His tongue was moist and furred, not particularly red. He complained of some soreness of the throat, but swallowed without any difficulty. He had no cough. His skin was hot and moist, and covered over the chest and abdomen with a bright-scarlet rash, which disappeared on pressure. On the arms, legs, back, and beneath the clavicle the rash was more dusky; and, after pressure, minute purple spots (petechiæ) were perceptible over the whole of the body. On the night of the 13th he was violently delirious; a man had to be with him to keep him in bed. He was restrained with difficulty, bit and struggled with great violence. At five in the morning he became quieter, his breathing became difficult about six, and at half-past eight he died.

On the 13th the rash had not spread to his legs, so that we may suppose that he had not been ill above four days. His post-mortem examination was made, and, as he died in the hospital, it was made very carefully. The notes of it, which I have here, occupy eight closely-written quarto pages. I am not going to read them to you, for all that we found was just what we found in the last case—petechial spots under the mucous membrane of the stomach and the bowels, under the endocardium, under the pericardium, under the pleura. There was a little recent lymph at the base of the lung, probably of only a few hours' duration, and a spleen which weighed a pound and a half. There was a little congestion of his lung, such as might have resulted from his struggles; but beyond these appearances we found nothing.

Here is another case. A young man came under my care with a very little sore-throat, but covered with the rash, and with considerable febrile disturbance. After a violent fit of delirium, he sank exhausted, with a frequent, feeble pulse. He died in half an hour from the time he was running furiously about the ward. After death the appearances we found were precisely what I have described to you—enlarged spleen, subserous and submucous petechial spots, and that was all. In these cases of death from the general disease, you will have observed that in several there was a full, free, and even bright rash.

Sometimes, in place of the rash being bright red, it is more dusky. I told you it was dusky over part of the skin in one of the cases I have described. Sometimes there is more congestion of the lung. Sometimes, instead of violent delirium, there is muttering delirium. The girl, I told you, had muttering delirium, and then became violent again. Thus there may be extreme prostration instead of active delirium, or active delirium followed by extreme prostration. I have no account of the temperature in these cases, but it has been taken in similar cases, and has been found, it is said, to be high, very high; not, however, equal, so far as I am aware, to that which has been met with in acute rheumatism, but 104° , 105° , 106° . Some have supposed that in these cases the death is due to the high temperature. I am doubtful of that, myself, in scarlatina. I think that the death is due to the changed condition of blood. It is evidently very much changed, from the occurrence of the petechiæ; and the change in the tissues is dependent on the change in the blood. More observations on the temperature in these cases are required before its influence can be clearly admitted.

There are two other forms of scarlet fever where there is only the general affection, to which you ought to have your attention directed: both are very mild forms. One of these is scarlatina without inflammation of throat—*scarlatina sine anginâ*. I doubt its existence; that is all I can say about it. There are cases without soreness of throat, but if you look into the throat, in all the cases I have seen, there is distinct redness and swelling of the tonsils, at any rate—considerable abnormal vascularity of the fauces and pharynx. I have often known patients to say they had no sore-throat, but when I have looked I have always seen abnormal vascularity. So I do not believe in scarlatina without any throat affection.

The other form is scarlatina without any eruption. About this, too, I have some doubts. Of the other I speak from having seen a large number of cases in which the patients have vowed that they had no throat affection at all, and yet I saw it. The subjective symptoms were absent, the objective were present. But it is difficult to prove the absence of the eruption. Certainly people sometimes have the disease with a very little and a very transient eruption, just a little scarlet blush, lasting a few hours, and probably there are some cases in which there is none.

