plished indirectly what we otherwise would have done had the patient borne prolongation of the operation well. The fact that we did not do and find what we expected does not detract from the merits of the operation and the benefits to the patients, and we shall always feel that we did the correct thing and are ready to do the same operation again for similar conditions, only hoping it may not be necessary to hurry or change plans.

A half-completed operation is better than a dead patient, and the beneficial results from the half-finished and changed plan of operation are just as satisfactory to the patient if not to the operator. These patients knew exactly what they had to expect—discomfort, suffering and certain death under the usual let-alone or round-about medicinal plan of treatment on the one hand, and a slim chance for betterment by the proposed operation on the other. The second patient was being examined in my office when the first came in, looking so robust and well that these men, the one recovered and the other considering the same plan of treatment, were introduced to each other and had many talks before the second consented to operation, and that he did so was only due to the proof of benefit exhibited to him by the other's recovery and his satisfaction with operative treatment.

I wish to acknowledge my indebtedness to Dr. J. de Bart Shorb, of Los Angeles, for his valuable suggestions and assistance in the treatment of these patients.

DISCUSSION.

DR. JOHN B. MURPHY, Chicago—The purpose of drainage of the chest is a very important one, but I will not go into the objections attending it. The methods of drainage are two, internal and external. We can either drain the abscess through the bronchi or an external opening. We drain through the bronchi where we have no adhesions, but where we have adhesions the life of the patient would be jeopardized. If we make an incision into the chest cavity we immediately have the entrance of air. Through the opening you can grasp and palpate the abscess. Unless great care be used you are likely to have a septic pleurisy develop. You have an operation that can be performed with comparative safety, which is that of compressing the lung, allowing the pus to escape through the bronchus. You can also puncture the chest and allow the patient to inspire air. About 170 cubic inches of air may be aspirated, and the patient can have his chest filled every third day if you wish to hasten the process. There is no inconvenience from air taken in that way. Of all the injections of air into the pleural cavity we have had but one accident, and that was where the needle was inserted while the tube was connected. I never have the tube connected when I wish to insert air into the chest. By this method you can fill air into the chest and drain the cavity effectually, but in cases of adhesions this is impossible. Many surgeons fear to open the chest, but their fears are groundless. You can palpate the lung just as well as you can palpate in the pelvis, and you can locate your abscess exactly. Do not make the mistake of endeavoring to reach it with a Paquelin cautery, for you will find the lung will retract in the chest out of reach of the cautery, unless an incision be made in the chest wall large enough to enable you to seize the lung or unless it be adherent. You will have less oozing in these cases than you might think for, but the abscess will heal perfectly.

DR. LE MOYNE WILLS—Of course we have the advantage of a dry climate, which means a great deal in these cases; otherwise my patients would almost certainly have died. The second case was operated on only last January, while the other case was operated on two years ago.

The first case spent fifteen months, after leaving the Los Angeles County Hospital, in the San Jacinto Valley, in the open air, at light work, and was undoubtedly greatly assisted in his cure by climatic conditions. Had he remained in the County Hospital he would certainly have died. You do not have the opportunity to place your patients in such an at-

mosphere. These are not the kind of cases you see, whereas we see many of them.

I have experimented on rabbits from time to time since 1892, and have learned much from my work. My patients had the courage to consent to, and go through with, the operation and are perfectly satisfied with the result, although the surgeons may not be. The second case is not likely to live long, but is much more comfortable and will live longer than he otherwise would have done, without external drainage. The first man practically got well by over-flushing with a strong solution of mercury bichlorid, for I really believe the bichlorid did it. He is doing manual labor, keeps up his weight and strength, and is perfectly satisfied. I hope to be able to continue this subject at a later date.

SURGICAL ERRORS IN SKIAGRAPHY.*

CARL BECK, M.D.

NEW YORK CITY.

The ease with which some small bones of the human body can be reproduced by the Roentgen rays on a photographic plate led many medical novices, and even ignorant laymen, to the indiscriminate use, or rather abuse, of the rays.

For the ignorance of unskilful operators many distorted Roentgen ray pictures are responsible. Some of them caused such fatal errors that even well-experienced medical men felt a great disappointment in the results. But in considering all these errors, some of which were indiscriminately heralded through the medical as well as the lay press, it becomes evident that the Roentgen rays never lie, but that it is entirely our own imperfection which induces some of us to err under peculiar circumstances.

