ginal discharge from the second to the sixth day, at times quite profuse. A thorough examination of the patient at the end of the fourth week showed no laceration of the cervix or perineum, and an involuted uterus. The antiseptic precautions used were those suggested by Dr. Wm. L. Richardson in his paper before this Society, and included a thorough washing out of the uterus at the close of all the intra-uterine manipulations with a hot corrosive sublimate solution, 1-2000, followed by a douche of boiled water.

The points that clinically renders this case of especial value, and in which it differs from any other that I have found recorded, is the evident persistence of the cervix as an independent portion of the uterus throughout labor. The ring of contraction in this case certainly could not have been formed from the os internum, because the cervix dilated *en bloc*, as it were, never thinning out or retracting. Also, there was not, as is usually noted, a prominent caput succedaneum, but the head showed rather the absence of a strong propelling force, and this is entirely in accord with the condition as I found it under ether, that is, paralysis of the muscular structures below the ring of contraction, so that, even during a pain, the cervix could be readily stretched, and the lower portion of the uterus exerted no pressure upon the hand, which, above the ring, was firmly grasped; nor did the walls mould themselves closely about the foetus, that is, the only expulsive force was in the small portion of the uterus which lay above the ring of contraction.

I did not recognize that marked attenuation of the lower uterine segment, which must have existed had this annular contraction been the usual ring of contraction, formed by shortening and thickening of the longitudinal fibres. Another point that impressed itself upon me was that, at the end of a pain, the head, even after it had entered the pelvis, receded as though snapped back by an *elastic force*, and the uterus immediately resumed its high position. In fact, I could observe no descent of the fundus during the labor. The abdominal depression has often been observed, but this fact, as a fact, had slipped from my memory, so I did not, perhaps, assign to it its proper diagnostic value.

I have ventured to separate from the mass of clinical details these especial points, and so isolate them into prominence, for I believe them to be in a measure diagnostic of a most dangerous complication of labor. The more dangerous because the fatal termination comes suddenly, with no warning, and where all seems well, except that progress is slow. I think, in this case, my patient was in danger perhaps several hours, while I, feeling that ineffectual pains, the usual accompaniment of a posterior position, had wearied the uterus, waited until after a good rest, strong expulsive pains should start up and terminate the labor. I interfered finally, not from any sign of exhaustion, maternal or foetal, but because the head having partially engaged, and making no advance, I wished to avoid too long-continued pressure. Perhaps, in another case, if the labor were tedious, I should sooner introduce my hand for exploration, but I cannot see at what time I could have been certain that I was dealing with a worse complication than inertia uteri and a posterior position of the occiput.

With regard to treatment, I propose to say but one word. There is the choice between forceps, version, or laparotomy, for I cannot think that a case of tonic annular contraction can terminate favorably, unaided.

I differ, perhaps, from most in that I should make a decided effort to employ the forceps before resorting to version, unless I found a decidedly narrow pelvis or a rigid cervix, either of which conditions, by increasing the resistance, would favor rupture of the uterus. I should elect forceps because it appears that it must be easier to drag the foetal body through the constricting ring than to double that same body upon itself, and thus force it through by version. In performing version, bring *both* feet down before attempting to turn, otherwise a foot or knee hitching above the ring may defeat all efforts. The question as to the limits within which version is safe must be decided in each individual case. In the case reported, the lower portion of the uterus not being as thinned as in those cases where it is formed from the dilated cervical portion, I think version was a fairly safe procedure; but in the thin, paper-walled uterus of the true ring of contraction, if a gentle attempt at version fails, I decidedly favor Cæsarian section.

A FEW STATISTICS ON THE COMPARATIVE FREQUENCY OF THE CHANCROID.¹

BY F. B. GREENOUGH, M.D.

