As to the correction of astigmatism, just so much of the error should be corrected as can be worn with comfort after a few days exercise with the correction. If a compound error, where the myopia or hypermetropia and the astigmatism are of about equal amounts, the astigmatic error is of the greater import, and the ametropic conditions may be best corrected and the symptoms sometimes completely relieved by correcting the astigmatism alone; whereas, the correction of the hypermetropic or myopic error alone would leave the patient in just as great suffering as before the correction. In cases where the hypermetropia or myopia is much greater than the astigmatic error, the correction of the astigmatism alone would be of little value. So, in these cases, a portion of the hypermetropia or myopia must be corrected with the astigmatism. Where large errors of astigmatism exist and there are no symptoms referable to the eyes directly or reflexly, then the oculist is perfectly justifiable in leaving the errors uncorrected.

In presbyopic cases, however, it is the writer's opinion that in nearly every case the correction for astigmatism should be added to the presbyopia.

I suppose that we all have considerable difficulty with our lady patients in regard to their wearing spectacles. At least I find nine out of ten who beg to have nose glasses, or threaten that they will not wear the spectacles, and probably five out of ten will get the nose glasses any way. This factor, I think, is encouraged by the habit of some of the oculists in acquiescing in the whims and notions of their fair patients, and also by the habit that they have of wearing nose glasses themselves, either because of convenience or looks. I must enter my protest, however, against their use in all cases of astigmatism.

"UREINE."

EXPERIMENTS TO DETERMINE THE TRUTH OF THE
RECENTLY ANNOUNCED DISCOVERY BY MOOR OF
THE TRUE CAUSE OF UREMIA.*

JOHN WEATHERSON, C.E., M.D.

CHICAGO.

When the article by Dr. William Ovid Moor,¹ of Rome, Italy, on "The Discovery of Ureine," appeared and subsequently formed a communication to the Thirteenth International Medical Congress, Paris, 1900, it was pointed out that, if true, it was the most important contribution to urinology, of recent times.

As it offered new fields in the study of the etiology of uremia, dropsy, etc., I began experiments² to isolate and to determine its toxic properties.

The method given by Moor for its isolation is substantially as follows: As "ureine" is supposed to be destroyed by a temperature of 80 C., a large quantity of urine is heated at the low temperature of 50 C., until no more evaporation is seen to take place. Silver nitrate is used to precipitate the chlorids; the liquid is then cooled to promote separation of the phosphates, when both chlorids and phosphates are filtered out. The filtrate is again heated at 65 C., until no more evaporation is seen to take place. Alcohol and oxalic acid are used to precipitate the urea which is filtered out. The alcohol is evaporated at 55 C. The sulphates and remaining solids are separated out by cooling and filtering. The color pigments are removed by treating

with mercuric nitrate, neutralizing with sodium carbonate and filtering. The remaining alcohol and water are evaporated at 55 C., when nothing is supposed to remain but "ureine," a pale yellow, oily liquid of a heavy specific gravity and resembling, in aspect, olive-oil.

I have repeated this experiment again and again, following exactly the method given by Moor, and using normal urine in amounts varying from 1000 c.c. to 1700 c.c. for each experiment. I never obtained anything—except in one well-defined case which I will mention later—but a yellow alcoholic liquid which could easily be evaporated at the low temperature of 55 C., and leaving scarcely any residue.

Moor states that this mysterious organic liquid is destroyed by high temperatures, and that, to be obtained pure, an excess of reagents is to be avoided.

To know exactly when every milligram of solid substance of the urine is precipitated, without having added an excess of reagents, is no simple matter. In my later attempts to isolate this organic toxin, I employed the following method: After examining a part of a twenty-four-hour specimen of urine for specific gravity, color, amount of urea, etc., I place from 1000 c.c. to 1500 c.c. of the urine in a large porcelain evaporating dish and evaporate over a low yellow flame at a temperature of not over 50 C. A smaller dish may be used and urine added as the water evaporates. In about thirty-six hours, or as soon as no more vapor is seen to arise from the liquid, the remaining fluid is carefully treated with a saturated solution of silver nitrate until all the chlorids have been precipitated.

To exactly determine this point, I use the following test: On a watch-glass resting on white paper, place a few drops of a saturated solution of neutral potassium chromate. On to this, from time to time, add a drop of the urine and silver nitrate mixture, until a faint pink tinge appears. This pink tinge is due to silver chromate and signifies that all the chlorids have been precipitated and that a trace of excess of silver nitrate now exists. This excess is used up by adding 5 to 10 c.c. of the original concentrated urine set aside for this purpose.