In the first place, we should never forget that a so-called Roentgen-ray picture is by no means an ordinary photograph of an object, but a silhouette only (skiagraph), i. e., a photograph of its shadow. To interpret such shadows properly a thorough knowledge of the normal anatomic relations of the tissues, especially of the bones, that produce such shadows, is required. As the most minute gradation of density is registered, the importance of being thoroughly acquainted with the anatomic relations of the bones producing the doubtful shadow is obvious, and for this purpose it is necessary to have the pictures of the shadows of the normal structures of the body always at hand for comparison.

There are slight irregularities of the surface of normal bone, which we could neither recognize nor appreciate before the Roentgen rays were discovered. Their shadows may at first sight appear to represent pathologic processes, but prove to be normal by thorough comparison with a normal skeleton. On certain portions of the normal skeleton the muscles and tendons would naturally cause obscure shadows. The carpus is especially likely to produce such errors in the skiagraph: the tuberosities of the trapezium, the scaphoid, the hamulus ossis hamati, the os pisiforme, and the eminentiæ carpi volaris radialis and ulnaris double up the thickness of the carpus, thereby causing dark shadows which might be mistaken for foreign bodies. Similar considerations and similar cautions apply to the other diagnostic opportunities offered by the rays.

If a skiagraph of the human hand is taken, for instance, the plate will show the least light where the bones rest, while the soft tissues appear opaque. There is also a difference of opacity according to the thickness of the tissues, their blood-supply, and their air capacity.

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The foot, while easily skiagraphed in the direction of the dorsum toward the planta pedis, from the toes up to the upper third of the metatarsus presents an obstacle farther backward in the first and third cuneiform bones and the scaphoid, so that it is necessary also to skiagraph the foot on these portions transversely by having the outer surface rest on the support. It is by this procedure only that the isolated shadows of the astragalus, the calcaneum, the os cuboidum, the scaphoid, and the fourth and fifth metatarsal bones can be distinctly outlined, so that false interpretations may be excluded.

In the early era of the Roentgen rays the normal sesamoids were also sometimes incorrectly interpreted.

How important the knowledge of minute anatomic detail is, especially of non-pathologic abnormalities, will be evident from the fact that the os intermedium cruris—os trigonum tarsi—has been mistaken for a fragment severed from the astragalus. This bone is a typical part of the tarsus of all mammalia, and is estimated at from 7 to 8 per cent.

Shepherd, who mistook this bone for a fractured fragment, says: "The fact that this fracture is not mentioned in any of the text-books of surgery or in special treatises on fractures would easily be accounted for by its only being discovered by dissection; it causes no deformity, and the symptoms it would give rise to during life would probably be obscure." The same author tried to produce this fracture artificially on the cadaver, but "in every case," he says, "where this maneuver was performed, I failed, even when the greatest force was used, to break off the little process of bone mentioned above." Pfitzner regards the os trigonum tarsi as an integral part of the posterior process of the astragalus in the adult, which is analogous to the os intermedium antibrachii.

The practical significance of this bone is evident from a case described by Williams, which is also highly interesting from a medicolegal standpoint: A laborer claimed that he was injured by an iron bar, on Jan. 20, 1897, but was able to work during the whole day, but the following day he called on Dr. Wilmans, complaining of intense pain at his internal malleolus. He limped and emphasized his inability to work. Dr. Wilmans found a slight swelling below the right internal malleolus. Ecchymosis of the skin being absent, the swelling was attributed to the presence of a considerable degree of talipes, from which the laborer suffered at the same time. The leg was elevated and fomentations were applied for several days. The laborer still complaining of great pain, it was decided to transfer him to a hospital for observation. When discharged, after several weeks of treatment, he made an effort to resume work, but at once declared that he was unable to keep it up. He was therefore admitted to another hospital, where he repeated this maneuver several times during a period of six months. Finally he claimed damages for having been crippled by the injury sustained on the date above, but in view of the negative objective condition found by Dr. Wilmans, the society decided not to grant any claims. The consequence was that the laborer was transferred to the surgical division of a third hospital for further observation. There he complained that he had continuous pains below the right external malleolus, even while in the recumbent position. The pain increased while walking or sitting. Stepping on the right heel, he also declared to be impossible. By distracting his attention, however, it was noticed that he could stand well on his heel, and he would undoubtedly have been declared a malingerer had not the Roentgen rays.

mirabile dictu, come to his rescue, at least temporarily, for a skiagraph showed a bone-fragment at the junction of the astragalus with the posterior surface of the calcaneum. On the strength of this skiagraphic "proof," Dr. Wilmans, although still mistrusting, was forced to modify his original opinion, and certified that the patient suffered from "fracture of the astragalus, in consequence of which he was damaged for life." The laborer therefore received an annuity of 30 per cent., in proportion to the estimated curtailing of his wages.