THE portrait of the chancroid, as given by the textbooks, is certainly clear, concise, and clean cut, such as would lead one to suppose that there would be no difficulty in recognizing a typical case of this lesion. An ulcer, starting as a pustule, appearing always in a few days, as the result of contagion from a similar one, having decided and well-marked characteristics as to its base and edges, with a tendency to affect the lymphatic glands in its vicinity, and capable, in its turn, of reproducing itself by contagion, is certainly very different from any other cutaneous lesion.

In spite of this individuality, the very different results given by the statistics of different observers as to its comparative frequency, would seem to show that they cannot have used the same rules in their classification of venereal lesions. Through all this diversity of statistics, however, a pretty constant regularity is to be found in two directions: first, that since the time following the period, which Bumstead has so appropriately called that of confusion, in venereal disease, when the chancroid began to be accepted as distinct from the true syphilitic chancre, there has been a steady diminution in its comparative frequency; and second, that it is found to occur much oftener in hospital or public practice than in private; in other words, that the lower classes are more subject to its contagion than the upper ones.

A marked exception to the first statement is, however, noted and exhaustively treated of by Mauriac, when, at two different periods, in 1870 and 1871, the chancroid increased in a most extraordinary manner for a time in Paris. The early statistics on this subject give, out of 341 venereal ulcers observed at the Midi, 215 chancroids, in a period of three months. M. Puche collated all cases at the same hospital from 1840 to 1850, getting a total of 10,000, the ratio being nearly 4 to 1 in favor of the chancroids, that is, 8,045 to 1,955. These figures are taken from an early edition of Bumstead's admirable work, in which it is also stated that the French observers have found that

¹ Read before the Association of Genito-Urinary Surgeons, at its first annual meeting, May 17 and 18, 1887.

in private practice this ratio is reversed, that is, that the true syphilitic chancre occurs more frequently than the chancroid. In a table drawn up by Fournier, the proportion given is: chancroids, 82; syphilitic chancres, 252; ratio, one-third. Fournier's explanation of this difference is that men of the lower classes frequent old prostitutes, who, being syphilized, are protected from another attack, whereas they are liable to an indefinite number of chancroidal contagions.

This explanation would be perfectly satisfactory were the primary lesion the only source of syphilitic contagion in a woman, but inasmuch as the secondary manifestations, such as mucous patches, broken-down papules, etc., are equally so, it is hardly conclusive.

From my own experience, and judging from the comparatively few cases in which I have been able to examine a suspected woman, I should say that the cases where a man was infected from a primary lesion were very rare, and that in the great majority of cases the source of the contagion was some constitutional manifestation, of the existence of which the woman often was unaware. That this difference in ratio exists between hospital and private practice is shown by several of the best French syphilographers' statistics. Is it not possible that many cases may apply for treatment as out-patients on the first appearance of a venereal sore, which looks like a chancroid, and is entered on the books as such, but which, if kept sight of and watched, as would be the case in private practice, would be found, in the course of time, to develop induration, inguinal reaction, etc., and turn out to be a case of syphilitic infection? That a large proportion of the cases from which the statistics are derived in Paris are only seen as out-patients, is shown by Mauriac's tabulation of his cases at the Midi, during the year 1880, from which it appears that, out of 969 cases of chancroid without buboes which were seen, 80 were admitted into the hospital, and 889 were "consultants." To return to the comparative frequency, it can be said that all the earlier statistics give to the chancroid a very decided preponderance numerically over the true chancre, Bassereau placing the proportion during 1837-1838, in Ricord's service, as high as 30 to 1! Later observers have universally noted a change in this respect. The edition of Bumstead and Taylor of 1879, which gives a very complete *résumé* of the literature of the subject up to that date, gives the ratio of the chancroid to the true chancre, during the years 1869 and 1870, as nearly two of the latter to one of the former.

