pupils active. This is a very extraordinary fact, considering the serious injury. There was no reaction for two hours; after that he vomited, and became partly sensitive for a few seconds, then relapsing into unconsciousness, and snoring slightly. The next morning he took some tea. His pulse continued feeble and oppressed through the day. No serum had followed the bleeding from the ear, which stopped at the time of the accident, and was very slight, apparently arterial. The head becoming hot and slightly painful, ice was applied to the scalp. He has passed urine, arising for the purpose; very little power to assist himself, but no paralysis. A purge, containing five grains of calomel, was now given, followed by a black draught in four hours. The bowels were comfortably relieved, and he expressed himself as feeling better; less lethargic; more sensible; pulse 65, still laboured and weak; the pupils continuing active and alike. He took tea and arrowroot; more himself, but rather more restless; pulse about 70, but uneven and laboured. Observed that the right pupil was more dilated than the left, although it contracted slowly on the application of strong light. His tongue was moist and furred; there is occasional wandering and incoherence, on attempting to converse.

Dec. 28th, 1857.—When I first saw him, I found him lying in bed, very much flushed, his head very hot. Manner rather excited, apparently not knowing Dr. Boulger, and not answering him rationally. The right pupil of the eye rather more dilated than the left, and though it answered to the light of a candle, it did so much more sluggishly than the left; the tongue moist but furred; pulse very irregular, both in force and frequency, but on the whole full and slow—it was an oppressed pulse.

In reply to my question, "Does your head ache?" he said, "Yes, it does," and put his hand to the forehead. Dr. Boulger told me that he was much worse now than when he saw him in the morning, about six hours previously. He was then quite rational, though heavy. Under all the circumstances we determined to perform venesection, as leeches could not be applied in less than two hours, and then only twenty.

Dr. Boulger opened the median cephalic vein, and let blood *pleno rivo*, which we allowed to the extent of twenty ounces. This loss did not produce any faintness, but a very decided though only a temporary improvement. From this and the other facts of the case, I argue that there was a fracture of the base, with internal cranial hæmorrhage at the time of the injury, but that, in consequence of the effusion taking place at the base of the brain, the old reputed symptoms for diagnosing compression were absent. The practical points to be derived are the same as those from the other cases—namely, the apparent *slightness* of the symptoms in the first instances, so much so that the friends of the poor fellow would not believe that he was seriously injured, and were inclined to believe that Dr. B. was making too much of the case. Like one of the patients I have been speaking about, he would get out for every motion of his bowels. Indeed, his enormous weight rendered it almost impossible for his attendant to place the bed-pan under him, unless he had been placed on one of those beds which are made purposely for lifting heavy weights. Of these contrivances, I have seen none equal to one invented by my friend and brother-in-law, Mr. Denne, Medical Superintendent at the Bedford Asylum, and patented by Mr. Hooper, Pall Mall. This bed, I think, will prove of great value to the profession. An account of it, with a wood-cut, may be seen in Mr. Denne's Report of the Asylum.

But to return to our patient. On the following day, Dr. Boulger shaved the head, applied twenty leeches, gave three grains of calomel every three hours with the antimonial wine, and applied ice to the head.

29th.—Symptoms better; the pupils more alike; the expression more natural; less restlessness; pulse 80, but more even and regular. Arrowroot and beef-tea taken at intervals. Mercury beginning to affect the mouth. Head still hot. In the evening, twenty more leeches were applied to the scalp.

30th.—System decidedly affected by the mercury; the pulse 80, and the head much cooler, but great restlessness and prostration.

31st.—Much the same, but more restless. From this date he gradually sunk. Squinting took place, but there was no other feature of note. He died comatose on the 7th day from the receipt of the injury. The antiphlogistic measures which were adopted appeared to arrest for a short time the rapid progress of inflammatory action, but the injury was too severe to admit of cure, and the case adds another to the list of fatal injury with comparatively slight signs of cerebral mischief for the first forty-eight hours.

Nettsonian Lectures,

DELIVERED BEFORE

THE MEDICAL SOCIETY OF LONDON,

March the 17th, 1858.

BY VICTOR DE MÉRIC, Esq.,

SURGEON TO THE ROYAL FREE HOSPITAL, AND TO
THE GERMAN HOSPITAL, DALSTON.

