CORRESPONDENCE.

Letters were read from Dr. J. Hall Davis, and Mr. Gascoyen of London, acknowledging the receipt of the announcement of their having been elected honorary members of the society; from Dr. Barker, transmitting a pamphlet on Provident Dispensaries.

EXPLANATION.

Dr. Bowen, the hon secretary, who was present for the first time since his return from Europe, gave some explanation with reference to his re-election during his absence. He had written to the acting hon secretary (Dr. Lilienfeld) making mention of his possible longer stay, but the Society had not been made aware of that communication.

NEW MEMBER.

Dr. Macqueen of Camperdown was elected a member of the Society.

The following paper was then read for the author by Dr. Neild:

ON A CASE OF PARACENTESIS THORACIS.

By JAMES JAMIESON, M.D.

J. C., a boy in his twelfth year, complained of a violent pain in the left side. This gradually wore off, but as he continued ailing, I saw him four days after, on the 16th August. I found a considerable amount of effusion into the left pleura; but as there were no severe symptoms I prescribed a diuretic mixture, and a blister to the side, hoping that absorption would take place naturally. On the second day, however, the effusion had increased greatly; dullness...
extended quite to the clavicle and across the sternum; the heart
was felt beating on the right side. The general symptoms too had
gained in severity; the pulse was 144; and when he was raised,
the parents had been alarmed by approaching syncope. In view of
all the circumstances, I thought it at least advisable to remove the
fluid by operation, the following method being adopted: The
point of an ordinary hypodermic syringe was made to perforate the
thoracic wall in the usual situation, and after being inclined upwards
was pressed freely on. The piston of the syringe was then drawn
back till the fluid showed itself, when the canula was unscrewed and
left in the wound, a piece of elastic tubing being at once slipped
over the end of it and fastened on. A piece of a catheter was
inserted into the other end of the tubing, and suction made till the
fluid rose, when the end was dipped into a test-tube filled with
water, standing in a larger vessel. Any remains of air were
removed by stripping the tubing down, and the vessel was placed
on a stool at the side of the bed. The serous fluid trickled away
pretty freely, but as it began to come away more slowly after about
three or four hours, I attached a suction enema apparatus to the
tube, and thus removed about half a pint in a short time, the whole
amount being about fifty ounces. The canula was then withdrawn,
and no dressing was wanted. It is unnecessary to give details of
the progress of the case, as the whole interest centres in the mode
of performing the operation. Iodine was painted on the whole side,
and small doses of iodide of potassium given for a few days, the
improvement being regular and uninterrupted.

The state of opinion with reference to the operation of paracent-
esis in acute pleuritic effusion has altered considerably within the
last few years. On the one hand, the risk attending the entrance
of a small quantity of air is now considered to have been once
much exaggerated; and on the other hand, the propriety of operat-
ing earlier and of course more frequently, is much insisted on by
the best physicians, as shortening the course of the disease, and thus
preventing the occurrence of the alarming symptoms caused by the
displacement of the viscera, especially the risk of fatal syncope or
asphyxia, as well as the after danger of the lung being permanently
bound down by adhesions. That the operation is generally con-
sidered an important one, and the admission of air a thing to be
carefully guarded against, are proved by the multitude of special
methods proposed and complicated instruments devised for rendering
the procedure safer, if not simpler. By the method now described,
the operation may be said to be removed into the domain of minor
surgery, having this further advantage, that its first stage may be
made to serve diagnostic purposes with perfect safety, if there should
be any doubt as to the exact nature of the case. The only dis-
advantage attending it is the length of time required, the objection
being lessened by the circumstance that the patient may be left
without fear of any untoward accident. Of course the time required
may be very much shortened by having recourse to suction by means
of some elastic apparatus, the most convenient probably being one of
the elastic ball syringes, which should be emptied of air by pressure, the pipe inserted into the elastic tubing attached to the canula, and the ball allowed to fill itself, when it can be emptied and reapplied. If it is considered necessary, the tubing may be closed by pressing its sides together while the syringe is being changed; but it can scarcely be said that air can, almost by any possibility, pass back through the canula after it is filled with fluid.

I shall be glad to learn the opinion of the members of the Medical Society on the operation now described, which may not be quite original, but which I have not seen elsewhere described.

In the discussion which followed:

**Dr. Martin** observed that he had seen by a late number of the *Medical Times and Gazette* that the subject of paracentesis thoracis was receiving a good deal of attention at home. There appeared to be two modes of practice in this operation, one consisting in the drainage method, the other in the subcutaneous puncture. The dread of admitting air into the pleural cavity was now however only a bugbear of the past. He (Dr. M.) had never witnessed any injurious effects from the admission of air. He was, however, a little surprised at a gentleman of Dr. Jamieson’s erudition giving elaborate instructions as to the apparatus he described, when so much more perfect a one was available. Dr. Martin exhibited and explained one of the latest kinds of aspirator, and showed how convenient and suitable for the purpose it was. The aspirator, however, was more suitable for removing fluids connected with the liver, on account of the risk—when used in the chest—of the sharp trocar wounding the pulmonic pleura. Dr. Jamieson’s mode of draining the fluid under water had its advantages.