Mauriac, in his "Maladies Vénériennes," 1883, treats this subject most thoroughly, and, without going into details, it may be said that there was a gradual diminution in the frequency of cases of chancroid, relatively to those of the true chancre, up to 1870-71, during which year the ratio of 3 to 1 was reached. From 1871 to 1875, the relative frequency of the chancroid decreased, its ratio being in that year as low as 1 to 10. This ratio increased again from that period up to 1880, when it was a little over 1½ to 1. These two rises in the ratio, that is, in 1870-71, and after 1875, are considered by Mauriac to be due — the first to the Franco-German War, the Commune, and sieges of Paris, and the consequent license; and the second to the great influx of workmen to prepare for, and strangers to visit, the Exposition in 1878. His explanation of the steady tendency of the ratio to decline when no such disturbing factors exist, will be re-

ferred to later. The first series of statistics of my own that I have to give are, in certain respects, very unsatisfactory. They are taken from the case-books of my service in the Department for Skin and Venereal Diseases at the Boston Dispensary, starting from July 1, 1873. Having carried them up to March 1st of the present year, they cover a period of thirteen years and nine months. The entries in the books give the name, age, sex, residence, and diagnosis of the case on the date of the patient's first applying for treatment. Future developments are not recorded, as a rule, although there is a space of about two inches on one line, under the heading of "Remarks," which can be utilized in any case which is especially interesting or important.

The system of classification which I have adopted for lesions supposed to be the result of venereal contagion, is to enter as chancroids those cases which show typical chancroidal character, and as primary lesions, or indurated chancre, those that evidently belong in that category. The quite large number of cases, which, when first seen, are doubtful in their nature, are entered as chancres. Besides these three classes, my records show that a certain number of patients came for advice, supposing that they had contracted venereal disease, who had herpes progenitalis. These Dispensary statistics, therefore, will not give the proportion of cases of chancroid to those of true chancre seen, but will show that out of so many cases of venereal sores, a certain number were chancroids. Inasmuch as a case would be put down in the doubtful class, because there was something about it which pointed to the possibility of its turning out to be a true chancre, the chances are that the great majority of these doubtful cases did, in time, develop into such, and I am convinced that such has been the case, but it is only a conviction, not backed up by evidence. Again, some of the cases entered as chancroids, later developed induration, inguinal adenopathy, and were followed by secondary symptoms. The ratio, as given by my table, between the chancroid and other lesions, is probably not very different from what it would be between the chancroid and true chancre, had it been possible to follow up each case until its character was pronounced.

CASES OF VENEREAL SORES SEEN AT THE BOSTON DISPENSARY, FROM JULY 1, 1873, TO MARCH 31, 1887. SERVICE OF F. B. GREENOUGH, M.D.

YEAR.	Chancroids	Doubtful	Indurated Chancre	Herpes Progenitalis	Total	Ratio of Chancroids to total
1st. July 1, 1873 to June 30, 1874	7	41	3	5	56	1 : 8
2d. July 1, 1874 to June 30, 1875	11	48	5	0	64	+1 : 6
3d. July 1, 1875 to June 30, 1876	2	49	8	0	59	-1 : 28
4th. July 1, 1876 to June 30, 1877	41	65	14	0	120	+1 : 3
5th. July 1, 1877 to June 30, 1878	57	61	15	1	134	-1 : 2
6th. July 1, 1878 to June 30, 1879	21	41	7	0	69	-1 : 3
7th. July 1, 1879 to June 30, 1880	37	47	14	1	99	+1 : 3
8th. July 1, 1880 to June 30, 1881	45	74	14	5	138	-1 : 3
9th. July 1, 1881 to June 30, 1882	48	75	11	4	138	+1 : 3
10th. July 1, 1882 to June 30, 1883	37	76	15	5	133	+1 : 3
11th. July 1, 1883 to June 30, 1884	22	93	26	10	151	+1 : 7
12th. July 1, 1884 to June 30, 1885	24	125	34	13	196	-1 : 8
13th. July 1, 1885 to June 30, 1886	14	83	43	6	146	-1 : 10
14th. July 1, 1886 to Mch. 31, 1887	25	53	10	2	90	+1 : 4
Total	391	931	219	52	1593	-1 : 4

The absence of something corresponding to my

"doubtful" column in the French hospital statistics, seems to me to detract somewhat from their value, as there are certainly many cases, which when first seen, no diagnostician, however skilful he may be, can determine the nature of with certainty. A consideration of the preceding tabulation shows, that while there are some irregularities in it, notably in the third year, where a most extraordinary and inexplicable divergence is found, there exists a fairly constant ratio, the frequency of the chancroid rather diminishing during the last four years, and in all of them being less than that given by the earlier statisticians.