LECTURE I.

ON THE UNICITY OR DUALITY OF THE VIRUS IN SYPHILIS.

Importance of the study of syphilis.—Specialism.—Diversity of opinions on syphilis.—The origin of the disease.—Syphilis in modern times.—Existence of a syphilitic virus.—Ricord's unicity.—Proposed duality.—Sketches of the doctrines of Hunter, Abernethy, Carmichael, and Wallace, as bearing on the plurality of the virus.—Bassereau's duality.—Ricord's opinion on a duality of poisons.—Present state of the question.

MR. PRESIDENT AND GENTLEMEN,—I beg your permission to offer a few observations on some points of interest bearing upon the pathology and treatment of the syphilitic disease; and as considerations respecting this malady have not yet been brought forward from this chair, I shall have the advantage of novelty as regards the subject.

It will hardly seem strange that a conference on surgery should hinge upon the syphilitic disease; for surgeons now-a-days, when coping with syphilis, are called upon to do a little more than to promote the healing of sores, and to gouge away carious bone. They are, on the contrary, expected to study the essence of the malady, trace it from its origin, examine its mode of propagation, its varied forms and aspects; observe its progress and the influence of remedies upon its manifestations, distinguish it from other diseases, watch over relapses, and devise the most efficacious and least dangerous therapeutical measures.

To do all this, it is indispensable that the inquirer be a surgeon in the fullest acceptance of the word; for the only way of mastering any surgical disease is to descend from the general knowledge of all medical and surgical affections to the particular malady to which especial attention is bestowed. General principles, and the close study of pathological laws, will allow the observer to move with judgment and ease in the circle which he has traced for himself; and he is then ever ready to apply his information and manual skill to improvements and sound practice in the particular branch to which he is confining himself. Such a surgeon need not be afraid of being considered a *specialist*, particularly if he succeed in avoiding some of the quicksands of specialism. Amongst these is the propensity of clothing many unkindred affections in the special garb; and of not paying sufficient respect to the opinion of men who, laying no pretension to specialism, are nevertheless entitled by their knowledge and experience to judge for themselves.

I suspect that we all may usefully bestow a portion of our time to the study of syphilis. Amongst the reasons for holding this opinion I may give the two following: the universal manner in which the disease is spread amongst the community, and the variety of methods employed by competent practitioners in combating the malady.

Before the audience I have the honour of addressing, it would be superfluous to dwell on the disastrous influence this fell disease has exercised upon mankind for the last three centuries; how it has poisoned the very springs of life, fearfully visiting both the innocent and the guilty; how aggravated and loathsome it was rendered by illiberal measures and imperfect treatment; how rapidly it destroys some individuals; how obstinately it fastens for a considerable number of years upon others; and how terribly, even in modern times, it has been known to break out and defy all control.

All these unfortunate circumstances are well known; nor are my hearers less aware of the diversities of methods employed in the treatment of the disease, all these methods being based upon deep conviction and candidly avowed theoretica

views. Amongst ourselves, at this moment, there may be some who dread secondary symptoms as much after gonorrhœa as chancre, and act in consequence; some may look upon every chancre, as Hunter did, as capable of exciting constitutional syphilis; some may suppose that the poison, after contagion, is immediately taken up into the organism, and that the primary sore is a consequence of general contamination; some may reject, as far as transmissibility is concerned, the distinction between primary and secondary symptoms; some may think mercury a beneficial agent, others a mischievous drug; some may believe that constitutional syphilis attacks a man but once in his life, others that the infection may be several times repeated.

So much difference of opinion and so much uncertainty overhangs hardly any other disease; I need, therefore, not apologize for endeavouring to throw a faint glimpse of light on some points of this very intricate subject.

