

THE  
CLINICAL HISTORY  
OF  
INTERMITTENT AND LATENT  
ALBUMINURIA.

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IN a leading article on "Albuminuria in Life Assurance," which appeared in the JOURNAL on January 5th, the following statement is made:—"Dr. George Johnson believes that albuminuria always indicates a condition of renal stress which sooner or later leads to organic disease unless this tendency be combated by very careful regimen. His long experience undoubtedly gives weight to his views, but *it may fairly be doubted whether, having failed for so many years to recognise the class of cases now referred to, he is the best authority as to their subsequent progress.*" The cases referred to are cases of chronic but presumably curable albuminuria. The italics are mine.

If the writer, whom, for brevity's sake, I shall in the course of this paper designate by the algebraic sign X, had made himself as thoroughly acquainted with all that I have published on the class of cases in question as he appears to be with the writings of Dr. Saundby, to whom he twice refers in the course of his article, he would not have made the statement which I have quoted in italics, and the inaccuracy of which I shall have no difficulty in demonstrating. It is probable that many readers of the JOURNAL are not better informed than X with regard to the history of this very important and interesting subject; I therefore propose, as briefly as is consistent with clearness, to set forth certain facts which may tend to elucidate the matter.

Dr. Moxon, in his paper "On Chronic Intermittent Albuminuria" in the *Guy's Hospital Reports*, 1878, is credited by X and some other writers with having directed attention to a class of cases hitherto unrecognised. Now I should be most unwilling to deprive a physician, who is no longer amongst us to speak for himself, of any credit that may be his due, but, having carefully read the paper in question, I cannot refrain from expressing my surprise that any physician who has had a large experience of cases of albuminuria should look upon that paper as an important contribution to clinical medicine. The cases, seven in number, are most imperfectly recorded, generally without dates, so that in most instances the duration of the albuminuria cannot be ascertained. Case v appears to have been the most chronic, its duration having been two years.

Cases VI and VII afford the most remarkable illustration of inexactitude on the part of the author. Mr. — at 20 had albuminuria always in the evening from September, 1877, "up to the time at which I write"—the date of writing is not given. The history of this case concludes with the following remarkable prediction: "I have no doubt that in a few months he will recover from this state of weakness." Whether this young man did recover, or whether he has since died or is living in an advanced stage of Bright's disease, we have no means of knowing.

Case VII was seen only once, with "urine considerably albuminous." The urine was not afterwards sent for examination; but Dr. Moxon says: "I mention the case because, eighteen months before, the lad was under my care for subacute pleurisy of the left side, lasting some months. I examined the urine repeatedly whilst he was under that trial, but it was never albuminous." Apparently it is assumed that because the urine was free from albumen eighteen months before it would cease to be albuminous at some subsequent period, and these two last imper-

fectly recorded cases are included amongst cases of cured, or at any rate curable, albuminuria.

But the greatest defect in Dr. Moxon's paper is that in not one single case does he attempt to trace back the albuminuria to its probable exciting cause. He speaks of the "albuminuria of adolescence," as if to give the condition a name were to explain it; and he quotes with gratification and approval the following passage from a letter addressed to him by Sir William Gull: "I have so often found albumen in young men from the age of sixteen to twenty-two that I regard it as common, and I have attributed it to atony of vessels and nerves, and, in fact, the urine is otherwise normal. I remember that when Dr. George Johnson brought forward two hundred instances of albuminuria, I asked what worth the mere statistics of albuminuria had, since it occurred in young and growing men and boys almost as frequently as spermatorrhoea." The statistics to which Sir William Gull thus refers are contained in a paper "On the Etiology of Albuminuria as Deduced from an Analysis of 200 Consecutive Cases."<sup>1</sup>

We can easily understand that a physician who thinks that the hypothesis of "atony of vessels and nerves in young and growing men and boys" affords a satisfactory explanation of albuminuria would pay little heed to an attempt to arrive at the real etiology of precisely similar cases of albuminuria occurring, not only in young men and boys, but in both sexes and at all periods of life from childhood to extreme old age.

I have for many years endeavoured to prove that, in the great majority of cases, the exciting cause of albuminuria may, by a careful inquiry, be ascertained. My most recent contribution to this part of the subject is contained in two papers published in this JOURNAL (March 3rd and July 14th, 1888), recording a series of cases of albuminuria the result of sewage-poisoning.

Since it has been suggested that I am indebted to Dr. Moxon's paper for the knowledge of the existence of such cases as he describes, I feel called upon in self-defence to indicate that the paper in question affords internal evidence that Dr. Moxon may have obtained from my published *Lectures* his knowledge of the fact that albuminuria is often intermittent.