Thus I have described to you cases of that form of scarlet fever in which the disease is the poisoned state of the system only, and I have described cases illustrating varieties of this form of scarlet fever, from the mildest to the gravest. In no case mentioned to-day was there any grave local disease, —nothing complicating the general disease. What is the treatment of uncomplicated scarlet fever with trifling local specific process? For scarlet fever proper, for the poisoned condition of the system, so far as I know, we have no remedy. There are those who say ammonia is the remedy; there are who say that hydrochloric acid is the remedy; and so on with a variety of drugs. But at the present moment my experience tells me that we have no remedy for the general disease. We can only act upon the broadest general principles of calming the patient when excited; of stimulating him, to keep his heart beating, when he is exceedingly weak; of cooling the surface when the heat is excessive. Give him pure, fresh air to breathe. It is a disease that runs a definite course, and if you can keep the sufferers alive for a certain time they get well from that general disease which killed some of our patients. So you watch for the symptoms which threaten death, and endeavour to avert them. The very grave cases are hopeless from the first. For the mild cases little treatment is required. A cool room, light clothes, unstimulating diet, a mild aperient, a little chlorate of potash, which seems to allay the irritation or inflammation of the throat, and that is all you need do. Some give a little dilute mineral acid; sulphuric acid was all that used to be employed at the Fever Hospital when I was there. I think myself that the best is the chlorate of potash drink. I put a drachm of the salt into a pint of barley-water, and let the patient sip it down as he pleases.

You will see that it is very different when I come to speak of the complications of scarlet fever. For some of the aggravations of the acute specific process, for some of the throat affections, we may do a great deal, and there we often save life directly; but for the disease itself, that which is the root of the matter, I repeat, so far as I know, you can do nothing. By the chlorate of potash you may do something towards allaying the throat affection; by a mild aperient you may allay a little the febrile disturbance which would be increased by confined bowels; by keeping the room well ventilated you prevent any increase of fever from the room becoming too close, and its air impure. And all the time you must watch the state of the urine, the skin, and the throat. One point I desire to impress on you—namely, the great importance of changing the clothes of the patient, as well as the air of the room. The blankets of the bed should be changed frequently, as well as the body-linen and sheets.

THE CASE OF LEOPOLD I., KING OF THE BELGIANS.

By DR. G. DE KOEPL,

FORMERLY SURGEON-IN-ORDINARY TO HIS LATE MAJESTY LEOPOLD I.

To the Editor of THE LANCET.

SIR,—I sent a copy of THE LANCET of Nov. 6th, 1869, containing the statement which appeared under Civiale's name, and against the accuracy of which I felt it necessary to enter a public protest, to Dr. G. de Koepl, surgeon-in-ordinary to his late Majesty Leopold I. I requested him, if he felt at liberty to do so, to express an opinion relative to those statements, whatever it might be; and I have received in reply a paper from him, addressed to you, for the purpose of publication, which I beg leave to forward.

I am, Sir, yours &c.,

Wimpole-street, Jan. 1st, 1870.

HENRY THOMPSON.

[TRANSLATION.]

To the Editor of THE LANCET.

SIR,—The number for Nov. 6th, 1869, of your esteemed journal contains "The Case of Leopold I., King of the Belgians," drawn up by the late M. Civiale, member of the Institute, and transmitted to you by Mr. Walter J. Coulson.

The publication of that incomplete account of the calculous malady of the late King Leopold I. appears to me to have been originated less by scientific considerations than by motives of a personal nature. I propose to notice these, and to point out some inaccuracies that I cannot permit to pass without rectification. The truth above all things, and to everyone his right.

The position which I had the honour to occupy about the person of his Majesty Leopold I., during eighteen years, justifies, I think, my intervention. I have been, conjointly with Dr. G. Wimmer, the present Surgeon-in-Ordinary to H.M. Leopold II., eyewitness and earwitness of all that has passed since the commencement of the calculous affection up to the cure of the august patient. While rendering my sincere homage to the merits, to the talent and great experience of the late M. Civiale in any question of lithotrity, I must assert that nothing has justified the opinion of that celebrated surgeon when he affirmed that the bladder of the patient contained *several* calculi; and that there was "a small tumour seated near the neck of the bladder, and rather towards the right side," independently of a hypertrophied prostate. All those who subsequently explored the bladder of the late King recognised the hypertrophy of the prostate; but as to any intra-vesical tumour or tumours referred to by Civiale, and to which he attributed in great part the difficulties which opposed his treatment and caused the ultimate non-success of his efforts, no one was able to verify the presence of these tumours—neither M. Langenbeck, nor Sir Henry Thompson, nor Dr. Wimmer, nor myself. A tumour which, according to M. Civiale himself, had constituted an "organic malady" of the bladder, and which, after the removal of the last portion of stone, gave rise to no vesical symptom, but, on the contrary, permits the bladder to perform its functions properly,—the existence of such a tumour, I say, must necessarily appear at least questionable to an impartial observer. The same must be said of the multiplicity of calculi in the bladder. M. Civiale was never able to tell me why he supposed this to be the case, and nothing ever occurred to corroborate the supposition during the progress of the complaint.

