To prevent misconception, I should state that I employ the term *antiseptic* to mean the local use of applications calculated to prevent suppuration and putrefaction, and to promote quick healing. By the term *aseptic* I mean a condition of wound and dressings absolutely free from these changes, whether or not micrococci be found in the outer portion of the dressings, for these can be rendered innocuous by the application of antiseptics superficially. I believe that I am right in asserting that the opinion of a great majority of surgeons at the present time is, that the safeguards against blood-poisoning may be much increased by the local use of antiseptics, and that the question of the hour is what is the best, most powerful, least irritating, and most convenient and economical antiseptic for general use? A great number have been tried, and many have been found wanting in one or other of these requirements. In many, there is little positive certainty that they aid the vital inhibitory force, or diminish the antagonistic agencies, which vary in almost every case.

After the publication of Lemaire's work on the use of phenic acid, in 1863, this substance was employed by many surgeons (the late Mr. Callender among them) in the form of carbolised oil, and by Sir Joseph Lister as carbolised putty, in cases of compound fracture. Although this substance fulfilled the requirements of cheapness and general usefulness, its absolute microbicide and antiseptic power is comparatively small, namely, 1 in 31 (Miquet). It was soon found to be irritating to the tissues, and apt to produce carbolic poisoning. Although the limits of dilution and efficiency had been sketched out by Lister (1 in 20 to 1 in 40), patients vary so very much in susceptibility, that both they and their surgeons often experienced the penalty of exceeding the limits. Its volatility, while rendering it more efficient in the deeper recesses and sinuses of a wound, caused it to lose a good deal of its efficacy superficially, by its want of persistence and staying power. This was partly, but not entirely, overcome by Lister's gauze, which, again, was expensive and uncertain.

Chloride of zinc, in the form of Burnett's fluid, had been used long before this at the Middlesex Hospital by the late Mr. De Morgan, to destroy cancer-germs in wounds; and it was found, when applied strong, in a saturated solution, to be a good antiseptic. Its microbicide power, however, is not so great as many other substances, namely, 1 in 52. In combination with solution of carbolic acid, 1, with many others, found, and still find it a valuable application to sweeten, and to keep sweet, wounds.

The sulphates of copper, iron, and zinc had been long used for what was called their detergent properties, which, I take it, meant nearly the same thing as antiseptic. The former two of these salts have an effective microbicide power of 1 in 110; that is to say, they destroy microbes when diluted to that extent, which is twice the dilution of carbolic acid nearly.

Iodine has a still greater antiseptic power (as high as 1 in 400), and has been used by Messrs. Savory and Bryant in solution as a wash to recent wounds with good effect, in the proportion of 1 in 1,000. To stop the putrefactive process when once formed, Mr. Savory uses it in the proportion of a drachm of the tincture to half a pint of distilled water. As a sedative as well as antiseptic dry dressing, its compound, iodoform, has been found very useful.

Condy's fluid, or permanganate of potash, with an antiseptic power less than, though nearly approaching to, that of carbolic acid, is inconvenient, from the discoloration which it causes. Like alum, boracic acid, and salicylic acid and its salts, the antiseptic power is not sufficient to make it a reliable application where sepsis is already present. Nearly all these substances, when more or less strong solutions are used, have the drawback of blunting and corroding the instruments used in operating and dressing, except boracic and salicylic acids.

In the choice of an antiseptic agent, it must be borne in mind that a much weaker solution suffices to prevent putrefaction than to destroy it when it has once commenced, and that the potentiality of antiseptics may be modified by any sort of dilution to any extent; so that, the greater the power of a given antiseptic, the more generally useful, and the more economical it is, within the limits of its market price and its supply.