In giving the history of Case II (p. 239) he says: "At that time [date not given] I was not in the habit of making observations on variations of the urine in these cases at particular hours of the day, and cannot say at what time of the day the albumen was present or absent." In the report of Case III it is stated that the urine was tested morning and night, and that "the albumen was always found in the night urine, if at all." Now at page 44 of my *Lectures on Bright's Disease*, published in 1873—five years before the publication of Dr. Moxon's paper—will be found the following paragraph: "Let me impress upon you one point of practical importance. Before you pronounce a patient to be entirely free from his malady, be careful to test the urine, not only after rest and fasting—that is, in the morning before breakfast—but after food and exercise. Albuminous urine is usually more copiously so after food and exercise; and you will sometimes find that while the urine before breakfast is quite free from albumen, that which is secreted after a meal is decidedly and even copiously albuminous. In some cases exercise has even more influence than food in exciting renal congestion and albuminuria."

This paragraph, published more than fifteen years ago, and indicating that albuminuria is often an intermittent condition, is a summary of the results of many previous years of clinical observation and teaching, and I would ask X if he can refer me to any previous publication in which the importance of testing albuminous urine at different periods of the day has been taught. Further, I venture to say that it would not be an unwarrantable assumption on my part if I suggest that Dr. Moxon, in the interval between his observation of Cases II and III, had referred to my *Lectures on Bright's Disease*, and had thence derived the information that the urine might be albuminous at one period of the day, and not at another. We shall presently see that the most numerous cases of intermittent albuminuria are found amongst the convalescents from acute nephritis.

At page 235 of his paper, Dr. Moxon refers to what he calls the interesting observations of Dr. Mahomed, who found "albuminuria in several members of a small group of healthy people after bathing in the sea." If he had been so disposed, he might have referred to a paper of mine on "Cases of Temporary Albuminuria the result of Cold Bathing." This paper was published in the seventh volume of the *Proceedings of the Clinical Society*, and I

<sup>1</sup> JOURNAL, August 2nd, 1873, p. 112.

believe that before that time the attention of the profession had not been directed to this frequent cause of albuminuria.

The four cases there recorded, four years before the publication of Dr. Moxon's paper, are examples of temporary or so-called intermittent albuminuria traced to their exciting cause, therefore more instructive than Dr. Moxon's cases, and, like some of his, terminating in recovery.

Towards the conclusion of the article in question, X says "it is conceivable that, dyspepsia and the consequent dyscrasia persisting, chronic renal disease would, in course of years, be brought to pass. But the space of time required to permit of this evolution is so long that we are not yet in a position to say whether this has occurred." In my *Lectures on Bright's Disease*, pp. 40, 64, and 79, I have given what I consider sufficient reason for my belief that renal degeneration is often a consequence of the long-continued elimination of products of faulty digestion through the kidneys. Dr. Murchison, in his work on *Functional Derangements of the Liver*, second edition, pp. 80-81, quotes my description of this class of cases, and then says, "Numerous cases, which have come under my own observation, and which I have carefully watched, have satisfied me of the strict accuracy of Dr. Johnson's description."

Although the writer X is at present of opinion that "we must wait almost another generation before this question can be finally set at rest," he will, perhaps, after a few more years' experience, find himself not indisposed to confirm the experience of Dr. Murchison and myself as to the causal relationship between some forms of chronic dyspepsia and degeneration of the kidneys. So long ago as the year 1852, in my work on *Diseases of the Kidney*, pp. 107 to 110, I described the structural changes in the kidney, which often result from the copious excretion of glucose in one class of cases and of bile in another.

As it has been suggested that I am disposed to over-estimate the evil results of albuminuria, I will here give an abstract from my *Lectures on Bright's Disease*, p. 42, an extract which expresses the views of prognosis which I have held and taught for the last thirty years.

"Acute Bright's disease has a tendency to terminate in complete recovery. It is essentially a curable disease, as much so as acute bronchitis or acute pneumonia. The earlier the patient comes under treatment the better is his prospect of recovery; and on the other hand, the longer the symptoms have continued without signs of amendment, the more grave does the prognosis become. The prognosis is, on the whole, more favourable in the young and middle-aged than in those more advanced in years; but the disease may prove mild and tractable even in very aged persons. For obvious reasons, the prospect of recovery is better in the case of those who can avoid exposure to cold and other injurious influences than when the patient's circumstances are less favourable. In favourable cases a copious secretion of urine, of comparatively low specific gravity, and of pale colour, with a diminishing amount of albumen and decrease of dropsy, are amongst the earliest signs of amendment. Albuminuria is usually the last symptom to disappear. The time of its disappearance varies in different cases of recovery from a few days to many months. If the urine continue albuminous for more than six months, it becomes more and more doubtful whether it will ever cease to be so;<sup>2</sup> but I have seen cases of complete recovery after albuminuria had continued for one, two, and even three years."

