus, at the cardiac end, or in the body of the stomach, an acute infection of the whole peritoneal cavity rapidly follows; if the ulcer be at the pylorus or in the first portion of the duodenum the fluid is directed down the right side of the abdomen, owing to the hillock formed by the transverse meso-colon at the pyloric end of the stomach. The right kidney pouch is filled and the fluid then escapes downwards into the right iliac fossa and there accumulating gives rise to symptoms and signs which mimic with extraordinary accuracy those caused by an acute appendicitis. Indeed, in 19 cases of perforating duodenal ulcer out of 51 recorded by me in a paper published in THE LANCET of Dec. 14th, 1901, p. 1656, the appendix was cut down upon after a diagnosis of acute appendicitis had been made.

One point which seems definitely settled by my experience in operations upon the stomach is the possibility of recovery from subacute and perhaps even from acute perforation of the stomach without operation. In two cases a diagnosis of perforation of an ulcer had been made by the medical man in attendance. Owing to the inaccessibility of a surgeon the patients were kept under morphia regularly administered and rectal feeding was adopted. At the operation innumerable adhesions were found, more especially near the pyloric end of the stomach, and the extent and position of these suggested that they were the result of an intense local inflammation set up by the bursting through of an ulcer of the stomach. Such cases as these, however, only seem to emphasise the importance of early operation in all cases, for the hazard of spontaneous recovery is immeasurable, whereas the risk of operation is definite. Out of the 10 cases which I have had under my care four patients have recovered. I should consider that this result is as good as one may reasonably expect when the difficulties of securing the case at an early stage for operation are remembered. If statistics are compiled from recorded cases alone a better impression may be derived than this, but every surgeon knows, and has probably experienced himself, the desire for announcing his successes and of quietly postponing any mention of his disasters.

Taking all cases into account, and including those in which death results, perhaps remotely from subphrenic or perigastric abscess, I should estimate the average recoveries at 35 to 40 per cent. In operating I do not excise the ulcer. A continuous suture of catgut taken wide of the ulcer folds the stomach walls over and unites the rent. A second continuous suture of Pagenstecher thread is placed outside the first. I generally flush the peritoneum if much soiled with

¹ A paper read before the Royal Medical and Chirurgical Society on Jan. 27th, 1903.