Solubility, a degree of volatility, no irritative property when applied to the living tissues, and no effect upon the system by absorption, seem to be the criteria of a good antiseptic. Some antiseptic applications combine the advantages of absorbent dry dressings. This gives to the treatment by salicylic wool, sanitas, wood-wool, etc., a preference with many surgeons. These substances possess also the advantage of close and comfortable packing; so that, although not highly antiseptic, they exclude the air, absorb the discharges, and so prevent the beginnings of putrefaction, and keep the wound aseptic for a greater or less time, according to the amount of discharge.

Salicylic wool is a very convenient dressing, and is at present much used. Encalyptol, at first much lauded, has not fulfilled the expectations of practical surgeons, and is now dropping out of use. In default of more powerful remedies, the compound tincture of benzoin (Friars' balsam) is a good primary application to slight wounds, excluding the air, and antiseptic to a considerable degree. A good deal of the power of these and other aromatic substances of the like kind has been lost by partial and imperfect modes of application.

The introduction by Sir Joseph Lister of the carbolic spray, by means of Siegel's spray producer, and its combination with the carbolic gauze and the judicious use of Chassaignac's drainage-tubes, improved the results of antiseptic surgery, especially in the treatment of compound fractures, and large spinal and other abscesses. The good effect of this combination, and the greater attention to the cleansing and surgical purification to which it led in the continental hospitals, was plainly shown in the vastly decreased mortality in the practice of Volkmann and Nussbaum in Germany. It led, also, to greater care in all the details of hygiene; and in this way, in no small degree, effected this beneficial change.

In large towns, and in crowded, poverty-stricken districts, the free use of antiseptics is rendered more imperative by the lowered vitality and dirty habits of the patients, as well as by the depressing effects of impure air. Accidents happen, and operations must continue to be needed, even in these unhealthy circumstances. And here I think the carbolised spray is useful to purify the air, and clothing, etc., which it does to a considerable degree; and the experience recorded by Mr. T. Holmes, at Wimbledon, shows that even pure air, great cleanliness, and favourable surroundings, do not entirely prevent erysipelas and pyæmia from being carried by infection.

(To be continued.)

A CLINICAL LECTURE ON RECENT ADVANCES IN ABDOMINAL SURGERY.

Delivered before the Pupils of the Medical Department of the Yorkshire College, October 28th, 1885.

By C. G. WHEELHOUSE, F.R.C.S.,
Consulting Surgeon to the General Infirmary at Leeds.

I AM here this evening to fulfil the promise I made to you last spring, that, if I were spared until the coming of another session, I would resume the subject I was then unable to complete, and would continue the story of the advances made in abdominal surgery in recent years; and that I would endeavour to show you in how entirely different a light we are able to regard the subject now, from that in which we saw it, even in so recent times as those in which, forty years ago, I took my seat upon the benches of the Leeds School of Medicine.

I have already told you that, in that day, intraperitoneal surgery, except under the direst necessity, was unrecognised; and that, save only for the relief of hernia, or some other necessarily fatal accident, the abdomen was a region yielded almost entirely to the physician.

If you will refer to the text-books and surgical works of fifty years ago, you will see the verification of this statement; and you will note that not only were the great operations I have already brought to your notice unknown, but that of those, concerning which I am now about to speak, no inkling had taken possession of the surgical mind. Indeed, had things which are now done every day been so much as even hinted at then, the hints would have been considered to be simply the thoughts of the enthusiast, and any attempt to carry them into effect would have been regarded as insane!

Thus, all the operations on the pelvic organs are the triumphs of men of comparatively recent days; and it is only the encouragement afforded by their unexpected success that has led on to others of equal, or almost equal, brilliancy.

I have already said all that I desire to say concerning the surgery of the uterine and pelvic organs; and I was engaged, when last we met, in the discussion of the surgery of the alimentary canal.

I had spoken of gastrotomy, as an useful and justifiable proceeding for the relief of those terrible forms of starvation arising from strictures of the pharynx, œsophagus, and cardiac orifice of the stomach; and had expressed an opinion that, in this operation simply

or in this operation combined with dilatation of the orifices of the stomach, as advocated by Professor Loreta, of Bologna, we had developed a means of affording relief, though not of cure, for a class of diseases, the agony of which, ere their course was run to its unaided bitter end, was beyond description, and oftentimes beyond all the strength of human endurance; and had found for the sufferers a form of euthanasia, at any rate, which enabled them to die in peace, and which robbed death of its supremest terrors.