The liquid is now cooled sufficiently for the separation of the phosphates and then filtered, and we wash the filter with distilled water until the liquid comes through colorless. We have now removed all of the chlorids and practically all of the phosphates. The filtrate is now put into a beaker and the water evaporated on a water-bath at a temperature of not over 65 C. To determine this point, Moor uses a large mercury thermometer, placed in the liquid and rapidly withdrawn at 65 C. from time to time. He says a puff of vapor will be seen to arise from the bulb as long as any water remains,

To the remaining liquid add one-half its volume of absolute alcohol. Moor now adds 1 gram of pure powdered oxalic acid for each 100 c.c. of urine used in the experiment. He then carefully adds an alcoholic solution of oxalic acid, stirring and allowing the precipitate to settle after each addition, until all the urea has been precipitated as oxalate of urea. I did not add any specific amount of oxalic acid, since this would vary as the percentage of urea of different urines varied, but I carefully added the oxalic acid and made the following test for excess: On a watch-glass resting on black paper, place a few drops of silver nitrate solution. To this, from time to time, add a drop of the liquid until a white precipitate occurs. This white precipitate is not due to chlorids, since they have been removed, but to silver oxalate ($\text{Ag}_2\text{C}_2\text{O}_4$), and signifies that all the urea has been precipitated, and that a trace of excess of oxalic

* Read before the Chicago Medical Society, Feb. 21, 1901.

1. Medical Record, Sept. 1, 1900.

2. These experiments were performed in the laboratory of Dr. Wm. E. Quine, of which I have charge.

acid now exists. This excess is used up by adding a few cubic centimeters of the liquid set aside for this purpose. Filter out the oxalate of urea and wash with absolute alcohol. Evaporate at 55 C. The sulphates and remaining solids may largely be removed by cooling and filtering, using alcohol freely to aid filtration. Next, to remove the coloring matter, treat the filtrate carefully with a saturated solution of mercuric nitrate [$\text{Hg}(\text{NO}_3)_2$] until no more precipitate occurs.

To determine this point I use the following test: On a watch-glass resting on white paper, place a few drops of a strong solution of potassium iodid. To this, from time to time, add a drop of the liquid until an orange or red precipitate appears, which is due to mercuric iodid. This signifies that all the pigment has been precipitated and that a slight excess of mercuric nitrate now exists, which is used up by adding a few cubic centimeters of the filtrate which has been set aside for this purpose. Now neutralize with sodium carbonate, testing with red and blue litmus paper. Filter again and evaporate at 55 C. Pure "ureine" is now supposed to remain.

This method is exactly the method given by Moor, except the tests for excess of reagents, which I have added. This method could be improved upon by using barium chlorid to precipitate the sulphates, instead of trusting to concentrating, cooling, filtering, etc.

I have repeated this experiment, with the refinements here given, many times, and have never obtained anything but clear yellow water and alcohol, which could easily be evaporated at the low temperature of 55 C.

Not succeeding in proving that the "ureine" of Moor is a constituent of urine, I began a series of experiments to prove that it is not. In this I was more successful.

1. Moor, in his publication, assumes that all the water of the urine is evaporated, when he can no longer see vapor arising from the bulb of a large mercury thermometer which is rapidly withdrawn from the liquid when at a temperature of 65 C. It will be remembered that he performed his experiments during the hot summer months. If he had performed them in winter, and as soon as evaporation appeared to cease he had opened his windows to chill the atmosphere of his laboratory, he would have discovered watery vapor arising long after it ceased to be visible in a warmer atmosphere. It is well known that the watery vapor of expired air of man and animals, which can not be seen in summer, is plainly visible on a cold wintry day. Again, he fails to consider the considerable amount of water which would be retained in thick concentrated urine as water of crystallization and which is very hard to drive off at the low temperature of 55 to 65 C.

Water is no doubt present long after Moor assumes it to be absent. But I have been able to evaporate 1600 c.c. of urine on a water-bath, at the low temperature of 55 C., to the consistency of a thick syrupy mush, amounting in volume to not more than two ounces, and driving off nearly all the liquid except what was retained as water of crystallization. The evaporation required several days, but the temperature was never near the point required to decompose "ureine" (80 C.) if any had been present.