Soon afterward the laborer was discovered by Dr. Wilmans carrying a heavy load without any apparent pain, while formerly he had claimed to be unable to walk without a cane or a crutch. The Doctor insisted on a second irradiation, this time also skiagraphing the uninjured left foot. The skiagraph showed the "severed bone-fragment," which had first been regarded as a sesamoid of the musculus flexor longus hallucis, but which was now recognized as a normal os intermedium cruris. The society, of course, refused the annuity, and the German Supreme Assurance Court, to which the laborer had appealed, not only sustained the verdict of the society, but also decided that the laborer must return the annuity which he had unjustifiably enjoyed for eighteen months.

The significance of a skiagraph for the purpose of estimating the degree of functional disability is not always conclusive. A skiagraph may show a considerable degree of bony deformity after a fracture, and still the function may hardly be disturbed at all. Skiagraphic test has shown that, as a whole, even our best functional results show by no means an ideal union. An unscrupulous patient who secures possession of a skiagraph of his own case which shows considerable deformity, may, although there is no functional disturbance, strongly appeal to a jury on the strength of his skiagraph, if he succeeds in simulating great impairment. On the other hand, there may be but little evidence of bone-injury on the skiagraph, but there may be severe impairment of function on account of the injury to the soft tissues—circulatory, trophic or inflammatory disturbances—which can be represented only faintly, if at all. This shows the necessity of considering all the other clinical symptoms in connection with the skiagraph.

While it is easy, even for a layman, to understand the significance of most skiagraphs, there are, as alluded to, injuries the correct interpretation of which presupposes, besides thorough anatomic knowledge, the greatest care and a vast amount of experience as to the different modes of delineation in various projection-planes. The greatest diagnostic difficulties are offered by the joints. The more complicated a joint is the more complicated the skiagraphs of its various positions will naturally appear. It is especially the elbow-joint and hip-joint which are kept in view. First of all, the interpretation of the displacement caused by supracondylar fracture of the humerus and the deformities resulting from it later on, may tax the power of discrimination considerably.

The older the fracture, the less conspicuous the fracture-line will appear, since it will be more or less overshadowed by the callus. In old fractures the lines can not be represented as such, and it is only in case of union in a displaced position that its features could be guessed. In one of my cases, for instance, a second skiagraph was taken three years afterward, which showed essentially the same features as the first one, which had been taken four weeks after the injury.

In the case of the entire absence of displacement, it is

only a very distinct skiagraph that shows the line clearly. It is natural that in such cases there is no skiagraphic evidence after recovery—that is, in from four to ten weeks, according to the type of the fracture.

If such a case fails to be skiagraphed shortly after the injury, no evidence of the fracture may be subsequently obtained. On the other hand, callus formation may be so abundant that, in spite of the absence of displacement, the fullest evidence of fracture may still be furnished months after. In one of my cases callus formation was so excessive that the attending physician was accused of malpractice, and it was the skiagraph only which convinced the patient that his physician had treated him correctly, the bones being in perfect apposition.

The intra-articular fracture types offer the greatest diagnostic difficulties, inasmuch as the fracture-line is also obscured by the callus formation. If, however, a skiagraph of the other joint is made at the same time, in the same position, and in the same projection, the various delineations of the shadows will be correctly understood and interpreted.

A normal skeleton should always be compared on the skiagraph. It should particularly be remembered that certain pathologic conditions—such as rachitis, for instance—influence the outlines of the bones and may deceptively be supposed to represent a portion of an injury. In such an event the skiagram of the fellow-extremity will set matters right.

In very young children the eminentia capitata appears as if entirely severed from the humerus, although the relations are absolutely normal. The explanation of this very important phenomenon is that the epiphyseal tissues are not sufficiently ossified to produce a shadow on the plate. If these points are not thoroughly considered, a displaced fracture-fragment might be erroneously diagnosed.

Union between the epiphysis and the diaphysis of the head of the humerus is not perfect before the twentieth year. The lower epiphysis of the humerus consists of four nuclei, which do not ossify before from the eighth to the seventeenth year. The epiphyses of the trochlea, as well as of the olecranon, do not ossify before between the seventh and twelfth years, which explains why an osseous nucleus that is still connected with its neighboring epiphyseal nuclei, and the diaphysis by cartilaginous tissue, appears as an isolated piece of bone which might erroneously be taken for a fragment. The acromio-clavicular junction sometimes shows, in the skiagraph, a hiatus the width of a finger, so that a diastasis of the joint might be assumed. But since our knowledge on this new subject has increased, we know that this apparent diastasis is by no means pathologic, and that there is a normal gap between the osseous ends of the acromion and the acromial end of the clavicle. The upper epiphysis and the diaphysis of the radius unite between the seventeenth and the eighteenth year, and its lower epiphysis and the diaphysis join in the twentieth year. During the early Roentgen era the translucent space above the epiphyseal cartilage in children was erroneously taken for a fracture-line. The head of the femur unites with the diaphysis at the eighteenth or nineteenth year and the lower epiphysis follows after the twentieth year. The upper epiphysis of the tibia unites with the diaphysis in the twentieth or twenty-second year, while the lower tibial epiphysis unites with the diaphysis between the eighteenth and the nineteenth year.