And now that we are fairly launched, I would fain comply with the wish expressed by Dr. Headland, my immediate predecessor in this chair. My colleague thought I might, perhaps, venture to offer a sketch of the origin of the disease; but such an attempt would merely lead you over often-trodden ground, without the benefit of arriving at a satisfactory conclusion. All I would ask permission to say on the subject is this: It is perfectly certain that previous to the general spread of the disease in 1493-95, no ancient or mediæval writer had given a description of the complaint, as seen and depicted by Joseph Grundbeck, Benedictus, and Leonicens, who wrote in 1496-97, these authors being the first who published an account of the malady. Such is not the case with small-pox, the essential fevers, hydrophobia, and other complaints, which are mentioned in the remotest records. We are, therefore, led to believe, that at the end of the fifteenth century the morbus pustularum, as syphilis was then called, appeared for the first time; and we are inclined to reject the ancient origin supported by some writers. Shall we give credit to the American story so eloquently and learnedly defended by Astruc? I fear not. Had the companions of Columbus brought the disease with them, they would have propagated the complaint, not at the siege of Naples, but in Portugal, where they landed. Was the disease generated by filth amongst the Jews and Moors, driven from Spain into Italy, where they encamped in the vicinity of Rome, and were decimated by the Marannic* fever, as maintained by Sanchez? Were the Yaws imported from Africa into Italy, where they became the origin of syphilis, as has been supposed by Swediaur? None of these statements are sufficiently supported by historical coincidences and documents. There is, however, one way of raising a corner of the thick veil which obscures the subject—namely, by taking into consideration the appearance of glanders amongst horses at the very siege of Naples so frequently cited. Now it was not known at the period alluded to, that this latter disease was communicable from the horse to man; and it has since been suspected that some of the loose female characters already labouring under simple ulceration, and hovering around armies, might have been infected by an individual suffering from glanders, or having about him some infectious secretion originating from a diseased horse. A mixture of the poison of glanders with the unhealthy and decomposing secretions of dissolute females may have given rise to the dreadful malady which has for centuries undermined the health of generation after generation. Proof there is none; but a tolerable share of probability.

I may not, on the present occasion, follow the ravages of the disease through the length and breadth of the world, trace its effects in different climates, describe its decrease and occasional exacerbations, compare it with the Sibbens of Scotland, the Yaws of the West Indies, and the Black Disease of Canada; but I shall just stop to inquire what aspect syphilis presents in this country, in our own refined state of society.

We now meet in hospital and private practice with all the symptoms of syphilis which have been described by the writers of the sixteenth century; nay, we are more complete than our forefathers, as may be learned by the works of Brassavolus, Fallopius, Fracastorius, and Fernellius; for it is only by degrees, and by successive authors, that loss of hair, ulcers of the tonsils, affections of the bones and of the testes, were mentioned. These manifestations of syphilis either did not appear at once, or their relation with the syphilitic poison was only gradually discovered. Surgeons now-a-days, however, see before them a considerable number of chancres, of which a rather large proportion, especially in the labouring classes, become phagedænic.

* These unfortunate people were driven from Spain upon their refusal to embrace the Christian faith, and were stigmatized by the term *Marranes* (pigs).

We frequently observe gangrene of the prepuce and glans, and spreading buboes; we have to cope with destruction of the soft palate, and large intractable secondary or tertiary ulcers; we are grieved at seeing, but too often, the vomer and hard palate attacked; and we meet with carious skulls, which now, as of yore, lead the sufferer to the grave. The picture is not overdrawn, and I am sorry to say that in the space of fifteen years, I have individually seen a very large proportion of fearful pustular eruptions, extensive tubercular ulcerations, destruction of the penis by gangrene, phagedænic ulceration of the greater portion of the abdomen or the back; and I have observed some of our fair delinquents, once adorned by freshness and youth, turned into objects of disgust and commiseration. Let us, therefore, not relent in our contest with this leviathan destroyer.

I need hardly mention that nomenclature is a matter of some importance, both in scientific investigations and in practice. From a desire, then, of being clearly understood, I shall comprise, under the term syphilis, (a word we owe to Fracastorius's poetic efforts, and the derivation of which is not settled,) the primary, successive, and constitutional symptoms which are generally included when syphilis is mentioned. But I must say, though I am, perhaps, forestalling a little, that there will eventually be advantage in restricting the term to such symptoms as result from the contamination of the organism at large. We all know that a man may have had a chancre and a suppurating bubo, and remain for the rest of his life perfectly free from any taint whatever. Such an individual should not be said to have suffered from *syphilis*; his frame is not infected, he has simply had a venereal ulceration, followed by a glandular complication. The words *chancre* and *syphilis* might, then, advantageously be reserved for the more serious disease, and it would at once be plain that syphilis is to be classed with other blood diseases, as cancer and scrofula.