Recently, on looking over my private casebooks, I find that in two cases of acute nephritis albuminuria had been more or less constantly, though often intermittently, present for a period of five years before its final disappearance, and I shall presently give the history of a recovery from an intermittent albuminuria of seven years' duration. The extract from my *Lectures* which I have before given will have shown that patients who are recovering from an attack of acute nephritis often pass through the stage of intermittent albuminuria on the way to complete convalescence. In fact, the only exceptions to this rule are cases of quick recovery from an acute attack while the patient is still in bed and restricted to liquid food. If, on the other hand, the albumen persists while the patient is walking about and taking more or less solid food, it will often be found that for a variable period the morning urine is free from albumen, while that which is secreted after food and exercise continues to be more or less albuminous.

Here I may remark that it has happened to me on several occa-

sions to be consulted by a patient who, his morning urine alone having been tested, had been declared free from an acute attack, when the subsequent history has shown that the disease had persisted and ultimately resulted in structural degeneration of the kidneys.

The next point to which I now desire to attract attention is this: that not only do most cases of acute nephritis pass through the stage of intermittent albuminuria on the way to convalescence, but that the majority of cases of intermittent albuminuria as they present themselves in the consulting room may on careful inquiry be traced back to a previous more or less remote attack of acute nephritis. This has been the result of my experience for many years past.<sup>3</sup> At the present time I have under observation a considerable number of cases of so-called intermittent albuminuria, and taking from my notebook twenty successive typical cases, I find that in no less than fourteen out of the number there is a distinct history of an antecedent acute nephritis, the cause of the acute renal attack being in five cases scarlet fever, in four exposure to cold and wet, in two repeated cold bathing, in one diphtheria, in one rheumatic fever, and, lastly, in one sewage poisoning. Of the six cases without a history of a previous acute attack, in two repeated cold bathing, and in four chronic dyspepsia, was the probable exciting cause. The age of these twenty patients ranges from 7 years to 86.

Then as regards the prognosis in this class of cases, the absence of albumen at any period of the twenty-four hours, the urine being of normal colour, quantity, and specific gravity, and quite free from tube-casts, warrants the conclusion that there is no serious structural change in the kidney, and justifies a hopeful prognosis if the patient can be induced to submit for a sufficient length of time to the prescribed regimen—dietetic, medicinal, and hygienic.

On the other hand, a restless and unruly patient who, in spite of advice and warning, commits errors in diet and exposes himself to cold and wet, will certainly aggravate his malady. The comparatively mild condition of intermittent becomes a persistent albuminuria, and ultimately, though it may be after many years, an incurable degeneration of the kidney is the result.

Here I would repeat a warning which I have often given before, namely, that we must not allow ourselves to be misled into giving a too favourable prognosis by the absence of all indications of disorder of the general health. Some writers finding that the subjects of this condition are apparently in good health, speak of a physiological, or, at the worst, of a functional albuminuria. But if the absence of indications of disordered health, apart from the condition of the urine, is held to constitute the case one of physiological albuminuria, I can affirm, from a not inconsiderable experience, that the greatest possible amount of albumen in the urine, persisting for months, and even for years, might, in some cases, be looked upon as physiological.

It has been proved, by abundant experience, that the mere fact of albumen, even in large amount, filtering through the walls of the Malpighian capillaries, does not seriously, if at all, interfere with the proper excretory function of the convoluted tubes. The long-continued percolation of albumen through the Malpighian capillaries is, however, attended by a double danger: first, that the walls of the capillaries may become so physically changed as to be permanently unable to prevent the escape of albumen; and, secondly, that, by the long-continued infiltration of albumen amongst the gland-cells of the convoluted tubes, these structures may by degrees become disorganised, and thus rendered incapable of discharging their excretory function.

Although the long continuance of albuminuria, whether in an intermittent or persistent form, must always be a source of more or less anxiety, yet the following case of recovery from albuminuria of seven years' duration should afford encouragement to persevere with the treatment of even chronic cases of this kind. The case was published in outline in the *JOURNAL* of December 13th, 1879; I now give it in more detail, with later particulars, and chiefly in the language of the patient, who furnished me with the history.