I am obliged equally to rectify that which M. Civiale has written relative to the general condition of the King at the period when the operations of lithotrity were commenced. The health of the King was then very satisfactory. It became more and more deranged as the surgical treatment progressed. It was not until after the 14th April, 1862, that the most threatening complications occurred; such as pneumonia, uræmic symptoms, rigors, loss of appetite, loss of sleep, frightful pains about the bladder, considerable prostration, &c. With this category of symptoms, M. Civiale found it necessary to depart from his principle of "renouncing lithotrity and substituting lithotomy" when "the manipulations cause much irritation, when the sittings are followed by rigors and fever." I believe that I should hazard the opinion, that M. Civiale sinned by excess of prudence at the outset of his treatment; and that which he has written relative to the first period of his treatment confirms this idea. From the 1st March to the 13th April there were six sittings. Nothing extraordinary happened. The proceedings were well borne; and the general health, which had been shaken, began to improve, dating from the time of the first sitting. If Civiale had turned to account these six weeks, if he had made the sittings more frequent and more efficacious, if he had availed himself of the tolerance of the bladder as well as of the heroic courage of the patient, he would probably have prevented some of his Majesty's sufferings and dangers, and might have ensured a success which would have made it altogether unnecessary for him thus to publish the case. Precious time being once lost, M. Civiale could no longer do as he wished, and could only do that which circumstances permitted him to do.

On the 17th of July, 1862, Civiale made the last exploration of the bladder, and declared to the King that he found there nothing more, neither calculus nor fragment of calculus; that the King's sufferings would cease, and that his (Civiale's) mission was finished. Afterwards, alone with me, Civiale said, that when he injected the bladder, more fluid returned from it than he had thrown in; that there might be a sac, perhaps containing calculous matter, &c. This proves that there was some doubt in the mind of Civiale himself as to the absolute freedom of the bladder

from any calculus or fragment there. It appears to me more than probable that Civiale was aware, at his last exploration, that there might still be a foreign body in the bladder; but, finding his efforts tending rather to produce unexpected difficulties, and some results not merely negative, he dared not, in the interest of the patient, repeat those efforts, but believed it his duty to put an end to all operations by his declaration of the 17th of July, which coincided with his last visit to the palace.

It is evident that "the case of H.M. Leopold I." has been published in order to explain and to excuse Civiale's want of success, and to prove that the calculus subsequently crushed by Sir Henry Thompson was a *new* formation, and was, consequently, a *second* calculous affection, for which Civiale could not be responsible. In order to strengthen this hypothesis, Civiale has striven to prove the cure of the King after this treatment by the fact that his Majesty was able to take part at the *fêtes* of September, and that he had received deputations from the kingdom. It is true these solemnities took place, but those who were around the King knew what he endured during and after these ceremonies, which he determined to submit to, in spite of the severe sufferings (*tortures incroyables*) which they occasioned.

M. Civiale has observed that it would also be necessary to make some exposition of the etiology of this supposed "return" (*rechûte*) of the calculous affection after a surgical treatment of four months and a half—viz., from the 1st of March to the 17th of July, 1862. It is for this, indeed, that he insists, in his letter of August 2nd, upon the danger of suspending the injections. But it is quite certain that these injections were practised every day, and sometimes more than once a day, without interruption, up to the cure of the affection, and even afterwards. If Civiale writes that his letter of the 2nd of August was never answered, it must be an error on his part, for on the opposite column of THE LANCET appears the following:—"On Sept. 22nd Dr. Koepl wrote: The King will proceed in state to Brussels on the 24th; his general health is very good, but I cannot say the same for the condition of his bladder."