The necessity for such a distinction will become plain when we come to consider the question of unity or duality of the syphilitic virus, the principal object of the inquiry which we are now instituting. This inquiry may, however, appear idle to some practitioners, as we have it stated in certain books that the poison of syphilis may be generated spontaneously. It has also been maintained that frequent promiscuous intercourse, coupled with uncleanly habits, may cause the development of syphilitic ulcerations. If notions of this kind were to gain ground, investigations respecting the intimate nature of the virus, its unicuity, duality, or plurality, might well be given up, for spontaneous generation, problematical as it is in natural science, is completely incompatible with the conception we form of a virus or morbid poison. In establishing a comparison between the modes in which small-pox and syphilis (two very frequently-observed morbid poisons) may be artificially propagated, we shall rest analogically satisfied that the purulent matter at the point of the syphilitic lancet contains a virus as well as the matter carried by the variolous blade. Analogy will aid us just as efficiently if we look at glanders and rabies, and we cannot consistently admit a virus for these diseases, and refuse it to syphilis. Nay, we concede a special virus for scarlet fever, influenza, rubeola, and typhus, although we are aware that it must be volatile, and unamenable to the senses by any vehicle; so we may, *a fortiori*, look upon the syphilitic virus as having a *bonâ fide* existence.

Nor does the analogy between syphilis and other contagious diseases rest here, for it is extremely probable that the organism is insusceptible of two syphilitic infections. A patient who has suffered from generalized syphilis is just as unlikely to see an indurated chancre take root upon any part of his body as an individual who has had small-pox to see a variolous pustule luxuriate by inoculation upon his frame. It is not strictly known, however, *how long* the immunity lasts respecting either of these diseases, or respecting the protecting effect of the vaccine virus, but I would venture to say, that the syphilitic influence is, from some facts observed, *more likely* to endure than that of either variola or vaccinia. This is extremely probable, because patients have been known to present constitutional symptoms of syphilis many years after the primary infection, a long lapse of time having intervened, during which the morbid disposition lay dormant. We have no such test regarding the two diseases just named; on the contrary, we do know that the vaccine virus will take a second time after the interval of a certain number of years. I wish to be understood as not denying *in toto* the possibility of a second infection on an individual who has passed through the phases of constitutional syphilis; but it may be affirmed that no reliable cases of such infection are on record; nor have experiments satisfactorily shown that the pus taken from a hard chancre will produce a

similar sore upon an individual whose organism had previously been tainted by syphilis.

This question is intimately connected with another very important one—viz., whether the disease *ever completely* leaves the organism which has once been infected. Ricord is inclined to think that something is always left, and that the disease is never entirely eradicated; but neither he nor other syphilologists are quite certain on that head; and no surer proof could be obtained that syphilis can be completely eradicated than the production of a patient who, after having had symptoms of generalized syphilis, and undergone an appropriate treatment, were some years afterwards to contract a hard chancre. This circumstance would incontestably show that his blood had been perfectly freed from the virus, he having again become susceptible of its influence. I need hardly say that *soft chancres* will very easily spring up on such individuals, but the hard does not.

A very interesting illustration of insusceptibility is frequently afforded in practice. I often see patients who present a hard chancre behind the corona. This kind of sore secretes but very little pus in ordinary circumstances; but uncleanly habits and negligence excite irritation and inflammation, and pus is then pretty largely secreted. The groove behind the corona becomes inoculated, wreaths of chancres spring up, but none take on induration.

But let us return to the *virus*, that alkaloid, as it were, which has not as yet been isolated from its vehicle. It is plain that we know it merely from its effects; and that its essential nature is still a matter of conjecture. Chemistry and the microscope have done their best to clear the mystery, but to no purpose. I have myself made a few efforts in that direction, kindly assisted by my friend, Mr. Jabez Hogg. We possess very delicate drawings of the microscopical appearances of various purulent secretions; but we have not hitherto obtained the reward of our industry. I am not, however, inclined to relax; and hope I may succeed a little better than some writers—such as Didier, who, in 1710, contended that the virus was composed of little worms; or, as Donné, who more recently laid great stress on the vibrio-lincola found in the pus; or as M. Castano, who maintained, in 1855, that the virus consists of a parasitical fungus.