CASE I.—A. P. G., aged 26, a very distinguished graduate in medicine and surgery of the University of London, first consulted me on November 26th, 1877. He says: "I had a severe attack of scarlet fever in June, 1871. As soon as ever convalescence commenced, I was treated with a very full albuminous diet—chop and eggs for breakfast, eggs for lunch, chop and custard for

<sup>2</sup> This refers to cases of acute nephritis.

<sup>3</sup> I directed attention to this relationship in a paper on Latent Albuminuria, which was published in the *JOURNAL* for December 13th, 1879.

dinner, eggs for tea, beef-tea for supper, milk through the night—was told to take 'all the nourishment I could.' At the end of three weeks from the attack I was sent into the country, and in another three weeks returned to London. The day after the journey and a cab drive in a shower, the urine was scanty, high-coloured, not bloody, but contained one-third albumen. Treated by counter-irritation to the loins, saline aperients, iodide of potassium, and a non-albuminous diet. At that time there was no albumen before breakfast or at bedtime. After about a month this treatment was discontinued. I was ordered tonics and 'to take as much nourishment as possible.' I continued to pass albumen after food until December, 1877, when, by the advice of Dr. Johnson, I put myself on a diet of five pints of boiled milk and two or three small biscuits a day. This at once reduced the amount of albumen, and made my digestion much more comfortable than before. I enjoyed the diet very much. I was lecturing five times a week, had private teaching, and altogether hard work, but was quite as able to do it as usual. After about six weeks I got tired of the diet, and the albumen, which had been less, increased slightly. I then tried a mixed diet, avoiding salted meat, pastry, and rich dishes, was careful to eat slowly, and to take only light meals. I always found that, however carefully I dieted myself, I passed more albumen after breakfast than at any other time of the day. I found that the best breakfast was a little bread and potted meat with half a pint of warm milk. For two or three months I took quinine and hydrochloric acid, and during the latter half of June and all July I took a wineglassful of Hunyadi Janos every morning; this was evidently beneficial, and during July the albumen was reduced to a faint trace only. In August I married and went abroad, and since my return there has been no albumen, although I have taken ordinary diet. I should add that I never had any œdema. Before commencing the treatment I used to suffer from flatulence and a little discomfort after any but a very light meal, but it was so slight and so constant that I did not observe it until I experienced relief from it on taking milk only. I began with raw milk, but after two days I had to give it up on account of an unpleasant taste in my mouth and a great dislike to the milk. Before last December I used to pass great quantities of urine. One night I got up and passed 84 ounces, although I had emptied my bladder on going to bed. As soon as I began to take milk only I passed less urine, and of a more normal (deeper) colour. I have been a teetotaller for about four years, and have smoked only occasionally during that time. I never indulged to excess in either alcohol or tobacco. I once took a drop of oil of turpentine night and morning for a month, on the advice of a friend; it had not the smallest effect."

On another occasion my former patient wrote to me as follows:—"I have lost an affection which has hung about me for seven years, and which, if it has done but little physical harm, has been a constant source of anxiety, and often a most troublesome depression of spirits. I cannot attribute any part of my cure to Switzerland, as I have had three weeks or a month there each summer for the last four years. Diet and the action of the Hunyadi Janos upon the liver must have the greatest share of the credit; and of course it is at once suggested that marriage may have something to do with it. Allow me to thank you most heartily for the advice you so kindly gave me. Although I had spoken of my case to several physicians, you were the first who ever held out any reasonable hopes to me, and tried to put me on the right track for recovery; and, had it not been for you, I believe I should have gone on as a melancholy 'albuminuric' to the end of my days."

In this case, which occurred in my practice before the publication of Dr. Moxon's paper, the albuminuria, when the patient first came to me, had continued three times as long as in the most protracted of his cases.

Knowing, as I do from long experience, that a patient who has once suffered from albuminuria is liable to a return of the condition from any fresh exposure to its exciting causes, I lately inquired of my former patient how he had fared since his last report of himself, and the following is his reply, dated January 17th, 1889:—

"I have had several slight returns of the albuminuria during the last few years. Indeed, whenever I get overworked I experience the old pain in my back, which to me is a very delicate test of the state of the urine. I then take an occasional dose of saline aperient, and a tonic mixture of arsenic and iron, or a few days holiday. The pain and the albumen promptly disappear. I was quite free from my trouble for some years after you cured me."