**Dr. Wigg** thought the risk of wounding the lung easily avoidable by a suitably modified apparatus. He remarked incidentally that in one case in which he had used the aspirator the fluid (25 ounces) almost immediately coagulated.

**Mr. Ellery** suggested that if the needle of the aspirator were made of a telescopic form so that one section could slide over the point, the risk of wounding the lung would be prevented.

The **President** believed that paracentesis thoracis was much more frequently resorted to in the colonies than at home.

**Mr. Gillbee** complimented the author of the paper in having suggested to those who entertained a fear of introducing air into the chest, a mode by which it could be avoided.

**Dr. Black** made mention of a method of fastening to the end of the canula a piece of gut which, collapsing as soon as the fluid ceased to run, effectually prevented the influx of air.

**Specimen from the Melbourne Hospital Museum.**

**Dr. Wigg** exhibited a number of kidneys illustrative of various forms of renal disease, which had been recently added to the Melbourne Hospital.
CLOVER’S CHLOROFORM APPARATUS.

Dr. Wigg also exhibited and explained Clover’s chloroform apparatus, the particular merit of which consists in its enabling those administering chloroform, to give the anaesthetic with a definite quantity of atmospheric air.

A conversational discussion followed, in which the opinion was somewhat generally expressed that the ingenuity of the apparatus hardly compensated for the absence of that simplicity in the mode of administration which, as the result of much experience, was now adopted. It was confessed, however, that the possibility of preventing an apartment becoming full of the vapour of chloroform—a common result during some operations—was an unquestionable advantage.

The following paper was then read:

ON A CASE OF TETANUS TREATED PRINCIPALLY BY CHLORAL HYDRATE.

By William Gillbee, M.R.C.S.
Surgeon to the Melbourne Hospital.

This case of tetanus, although put down in the notice-paper as treated by Chloral Hydrate, was, in fact, not treated by that drug alone, but also by the extract of Calabar bean. I do not think, however, that the latter had very much to do in bringing about the successful result. Some few years ago I read a paper before this Society on the treatment of tetanus by chloroform, in which I attributed the success to the chloroform, but unfortunately in that case the treatment was of a mixed character, as I had also given calomel and opium. I then stated that a similar case occurring I would treat it by chloroform alone. I did so, and it terminated fatally, not so much I believe from the failure of the chloroform, as the difficulty of keeping the patient continually under its influence, a condition imperative to render it successful.

In the chloral hydrate we have a remedy of the same character, with the advantage that its action is more permanent and more under the control of the surgeon.

Dr. Rae, in his paper upon chloral, in the *Edinburgh Journal* of last November says, that “It has the power of resolving muscular spasm or rigidity,” and that “in chloral the long-dreaded tetanus seems to have met its match, and if they have not hitherto fought fair, it is, to use a pugilistic phrase, only because the seconds have not been aware of the rules of the ring.”

The question of transformation of chloral into chloroform in the blood has not yet been settled, but all agree that transformed or untransformed, it acts directly on the cerebro-spinal centres, it modifies profoundly the number and rhythm of the movements of the heart, it causes progressive slackening of the movements of the diaphragm, and lowers considerably the temperature. This lowering of the animal temperature must be looked upon as important in the treatment of tetanus, for Dr. Macnamara of India...
who has had a large experience, states that where the temperature of the body is below 100° F. he would give a favourable prognosis. If the temperature of the patient’s body, however (especially in the morning), rises to 101° there is danger to be apprehended; should the temperature rise suddenly from 99° or 100° to 102° the patient is in imminent danger; and that he has seen few cases of tetanus recover after the temperature of the body has risen to 103°.

In the medical journals, during the past twelve months, there have been numerous cases recorded of the successful treatment of tetanus by chloral.

Darigo gave a boy ninety grains a-day for fifty-four days, and cured tetanus. Grandisso-Sylvester gave a girl, eight years old, half an ounce in five days and cured. Bensasson of Tunis gave a boy of thirteen, five ounces in thirty-five days, and cured; and in a case in New York they gave over five ounces in eighteen days. Dr. Alexander Ferguson, in a case of tetanus, gave 120 grains daily for four weeks; with this he continued the bromide of potassium, of which he gave ninety grains a-day; in the aggregate 3,600 grains of chloral and 2,700 of the bromide of potassium.

