resulting injury, though very serious, will compare not unfavourably as regards the possibility of recovery with similar wounds caused by the Martini-Henry or Snider rifles.

I have seen a good many penetrating wounds of the chest inflicted by Martini and Snider rifles, but I have never seen one recovery when the bullet passed through the lung. In all such cases that I have seen the patient either died within a few hours from shock or hæmorrhage, or subsequently from pneumonia or other complications. I attribute the recovery of the sepoy wounded in the chest, whose case I have quoted to the small calibre of the bullet and to the fact that there was no setting up. The small wound in the skin caused by the small bullet can be very effectually closed by the natural elasticity of the tissue and deep septic infection of the bullet track thereby prevented. The jemadar suffered so little from shock that he was able to walk into Camp Maidan, a distance of two miles; the sepoy wounded in the buttock had lost a good deal of blood before he was brought to hospital, and was suffering from shock, partly the result of that loss; the sepoy wounded in the chest was in a very precarious condition for some hours, suffering from profound shock.

I am inclined to believe that the high muzzle velocity of the Lee-Metford, associated with a bullet of small calibre, tends rather to drill a hole in even a massive bone than to smash it up into small fragments; and that at short range the dum-dum bullet does not set up, even if it impinges on a large bone.

THE ROENTGEN RAYS IN BULLET EXTRACTION.

By I. L. G. GILLANDERS, M.B., C.M.ABERD.

On February 11th I was asked to undertake a small operation. The patient, D. A., aged 14, whilst cleaning a pistol, accidentally shot himself (January 24th), the bullet lodging in

Fig. 1.

the palm of the right hand. The wounded hand had been subjected to the Roentgen rays on February 1st, the skia-

graph revealing the bullet lying in the second interosseous space close to the proximal ends of the first and second metacarpal bones (Fig. 1).

When I examined the patient (February 11th), I found the wound of entrance situated $\frac{1}{2}$ inch from the distal end of the third metacarpal bone on the palmar surface. The hand was oedematous, and the patient complained that he "was losing the power of his fingers."

The following day I operated, Dr. Tipper, Coldharbour Lane, acting as chloroformist. I made a free incision in the palm, carefully explored the first, second, and third interosseous spaces, but with a negative result. Then I controlled the chloroform, and Dr. Tipper operated. He also met with non-success. The operation was postponed, and next day the patient was taken to Mr. Glew's radiographic laboratory. I placed a strip of lead foil along the wound. Another skia-graph was taken which indicated the comparative relationship of my incision to the bullet (Fig. 2).

Fig. 2.

I then resolved to operate on the hand while the rays were actually passing through the tissues, and substituted the fluorescent screen for the sensitive plate. The strip of lead foil was removed, Dr. Tipper put the patient under chloroform, and the room was darkened. Mr. Glew controlled the Roentgen apparatus, and held the screen between me and the boy's hand. On introducing the bullet forceps into the wound a perceptible shadow of the instrument was thrown upon the screen, and my own hand appeared in skeleton outline. Removal of the piece of lead was a question of very few minutes. I was so struck by the success and rapidity of the operation that I venture to make the following suggestions with regard to the removal of foreign bodies.

First, that the direct use of the screen is preferable in many cases to operating by the aid of a fixed skia-graph, provided that the foreign body is lodged in the less dense tissues.

Secondly, that it is advisable to place over the skin a piece

of metallic foil, or other opaque substance, indicating the line of proposed incision before operating.

Of course a foreign body can be accurately localised by one of several methods known to the profession—for example, by Messrs. Rémy and Contremouline's plan, by the ingenious method adopted in Dr. Waymouth Reid's case, by the cross wires, etc. All these methods take time, involving as they do reskiagraphy and complicated mathematical measurements. Yet I see no reason why, with perfected apparatus, one could not extract bullets and other foreign substances from any part of the body by the direct use of the screen a few minutes after their lodgment in the tissues. In military field surgery, or in our large factories and ironworks, this method of operation for obvious reasons ought to be of great value.

The failure of my first operation, I believe, was due to the fact that the bullet had become flattened against the bone, and was bound tightly down by the powerful flexor tendon. After removal of the bullet recovery was uneventful, with the exception of the appearance of dermatitis. It is interesting to note that desquamation took place from both the dorsal and palmar aspects of the hand. It appears to my mind that if the Roentgen rays cause such extensive superficial disturbance, there must needs be an immense field for original research open to dermatologists. Yet curiously enough up to the present time the practical application of the rays in this respect has been of little or no therapeutic value. The rays have been proved to exercise no appreciable influence upon cultures of tubercle and diphtheritic bacilli, but has the effect been tried *in extenso* at our skin hospitals? Do the rays act simply as a depilatory, and is the curative action in skin diseases *nil*?

ON THE CURATIVE POWER OF THE ANTI- VENOMOUS SERUM

USED FOR THE TREATMENT OF AUSTRALIAN AND INDIAN
VENOMOUS SNAKE BITES.

By A. CALMETTE, M.D.,
Director of the Pasteur Institute of Lille.

In the *International Medical Journal of Australasia*, Melbourne, August 20th, 1897, Dr. C. I. Martin, Professor of Physiology in the Melbourne University, published a paper concerning the curative power of the antivenomous serum on animals inoculated with Australasian snakes' venom.