To prove that the remaining fluid is water of crystallization and not an oily liquid "ureine," the following experiment is conclusive: Pour the thick syrupy mass mentioned above, from the beaker on the water-bath into a large flat dish. On cooling, the liquid crystallizes into a solid mass, which soon becomes quite hard and dry. Fifty c.c.—nearly two ounces—of liquid "ureine,"

which is stated to exist in normal twenty-four-hour urine, should be plainly visible if present. The volume of the entire mass which I obtained by evaporation amounted to less than two ounces.

Now, since by supposition, "ureine" is an oily liquid, which remains intact and constant, at temperatures below 75 to 80 C., and since normal urine can—except some water of crystallization—be evaporated to dryness at the low temperature of 55 C., the conclusion is irresistible that there is no such substance as liquid "ureine."

2. It is well known that the total solids of average twenty-four-hour urine amount to from 60 to 70 grams. This includes both organic and inorganic substances. Also, it is known that, of the above amount, urea constitutes about one-half or from 30 to 35 grams. Moor states that the specific gravity of "ureine" is 1270, or it is as heavy as glycerin. That is, "ureine" is nearly as heavy as urea, whose specific gravity is 1323. Yet farther along in his article he states that "ureine" exists in amount equal to twice that of urea. That scientific investigators all over the world should fail to discover an organic substance in the urine amounting to 50 to 60 c.c., which would weigh 65 to 70 grams, or as great as the total known solids, is highly improbable.

3. Moor asserts that he examined the urine of a diabetic woman and found 6 per cent. of "ureine," 2.1 per cent. of urea, and that the specific gravity was 1025 and that the total urine passed in twenty-four hours was 1250 c.c. This statement alone would condemn the article before any scientific body in existence. Let us consider it in detail.

I carefully made up two solutions as follows: Solution No. 1 consisted of distilled water, in which was dissolved 2.1 per cent. of pure urea crystals, which equalled the amount found in this diabetic urine. Solution No. 2 contained 0.9 per cent. of sodium chlorid, the normal amount for 1250 c.c. of urine. By the Mohr-Westphal balance I found: Specific gravity of Solution No. 1 = 1005.5; of solution No. 2 = 1006.5. The formula for the specific gravity of mixtures gives slightly higher values.

To find what the specific gravity of a solution containing water and 6 per cent. of a liquid "ureine," whose specific gravity is 1270, would be, I make use of the following well-known formula:

$$X = (V^1 \times S^1) + (V^2 \times S^2) \div (V^1 + V^2)$$

where V^1 and V^2 represent the volume of the liquids used, and S^1 and S^2 their respective specific gravities. Substituting 94 per cent. water with a specific gravity of 1000, and 6 per cent. "ureine" with a specific gravity of 1270 we have:

$$X = (V^1 \times S^1) + (V^2 \times S^2) \div (94 + 6) = 1016,$$

the specific gravity due to "ureine" alone. This formula, while the one commonly used, is not absolutely correct when tested by experiment, because the actual volume of a mixture does not always equal the sum, in figures, of the separate volumes used. This is due to what is known in physics as "interpenetration" resulting from the porosity of substances, which exists even in liquids. But it is sufficiently correct for our purpose. We now have: Specific gravity due to 2.1 per cent. urea = 1005.5; due to 0.9 per cent. Na Cl = 1006.5; due to 6 per cent. "ureine" = 1016; total for only these three substances = 1028.

I have not considered the specific gravity due to sulphates, phosphates, pigments, sugar, etc., which without "ureine" would bring the specific gravity to about 1025, as Moor found it. But if 6 per cent. of a heavy

liquid had been present, the specific gravity would be about 1040 instead of 1025. This experimental and mathematical check on the specific gravities is the most conclusive proof we have that the whole proposition is without foundation.

But while there is no such a single toxic substance as "ureine," it is well known that normal urine is toxic. That it is not due to urea or uric acid has been proved many times. Bouchard states that there are at least seven different toxic agents in normal urine. None of these have been completely isolated. Their sum-total, including pigment, however, amounts to but a few grams.

It has been found by experiment that an aqueous extract of the urine, when injected into rabbits, causes contraction of the pupils and convulsions, but no coma; while an alcoholic extract of the urine causes deep coma, but no convulsions nor contraction of the pupils.