I had shown you that, rather than submit to such indescribable torture as is entailed by these conditions, men, and women too, had been found, who would even accept the risk of such an operation as pylorotomy; and that surgeons had been found sanguine enough to advocate the ablation of the pylorus, and even to hope that, in "favourable cases," they might occasionally thereby save, or at any rate prolong, an otherwise doomed life. But I mentioned these cases as surgical rarities, and as possibilities only; as not being likely ever to become otherwise; and as not unjustifiable in cases of dire and extreme necessity.

I had spoken, too, of the surgical treatment of obstruction, as occurring in the intestinal canal, and had given you my view as to the present position of the surgical aspect of that question. I had shown you that in some—the acute forms—such internal obstructions gave rise to symptoms so like strangulated hernia, that, if any external manifestation were present, a surgeon would feel bound to explore it, and that he was none the less bound, because it was internal, to open the body, search for the obstruction, and, if possible, remove or overcome it; that in some, more chronic in their general conditions, but with acute symptoms suddenly added, he had also a reasonable though considerably diminished prospect of successful operative intervention; and that there still remained a large class of cases, purely chronic in character, which, almost to a certainty, would be found to depend upon irremediable conditions, and in which, therefore, each man must be left to form his own opinion, and to act upon it; and in which, practice, and the results obtained, would probably continue to vary, as they have varied from time immemorial, and to depend upon the greater or less surgical bias of the mind of the attendant.

This, then, brings me to a review of other cases in connection with the intestinal canal, in which the aid of the surgeon may be invoked, and concerning which I have something new to tell you. Let me put before you a case of wound of the abdomen, complicated by a wound of the intestine also, and consider the best way in which to deal with it. If, through a wound in the abdominal wall, unwounded intestine be found to protrude, the rule of treatment is clear enough. See that the intestine is thoroughly cleansed; return it, carefully, well within the abdominal cavity; and so stitch the wound in the latter, under antiseptic precautions and dressings, as to place it in the most favourable conditions to heal and become sound again, and you may reasonably hope that the case will end successfully.

Clearly, therefore, if the intestine is found to be wounded, as well as the abdominal wall, our object should be to imitate, as closely as we can, the case in which it is not; and, before replacing the bowel, to remedy the wound, if possible; and then, having made it leak-tight, to restore it, as in the former case.

Now, whether the wound be large or small, it is fortunate that we have it in our power to do this, if only time and sufficient care be bestowed upon the doing of it; the great secret, we have learned, is not to be content simply to stitch the edges of the peritoneal coat, but, turning the peritoneal coat well in, so as to bring together a fair amount of peritoneal surface, to sew the rounded edges so firmly together as to insure their withstanding the pressure of even liquid contents in their passage through the canal; and that a case so treated will also generally make a favourable recovery.

But, let us suppose that this successful closure of the wound in the bowel has not occurred, then one of two things must happen: either the contents of the canal will be poured through the wound into the peritoneal cavity, and death must necessarily and speedily follow; or the edges of the wound in the intestine may, by good fortune, have adhered to the edges of the flesh-wound in the abdominal wall, and the contents be thus permitted to escape, forming what is called a fecal fistula.

As I write these lines, two cases of this kind come vividly to my remembrance—not only because of the distress and suffering to which they gave rise, but also because of their intractable character, and the persistency with which they resisted all my endeavours to close them.