In this note Dr. Martin describes the experiments he has undertaken in order to ascertain whether the antivenomous serum prepared in the Pasteur Institute of Lille is, as I have stated on several occasions, as active against the venom of Australasian snakes as against that of Indian cobras or American crotalus. By these experiments he has shown that in the Australasian venom there are two constituents: one is easily destroyed by heating, the other, separable from the former by filtering through a porous earthenware filter impregnated with gelatine, is very resistant to heat.

According to Dr. Martin my antivenomous serum might have a manifest preventive power against the latter constituent, which is the more poisonous, especially when the antivenomous serum is injected intravenously a few hours before the animal is inoculated with the venom. On the other hand, Dr. Martin says my serum has almost no action, either preventive or curative against the former constituent.

The experiments of Dr. Martin have been made in a manner so different from mine that I am not at all surprised at the results he has obtained, and what is more, if one follows the method described by him, I must allow that one will find that his statements are correct. I will, however, take the liberty to say that his experiments do not at all invalidate the results which I have obtained and published concerning the curative power of my serum.

Dr. Martin does not take into account an essential point which has to be kept in mind when experiments with snake venom are made—namely, that the resistance of animals increases with their weight; and, on the contrary, that in order to protect or cure an animal, the quantity of serum necessary is greater in proportion as their weight is smaller.

For example: in order to kill a guinea-pig weighing about half a kilogramme in twelve to twenty-four hours with the venom of *pseudechis porphyriacus*, it is necessary to use, on an average, $\frac{1}{1000}$ of a milligramme—that is, $\frac{1}{1000}$ of a milligramme for one kilogramme of guinea-pig; whereas, in order to kill in the same time a rabbit weighing 2 kilogrammes it is necessary to use $\frac{1}{80}$ of a milligramme—that is, $\frac{1}{80}$ of a milligramme for one kilogramme of rabbit. In order to kill a dog weighing 8 kilogrammes in twelve to twenty-four hours, a dose of 4 milligrammes is necessary—that is, $\frac{1}{2}$ of a milligramme for 1 kilogramme of dog. The following table brings out these numbers:

For a guinea-pig ...	$\frac{1}{1000}$	of a milligramme for 1 kilogramme.
For a rabbit ...	$\frac{1}{80}$	"
For a dog ...	$\frac{1}{2}$	"

If experiments are made upon other animals, offering various degrees of resistance, such as the ichneumon or the hedgehog, it is found that in order to kill an ichneumon it is necessary to use fourteen times more venom than is necessary for killing a rabbit of the same weight. Consequently, there is no exact correlation between a dose of venom fatal in twelve to twenty-four hours, and the weight of the animals to which that dose is fatal. Everything depends on the resistance of the kind, and on the size of the same.

These principles being clearly understood, it will be easy to see that a much smaller dose of venom will be required to protect or cure a dog or ichneumon than would be necessary to protect or cure a rabbit. For the natural resistance (or want of sensitiveness) of the dog and ichneumon to the venom is much greater than that of the rabbit. It will be evident, also, that in order to immunise a guinea-pig it will be necessary, weights being equal, to use four times more serum than to immunise a rabbit.

Dr. Martin in his experiments does not take any account of these differences of susceptibility, and he inoculated his rabbits with doses of antivenomous serum which were much too small. During the last seven years I have made a very large number of experiments with venoms of all kinds. I have studied the venoms of Australian snakes, the venoms of nearly all the kinds of snakes found in India, Indo-China, Africa, and America. I have been able to satisfy myself that all these venoms derive their toxicity from a substance which is the same in all these snakes. I have found, however, that some of them produce very intense local phenomena, some hæmorrhages, others simply bulbar paralysis.

The venom of viperidæ differs from that of the colubridæ generally by its very intense local effects. These local phenomena are prevented by heating the venom to 75° C., after which the venom of viperidæ produces exactly the same toxic effects as that of the colubridæ.

On the whole, apart from these local effects and the production of hæmorrhages, there are only differences of intensity in the various venoms; these variations are quite comparable to the differences of virulence observed in various races of streptococci or diphtheria bacilli.

I maintain in the most categorical manner, and in spite of Surgeon-Major Cunningham and Professor Martin's adverse opinion, that the serum of an animal hypervaccinated against a very active venom, such as of the Indian *naja tripudians*, can, when injected in a sufficient quantity, prevent the death of an animal inoculated with fatal doses of venom of the *daboia russelii*, *pseudechis porphyriacus*, *crotalus adamantus* or *bothrops atrox*.

The experiments I made in London (July, 1896) in the laboratory of the Royal College of Physicians and Surgeons were quite conclusive. On that occasion, in order to prove the preventive and curative effects of my antivenomous serum, I used a mixture of equal parts of the venoms of the Indian *naja*, the American *crotalus*, and the *bothrops lanceolatus* from the Martinique. The horses from which I had obtained the serum used in these experiments had not been inoculated either with the venom of *bothrops* or *crotalus*, but only with very large doses of the venom of *naja tripudians*.

If Dr. Martin had repeated these experiments in the way described in my publications, he would undoubtedly have obtained the same results. I consider that I have absolutely proved this first point, that is, that the toxic principle of various venoms is identical and that my antivenomous serum is efficacious against them all.

Dr. Martin's second contention, that my serum is too weak to be of any value for treating people bitten by snakes, I will answer by the following evidence.

First, let us consider the experimental facts.

If Dr. Martin be good enough to repeat his experiments