The essence of the poison is, to this day, unknown; but the last few years have, nevertheless, worked a complete revolution respecting this same virus; and it is in France that this revolution, like many others, has occurred. It consists in dethroning the monarchical government of one virus, and forcing it to share its power with another morbid poison. The unicity of the virus is attacked as despotic; and the sceptre is henceforth to be divided.

It is well known that Ricord was led by his experiments to admit but one syphilitic virus; and it is by no means surprising that he should have formed such an opinion. During his numberless inoculations, he observed that, of all the syphilitic ulcerations, the primary sore only was susceptible of artificial propagation. He saw this sore, whether hard or soft, equally reproduced, or almost so; and he concluded that as they both possessed this same property, they were the result of the same poison. But how did he explain that one of these sores was never followed by constitutional symptoms, and the other always? He explained it by the different constitutions upon which the selfsame virus was planted; and said, with his usual imagery, that the seed was ever the same, but that the ground wherein it developed presented differences. I can vouch, however, for his having his misgivings all the time. He more than once told me by the bedside, many years ago, in his hospital: "You may rest assured that some day distinct origins will be found for the infecting and non-infecting chancres." Nay, we find the following words, which sound almost prophetic, in the "Letters on Syphilis," p. 359:—"In computing the cases of syphilization, published in Italy and in France, I find that it was always matter from soft chancres that was used, and that the only time that an inoculation was practised at Paris with pus from a hard chancre, the healthy individual, who was the subject of it, presented the development of a hard chancre and of constitutional syphilis. If these results were constantly obtained, we must admit that there are differences in the symptoms which have reference not only to the peculiarities of the individual upon whom the pus is implanted, but also to differences in the pus itself." And it was eventually one of his own pupils who worked out the idea, and now bids fair to establish his duality in the place of his teacher's unicity. But before I attempt to sketch by what steps the new doctrine was brought to light, it will be useful to inquire whether, before Ricord's time, doctrines analogous

or partially so to those now promulgated had not existed. By this retrospective glance we shall be able to ascertain the real value of the new tenets which have been proposed.

The foreshadowing of the notions which were to lead to a belief in a plurality of syphilitic poisons was thrown by Hunter. It is well known that this great physiologist had arrived at the conclusion that gonorrhœa and chancre arose from the same poison; and he was considerably strengthened in this opinion by an experiment he made upon himself, and which was extremely likely to give support to the views he had adopted. He could not, however, deny that there was such a thing as *simple gonorrhœa*; nor could he help noticing that ulcerations were observed on the parts of generation, which, though resembling the chancre which was eventually to bear his own name, did not present all the characters of sores depending on actual syphilis. Hence we hear him say, in the chapter on the affections resembling the *lues venerea*: "Other diseases shall not only resemble the venereal in appearance, but in the mode of contamination, proving themselves to be poisons by affecting the part in contact, and from them producing immediate consequences similar to buboes; also remote consequences, similar to the *lues venerea*." The last few words contain a statement which has been found unsupported by facts; and it is almost strange that, deeply engaged as Hunter was in the consideration of these questions, he did not catch a glimpse of the soft chancre and its inability to infect the economy; the more so as he had so beautifully seized upon the characters of the bubo by absorption, which generally accompanies it. But such oversight is easily explained by the fact that mercury was given for every chancre; so that the use of this metal always had the merit, when no constitutional symptoms appeared, as generally happens with soft chancre, of having prevented the development of systemic syphilis. The distinction between the soft and hard chancre, as subsequently made by Ricord, could, therefore not be expected of Hunter. The latter, had, however, a kind of presentiment that such a distinction did exist; but he was not inclined to work out the problem himself, and merely says, "As the diseases in question (those resembling the *lues venerea*) are various, and not to be reduced to any symptom or order that I am acquainted with, I shall content myself with selecting the cases, and thereby put it in the power of others to judge for themselves, if they should not be inclined to adopt the conclusions I have drawn from them."—(American edition, p. 492.) This was certainly not a distinct declaration of plurality of virus—far from it, but it was equivalent to saying: I have observed on the parts of generation appearances which resemble those occasioned by syphilis, but some characters are wanting to make the resemblance complete. How shall we class this kind of false syphilis?