Picture to yourself a patient, otherwise healthy, but the subject of such a wound, with the liquid contents of the small intestines perpetually streaming away; the wound, and the surface round it for a long way, kept excoriated and raw, and all efforts futile to stop the dis-

charge; and you will realise the state of mind in which, time after time, I saw those two cases: and all this, not because nothing was done; but, in spite of everything that was done, or could be thought of. The edges of the wound in both the intestine and the abdominal wall were pared, and were most accurately and carefully stitched together, only to burst open in the course of comparatively very few hours. Plastic operations of all kinds were resorted to, with no better success; plugs, pads, trusses of all kinds, and all sorts of external contrivances, one after another, were tried, and failed.

Once, in early life, I had succeeded in curing such a case by the use of Dupuytren's enterotome; and in one of these, though very nearly at the cost of the patient's life, the enterotome was unsuccessfully tried; and, finally, both were abandoned to their fate.

Of one of them I have heard no more; he was an old man, and would probably not long survive his discharge from the hospital; but, of the other, I have heard a good deal, and have lived to rejoice, in his case, at seeing a success attained in other hands which I had failed to accomplish. He was a young man, and was drafted from the hospital into the workhouse; and there, with the prospect of a long life before him, he would of necessity have remained, had it not been for one of the great advances in abdominal surgery, of which I am endeavouring to-night to give you an idea, and which has been fraught with incalculable blessing to him. Many consultations had been held upon his case, and I believe that every member of the staff had contributed all he knew in the way of suggestion for his benefit; and I am not wrong in saying that, if the failure to cure the case was primarily mine, inasmuch as the man was under my especial care, so also was it a failure on the part of my colleagues, whom I had called repeatedly to my assistance.

Such was the state of surgery at the time that, as we believed, we had exhausted all our resources. Neither I nor they saw anything more that could be done; and so, as I say, the poor man was reluctantly abandoned to his fate.

Not long after this, a similar case came under the observation and care of Mr. Jessop; and, to my great satisfaction, I heard that he had succeeded in completely curing it.

I need not say that I immediately became all alive to ascertain how this had been done; and, in answer to my inquiries, Mr. Jessop gave me the following information: that he had made a free incision in the abdominal wall; had fully exposed the attachment of the wound in the intestine to its edges; had separated these completely; had cut out (across its entire calibre) the injured portion of intestine; had very carefully sutured together the severed ends of the bowel; and, having restored the direct channel through the intestine, had returned it into the abdomen; had then refreshed the edges of the external wound, and brought them together by interrupted sutures; and had been rewarded by complete success.

The fame of this case speedily reached the ears of the surgeon to the workhouse; and he laid it before my old patient, who, so great was his perpetual discomfort and distress, no sooner heard of it than he cheerfully undertook to submit again to anything that offered him a reasonable chance of recovery—submitted, and was also cured. Am I not therefore justified in speaking of this as a veritable advance?

No doubt, the doctrine that a wounded intestine, after careful stitching, may with safety be returned into the abdominal cavity, has been recognised and acted on for generations past; but it is not a novelty in surgical practice and doctrine that diseased, wounded, ulcerated, gangrenous, and otherwise damaged portions, may be cut wholly away, and the healthy edges of the severed intestine be so securely and accurately joined and sutured, that the ablated portion will never be missed? I could recall to mind no other recorded case in which this had been done; and, as it seemed to me to open up a great prospect for the future of abdominal surgery, I sought, not unnaturally, to learn from Mr. Jessop what had led him up to so great a success. With a courtesy and grace which I would gladly see as general in the profession at large as it is in Leeds, Mr. Jessop at once wrote me a letter, and gave me therein all the information he possessed, by placing this pamphlet in my hands.¹ Until he did this, I was fully under the impression that the improvement was the outcome of his own thoughts; and, had I not communicated frankly with him, I fear that I should not only have claimed it for him, but that I should have somewhat seriously misled you also.