The second set of statistics which follow, while much less, numerically, are more reliable and consequently more important. They are collated from a hundred cases that have consulted me in private practice, for venereal lesions, or lesions supposed to be venereal following exposure. In these cases the diagnosis was not made at the first visit, but has been taken from the case-books as resulting from a continued observation of the patients. The cases have not been selected, but have been taken by going back from the present time until a hundred were obtained. Even under these favorable circumstances I have found it necessary to have a column for doubtful diagnoses. It is of course a very much smaller number that come under this head, than in the Dispensary practice, but nevertheless there are certain patients, some of which I saw only once in consultation, at which time the diagnosis was doubtful, and others of whom I lost sight before the true character of the lesion could be definitely settled. The following is the tabulation of 100 cases seen in private practice.

Chancreoids	10
True Chancres	63
Doubtful	13
Herpes Progenitalis	14
Total	100

Ratio of chancroids to total lesions $\frac{1}{10}$.

Ratio of the chancroid to the true chancre, a little less than $\frac{1}{5}$.

These statistics therefore agree with those of other observers both as far as showing that the chancroid is less frequent in hospital or dispensary practice, than in private, and that it is very decidedly less seldom seen at the present time than formerly. What the cause for this falling off in its comparative frequency is, is a very interesting question, on which I have a few suggestions to offer. Mauriac, who devotes several chapters to the discussion of this subject, gives reasons, which with one exception, which will be referred to later, are strictly local. Briefly stated these are: the more perfect police regulations regarding the tolerated houses of prostitution and registered prostitutes, and the great improvements that have been made in the general sanitary and hygienic condition of Paris. He refers to the fact that a chancroid in a woman is apt to be a more decided lesion, and more easily seen on examination than some of the syphilitic lesions which would be contagious. This fact is of some importance with us, as although we have no system of inspection of public women, one can easily conceive that that class of prostitutes who depend upon the men of good social position for their living, would not be likely to infect a patron knowingly, whereas they might conceivably give him syphilis from some relapse of which they themselves were entirely ignorant. In 1876, I read a paper before the Boston Society for Medical

Improvement "On the Treatment of the Chancroid," the main object of which was to protest against what at that time, was universally laid down in the text-books as the proper treatment for the chancroid, destructive cauterization, which was, however, even then, by no means invariably practiced. The reasons advanced were briefly, the painfulness of the treatment, the almost certainty of producing an inflammatory phymosis in cases where the prepuce was long, the masking of the diagnosis by setting up a pseudo induration of inflammatory origin, the likelihood of starting a suppurating inguinal adenitis, and last, but most important, the fact that the lesion did better when treated in a milder way. The ten years and more that have elapsed since then have only confirmed me in the correctness of the views advanced, and I have not since then once used cauterization in the treatment of a chancroid. It may not be apparent what connection there is between the treatment and the frequency of this lesion, but if I am correct in assuming that destructive cauterization is worse than useless, the older syphilographers were entirely mistaken, and such men as Ricord, Diday and Clerc went on for years needlessly putting their patients to great pain, unless there has since their time been some change in the character or type of the disease. And such I believe must be the case. In the paper which I have referred to, which appeared in the *Boston Medical and Surgical Journal*,² I made the statement, that in my opinion, besides being less commonly met with in practice than formerly, we did not see such severe cases of chancroid, one destroying much tissue, and resulting in a deforming cicatrix, being very rare. Of the correctness of this statement also, I am still further convinced, not having seen since that time a single case where decided loss of tissue resulted from a sore, which came under my observation in an active state. I have no doubt but what, if I visited Deer Island or Tewksbury, or the Chelsea Marine or Naval Hospitals, I could see such, but I believe many fewer than twenty to thirty years ago. It was with much pleasure that I came across the following statement by Mauriac, which I translate. After describing the serious result of phagedenic action on chancroids in the past, he says,³ "Phagedenism which so frequently formerly interfered with the cure of chancroids, which made them very serious and even disastrous affections, has become an entirely exceptional phenomenon. We scarcely see two or three cases yearly in our service, and even those are of a benign character, and are not difficult to control."