Here we have an instructive, practical illustration of the need for long-continued care and watchfulness after recovery from an attack of albuminuria, and especially when the disorder has been of long duration. The safety of such individuals depends upon the early discovery and prompt treatment of a recurring albuminuria. It is obvious that the overfeeding during the convalescence from the fever, and the subsequent advice to take as much nourishment as he could, were calculated to induce, and then to aggravate, the albuminuria.

And now, having shown that albuminuria of seven years' duration may ultimately pass away, I will give the history of a case in which albuminuria, having been overlooked and neglected in its earlier stages, had persisted for thirty years before the fatal termination of the malady.

The early history of this case was published in my book on *Diseases of the Kidney* (p. 374, 1852), and a brief report of its subsequent progress and termination appears in my *Lectures on Bright's Disease* (p. 93, 1873). I will now give some details which have not hitherto been published.

CASE II.—Mr. S., aged 33, came before me, as the medical adviser of an insurance office, on April 23rd, 1852. He stated that in the year 1836 he had scarlet fever, followed by dropsy. He soon recovered from the dropsy, but the doctor who attended him was not careful to guard him from exposure to cold. Believing himself to be well, he had at different times been much exposed to cold, especially while out shooting in the snow. In the year 1841, while he was a medical student at the London Hospital, one of his fellow-students, happening to test his urine, found it to be albuminous. Since then, on the many occasions on which it had been tested, albumen had never been absent, and it had doubtless been present since the attack of scarlet fever sixteen years before. There had been no return of dropsy, nor had he been troubled by any serious ailment. His health had generally been good, but on four or five occasions, during an attack of influenza, there had been a temporary increase of albumen, which passed off after lying in bed for a few days. In 1843 he went to India, and remained there for three months, but he acknowledged that any benefit which he might then have derived was counteracted by the fact that during the voyage he lived as others did, and took stimulants rather freely, his health remaining much the same. He stated that his urine was natural in colour and quantity, with an average specific gravity of 1020.

The specimen which he gave me was of normal colour, specific gravity 1020, and very slightly coagulable by heat and nitric acid, which rendered it scarcely more than opalescent, but after standing the albumen collected in distinct floculi. Standing in a conical glass the urine deposited no cloud, and contained no tube casts. He was of short stature, and rather disposed to obesity, but he had a healthy look and a good colour. The tongue was clean, and the heart and pulse were normal. The insurance office, acting upon my advice, granted him a policy of £500 on payment of an annual premium calculated at an advance of ten years on his actual age. He lived fourteen years after this, and died, as was announced in the *Times*, on "May 16th, 1866, in his forty-seventh year, from dropsy, after a long and painful illness."

During the last year or two of his life his urine, which had continued to be albuminous, also contained sugar. The last occasion on which I saw Mr. S. was on September 14th, 1865, when he was confined to bed with a bronchial attack. I regret that the only note which I made of the urine on that occasion was the following: "Specific gravity 1027; albumen  $\frac{1}{2}$ , dark brown when boiled with liquor potassæ" (glucose). Until within about two years of his death this man was a hard-working general practitioner, and to all outward appearance in fairly good health.

It is probable from the history of this case that the persistent albuminuria, of thirty years' duration, was the result of the medical attendant's negligence in not keeping watch upon the boy and warning him against exposure to cold after the dropsy had disappeared. There can scarcely be a doubt that the albuminuria had persisted from the first onset of the disease to the time when it was accidentally discovered five years later.<sup>1</sup> Whether careful

<sup>1</sup> Dr. Grainger Stewart, in his recently published volume on *Albuminuria*, has recorded (pp. 70-71) a case similar to the above. A girl about 8 or 10 years of age had scarlatinal nephritis, followed by persistent albuminuria. In spite of this she lived like other people till her marriage, at the age of 22. She had no bad symptoms at the birth of her six or seven children. Afterwards she suffered from gout, gradually lost ground, and ultimately died of a combination of dropsy with uræmia, more than thirty years from the onset of the renal disease.

treatment at that time would have been successful in restoring him to health must, of course, remain undecided. If he then obtained good advice, it is clear from the account which he gave me of his mode of living on the voyage to India that he disregarded it, and the result was what might have been anticipated.

Since the publication of Dr. Moxon's paper I have from time to time protested against the assumption that the presence of even a small amount of albumen without other evidence of impaired health, which some writers designate "functional albuminuria," is a matter of small importance, and requiring no special care. I may refer especially to a paper on "Latent Albuminuria" in this JOURNAL, December 13th, 1879; also to a paper "On Intermittent, Recurring, or so-called 'Cyclical' Albuminuria," in the *Lancet*, January, 1888; and to another paper "On Cyclic or Physiological Albuminuria," *Lancet*, May 19th, 1888.