If you will consult the thirteenth volume of the *St. Thomas's Hospital Reports*, you will find "A Case of Artificial Anus treated by Resection of the Small Intestine, by G. H. Makins;" and you will not only learn all the details of the procedure, with its bibliography, but you will also see that its initiation in this country is entirely due

¹ Extract from *St. Thomas's Hospital Reports*, vol. xiii, containing report of case by Mr. G. H. Makins.

to Mr. Makins. The case he there relates is so exactly like my own in everything (except its cause); in its nature, its position, and in what had been done, unsuccessfully, both by himself and by others, for its cure—that, in reading it, I could almost have believed that he had had my very patient himself in hand, and had cured him after all.

I pray you to study carefully the thoughtful and truly scientific paper which Mr. Makins has published; and if, in your after-life, it should fall to your lot to be entrusted with the management of such a case, you will feel that you are armed at all points to meet it.

In like manner, I now desire to bring to your notice another novelty in connection with the surgery of the intestines, which has only come into practice within very recent times.

For some years past, the subject of the radical cure of hernia has been before the profession. Introduced originally by Mr. John Wood, of King's College, modified—shall I say improved upon?—by Mr. Spanton and others, the proposal has had a chequered existence; and, though attempted by many others—ourselves amongst the number—it has not made much progress, nor, except by its originators, has it been very largely practised.

The reasons for this are not very far to seek. First, the operation itself is a most complicated one, and its details are very difficult to comprehend and carry out; secondly, its results, even when successful, are by no means certain, or certainly permanent; and, thirdly, it is not unaccompanied by very considerable risk.

And yet, when a hernia which—it may be for many years—has been a source of discomfort, trouble, and danger, has at last become strangulated, and an operation has become necessary to save life, it has come about, not unnaturally, I think, that many operators have had it in their minds not only to remove the present danger, but, at the same time, to do something, if possible, to lessen that of the recurrence of the strangulation, and also for the cure or more permanent relief of the condition.

Now, gentlemen, if there be one thing more dangerous for a surgeon to do than another, it is to claim priority of discovery in any surgical procedure. No one was ever the first to do anything now in surgery or medicine, or to invent any new surgical instrument or appliance. We in England claim that Harvey was the discoverer of the circulation; the Italians say no, and claim the honour for Cæsalpinus. To Sir James Simpson is usually accorded the discovery of chloroform as an anæsthetic; and yet only the other day I saw his claim disputed, and read the assertion that he had only pieced together other men's ideas, and was not himself the true discoverer.

Whatever, therefore, in any similar case, your belief may be, never profess or claim to be the first to have done a thing, or especially to have invented anything. Have you thought out some new contrivance, or some novel instrument? For the sake of your happiness, make no public claim to it. Enjoy the use of it yourself; let all your friends enjoy it, too; but do not call it yours. Some one else, you may be quite certain, has been in the field before you, and has forestalled you; your instrument has a tap which turns one way, and his only differed in that his tap turned another; or it may be that, though yours was brought before the public on a given day, he had made his known a few days earlier, and you are nowhere, except in a wearisome correspondence, of no earthly moment to yourself, or anyone else, about a matter which interests no one in the world except yourself and your competitor.

Now, if I were to say to whose sagacity I believe the endeavour to combine the operation for the radical cure of a hernia with its relief from strangulation to be due, I think I should say that we owe it to Dr. Mitchell Banks, of Liverpool; and yet I might retain a lingering doubt in my mind whether we in Leeds had not been working in that direction almost as early as he. At any rate, I have a very distinct recollection of the first case in which it entered into my mind—without the aid of any previous suggestion—to attempt the two together. I had been operating upon a very large hernia, in which the coverings of the sac were very difficult to identify; and I had, in trying to identify the sac itself, unconsciously stripped it a good deal from its connections. When I had released the hernia, and had returned the contents of the sac into the abdomen, I was puzzled how best to deal with the sac itself; and, finding that I had no difficulty in continuing the stripping I had begun, I readily cleared it to its neck, and, putting a stout catgut ligature around that part, I cut away all the rest, and I never had a case that made a better recovery.