At the same time it cannot be denied there are others who do not place the same confidence in it as a specific for tetanus.

Dr. Macnamara, who has had opportunities of seeing this disease on a large scale, states that he treated ten consecutive cases of tetanus in the Chandine Hospital with the hydrate of chloral; this drug being the only medicine prescribed. The quantity of chloral administered varied from 40 grains to 160 grains per diem, according to the severity of the symptoms and the age of the patient.

Of these ten patients treated exclusively with chloral, seven were instances of traumatic tetanus and only one recovered; two cases of idiopathic tetanus were thus treated, one of these recovered, as also did a case of the disease occurring fifteen days after childbirth, and his experience had led him to the conclusion that he was hardly justified in trusting to the chloral alone in the treatment of severe cases, but recommends that the extract of Calabar bean be also used.

In his treatment of a case by the extract of physostigma, he gave one grain every second hour the first day, with 40 grains of the chloral hydrate at bedtime. This was continued for two days. On the third day he had increased the Calabar bean to one grain every hour; on the fourth day to two grains every hour, with the chloral at bedtime; on the seventh day he gave three grains every hour. When the patient had taken no less than 48 grains of the extract of physostigma during the 24 hours, the tetanic convulsions became then much less violent. The medicine was continued however in the same doses throughout the following day, in consequence of a return of the tetanic spasms, and so on till the tenth day, when the patient was much easier; it was then gradually diminished. After the thirteenth day the tetanic spasms were merely nominal, and the patient gradually recovered. He consumed altogether one ounce and
six drachms of the extract of Calabar bean, and an ounce and five drachms of the hydrate of chloral. He observes that it is impossible to lay down any rules regarding the amount of physostigma which we can administer, but as soon as it produces vomiting, diarrhoea, or a rapid small pulse, and clammy sweat, we are bound to discontinue its employment at once.

In the present case, the quantity of Calabar bean I gave was small, the eighth of a grain every two hours, and its action as a therapeutic agent was not observable; the good results were in my opinion derived from the chloral hydrate, of which I gave 20 grains every two hours, with an additional dose at bedtime. From the 15th April to the 24th he had from 140 to 170 grains per diem, altogether he had 4,835 grains, or a little over 10 ounces of the chloral hydrate, and some 70 grains of the Calabar bean. The pulse varied from 80 to 123, and the temperature from 100° to 105°.

For the notes of the case, which have been taken with great care, I am indebted to my dresser in the hospital, Mr. D. Wilkie.

CASE—James Fagan, set. 10, residing in Hotham, admitted 12th April, 1872. Eight days before admission was cleaning part of the machinery in the rope-walk in which he was employed, when a portion of the machinery fell, inflicting ten small punctured wounds on the back of his hand. He immediately applied at the hospital, and his hand was dressed with water-dressing. He attended two or three times as an out-patient, and was then discharged, the wounds having cicatrized.

Twenty-four hours before admission he complained to his mother of violent pain in back and loins, and slight stiffness of the muscles of the jaw and the back of the neck, and she noticed that he had arching of the back.

April 12. — On admission: — The countenance was pale and anxious, the “risus sardonicus” being well marked. Spasms were frequent and severe, occurring at intervals of about thirty minutes. He was ordered an enema of Ol. Ricini and the Extract. Physostigmasis Calab. gr. ½ every two hours; also, ice-bag applied to the spine.

April 13. — Very much the same to-day. Bowels not having been moved, he had Ol. Crotonis m. ss.

April 14. — The spasms are becoming more frequent. Opisthotonos is well marked, the body being bent up into the form of a bow. He complains of intense pain. The abdominal muscles are constricted and tympanitic. Respiration is quick. The pulse is quick and irregular. He has had no sleep during the night. At 9 p.m., three doses, each containing twenty grains, of hydrate of chloral were ordered, one to be taken every hour.

April 15. — This morning all three doses of the chloral were taken, after which he slept for several hours. He still continued taking the Calabar bean, gr. ½ every two hours. Pupils normal.

April 16. — Did not sleep well during the night. Had a great many spasms. Chloral-hydrat. gr. xx. was ordered to be taken every two hours.
April 17.—Restless during the first part of the night, but afterwards he obtained a few hours good sleep. At 4 p.m., became much worse, the spasms becoming very frequent. He cried out with the pain in his back, and bit his tongue several times during the spasms. Complains of stiffness of the muscles of the jaw. Physostigma every hour gr. $\frac{1}{3}$. 10 p.m.: Spasms much more frequent; brought on by the slightest movement or attempt to swallow. Ordered Chloral-hydrat. 3 ss. to be taken immediately, and also to continue taking twenty grains every two hours. During this time the spasms were extremely irregular; at times occurring every minute; seldom five minutes intervening.