The hint was not lost on Mr. Abernethy, who diligently collected facts bearing on this important question. It is a pity, however, that no attempt was made by the author to classify these facts, and by the assistance of analogy to introduce something like regularity into the mass of data which public and private practice brought before him. Our author was, besides, considerably cramped by the belief that the action of mercury is quite sufficient to prove the venereal nature (thereby was meant the syphilitic nature) of every symptom. This belief must have necessarily obscured the otherwise keen perceptive powers of Abernethy; hence we are not surprised at remarking a little confusion in the book called "Surgical Observations on Diseases resembling Syphilis," 1810; for we perceive that secondary symptoms are attributed to primary affections which are not venereal—that is to say, not syphilitic. We must naturally suppose that the diagnosis was not quite correct, either as regarded the primary or secondary manifestations. Nor is Abernethy at ease when he comes to think of this *pseudo* syphilis; for he says at the very beginning of the book alluded to:—"It cannot, I think, be denied, on due consideration of the subject, that many sores are induced on the genitals by sexual intercourse which are not the effect of the venereal poison, and that many of these infect the constitution, and produce secondary symptoms resembling those of that disorder. It may be asked, however, if these diseases be not venereal, what are they?" One would almost feel inclined to try and answer the question, were it not that the author erroneously introduces secondary symptoms, which merely tend to perplex.

Abernethy was, however, on the eve of disentangling himself from one of the trammels which cramped Hunter, and therefore had more advantage than his predecessor. Abernethy had, namely, a foreboding that mercury was, perhaps, not so infallible a touchstone as was commonly imagined; for we hear him say (p. 28):—"I shall prosecute the subject by

relating some unequivocal cases of diseases strikingly resembling syphilis, but which, however, were disorders of a different nature, provided it be admitted that syphilis does not spontaneously get well without the aid of medicine." Nay, as we advance in the book, we find in the author a desire to erase constitutional symptoms from the diseases resembling syphilis; for he remarks, (p. 59,) in speaking of sores: "It is from their effects upon the constitution alone that we can judge whether they were syphilitic or not." Thus we have Mr. Abernethy actually paving the way for the doctrine of duality which has so lately been proposed, and which we shall presently examine. It is to be regretted that the author contented himself with noting down and describing the various phenomena which came before him in his practice, without trying right earnestly to find a string to lead him through the labyrinth. Indeed, he seems to have been fairly puzzled and worried by the varieties of symptoms that were brought under his notice; so much so, indeed, that he closes the work with the following words:—"Such is the result of the observations I have made on the treatment of these most vexatious diseases—diseases which must, I believe, perplex all surgeons, whatever opinions they may entertain respecting them, and whatever conduct they may pursue with a view to their cure."—p. 167.

(To be continued.)

ON THE MEDICAL TREATMENT AND DIAGNOSIS OF STONE IN THE BLADDER.

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It has long appeared to me that stone in the bladder is one of the most artificial and unnatural affections to which the human subject is liable; and that the very circumstance of its occurrence affords a convincing proof of the incompleteness of the present state of medical science.

That this view is correct will become apparent as we consider the circumstances under which stone is formed.

First, the very words, stone in the bladder, sound unnatural—a stony substance, usually rolling about free in the cavity of a viscus having a communication with the exterior of the body.

Again, the formation of stone in the bladder is not a sudden occurrence—is not the work of a few hours, days, or weeks, but occupies ordinarily months and even years.

Neither is a calculus formed in the bladder without there being a very evident manifestation of the risk and probability of such a catastrophe.

There is invariably a persistent condition of the urine going on for months or years obvious to the unaided sight, and which ought to attract the attention of both patient and physician.

That is, for a very long period prior to the formation of a stone, there is a very sensible and obvious deposit in the urine; for it is only those substances, be it remembered, which are but sparingly soluble in the urine which ever form calculus.