My casebooks contain abundant illustrations of the disastrous results of negligent treatment in the early stages of what were looked upon as trivial cases of albuminuria. The details of most of these cases could not be published without giving pain to the families, friends, and, in some cases, the medical attendants of the patients; but I may venture to give an outline history of two illustrative cases.

CASE III.—I was first consulted by Mr. — when he was 30 years of age. The history which he gave was that five years before he got a chill after being overheated and fatigued in rowing, with the result that his urine became blood-tinged. The slight hæmaturia had recurred on several occasions since, but there had been no recurrence for two years until seven weeks before his visit to me, when he got overheated by riding fast to the railway station in cold weather and then chilled by getting into the train without a rug. The urine was of normal colour, specific gravity 1022, contained  $\frac{1}{8}$  albumen, and on standing deposited a cloud in which were found a few small hyaline and oily casts. The general health was good. The heart and pulse were normal. This gentleman had been well advised and cared for from the commencement, but being a zealous sportsman as well as an energetic and most successful merchant, he had on various occasions imprudently exposed himself to the risk of being chilled. I saw him occasionally in consultation with his medical attendant during the next five years, and the report on each occasion was to the same effect—the general health good, heart and pulse normal, urine rather pale, specific gravity normal, containing a small amount of albumen, but no casts. After this a period of eight years elapsed before I again saw him in consultation. He had lately been much overworked in business. He had married four years before, and had three children. He was pale, and had lately suffered from sciatica. The radial pulse was full and tense, the heart's impulse strong and diffused, with reduplication of the first sound. The urine was of light colour, specific gravity 1010, contained 1.2 gramme of albumen per litre, but deposited no casts. It was evident that the kidneys, which probably had been enlarged, were then in the stage of atrophy and contraction. He lived two years after this, during which I saw him occasionally in consultation. The condition gradually became worse, with headache, dyspnoea, vomiting, some dropsical swelling, pericarditis, convulsions, coma, and death twenty years from the first occurrence of the hæmaturia and fifteen years after his first visit to me.

CASE IV.—A boy, aged 12, had a severe attack of scarlet fever and suppression of urine. He was supposed to have completely recovered. At the age of 13 he went to Eton, where he remained four years. While there he often bathed in the river. The year after leaving Eton he went to Oxford. During the second year of his residence there he suffered from severe abdominal pains, which compelled him to leave the University. Soon after this, he being then 20 years of age, the vision became seriously impaired. He went to an oculist, who found albuminuric retinitis in both eyes, and the urine, which had not been tested since his recovery from scarlet fever eight years before, was found to be albuminuric. He then came to me. The urine was copious, nearly colourless; specific gravity 1010; albumen  $\frac{1}{2}$ ; no casts. The heart's apex was in the sixth interspace, and the impulse was very strong. The radial and all the superficial arteries were large, tense, and tortuous. The vision was much impaired. Both discs were blurred, and there were extensive, white, oedematous patches in both retinae. The face was pallid. He suffered often from violent headache, with vomiting. Evidently there was degeneration of the kidney of long standing, and in a very advanced stage. The prognosis was most unfavourable. He continued to get worse, and

died of uræmia six months after his first visit to me. It is probable that the recovery from the scarlatinal nephritis was incomplete, and that repeated testing during and after convalescence would have detected a trace of albumen. It may be, however, that for the time the cure was complete, and that the frequent cold bathing at Eton, acting injuriously upon kidneys rendered more vulnerable by the previous acute disease, caused a return of the malady.

This case affords another illustration of the vital importance of, from time to time, testing the urine of a patient who has once been the subject of acute nephritis.

The following are the main points which in this and in previous publications on the same subject I have endeavoured to establish:

1. The presence of albumen in the urine, though small in amount and occasionally intermittent, is always pathological.

2. The practice of testing the urine in all cases of ailments, even the most trivial—the importance of which I have for many years past insisted upon—has led to the detection of albuminuria in many youths and adolescents who are especially liable to be exposed to the commonest of its exciting causes, namely, cold and wet and over-fatigue, and who have not lived long enough for the ultimate evil results of a neglected albuminuria to have become developed.

3. The albuminuria, whether intermittent or persistent, of persons apparently in good health has no such special features as to require it to be designated by such misleading terms as "physiological," "functional," "cyclical," and "the albuminuria of adolescence." The last term is especially inappropriate, since the condition is of common occurrence in both sexes and at all periods of life, from childhood to extreme old age.