For the thorough interpretation of skiagraphs in children, it is important to know that at birth the diaphyses

of the radius, the ulna, the metacarpal bones and the phalanges are ossified, while their epiphyses, as well as the whole carpus, are still cartilaginous. It is not before the seventh year that an osseous nucleus shows at the lower epiphysis of the ulna. Union with the diaphysis sometimes begins with the twelfth year, but, as a rule, not before the fifteenth. Even then a small epiphyseal disc remains, which does not disappear before the seventeenth year in the female, and not before the nineteenth in the male.

The osseous nuclei of the carpus show at different periods, viz., at the os capitatum, at the fourth month; at the hamatum, at the fifth month; while the triquetrum shows its nucleus between the second and the third year, the lunatum between the third and fifth, the naviculare between the fifth and the seventh, the trapezium and trapezoid between the sixth and the seventh, and the os pisiforme between the eleventh and the fifteenth year. After five years the capitatum, hamatum and triquetrum have assumed their regular shapes, while the others, with the exception of the pisiforme, are perfectly developed at the twelfth year.

The osseous nuclei of the epiphyses of the metacarpal bones show at the second year, their synostosis with the diaphysis taking place between the twelfth and the seventeenth year in the females, and at the age of 19 in the male. The epiphyseal nuclei of the phalanges are ossified between the fourth and the fifth year, their synostosis with the diaphysis taking place at the same age as that of the metacarpal bones—from the twelfth to the seventeenth year in the female, and between the sixteenth and the nineteenth in the male.

Regarding the elbow-joint, it must be considered that an osseous nucleus appears at the medial side of the capitulum humeri between the second and third year, another one in the internal epicondyle at the fifth year, a third in the trochlea between the eleventh and the twelfth, and soon afterward a fourth in the external epicondyle. The nucleus of the internal epicondyle unites with the diaphysis between the sixteenth and the twentieth year, but the other three nuclei form a synostosis among themselves at the seventeenth year and then form the uniform osseous epiphysis, which completes its synostosis with the diaphysis at about the twentieth. In the capitulum radii an osseous nucleus appears between the fifth and seventh year, and in the olecranon between the sixth and the eighteenth, both uniting with the diaphysis between the twentieth and the twenty-fifth and between the sixteenth and the twentieth year.

Regarding the knee-joint, it must be considered that the lower femoral epiphysis contains an osseous nucleus at birth, while the nucleus in the tibial epiphysis shows shortly afterward. At the fourth year both these epiphyses have completed their development, but they do not unite with the diaphysis before the fifteenth year. The anatomic text-books say that union takes place between the seventeenth and the twenty-fourth year, but a skiagraphic experience points to an average period of only sixteen. The osseous epiphyseal nucleus of the fibula appears between the second and the fifth year, and unites with the diaphysis between the eighteenth and the twenty-fifth, but the skiagraph dates this period earlier, viz., the fifteenth year. The osseous nucleus in the tibial spine appears between the eighth and the tenth year; the epiphyseal line disappears between it and the diaphysis at the fifteenth.

As to the bones of the foot, it may be said that the lower epiphyses of the tibia and fibula show their osseous nuclei in the first and second years, and unite with the

diaphysis between the eighteenth and twenty-fifth; according to skiagraphs, as early as before the eighteenth year. The osseous nucleus of the astragalus and calcaneum appears intra-utero, that of the cuboid shortly before or after birth, that of the cuneiform bones between the first and the fifth year, and that of the os naviculare from the first to the fifth. The osseous nuclei of the metatarsal bones and the phalanges appear from the second to the tenth year, and unite with the diaphyses between the sixteenth and the twenty-second.

In elbow-joint fractures occurring in childhood it is necessary, therefore, to take at least two skiagraphs in different projection-planes and to compare them thoroughly with the normal fellow. In a case of fracture of the femoral head, for instance, the deformity had appeared three times as large as it actually was, on account of inappropriate projection. The degree of shortening of the limb was accordingly overestimated. This shows the necessity of considering the other clinical symptoms and data in connection with the skiagraph.