Now this deposit, in the great majority of cases, although observed by the patient, is allowed to continue and to go on unchecked without his seeking for medical advice; and it is only when symptoms of stone have presented themselves that he is led to seek for aid; but then great mischief is already done, and the physician is called upon to treat not only the condition of the urine which has occasioned the stone, but has to endeavour to act by his remedies upon the stone itself. This is generally regarded as a task so difficult that the physician usually abandons it in despair, and the case is handed over to the surgeon, the stone being removed ordinarily by the extreme and heroic remedy of vivisection and forcible extraction.

In ordinary cases, then, the physician and the patient are alike ignorant of the impending calamity; or if they entertain any suspicion that the condition of the urine is one likely to tend to the formation of stone, they do not usually attach sufficient importance to it to make it the subject of active and immediate treatment. The presence of a stone is usually first determined by the surgeon. Again, when even the calculus is

discovered, the physician is rarely called upon to put into operation the resources of his art; but the treatment generally pursued is strictly surgical.

Now, I contend that the physician ought to take a prominent position in the treatment of stone. The conditions of the urine leading to calculi are chemical, physiological, and pathological, for all which reasons medical treatment is required.

Further, the medical treatment pursued should not be limited to the period of the actual presence of a stone; for then, although more can be accomplished medically than is usually supposed, the best opportunity for medical treatment has passed. In fact, the treatment should be anticipative, and preventive, and not merely palliative or curative.

That is, every person whose urine contains an habitual and persistent deposit should seek medical advice, with a view to the correction of that non-natural condition of urine which predisposes to the formation of calculus.

The particular nature of the treatment demanded is determined, of course, by the nature and composition of the deposit, and need not here be described; that for persistent deposits of uric acid the urates and cystine has already been fully detailed in my Lectures, published in THE LANCET for the first half of the current year. We would in this place limit the observations we have to make to some few general particulars relating to the treatment of stone itself when actually formed, and first we would treat of solvent and especially alkaline remedies.

These remedies, as ordinarily employed, are not exhibited to anything like the extent to which they ought to be given. As usually prescribed, they are administered in doses of from ten, twenty, to at the most thirty grains two or three times a day.

This fact has been forced particularly upon my attention by the analysis of a quack remedy, called "*constitution water*," and which has acquired some celebrity in the treatment of some forms of stone. It consists of impure carbonate of potash, and the dose is the eighth part of a bottle, about the size of a wine bottle, four doses to be taken daily, equal to about 184 grains, or upwards of three drachms of carbonate of potash, a quantity which in some cases may be greatly exceeded with advantage.

Now, the doses of the other alkalies, alkaline carbonates, citrates, acetates, and tartrates, and indeed of nearly all the solvent remedies employed in the treatment of stone, may be increased in like proportion, and with similar advantage.

Another very important deficiency in the present mode of treating stone is, that the remedies resorted to, being usually only administered by the mouth, are conveyed to the stone in the bladder by the circuitous route of the circulation: in the blood, of course these remedies become much diluted, so that they reach the bladder in a greatly weakened form. Again, administered freely, alkaline remedies are apt to disorder the general health, modifying as they do so materially the vital and physical properties of the blood, as well of the various fluids secreted from it.

Now these disadvantages may be obviated by employing the solvent remedies used in the form of *injection*; these should not only be strong and frequently repeated, but they should be retained in the bladder as long as possible, or should be injected in a continuous stream.

I am of course aware that injections have been tried occasionally in the treatment of stone, with more or less success; but they are not resorted to nearly so frequently as they ought to be, nor have they been employed in the manner most conducive to success.

I would not be understood as recommending the use of vesical injections to the exclusion of constitutional treatment; this of course should be persevered in as well.

Again, in some cases medical may be combined with surgical treatment: that is, the stone having been reduced to fragments by the lithotrite, the solution of the fragments should be attempted by the employment of suitable remedies, both through the medium of the constitution and locally by injection.

The conditions of the urine tending to the production of stone which call for treatment are those in which that fluid contains persistent deposits of uric acid, urates, oxalate of lime, cystine, and the earthy phosphates. The calculi ordinarily met with are composed entirely or in part of one or other of these compounds; those composed of uric oxide, silicic acid, and carbonate of lime, are of such rare occurrence that it is scarcely necessary to take them into consideration at all.

The following figures show the relative frequency of the occurrence of the calculi most commonly met with:—

Of 1000 calculi, the composition of which has been ascertained, 372 consisted of *uric acid*, alone, or mixed with small