That a disease may in the course of time become modified as to its severity, is, I think, shown in the case of syphilis, as far as its manifestations on the skin go. We certainly see fewer of the ulcerative syphilides in proportion to the number of cases treated at the present time, than were seen twenty or thirty years ago, judging from the literature on the subject, and also from the cicatrices seen in old cases. That this difference is in a great measure due to the improved treatment, I have no doubt, but we do see certain cases where through ignorance, or recklessness, practically no treatment at all has been taken, and yet the patients never have any relapse either in themselves or offspring. It must, however, be said that while the large majority of cases of syphilis are com-

² Vol. xcvi. January 11, 1877, No. 2.

³ Leçons sur les Maladies Vénériennes, Paris, 1883.

paratively benign, the lesions of the nervous system are much more frequent than formerly, especially during the early stages of the disease. We must either admit that these nervous lesions have increased in frequency, or else assume that the older writers did not recognize them, and it hardly seems possible to suppose that such close observers could see young patients showing cerebral or spinal symptoms, during an attack of syphilis, as often as we do, without connecting the two together. Apart from the question of possibility of change in type in the chancroid, what other reasons are there for its less frequent occurrence? The extreme predominance numerically, given to it in the earliest statistics, might be perhaps explained by the fact that it had just been discovered, so to speak, and that Ricord's and Bassereau's writings called attention to the fact that there was such a lesion as a local, non-syphilitic sore, and consequently everybody was anxious to avail themselves of the new doctrine. In other words, for a certain time, chancroids were the fashion in the medical world, and very possibly every lesion that did not show marked induration and glandular enlargement in both groins, was recorded as such. Inasmuch as the earlier syphilographers make no mention of herpes proies, and as their treatment was to cauterize all suspected lesions following exposure, it is more than probable that at that time, many crops of herpetic efflorescences may have been diagnosticated as chancroids. There is another, not very uncommon lesion, which may have been mistaken for a chancroid, as is not unfrequently done at the present time. I refer to an inflamed sebaceous follicle, on the shaft of the penis, which begins as a pustule with an inflammatory areola, and which when the pustule is destroyed leaves a circular, punched-out looking ulcer, which will only require one or two applications of some strong caustic to make it a very good copy of a chancroid.

There can be no stronger proof that some modification must have taken place in the chancroid, a lesion which as described, is one most essentially *sui generis*, than the fact that some of our most distinguished syphilographers have gone so far as to deny its individuality, or at least to claim, that a lesion which is identical with it, may result from immediate contagion from, or artificial inoculation with, any pus resulting from inflammatory action.

That pus inoculated will produce a pustule, has been proved, but that healthy, non-septic pus brought in contact with the cutis or a mucous membrane can cause an ulcer resembling the chancroid, has not, at least in my opinion. When we think of the enormous number of cases where inflammatory pus is brought in contact with the skin of the patient suffering from the inflammation, as in cases of amputation, operations for tumors, abscesses, in short, all surgical operations that do not heal by first intention, in which nothing like a chancroid is found, and then remember what every practitioner who sees venereal disease must have observed, namely, cases where somewhere in a circular line, drawn on the lower abdomen and inner part of the thighs, a circle of which the penis is the radius, a chancroid is seen as the result of auto-inoculation from chancroidal pus flowing from the orifice of the prepuce, we must admit that the chancroid is a fact, even if not as frequently seen as formerly.