Moreover, when recovery was complete, and the man came to apply his truss again, he found that his hernia, which had been most difficult to command before, was now retained with ease, and seemed to him to be cured. Mentioning this to my colleague, Mr. Teale, I found that he had already done exactly the same thing a little while before; and it was not long before I received from Dr. Mitchell Banks

a pamphlet containing a full account of this as a regular method of procedure with him. Therefore, if I had been presumptuous enough to claim priority for my procedure, how sad and undignified would have been my fall.

Nevertheless, the procedure has taken root; and whereas I, when I was of your age, was taught that it was my bounden duty, wherever I found it possible, to relieve a strangulated hernia without opening the sac at all, I venture to predict that you will not unfrequently see the sac not only opened, but taken altogether away, when once its contents have been safely replaced in the abdomen.

To this procedure some surgeons now add that of stitching together the pillars of the opening through which the hernia has protruded; but of this I have no personal experience; and as it is only yet on its trial, in the hands of others, I will leave it to them to tell you hereafter what results they have obtained, saying only at present that where I have seen it done, it has seemed to me to involve great additional complication, and has added to the operation, necessary for the relief of the strangulation, an amount of further risk which would have caused me to hesitate in its adoption.

And now, gentlemen, before I take leave of the alimentary canal, I desire to say a few words on the subject of lumbar colotomy; an operation which, though by no means of a novel character, has come, at any rate in my experience, into so much more frequent use of late than in former years, that I should not deem it right to pass it over without a word of comment. In my early days, colotomy was looked upon as one of the rare and difficult operations of surgery, and was only resorted to in cases of extreme obstruction, the actual presence of which could be detected, and demonstrated within reach of the fingers or of some instrument in the rectum. In proof of the rarity with which it was undertaken, I may say that I never saw it performed by any one of the surgeons to this institution during the whole of my pupilage, and only once, by the late Mr. Teale, in all the enormous practice of the then acting staff of this great hospital. Now, you will see it done time after time; will come, I doubt not, to regard it as one of the common and more ordinary operations of surgery, and will wonder why it was so rare in former days. It could not be because of any inherent difficulty in the performance of the operation; for the proceeding is, indeed, as a rule, a simple and straightforward one, the only difficulties being, first, the uncertainty of finding the colon, and, secondly, the danger of opening the peritoneal cavity by missing the points of reflection of that membrane from the sides or back of the intestine. The first of these Mr. Bryant has cleared out of the way, by the very precise manner in which, as the result of repeated dissections, he has shown the normal position of the gut; and of the second we have ceased to have somewhat of our former dread, since we have learned that, even should the accident occur to us, it is not irremediable, but that, if the opening unintentionally made be carefully closed by accurate suture, it need not interfere with the successful issue of the case. In both these matters, experience, when you come to acquire it, will show you that accidental circumstances play a very large part.

If the obstruction have been complete, and the colon be greatly distended, you will think the operation amongst the easiest you have ever undertaken; but if, on the other hand, the gut be empty, the mesocolon long, and the patient greatly attenuated by starvation, you may have such difficulty in finding the bowel as to reduce you to the verge of despair. To meet this difficulty, however, we have recently received a common-sense hint from our American friends, for which we owe them much gratitude.

As the patient lies upon his belly to expose the flank for operation, an empty colon naturally falls forward into the abdomen, especially if its peritoneal attachment be loose, and is very difficult to find. I have seen it inflated from the rectum with air to bring it into view. I have seen it injected with water for the same purpose, and I have seen both these procedures fail to effect the object in view; and though I have never seen it, I know that surgeons of the highest manipulative skill have, under such circumstances, been compelled to abandon the operation.

The American solution of the difficulty is this: do not abandon the operation; only delay its completion for a little. Turn your patient over on to his back, let the gut fall backwards, by gravitation, instead of forwards, and then, if with well nibbed forceps you will seize whatever has fallen into the wound, if that have been accurately made, you will find, in a large majority of cases, that you have seized the uncovered back of the intestine of which you are in search.