Since we have shown that Moor probably did not drive off all the water of the urine by evaporating at low temperatures, and since he used alcohol freely in all his experiments, and since the rabbits into which he injected his supposed "ureine" died with both coma and convulsions, the irresistible inference is that Moor's "ureine" is a mixture of aqueous and alcoholic extracts of the several toxic ingredients of normal urine. His 50 to 60 c.c. of "ureine" probably consisted of water and alcohol, in which were dissolved a little pigment with the several organic toxins, and some compound of oxalic acid.

It will be remembered that in precipitating the urea, Moor uses an alcoholic solution of oxalic acid, after he uses the powdered form, and which he makes, by dissolving 3 grams of oxalic acid in 10 cubic centimeters of hot alcohol. I made up such a solution, but used it only a few times and then only in small quantities. From Watt's "Dictionary of Chemistry," I learn that if alcohol and oxalic acid be heated together at a high temperature, one part of oxalic acid will combine with nearly the same amount of alcohol to form di-ethyl-ether, or oxalic ether, which is a colorless oil whose specific gravity is 1079. Whether true oxalic ether, or some closely related compound was formed in this case, I do not know, but at any rate when I evaporated the excess of alcohol from the solution I had made up, I found a liquid which left a permanent oil stain on paper, similar to that produced by olive-oil, but not so marked. Oxalic acid dissolved in cold alcohol did not leave these oil spots.

It is interesting to note that at the close of the second attempt at isolation which I performed by Moor's method, I found about 4 c.c. of a liquid which resembled "ureine." But this was the only time, and it was in this experiment that I used the extra quantity of the alcoholic solution of oxalic acid mentioned above. This liquid looked like olive-oil and left a permanent oil stain on paper. It did not have the odor of urine, but it was a pale-yellow color and had the aromatic odor of alcohol. This liquid I believe to be, principally, some oil closely related to oxalic ether. At any rate, a drop of it, with a drop of silver nitrate solution, gave the white precipitate of silver oxalate. It is the presence of some oxalic compound, no doubt, that leads Moor to ascribe de-oxidizing powers to "ureine," oxalic acid being a great reducing agent and readily decolorizing solutions of the permanganates. The presence of this substance, no doubt, helped to mislead Moor. Again, he states that "ureine" belongs to the alcohol group of the aromatic series, and that it splits up at a temperature of about 80 C. There is

undoubtedly alcohol there, because he put it there, and his statement that "ureine" splits up at 80 C. is no doubt due to the fact that the boiling point of alcohol is exactly 78.39 C. Again, he states that the characteristic odor of urine is due to "ureine." In my own experiments I found that the odor disappeared when the pigment was removed, due probably to some substance precipitated at the same time. Considerable pigment remaining, however, would give an odor, and also add toxic properties, since the researches of Ritter, Bouchard and Lenoir show that when normal urine is deprived of coloring matter its toxic powers are reduced one-third.

The subject of toxins of the urine is very interesting and highly important, but I can not see that Moor has added anything to our present knowledge of urinology.

DISCUSSION.

DR. JOHN A. WESENER stated that after going over Moor's article very carefully he finds that there is no chemistry whatever in his paper. In fact, it is an article made up simply of words and facts relegated to the background. He was very glad to see that the essayist has shown most emphatically the absurdity of Moor's new compound—ureine. He said that there are a few points to be considered in this article, and when these are thoroughly brought out the so-called "wonderful compound," "ureine," will immediately fade from sight. Moor says that the specific gravity of ureine is 1.27, and the amount of ureine found in the urine under normal conditions is nearly twice that of urea; therefore, assuming that we have a 6 per cent. solution of ureine, the specific gravity of that solution alone would be 1.015. It is known that when the last two figures of the specific gravity are multiplied by 2.33, and then these figures computed to the total amount of urine passed in twenty-four hours, we get approximately the total solids. Furthermore, we find that if a given quantity of urine is evaporated to dryness, and then weighed, the total solids would correspond very well with the above principle. Moor says that ureine decomposes very readily at low temperature, 80 degrees splitting it into new compounds. The speaker suggests that evaporation under those conditions should have been done in a vacuum. Moor's method of removing salts and coloring matter from the urine is a very poor and inaccurate one. Furthermore, Moor says that potassium ferrocyanid and ferric chlorid, when treated with ureine, turn blue. The speaker has always been taught that potassium ferrocyanid and ferric chlorid, when mixed, always gave Prussian blue. Moor probably means to use the red prussiate of iron instead of yellow, but even a mixture of red prussiate of iron and ferric chlorid will turn blue when treated with organic substances. For example, tincture of guaiac will change this solution to a blue color; therefore the speaker would not be surprised that the coloring matter in the urine should, when treated with these two salts, give a blue reaction.