4. In almost every instance these cases of albuminuria may, by a careful inquiry, be traced back to some recognised exciting cause.

5. Nearly all cases of acute nephritis pass through the stage of intermittent albuminuria in their progress towards convalescence, and, on the other hand, the majority of cases of intermittent albuminuria may be traced back to a more or less remote attack of acute nephritis.

6. While, on the one hand, intermittent albuminuria—even though it may have existed for years—may be looked upon as a curable condition, if only its exciting causes can be ascertained, avoided, and counteracted by suitable dietetic, medicinal, and hygienic means, on the other hand, the neglect of such means may convert an intermittent into a persistent albuminuria; and a persistent albuminuria, although for many years it may be unattended by symptoms of disordered health, ultimately results in a fatal degeneration of the kidneys.

7. Since it is notorious that albumen, even to a very large amount, may exist in the urine of persons who are apparently in perfect health, it is obvious that the urine of every patient, no matter how apparently trivial his ailment, and the urine of every applicant for life assurance, no matter how robust his appearance, should be tested for albumen.

8. For many years past the fact that albumen may be abundantly present at one period of the twenty-four hours and entirely absent at another has been publicly demonstrated, and ought to be generally known. It is, therefore, necessary to test the urine, not only after rest in bed and before breakfast, but also after food and exercise.

With regard to the question of albuminuria in life assurance, no prudent medical officer would advise that a proposed assurer with a trace of albumen in the urine should be accepted at the ordinary rate of premium. Such a condition, even in a careful and prudent man, must involve some extra risk, and no office can insure care and prudence.

Whether an albuminuric should be accepted on any terms can be determined only by a careful inquiry into the particulars of each case. It goes without saying that such unquestionable signs of advanced organic disease as albuminuric retinitis and cardiac hypertrophy would exclude the applicant; but Dr. Tyson, of Philadelphia, with whom the writer X agrees, would not consider that an amount of albumen not exceeding one-fifth of the bulk of urine examined, and unaccompanied by tube-casts, "formal ground for exclusion." As to this, I venture to remark that while small hyaline and epithelial casts are often present in the albuminous urine of patients whose speedy recovery may be confidently predicted, on the other hand, in the advanced stages of a contracted granular kidney (the so-called cirrhotic kidney), the amount of coagulated

albumen is often less than one-fifth the volume] of urine examined, and the characteristic large hyaline and granular casts may be absent for days, and even for many weeks, consecutively. Cases III and IV, before recorded, afford illustration of this statement.

The main purpose of this communication has been, not to refute the ludicrous suggestion that I had failed to recognise a class of cases which for many years past I have been diligently observing and recording, but rather to reiterate and emphasise certain facts and principles regarding the clinical history of albuminuria which even now are less generally recognised and acted upon than their great practical importance demands.

## ANTERO-POSTERIOR COMPRESSION FORCEPS FOR APPLICATION AT THE BRIM OF FLAT PELVES.<sup>1</sup>

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THESE forceps are not presented as an improvement on the classical long pelvic curved forceps. With many modifications to suit many tastes, these latter serve their purpose admirably; and, until recently, when these instruments failed in simple cases of obstruction at the brim, whether in flat or in generally contracted pelvis, I believed that the only alternative to them was craniotomy; turning being reserved for special cases, the nature of which I have referred to in a paper published in the *Edinburgh Medical Journal*, for January, 1886.

I shall not here enter into the question of turning or craniotomy, as an alternative to forceps at the brim, in cases of flat pelvis. This subject I have discussed pretty fully in the paper referred to; and I am justified in believing that the views I have there expressed are now generally accepted by the profession; since, in two and a half years, no attempt, so far as I know, has been made to controvert them.

What I am now submitting to your criticism is a new instrument designed to take the place of craniotomy, where the other forceps have failed, in a large number of cases of obstruction at the brim. It is known that in flat pelvis the head of the child, which as a rule lies in the transverse diameter of the pelvis, or nearly so, and in an extended position, may, according to the relation of the pelvis to that individual head, pass in various ways; each having its own mechanism. These are amply detailed in textbooks, and need not detain us; I shall only add that, in my experience the same head may pass through the same brim with varying mechanism, according to the position the head is placed in, or is allowed to take naturally above the brim. The most satisfactory mechanism, and the one most favourable for the ordinary forceps, is that of simple flexion of the head in the transverse diameter of the pelvis, rotation following immediately after, as the head is then in the cavity of the pelvis.