In fractures of childhood it should also be remembered that the process of ossification is influenced by various affections of the bone, as, for instance, in rickets.

How important the question of projection is becomes evident when we consider that grave errors may sometimes occur even if all the preliminary conditions required for a thorough understanding of the case seem to be fulfilled. This will appear from the following experience, which has probably not been paralleled in the literature of this subject:

A boy 4 years of age, while playing in the street, fell against an iron bar. Being unable to rise again, he was taken up and carried to St. Mark's Hospital, where the first instance of moderate pain was noted, besides the functional disturbance. There was no difference in level nor any other deformity, nor any shortening, nor the typical equinus position. A photograph taken two days after the injury only showed a very moderate and uniform swelling of the leg. Abnormal mobility and crepitus in accordance could be produced only by very rough manipulations.

On the day following the injury two skiagraphs were made, one in the dorsal and the other in the lateral position. To my surprise, the one which had been skiagraphed by a direct irradiation, the center of the platinum disc of the tube being perpendicular to the anterior surface of the leg, did not show the slightest indication of fracture, while the one which represented the leg irradiated from the outer aspect of the tibia showed a marked fracture-line. The fracture presented the typical oblique type in the middle of the tibia, the fracture-line running from anteriorly to above posteriorly, the upper, tapering fragment overlapping the lower end. No side-ward displacement having been present, it can be understood why the rays reaching the long axis of the tibia in a vertical direction did not show the fracture-line. A very slight change in position, where the inclination toward the fibular direction amounts to less than 1 millimeter, brought out the fracture distinctly. If I had, as is the custom in general, taken a skiagraph in the antero-posterior direction only, and if the manipulations made during the first examination had been carried out as gently as they properly should be, the fracture might have been overlooked entirely. And if, in view of the local pain and tenderness, the swelling and the functional disturbance, the possibility of a fracture had been seriously considered, the skiagraph might have silenced the uneasy conscience. This experience teaches the necessity of adopting the principle of always taking at

least two skiagraphs in two different positions in all cases of suspected fracture.

In taking skiagraphs of foreign bodies it must be considered that their size varies according to the distance from the tube. In oblong bodies great errors as to their extent may be committed. Once I was very much surprised in a case where a needle-fragment had entered the palm of the hand in a perpendicular direction. The plate, while indicating the presence of the needle, distinctly created the impression that the fragment was only about 2 millimeters in length. When extracted it was found to be more than an inch long, the rays having reached the hand in a perpendicular direction so that the circumference of the fragment was reproduced rather than its length. A side view would have cleared up the error at once.

Misinterpretations have also arisen from unavoidable mechanical and chemical effects, causing markings in the photographic plate, the significance of which must be well known to the skiagraphic interpreter. Blemishes may also be produced by spots caused by pus from wounds or by perspiration. In the location of foreign bodies, especially in the skull, many errors were and are still committed. Their avoidance will be considered in a special article.

DISCUSSION ON PAPERS OF DRS. BECK, LEONARD AND DAVIS.*

DR. HOWARD KELLY, Baltimore—Dr. Leonard has presented a remarkable paper, and I feel sure he is doing the finest work of this kind. I have had a chance to test his skill in several cases of calculi. I had supposed until recently that in the female I had occupied this field pretty much for myself by making a diagnosis by slipping into the kidney my wax-tip bougie and finding the scratch marks on it when a stone is present; but recently I have had two cases in which I have been unable to detect the presence of the calculus in the kidney by this method, and in one of these cases Dr. Leonard cleared up the diagnosis. A patient came to me with a renal calculus on the left side, and on catheterizing the kidney I drew off a small amount of urine containing pus, but got no scratch marks. I then catheterized the other side simply to ascertain its condition, and there I unexpectedly got the scratch marks on the bougie. I therefore had a pyonephrosis on one side and calculus on the other. I passed the catheter a second time and got scratch marks again. Dr. Leonard happened to be in Baltimore at that time and took a skiagraph which located a calculus also in the side first tested. I operated first on the worst side and ten days ago on the other side. We must all learn to do this work as Dr. Leonard does it.

To facilitate end-to-end anastomosis of the ureter, I have invented what I call an anastomosis guide, which is shaped like a long thin cartridge with a long handle to use it. I slit the ureter above the division and slip in the guide and bring the ends together and suture them over the guide. The ureter can be anastomosed into the bladder in the same way. It is sometimes necessary to make a long anastomosis of the ureter into the bladder, as shown by some of my cases. I remember a patient in whom a carcinoma obstructed a ureter. I brought the ureter into the bladder by devising a plan which originated some years ago by simply freeing the bladder from its attachments and pulling it up until it reaches the ureter; the bladder was then retained in this extreme displacement by suturing it to the psoas muscle on level with the posterior border of the pelvis. By this means I have secured a perfectly satisfactory anastomosis.