Moor's formula of oxalate of urea can not be recognized by chemists as belonging to any known compound. How he estimated urine is beyond the speaker's comprehension. If he simply weighed the solution, then what he weighed was a most impure product. He not only weighed a certain amount of coloring matter, but also salts, which he failed to remove by his process. The statement is also made that the urine is a liquid composed of water and inorganic solids. It is known that the urine contains between 3.5 and 4 per cent. of solid matter, of which 1.75 per cent. is inorganic and 2.25 per cent. organic. Moor tells us that ureine belongs to the alcohol aromatic group, and when heated it splits into oxy acids, etc.

Probably the most emphatic and absurd statement made by the author is that "the fermentation of urea is caused by ureine, and that bacteria alone could not bring about this change, and without ureine all organic matter would become converted into urea, which would remain in Nature without any use, and thus within a limited period of time all vegetable and animal, as well as human, life would cease." It has been shown most emphatically that pure solutions of urea are changed to ammonium carbonate by a great variety of bacteria. In order to have the fermentation brisk and rapid a certain amount of nourishment in the form of peptones, ammonia salts, glycochol, etc., is necessary. Furthermore, if a decomposed urine is precipitated with alcohol, this precipitate dried and powdered and an aqueous extract made of it, and then added to a solution of pure urea, fermentation takes place quickly, ammonium carbonate being the result.

In this particular instance the enzymes of the bacteria change the urea into ammonium carbonate.

The speaker does not think any members of the profession need have any fear that without ureine urea would remain as an end product of nitrogenous metabolism. The bacteria will very readily convert it into ammonium carbonate in spite of the fact that it is, in the opinion of Moor, a most stable compound. In fact, the air itself would in time change urea to ammonium carbonate. Bacteria not only change ammonia to nitrites and nitrates, but we have a reversible action where the nitrates are changed back to ammonia. These salts are much more stable than urea, being inorganic instead of organic.

DR. WILLIAM A. EVANS said that the essayist had answered the statements of Dr. Moor so thoroughly that neither the original paper nor the refutation thereof would require any attention. From the statement, in Dr. Weatherston's paper, that urea was non-toxic, he gave some observations drawn from a paper written by Dr. Herter for the Welch "Festschrift." Since 1865, when Frerichs popularized the belief that uremia was not due to urea, there has been a growing disposition to regard this substance as almost, if not entirely, harmless. The work of Dr. Herter demonstrates that while urea is not a very actively toxic agent, and while it is readily eliminated by any one of several routes, at the same time it has some toxic action, and is responsible for some part of the symptoms known under the symptom-complex of uremia. He determined that the dog's kidneys secrete twenty times the normal quantity of urea; that ten grams of dog's kidney can secrete 1.57 grams of urea in one hour; that the kidney of the Batrachian was less efficient, and that the mammalian kidney rose in efficiency as it advanced in the scale of life.