The history of the "third period of the case," which, according to its author, comprises the supposed "return" of the malady, is the most feeble part of Civiale's defence. The truth is, that *all the painful symptoms of the stone continued, without any notable change*, after the declaration of Civiale that there was no longer anything in the bladder, and that his mission was accomplished. The symptoms augmented, and at length became so violent that his Majesty summoned Prof. Langenbeck from Berlin towards the end of December, 1862—five months, and not "nine months," after the last visit of Civiale. Without entering into details—which I reserve for any future communication if necessary,—I limit myself to pointing out that the illustrious Berlin surgeon verified the presence of a calculus, but he could not, in spite of his dexterity, and in spite of repeated efforts, free the august patient from his stone, which rendered existence insupportable. After Langenbeck's attendance, which terminated on the 27th of March, 1863, there was no change in the King's condition; none in his sufferings, nor in the condition of the urine, nor in the general condition of the system; there was nothing but despair and—resignation. In this situation Sir Henry Thompson was called. He can publish, if he thinks it desirable, the result of his explorations, the details of his operations, and his remarks upon them. For my part, I wish to be only the faithful reporter of the result obtained.

Sir H. Thompson practised three short sittings of lithotrity, which were followed by the expulsion of a large quantity of phosphatic *débris*, identical with that which Civiale had obtained by his operations. These three sittings, made by Sir H. Thompson on the 6th, 10th, and 18th of June, 1863, had the following result: the consecutive, and soon complete, cessation of all pain before, during, and after micturition, the disappearance of every vesical symptom, a healthy state of the urine, and the complete cure of the calculous affection.

If the publication of "The Case of H.M. Leopold I." had for its object to lessen the merit of Sir H. Thompson, to whom exclusively belongs the honour of having cured the august patient of stone in the bladder, I am compelled to oppose myself with all the force of truth to this design. M. Civiale crushed a stone for the King without obtaining a cure; Sir Henry Thompson cured the King by crushing

the calculus he found. It is this which constitutes the difference in the results arrived at by the two surgeons respectively. Whether this latter calculus was a fragment which escaped the experienced hand of Civiale, or whether it was the product of a new formation, makes no change as to the fact of these results; but it still remains that Sir Henry Thompson put an end to the cruel sufferings of the King, which M. Civiale was never able to accomplish. I have already established in this letter my incredulity as to the supposed "return" of the calculous affection, on which Civiale insists so much. Langenbeck, Thompson, and Wimmer all joined in that opinion; and I have not the slightest recollection that they ever considered the calculus, which was the object of their operations, as a "return," or as a new formation.

There is a passage at the end of Civiale's paper which demands especial remark from me. It is there said, "In order to calm the apprehensions of the King, he was given to understand that the stone was merely a fragment which had remained behind since the former operation, and had become fixed in the walls of the urethra." This is inaccurate and incomplete. King Leopold I. was not a man one might dare to soothe by questionable statements. His Majesty had, in truth, after the departure of Civiale, a fragment embedded in the superior parietes of the urethral canal; but, independently of this, we never concealed from his Majesty the real state of affairs as regards the bladder.

All that I have related in this letter of "the case of King Leopold I." cannot, as far as regards the facts, be contradicted by those who had the honour to surround his Majesty during his illness, or those who have been in communication with his Majesty by writing. I might call to witness Sir James Clark and Sir Wm. Jenner among his physicians, as well as many other persons not belonging to the medical profession. As to the opinions I have here expressed, they may be erroneous, but they result from my convictions.

I beg you, Mr. Editor, to accept the expression, &c.,

DR. G. DE KOEPL,

Formerly Surgeon-in-Ordinary to his Majesty Leopold I., King of the Belgians.

Graz, Austria, December, 1869.