And now, I have only one other hint to give concerning this operation, but it is a clinical observation of which, though I have seen no authoritative mention of it in surgical works, I think I have made myself fairly certain, namely, of its great value as an aid in

he cure of syphilitic disease of the rectum. It is by no means easy, at all times, to say positively whether a given case of obstructive disease of the rectum be malignant or syphilitic in nature. In either case, if the obstruction be such as to render life miserable, I would urge the performance of colotomy. If the disease be truly malignant, you will give your patient great relief, and will materially smooth his passage to the grave; and if it be syphilitic, by placing the parts physiologically at rest, you may be enabled, by constitutional treatment, to cure the constitutional disease; and you will then see the opening in the loin slowly contract and close, as the local disease in the rectum passes away, and the gut resumes its normal function. I have seen such instances, and I know that they have been noted by my colleagues also.

[To be continued.]

REPORT TO THE SCIENTIFIC GRANTS COMMITTEE OF THE BRITISH MEDICAL ASSOCIATION.

A NEW METHOD FOR THE QUANTITATIVE ESTIMATION OF URIC ACID.

By JOHN B. HAYCRAFT, M.B., B.Sc., F.R.S.E.,
Professor of Physiology, Mason and Queen's Colleges, Birmingham.

WISHING to continue a research upon albuminoid metabolism, and having been much disheartened by the unsatisfactory nature of the processes used for the estimation of uric acid, I determined, if possible, to introduce one which should be at the same time exact and easy of application.

The method of Heintz is certainly not very reliable, and is not applicable when small quantities of the acid are present in the urine. Salkowski's modification, although certainly more reliable, is far too laborious, and is not free from other objections. The same may be said of a somewhat similar process introduced by Fokker. Other methods, depending on the power which the acid has of reducing certain metallic salts, or upon the decomposition of the uric acid, and the collection of its decomposition products, I have tried, but soon had to give them up. Inasmuch as uric acid forms some salts more insoluble than itself, and as some of these are with metals easy of estimation, it occurred to me that it might be possible to precipitate it as a salt, as of lead, barium, or silver; and then, by estimating the metal in the precipitate, to calculate the uric acid in combination with it.

Many experiments were undertaken with the uric acid salts of barium, mercury, and lead; their solubilities in various acids, etc., were tested; and endeavours were made to separate (in these forms) the acid from solutions of mixed salts and other compounds found in urine, and likely to interfere with the process. One or two rough methods were devised, but were soon discarded from their want of accuracy. An attempt to separate out uric acid as a silver salt was more successful.

Urate of silver is, as is well known, very insoluble in water. I find it to be very soluble in 20 per cent. nitric acid, and in somewhat stronger sulphuric acid. It is insoluble in ammonia, and almost insoluble in strong acetic acid.

After a futile attempt to separate out the uric acid from the phosphates of the urine, after acidulation with acetic acid, I succeeded in elaborating the following process.

The fact that urate of silver is insoluble in ammonia water is a very curious one, inasmuch as this is a solvent for nearly all the silver salts—for example, the chlorides, phosphates, and oxalates. On adding a solution of silver nitrate to a solution of acid urate of sodium, I found that, instead of a precipitation of silver urate, an immediate reduction occurred, the black precipitate not redissolving in ammonia water. If, however, the solutions be previously rendered ammoniacal, a white gelatinous precipitate of the urate at once forms. The silver, however, becomes partially reduced before it is possible to collect and wash it. I find, however, that the previous addition of bicarbonate of sodium prevents this reduction. The same obtains with the acid urate of sodium normally present in urine, the chlorides and phos-

phates remaining in solution, the urate of silver alone falling on the addition of the ammoniacal nitrate. In order to estimate the silver in this precipitate of the urate, the latter was collected in a filter, and washed with distilled water. Taking advantage of its ready solubility in nitric acid, it was dissolved in that reagent.

A method not long introduced by Volhard,¹ and one which is available for the estimation of silver in an acid medium, was used as the final step of the process.