There is one form of venereal lesion of which I have seen a great many examples, to which I

want to refer in this connection, and that is what has been very thoroughly described as the *ulcus exulcerans*, or *elevatum*. The reason that I do refer to it, is, that I have often been in doubt as to its nature, that is, as to whether it belonged to the chancroids, or true chancres, and have thought that it suggested the "mixed chancre" of Diday. The lesions I refer to are always multiple, situated on the free margin of a long prepuce, suggesting inoculation from a sub-preputial sore, or auto-inoculation from each other, or both. They are not larger, as a rule, than a small pea, raised above the niveau, ulcerated on their summit, with the base of the ulceration like that of a chancroid. They usually are situated in a fold or wrinkle of the free margin of the prepuce, which fold when the prepuce is retracted as far as the usually accompanying phymosis will allow, is reproduced in the lesion as a split. They do not show any marked induration, but evidence of an indurated sore is apt to be obtained under the prepuce. They are always most obstinate against treatment, although they do not show any decided tendency to increase in size. They almost invariably are followed by secondary symptoms. In the few cases in which I have experimentally used specific constitutional treatment, the duration of the lesion has been somewhat, but not markedly shortened.

In cases where we get evidence of an indurated chancre under the prepuce, of course the consequent secondary symptoms are explained, but what is the nature of the raised lesions at the preputal orifice? Their situation is exactly that which would suggest auto-inoculation, namely, in the folds where the sub-preputial secretions would be retained, and also their position with regard to each other is very suggestive, as they are very apt to be found in pairs which come together in exact apposition. Their situation at the preputial orifice also would suggest the possibility of the daily irritation during the act of micturition, having something to do with their obstinacy against treatment, and also, perhaps, having some influence on their peculiar raised character. Certainly I have never seen typical specimens of the lesion I refer to, except in this locality, although something like them is seen at times as a single lesion, in the sulcus, or on the reflected prepuce. I must confess that I have at times in these cases been tempted to make an exception, and try the use of destructive cauterization, but the fact that an already troublesome inflammatory phymosis would be decidedly aggravated, has deterred me from so doing.

There is one other respect in which I think the chancroid of the present day differs from that of the past, and that is in its being complicated by a suppurating, or virulent bubo. I will not take time to go into statistics of the past, as I have none of my own to offer, except to quote from Rollet's service at Lyons, where out of 140 cases, 60, or 1 to 2½ had virulent buboes. The fact, however, that chancroids were liable to have this complication, has been universally admitted, in fact it has been made one of the important diagnostic points. Without having any evidence to fall back upon, I should say that in my experience virulent adenitis was not more frequently seen with the chancroid, than with the true chancre, and I cannot help thinking that this is due in a measure to the fact that irritating the lesion by caustics is at the present time not usually practised. This is, of course, only my opinion, but I have seen some cases

where an acute adenitis followed at once on the use of caustics.

There are, of course, many points of interest with regard to this subject that I have not even touched; all I have attempted to do has been to lay before you some statistics which I had, with a few suggestions resulting from my observation of the cases. These statistics would go to show, what others on the same subject do, that the chancroid is decidedly less frequent at present, than it was formerly; and in addition to the reasons I have given that may have had an influence on that change, I must say, in conclusion, that I believe chancroids are less frequent because we have in a great measure ceased manufacturing them, by cauterizing every sore that is not a typical primary lesion.

ABSCESS OF THE KIDNEY FROM OBSTRUCTION TO A URETER.¹

BY JOHN G. BLAKE, M.D.,
Visiting Physician, Boston City Hospital.