It has been determined by other authorities as well as by Herter that when a large amount of urea is thrown into the circulation the amount recovered from the urine is considerably less than the amount injected. This difference represents the urea which was stored in the liver, the brain, the muscle and other structures, as well as the urea which is eliminated by the gastrointestinal tract, and the sweat glands. In 1898 Drs. Hoelscher and Wesener showed that in some cases of chronic nephritis and acute articular rheumatism the proportion of urea in the sweat amounted to .3 of 1 per cent. Herter and Wakeman have shown that whereas there is habitually some little elimination of urea by the gastro-intestinal tract when the percentage of urea in the blood amounts to .2 of 1 per cent., there is a great increase in gastro-intestinal elimination, accompanied by congestion of the mucosa and submucosa, dropsical accumulation in the epithelium, together with considerable desquamation thereof; that if the proportion of urea amounts to considerably above .2 of 1 per cent., the gastro-intestinal epithelium fails to eliminate urea further. In the experiments of Herter if an urea solution stronger than 4 per cent. was injected, there followed hematuria, albuminuria, decrease in the quantity of urine, and finally total suppression; that the decrease in the waters of the urine preceded the decrease in urea. It was demonstrated that in the case of dogs and monkeys convulsions, bearing a close resemblance to uremic convulsions, came on when the blood contained .3 of 1 per cent. urea, and death invariably followed when the blood contents of urea had amounted to .6 of 1 per cent. The urea in normal human blood amounts to .05 of 1 per cent. In pathologic conditions accompanied by a normal kidney epithelium, there is but little danger of accumulation of urea in the blood or in the tissues, regardless of the amount that is manufactured or absorbed. If, however, there is destruction of kidney epithelium as the result of causes acting at present, or as the result of causes previously acting, for example chronic nephritis, there is liable to be an increase in the blood-current of urea. Bradford offers a study of three cases of anuria, in which the blood percentages were .277, .324 and .44, respectively. Herter, in a study of fourteen cases of lobar pneumonia, found increases of urea ranging from an increase of 64 per cent. in one case to an increase amounting to 1200 per cent. in another. In every case of chronic nephritis examined the urea in the blood was found to be increased. He divided his cases into three groups: The first, in which the percentage of urea in the blood was .1 of 1 per cent. or less; the second, in which it varied between .1 of 1 per cent. and .3 of 1 per cent.; the third, in which it was .3 of 1 per cent. or over. The symptoms of these cases were those ordinarily recognized as the symptoms of uremia. The mode of death of the dog that died after taking unusually large injections of urea was the same as that in the condition known as uremia. It is difficult to determine whether the symptoms of uremia are due to urea or to other urinary ingredients, but it is to be remembered that in the case of the dog no urinary ingredi-

ent was injected except urea. Dr. Herter makes the statement that creatin, uric acid and other members of the xanthin group could not be the pathogenic agents in these cases. He called attention to a point that clinically has been observed most frequently by all of us, namely, that there is great difficulty in determining whether the symptoms of uremia were due to urea by quantitative examination of the urea in the urine. It would be necessary to examine the urine quantitatively for urea day by day for a considerable time, and to know with certainty how much urea was absorbed from the food, and how much was excreted by the gastro-intestinal tract and by the perspiration. As these considerations are altogether impossible, the quantitative determination of the urea in the urine gives us an altogether incomplete idea as to the amount of urea in the blood, and it is the urea in the blood and in the tissues that is responsible for any effects that urea might have.

DR. WEATHERSON, in closing the discussion, stated that Professor Haines, of Rush Medical College, had been working on the same subject and had conducted a series of experiments which corroborated his own conclusions.

ANASTOMOSIS OF THE URETERS WITH THE INTESTINE.

A HISTORICAL AND EXPERIMENTAL RESEARCH.*

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CHICAGO.

(Continued from p. 635.)

AUTHOR'S EXPERIMENTAL WORK.

The following experiments, begun nearly eighteen months ago, have been performed at the Post-Graduate Hospital and Experimental Laboratory and in the Physiological Laboratory of the Chicago University. The bacteriologic and microscopic work has been conducted entirely by my friend, Dr. F. Robert Zeit, professor of pathology at the Post-Graduate Medical School, and I gratefully acknowledge my indebtedness to him for the thorough manner in which he has performed a most arduous task.

Dogs were used for all the experiments, and chloroform was always employed for anesthesia. As the work was to include the intestinal tract, various methods of emptying the animal's bowels preparatory to the operation were tried. The administration of cathartics the day previous to the operation was a dismal failure, as a diarrhea always ensued, and even after the exercise of the greatest care liquid feces escaped through the incised gut wall. As the operation was to be upon the lower bowel, attempts were made to empty it by large rectal injections, but this method failed until it was accidentally discovered that it could be most satisfactorily accomplished on the anesthetized animal. The rectum and descending colon can be so thoroughly cleansed by this method as to enable one to entirely dispense with bowel clamps. This has been dwelt on at some length because fecal contamination was responsible for many deaths in the first series of cases.

The most rigid asepsis was employed in every operation. Silk ligatures and sutures were used. The abdominal wound was closed by interrupted through-and-through sutures. In the large majority of cases it was found impossible to keep the abdominal wound aseptic, and suppuration ensued, but in only a few cases was this followed by any serious results.

For the first twenty-four hours the animal was given nothing but water, and the diet was limited to milk for the next two days. After the first few days the surviv-

* Read before the American Gynecological Society, Washington, D. C., May 1, 1900.