Dr. Wimmer, Physician-in-Ordinary to his Majesty Leopold II., who was associated in attendance on his late Majesty throughout the whole case, having carefully perused the above, writes to signify his entire assent to all therein contained.

ON THE
EFFECTS OF THE ANTISEPTIC SYSTEM OF
TREATMENT UPON THE SALUBRITY
OF A SURGICAL HOSPITAL.

By JOSEPH LISTER, F.R.S.,

PROFESSOR OF CLINICAL SURGERY IN THE UNIVERSITY OF EDINBURGH.

(Concluded from p. 6.)

BUT, returning from this digression, I must now speak of pyæmia after the major amputations, before and after the introduction of the antiseptic system.

The hospital records are unfortunately imperfect for one of the three years immediately preceding the antiseptic period. In the other two years, the mortality after amputations in my wards may be gathered from the following tables:—

Before the Antiseptic Period.

1864.

Seat of Amputation.	No. of Amputations.	Recoveries.	Deaths.
Shoulder ...	1	0	1
Arm ...	3	1	2
Forearm ...	3	2	1
Thigh ...	1	1	0
Leg ...	4	3	1
Knee ...	2	1	1
Ankle ...	3	2	1
Totals ...	17	10	7

1866.

Arm ...	2	1	1
Elbow ...	1	0	1
Forearm ...	2	2	0
Thigh ...	4	0	4
Knee ...	6	4	2
Leg ...	1	1	0
Ankle ...	2	1	1
Totals ...	18	9	9

On the other hand, we have—

During the Antiseptic Period.

1867.

Seat of Amputation.	No. of Amputations.	Recoveries.	Deaths.
Arm ...	1	1	0
Forearm ...	2	2	0
Knee ...	2	2	0
Leg ...	1	1	0
Ankle ...	1	1	0
Totals ...	7	7	0

1868.

Shoulder ...	1	1	0
Forearm ...	2	2	0
Thigh ...	1	1	0
Knee ...	8	5	3
Ankle ...	5	5	0
Totals ...	17	14	3

1869.

Shoulder ...	2	2	0
Arm ...	2	2	0
Forearm ...	2	1	1
Thigh ...	1	0	1
Knee ...	3	2	1
Leg ...	3	3	0
Ankle ...	3	3	0
Totals ...	16	13	3

Comparing the aggregate results, we have—

Before the antiseptic period, 16 deaths in 35 cases; or 1 death in every $2\frac{1}{5}$ cases.

During the antiseptic period, 6 deaths in 40 cases; or 1 death in every $6\frac{2}{3}$ cases.

These numbers are, no doubt, too small for a satisfactory statistical comparison; but, when the details are considered, they are highly valuable with reference to the question we are considering. This is especially the case with amputation in the upper limb, where neither injuries requiring primary amputation nor the operations involve, as a general rule, much loss of blood or shock to the system; so that, if death does occur, it is commonly the result of the wound assuming unhealthy characters. It happens that there were 12 amputations altogether in the upper limb in each of the two periods referred to. Of the 12 cases before the antiseptic period, no fewer than 6 died—a frightful mortality certainly. And it is recorded that, of those 6, 4 died of pyæmia, and 1 of hospital gangrene. Also that one of those which recovered had pyæmia; but, though the symptoms were well marked and severe, presented an example, unhappily too rare, of recovery from the disease.

Very different was the result of the corresponding amputations during the antiseptic period. Eleven of the 12 cases recovered; and the one death which did occur was not the result of the operation, but took place in spite of it, from pyæmia, which had resulted from fetid suppuration in a metacarpal bone, and continued after I had removed the hand, in the faint hope that the constitutional mischief might be thrown off when its original source had been taken away. Some of the successful cases, I may add, were by no means favourable subjects for operation: as, for instance, a completely shattered hand in a very aged person; the avulsion by machinery of nearly the entire arm, one of the flaps of the amputation at the shoulder-joint being left contused and lacerated as it had been formed by the injury;*

* This case was treated by my colleague, Dr. Dunlop, during my temporary absence.