The only difficulty that occurs is in filtering and washing the precipitate of urate of silver. It is gelatinous, and clogs up the filter. It is imperative to use a Sprengel's pump, and a properly made asbestos filter is advisable. In this case, the whole process may be completed in little more than half an hour. The filter should be prepared in the following way. A small funnel is half filled with broken glass. A sufficient quantity of asbestos is shaken in a flask with water until all the fibres have separated, forming an uniform pulp. This is poured on the glass, and should form an uniform felt, one quarter of an inch thick. The filter can be used again and again for several analyses.

DESCRIPTION OF METHOD.

Solutions Required.—1. Centinormal ammoniac sulphocyanate. Dissolve about 8 grammes of crystals in a litre of water, and adjust it to decinormal silver solution. Dilute with 9 volumes of water. One cubic centimetre is equivalent to 0.00168 of uric acid.

2. A saturated solution of potash-alum.

3. Pure nitric acid (20-30 per cent.). Dilute the commercial acid, boil and preserve from light in a blackened flask.

4. Strong ammonia.

5. Ammoniacal silver solution. Dissolve 5 grammes of nitrate in 100 cubic centimetres water, and add ammonia, until the solution becomes clear.

Process.—Measure off 25 cubic centimetres of urine in a pipette, and place it in a small beaker, with about 1 gramme of bicarbonate of sodium. Add 2 or 3 cubic centimetres of ammonia, which will produce a precipitate of ammonio-magnesium phosphate. On adding 1 to 2 cubic centimetres of the ammoniacal silver solution, the uric acid falls as a white gelatinous precipitate of urate of silver.

This is collected on the asbestos filter, and carefully washed, until the washings give no trace of silver, with a drop of salt-solution. The urate is then washed through the filter, by the aid of a few cubic centimetres of the nitric acid, and the silver in this solution estimated by Volhard's method.

Add a few drops of the saturated solution of potash-alum, which is the indicator, and drop in the centinormal solution of ammoniac sulphocyanate. A white precipitate will form, and, with a reddish coloration, which latter disappears as soon as the red colour is permanent, the process is at an end.

It is easy to calculate the uric acid, which will be the number of cubic centimetres of the sulphocyanate used multiplied by 0.00168.

If the urine contain albumen, this should previously be removed. If uric acid or urates be present in such quantity as to cause turbidity, the secretion should be warmed and diluted. In order to test the accuracy of the process, I prepared several solutions of acid urate of sodium of known strength. To these, I added various quantities of common salt, sulphate of magnesia, and phosphate of soda, in order to imitate, as far as possible, the urinary secretion. On estimating the uric acid in these solutions, I obtained wonderfully correct results, as the subjoined tables will show. In all cases, a quantity not much more than a milligramme was lost during the process, and may be simply accounted for by the fact that no salt of uric acid is absolutely insoluble.

	Experiment 1.	Experiment 2.	Experiment 3.
Uric acid present as urate ..	0.0486	0.0459	0.0211
Uric acid found	0.0470	0.0437	0.0198
Loss during process	0.0016	0.0022	0.0013
	3 per cent.	5 per cent.	6 per cent. loss.

In order further to test its accuracy, 50 cubic centimetres of urine were divided into two equal portions; to the first, 25 cubic centimetres of a solution of acid urate of sodium of known strength were added; to the second, 25 cubic centimetres of water were added.

When estimated, the two fluids should shew a difference equal to the quantity of the salt added.

	Experiment 1.	Experiment 2.	Experiment 3.
Uric acid found (a)	0.034	0.026	0.041
Uric acid found (b)	0.0144	0.0105	0.0254
Difference	0.0196	0.0155	0.0156
Uric acid added	0.021	0.016	0.0160

The above figures show how delicate the process is, and what accu-

¹ Die Anwendung des Schwefelcyammoniums in der Maassanalyse. (Liebig's *Annalen*, Band cxc, p. 1.)