MR. B. S., thirty-five years old, first came under treatment five years ago for irritation of the bladder, and symptoms suggesting stone. Pain in the region of the left kidney was constant, but not of such a severe character as to draw attention away from the bladder, which seemed to be the seat of the severest pain. The urine, at this time, was examined frequently, and showed the ordinary conditions found in cystitis: pus, some blood, epithelial scales, and mucus. The treatment was directed chiefly to the bladder, and the existence of a renal abscess was not suspected. In three weeks after beginning the treatment, a quantity of dark-green pus, resembling chewed grass, was found in the vessel, mixed with urine, the quantity of pus being estimated at ten ounces. After a few days, the pain in the side subsided, but the vesical irritation continued without much relief. Permission for an examination under ether was at last obtained, and the bladder carefully sounded, but no stone could be detected. The general condition, however, improved a good deal, and he passed from under my care to resume business.

Six months after I received a note from him, inviting me to call at the Massachusetts General Hospital. He had been again under treatment, and had been examined by Dr. Bigelow, who found a stone, crushed, and removed it. It may here be stated that, three years before this, the patient was attended by a member of this Society, for what proved to be an abscess of the kidney, which ran its course without giving rise to severe constitutional symptoms. At least, this was the patient's statement.

The present attack began February 20, 1887. Since the termination of the illness attending the removal of the stone, Mr. B. had been quite well, and attending to business that necessitated much railroad travel. It was to exposure to cold during one of his frequent trips, that he attributed his last illness. Pain in the side, extending down toward the groin, and resembling that of an attack of gravel, was the first symptom. Local treatment relieved this somewhat, but never wholly. The urine showed no sign of either gravel or pus. Day after day, the pain continued, with a ris-

ing temperature and pulse, and constantly requiring the free use of morphine subcutaneously. On the seventh day he had a chill. There was no doubt about the diagnosis, and the treatment aimed at saving strength and pain until the abscess should evacuate through the ureter and bladder, as it had done twice before. After twelve days, the symptoms began to look more serious, and surgical counsel was sought, with a view to interference, if it were deemed advisable. Dr. Gay examined the case, but decided not to operate, the external evidences of the presence of pus, and the uncertainty of the result making this course, in his opinion, the safest. The favorable termination of the previous attacks justified this conclusion, and held out reasonable hope of a spontaneous cure.

The following day, severe pain, extending toward the nipple, and attended by difficult and gasping breathing, was complained of. It was evident that inflammatory action was progressing upward, and percussion and auscultation gave signs of lung invasion and effusion. From this time, the patient began to sink rapidly. Dr. Cabot saw him next morning, but too late to attempt an operation, even if it had been considered advisable. He died in the afternoon. This is the outline of the case in brief, omitting all details, which are not of necessity. It is reported chiefly for the purpose of bringing before the Society the subject of surgery of the kidney, and the question of operation in cases of like character. It was of special interest to me from the fact that I had, at the same time, two cases of chronic pyelitis under my care at the City Hospital, which did not improve under medical treatment.

As the autopsy showed, there was no possibility of a successful operation in the case. At the same time, the conditions found were of such an unusual character as to be seldom met with. I shall not presume to discuss the surgical aspect, leaving that to others better qualified, but shall be glad to hear the sense of the Society on the general question, as a guide for the future. It may not be out of place to state that the subject of the removal of the kidney was discussed with the patient and his relatives during the early stages of the disease, and would have been agreed to had the present attack terminated favorably. The obstruction of the ureter, found at the autopsy, was suspected as the probable cause of the abscess not emptying, but could not be demonstrated during life. The uncertainty of the duration of the abscess of the kidney before finding an outlet into the surrounding parts, together with the poor success generally attending surgical interference, were considered additional reasons for not operating.

So far as I can learn from conversation with surgeons, the surgical treatment for abscess and kindred affections of the kidney, and the removal of that organ, have not been attended with a degree of success, in this community, which would encourage such methods. Still, these operations have very frequently been performed successfully in other places, and I am not willing to admit that we are wanting either in the skill or the means to obtain equally good results. Larger experience in this, as in other fields, will undoubtedly lead to improved results; and, although the cases are comparatively rare, a sufficient number will come to us to furnish an improvement in statistics, and to stimulate us to more frequent operations.

¹ Read at the Boston Society for Medical Improvement, April 25, 1887.