DR. A. D. BEVAN, Chicago—I want to congratulate Dr. Leonard on the very fine work he has been doing. Some of the early work in finding renal calculi by means of the X-ray was done by Dr. McArthur and myself in Chicago. At that time

* Dr. Davis' paper on "Treatment of Injuries to the Ureters" appeared in last issue; Dr. Leonard's paper on "Diagnosis of Calculus Disease of Kidney, Ureters and Bladder by Roentgen Method" appeared elsewhere.

the work was rather crude and the only stones found were those of large size. I am inclined to believe that some of the cases which had stone were not always demonstrated by the X-ray, but recently the technique has been very much improved. I regard it as of the greatest possible value in the diagnosis of stone in the kidney and recall a case where its value in this connection was well demonstrated. The patient was 30 years of age and had all the symptoms of a renal stone. The patient was sent to me by Dr. Billings, of Chicago. Dr. Crane, of Kalamazoo, who has been experimenting somewhat with the X-ray, took much interest in the case and examined it with his own apparatus. He presented three pictures which showed a stone three-fourths of an inch below the last rib and also a very small shadow one-third of an inch in length and not more than one-twelfth of an inch in width below this large stone. To the outer side was seen a kidney-shaped shadow about the size of a bean. At the time of the operation the kidney was pulled out and the vessels were controlled by pressure so that the posterior border of the kidney could be split. The large and small stones were easily found, but there was no evidence of the third stone. Very thorough examination was made, but was unsuccessful. I had the X-ray picture in the operating-room and I measured with my eye as nearly as I could where the third stone was supposed to be. I then took a hypodermic needle and very readily found the second size stone in the kidney itself, which shows the great value of the X-ray. You would be surprised at the small size of the smallest stone, and I am satisfied that this method is the most valuable for diagnostic purposes that we have at our disposal. Where it fails it is largely due to improper handling or wrong interpretation: when you hear a man say it is misleading, be sure that either he does not take good pictures or he does not know how to interpret them. We must learn something about the technique and how to interpret the result gained.

DR. J. WESLEY BOVEE, Washington, D. C.—Until recently we had very little evidence of the value of the X-ray in the diagnosis of biliary and renal calculi and I am very glad to learn that the work can be done so satisfactorily at the present time.

I was very much pleased with the way Dr. Davis presented the subject of ureteral injuries, and I have no doubt that the best method of uniting a severed ureter is by uretero-ureteral anastomosis. The end-to-end method is done in nearly all cases transversely, and, in one of my own, obliquely. The experiments with this method on dogs has not been carried out on man in but the one case. The lateral anastomosis by Dr. Weller Van Hook is a good method, and recently another method of lateral anastomosis similar to the way it is performed in the intestines has been brought forward in Italy. The operation by different methods has been done 21 times on man, and an unfavorable result followed in but one case. Anastomosis with the bladder is a good operation and has been done something like 70 times. The point on which I do not agree is concerning the failure of the rectal or colon implantation, which has been done over 80 times and in a large number of cases has been successful. If we remember the work done by some of the foreign surgeons and get their statistics we shall find that they are successfully implanting the ureters into the bowel. The implantation of the severed ends of the ureter, however, has not been very satisfactory here, if we may judge from experiments on dogs, but the dangers have been greatly overdrawn. There has not been a case reported of marked stenosis in uretero-ureteral anastomosis, except one where tuberculosis was present; this may have had something to do with it, and the operation was not necessarily faulty.

DR. GEORGE GOODHUE, Dayton, Ohio—It was my experience six years ago to have a patient who complained of pain over one side in the kidney region, but not on the other. A surgeon of large experience was called in to operate on this man, and removed the kidney from the side which gave him the trouble, although it was very slightly, if at all, diseased. The other kidney I removed at the autopsy several months later, and found it packed with stone. I simply mention this to show what the X-ray might have done had we had it at that time.

Only an hour or two before I left home I had a patient, who gave every symptom of renal calculus, subjected to the X-ray, but I came away too soon to learn the results. I would like to ask Dr. Leonard to state what part of the surface should be exposed to get the best results, i. e., whether the posterior, anterior or lateral. Our work is being done by a man who is doing excellently in this line. Later I shall report a case of successful anastomosis of ureter by implanting the upper section into the lower through an opening made into the side, as recommended by Kelly.