The position of the head, which renders the ordinary forceps a peculiarly unsuitable instrument, is where it lies well above the brim, and with the anterior parietal bone overhanging the pubes. Here the well-known tendency of the ordinary forceps to cause flexion and rotation of the head comes into play too soon; that is, before the head has even become engaged in the brim. Under such circumstances the difficulty is really increased by the forceps, and the more the traction the more the difficulty. Delivery is, in these cases, utterly impossible with such instruments; and version, as I have elsewhere shown, is unsuitable. To meet these difficulties, and thus in some cases be able to avoid the horrible operation of craniotomy, in which not only is the woman exposed to increased danger, but an innocent babe is deliberately killed—this is the object of these new forceps; and, since they are never to be used until craniotomy is the only resource left, it follows

that every child born alive with these instruments is one life saved, without any increased danger to the other.

These instruments are first compressors and then tractors. Now it is obvious that theoretically an instrument which is able to compress the head in the proper direction is preferable to one which only or mainly acts by traction in the cases we are considering, for naturally the head must undergo compression; but, without compressing instruments, that must be done at the expense of the maternal soft parts.

The first difficulty, however, is how to manage this without taking up so much room in the already diminished diameter of the pelvis as will more than make up for any reduction made in the bulk of the head, and the measure in which I have succeeded in doing this must be the measure of the success of the instrument. This difficulty is so real a one, especially considering the strength of blades required, that probably it has deterred others from trying it; for any instruments apparently designed to lie in the antero-posterior diameter above the brim (like my friend, Dr. Reid's) are really meant to lie in the oblique or transverse, like the ordinary forceps. These have blades of the usual length, so as to be able to grasp the head in the occipito-malar direction; whereas antero-posterior compression forceps must have short blades so as to grasp the head vertically.

The first method which suggested itself was to make the blades very narrow, and apply them in the lateral conjugate of that side of the pelvis in which the bi-parietal diameter lay. This, however, I soon found was impracticable, as there was no room in the side of the cavity for the instrument to work. I next tried the blades so wide that the rims lay in each of the lateral conjugates. This kept the instrument in the middle of the cavity; and, by permitting the promontory to completely pass through the fenestrum of the posterior blade, removed entirely the difficulty of finding room for it. The great width of the anterior blade served to protect the bladder from pressure; but I found that fully an eighth of an inch of the anterior pole of the diminished diameter was lost by this width of blade, the head during extraction having this space between it and the pubes. By altering the transverse curve, however, of this blade, so as to make it coincide with the anterior curve of the pelvic brim, even this loss of space was rendered unnecessary; little more than the thickness of the rims requiring to come off, and this not actually in the diminished diameter. The transverse curve of the posterior blade I have made a segment of a large circle with its convexity forwards, so as to permit the head during compression to pass backwards into the space on either side of the promontory. Another difficulty which early presented itself was that the frontal portion of the head being much narrower than the bi-parietal portion, the blades must be able to lie flat over this wedge-shaped surface. This, however, was easily managed by making the lock unusually loose, permitting so much rotation of the handles on their long axes as to make the distance between the blades at the two sides differ by fully two-eighths of an inch.

Before venturing to introduce these new instruments into the pelvis of a living woman I used them and compared their action with the ordinary forceps perhaps more than a hundred times with dried pelvis and foetal heads either fresh or preserved in spirits and water. When I speak of the ordinary forceps in these experiments I refer to those with which I am most familiar, and therefore have most confidence in, namely, the late Sir James Y. Simpson's. In these trials I found in some cases both instruments equally successful, whilst in some instances the Simpson's forceps had the advantage, and in some others the new instruments succeeded where the ordinary forceps failed. The result in these cases depended somewhat on how the head happened to be placed above the brim. It may be worth while detailing a few of these experiments, noting that, in many of them, these new instruments were at the time in an incomplete form, and had some disadvantages which they are now free from.

1. Placing a premature foetal head above the brim of a flat pelvis with a conjugate diameter of two inches and a half, the new forceps with moderate traction drew the head through the pelvis without any appreciable indentation of the head by the promontory. Whilst the outer edges of the anterior blade were pressing on the brim of the pelvis, the inner edges of the same blade were about an eighth of an inch from the anterior surface of the pelvis, thus preventing the head from quite reaching the pubes by about this distance.

Applying Simpson's forceps to the same head at the brim of the same pelvis, it was found that they could only lie in the trans-

<sup>1</sup> Read in the Section of Obstetric Medicine, at the Annual Meeting of the British Medical Association, held in Glasgow, August, 1888.