DR. RANDOLPH WINSLOW, Baltimore—Four years ago, while doing a hysterectomy, I was unfortunate enough to cut a ureter. I was aware of the work that had been done by Van Hook and Kelly, and I made an incision into the ureter, slit it up, invaginated the upper end into the lower, closed the lower end over the upper, and the woman recovered. The operation, so far as I can see, does not differ from the one the reader of the paper has presented, except that I used silk instead of catgut. The woman has remained well for three years, and I believe is still so. All of these operations are merely modifications of the original method of Van Hook, and to him is due the credit for whatever success has been accomplished. The ureter is ordinarily described as terminating in the pelvis of the kidney, which, passing through the sinus of the kidney, forms a large dilatation within the substance of this organ. This description is not correct in most cases. The pelvis of the kidney is usually quite a small structure and may be either fusiform or triangular, or more or less quadrilateral in shape. From the pelvis issues several long and narrow tubes, usually at least four in number, which terminate in calices after passing a considerable distance, perhaps an inch or more in length. You may have a stone in one of these calices. You may pass a ureteral catheter, which will go into the pelvis or into one of the calices, but not in the one where the stone is impacted. A stone developed in the lower calices can not be reached in this manner.

DR. CARL BECK, New York City—It seems to me to be a deplorable fact that the great importance of the X-ray has not been realized by the profession, and I only wish that more such beautiful and convincing explanations could be brought forward. I agree with every point but one, and that is that the skiagraph will always show whether a renal calculus is present or not. I think this is a dangerous statement. Although a man's technique may be splendid, yet he may commit some errors. I would not make a diagnosis on a negative skiagraphic report regarding the presence of renal calculi. While a positive skiagraph is proof enough for the surgeon, a negative skiagraph will not convince me there is not a stone. In a doubtful case at least two short and long exposures should be made, and if possible, with different tubes. It has occurred to me that renal calculi, if they be elliptical in shape, may be confounded with biliary calculi, particularly if the body is irradiated in the oblique direction, so that the calculus appears near the spinal column.

DR. L. L. MCARTHUR, Chicago—I agree with the last speaker, Dr. Beck, that not all calculi may be expected to be found with the skiagraph. I believe Dr. Leonard, in his paper, failed to emphasize sufficiently the fact that the chemical composition of the calculus determines the intensity of the shadow. A uric-acid calculus will give a very slight shadow. I made some investigations before attempting to photograph a stone in the kidney, and on taking a group of calculi of varying types I found that a pure uric-acid calculus gave a very feeble shadow. Since that time I have cut down and found uric-acid calculi which were not demonstrable by the X-ray in the hands of our expert in its use, Mr. Fuchs.

DR. J. E. SUMMERS, JR., Omaha—Dr. Davis' technique is practically identical with that adopted years ago by Joseph Price, and is, I believe, as simple as any, being particularly advantageous when there has been a loss of substance and under such circumstances has an advantage over Van Hook's method. One must remember when operating on the ureter that it may have a fellow on the same side and that these ureters may come from a horse-shoe (more or less fused) kidney. Those who have examined the pathological exhibit will

remember a specimen of double ureter on both sides, there being two separate kidneys. Recently I did a left-sided nephrectomy for tuberculosis; the patient was a female, aged 2½ years. There were two ureters on this side and a large kidney. Fearing the possibility of a single kidney, I button-holed the other side in order to examine for the presence and condition of the right kidney. The kidney was removed together with the ureters, the lower one, which was healthy, was removed only in part, the upper, which was tubercular, was removed to below the pelvic brain. The child recovered nicely.

DR. F. B. CARPENTER, San Francisco—Seeing Dr. Kelly requested that all cases like his own should be reported, I will report one to add to the list. I brought the upper end of the ureter down to meet the lower, and consequently tied off the lower, destroying the mucosa that remained. I then made a large incision in the posterior wall of the bladder and passed three catgut sutures from within out to the upper end of the ureter. I brought the ureter firmly down from the lower angle of the incision into the bladder. When this was once secured I closed the remaining portion of the long incision into the bladder, and the patient made a good recovery, without any bad symptoms. I would call attention to an idea I derived at the time of the operation. It occurred to me that hereafter I could make use of Murphy's idea of a button in making anastomoses between the lower end of the ureter and the bladder. One important thing in this connection was that when the button came away it must necessarily fall into the bladder and could then be brought out through the urethra. The button will probably be slow, as our experience with buttons passing into the intestine has been. Phosphatic deposits may accumulate, which will cause some delay.

DR. C. L. LEONARD—I purposely refrain from speaking of the technique of skiagraphy, as I have just published a full description in the *Annals of Surgery*, and reported 59 cases in which I had found 12 calculi. Of those calculi a chemical analysis has been made, which shows that they consist of all the varieties except cholesterin. Uric-acid crystals, carbonates and phosphates are all represented. The reason why these calculi may now be detected by the X-ray, but could not at first, is due to the different quality of the X-ray I use. If you use a tube that has an equivalent resistance of 2 inches you will penetrate a uric-acid stone, but if, on the other hand, you use a tube which has a resistance of 1½ inches and will stay at that point, you will not penetrate the uric-acid stones. I found a calculus that weighed nine-tenths of a grain, while another of pure uric acid was removed from the ureter and weighed two and three-quarter grains. I had hoped to be able to show you some slides demonstrating these calculi. One of those slides shows 7 calculi that have been taken with different qualities of the X-ray, but with all other conditions equal. It shows that vacuum tubes will penetrate uric-acid stones, while the low vacuum gives opaque shadows, and it is this improvement in the technique that has made it possible for me to develop the method which I now use and which is so essential. I use a light which will not penetrate the less opaque calculi. If you get in the negative a shadow of the lumbar muscles you may be absolutely sure that no calculi exist. The only case of error in either positive or negative diagnosis was made in a case examined for Dr. W. W. Keen. It was an error in technique and served to show that the method *per se* was not at fault. The plate had not been placed sufficiently high, so that a calculus in the upper pole of the left kidney escaped detection. This error only served to strengthen my belief in the correctness of the method and helped to improve my technique. A second case for Dr. Keen showed that I was amply justified in my faith in the method, even to the extent of opposing his diagnosis. The symptoms were sufficiently severe to justify operative intervention, but the result of the operation coincided with the negative diagnosis. It is the method which I believe is absolute, the errors are entirely the result of faulty technique in applying it.

DR. B. B. DAVIS—I have certainly succeeded in getting some more cases reported, if my paper has accomplished nothing else, and this will make these cases of record. In practically all instances where there has been a ureter severed at the time

of operation the patient is exhausted and the surgeon is tired before the operation is completed, and consequently we want to have a method of repair which will be as simple as possible. The quicker it can be done the better will it be for all concerned. Kelly will do his operation very quickly, of course, but for the average operator I think it will take a good deal longer time than the method I have described. Although I do not profess to be a rapid operator, I am sure it did not take me more than five minutes to finish up the anastomosis. Catgut soon dissolves, so that there is no danger from the suture. As to the button which has been shown, I can not but think that with such a small lumen as it must necessarily have, it is almost sure to be filled by the phosphates before it separates.

WALLED OFF.*

JOHN B. DEEVER, M.D.

PHILADELPHIA.

"Walled off" is an expression so often heard in connection with cases of appendicitis which have gone on to pus formation, that I have selected it for the subject of discussion. When I hear a physician boast that his case has advanced to this stage and is ripe for operation, I often wonder how it is possible for him to have been unaware of the dangers to which the patient has been subjected and feel sorry for the physician's or surgeon's lack of knowledge of the living pathology of this vicious and deceptive disease that he has allowed to stealthily advance to that point where he believes he has every reason to congratulate himself. The most charitable view one can come to is, that he is familiar with operation under this one circumstance only.

In order to make clear the object of this paper, let me draw the comparison between an operation for walled-off pus, and operation in the absence of pus, or before the pus has become walled off.

Before taking up the arguments in favor of early operation and against late interference, let me impress on you that the mere evacuation of an appendicular abscess, drainage and subsequent healing of the abscess does not insure the patient immunity from a subsequent attack of appendicitis. A recurrent attack is much more likely to follow a partial operation—where the appendix is not removed—on account of the retention of an appendix which is damaged, therefore more vulnerable, and a fruitful source for subsequent attacks. That the appendix, in the case of a walled-off abscess, is frequently discharged as a slough, I am prepared to deny. The surgeon or the physician deceives himself, the patient, and the patient's friends when he makes this statement. I have repeatedly operated on cases in which a walled-off abscess had been drained, and in not one have I failed to find the appendix. At this time I may say that I have never met with a case where the appendix was normally absent.

Walling off of an abscess is nature's means of protecting the general peritoneal cavity. It has been stated that nature should be allowed to pursue its course unhampered by human skill or interference. The fact must not be lost sight of, that nature in dealing with pathological conditions does not work with the same immutability that it does with other normal laws, and therefore there is always to be considered the probability that the lauded and longed-for walled-off condition may fail to materialize, thus leaving the patient far worse off in the second state than he was in the first. The fact that a sufficient number of cases which constitute a large series is warrant enough for a decided opinion enables me to

* Read before the Profession of Springfield, Mass., at the Mercy Hospital.