April 18.—Much better this morning, after taking three doses of chloral; the first of thirty grains, and the other two of twenty grains each. He slept well during the rest of the night, and has had very few spasms since. Very slight stiffness of the muscles of the jaw. Complains of pain; abdominal muscles still very hard; appetite good.

April 19.—Slept well last night, after having the extra dose of chloral of thirty grains, and the usual twenty grains every two hours.

Continued much in the same state till the 24th, when the chloral was discontinued during the day, and only one dose of thirty grains given at night, but still continued taking the Calabar bean gr. $\frac{1}{3}$ every hour. After this he had a relapse, the spasms becoming very frequent and severe, and on again continuing the chloral he improved rapidly.

May 8.—Physostigma discontinued, as it was considered useless, the pupils never having been affected. Still takes the chloral, twenty grains every two hours.

May 14.—Stiffness of the muscles entirely gone. At times feels slight pain in his feet. Ice to the spine discontinued. Still takes the chloral. No spasms.

May 17.—Much the same. Chloral only taken at night, gr. xx every two hours till he goes to sleep.

May 21.—Got up to-day and was walking about the ward. Chloral discontinued.

From the 12th April to the 17th April, he had $\frac{1}{3}$ gr. of physostigma every two hours, 1$\frac{1}{2}$ per day. From 17th April to 8th May, $\frac{1}{3}$ gr. every hour.

**QUANTITY OF CHLORAL TAKEN.**

From 15th April to 24th April, he had from 140 to 170 grs. per diem.

On the 25th, had only two doses at night, 40 grs.

From 26th April to 16th May, had from 140 grs. to 170 grs. per diem.

From 16th May to 21st May, only took it at night till he went to sleep, 20 grs. every two hours, generally had 3 doses; 31.

On the 21st May, chloral discontinued.

From 15th April till 21st May, he had altogether 4,835 grs. of chloral.
The temperature varied from 100° to 101°, but when the spasms were frequent, it was as high as 105°.

The next paper was as follows:

ON A CASE OF LITHOTOMY, FATAL FROM EMBOLISM OF THE PULMONARY ARTERY.

By P. H. MacGillivray, A.M.

Surgeon to the Bendigo Hospital.

Thomas S., aged 3½ years, was admitted to the hospital on 18th September. His father brought a note from a medical man who had attended him, stating that he was suffering from stone, and he informed me that the symptoms had been present for two years. He was sounded the same afternoon, and a stone was readily struck. Having arranged to operate on the 21st, a teaspoonful of castor oil was given the night before. This made him sick, and was repeated early in the morning. He was again very sick. At two the rectum was thoroughly cleared by an enema of warm water; and about half an hour before the operation a small quantity of brandy-and-water was given. During the earlier part of the day he was feverish and much upset, seemingly from the castor oil; but at the time arranged for the operation he was much better, and there seemed no reason for postponing it.

The usual lateral operation was done. Some difficulty was experienced in getting the knife fairly lodged in the groove of the staff. The urethra at first cut very tough, so much so that the surgeon holding the staff remarked that he thought my knife must be blunt. As there was no escape of urine or distinct feeling of entering the bladder, the knife was run along quite to the end of the groove. The finger was then introduced, the stone at once felt, and the staff removed. The bladder was very large, and the stone rolled about so that it could not be extracted with the scoop which I first used. I then introduced a small pair of forceps, withdrawing my finger, when there was an immediate gush of urine. The stone was at once seized and extracted. A small piece of the outer layer broke off, and was removed with the finger and scoop. The bladder
was finally washed out. After the first introduction of the knife into the groove, there was no difficulty about the operation; and the haemorrhage was very trifling.

After the operation, the patient progressed in all respects well. There was no sickness, and the urine soon commenced to escape by the wound. At night the pulse was 130. As he was rather restless, he had a draught with three drops of tincture of opium. On the morning of the 22nd, he was very well, pulse 120, urine coming away freely. During the day nothing unusual was observed. He talked to me at my visit, and was all that could be desired. At the evening visit, at nine, everything was right. Afterwards, he became more restless, and another opiate (3 drops) was given at 1 a.m. He then fell asleep. He was well watched, but not disturbed, as he seemed sleeping quietly. Shortly before seven, the attendant noticed that his appearance was changed, that he was pale and breathing becoming very short and quick. When he spoke to him, he opened his eyes and recognised him. Mr. Penfold, the assistant-surgeon, was at once called, and found him dying. He thought at the time that the appearances were those of embolism. The lad died at half-past seven.

The examination was made four hours after death. The operation wound was quite healthy. The rectum was uninjured. The urethra in front of the bladder was enormously dilated, and its walls thick. It had been opened for about an inch and a half by the knife. The neck of the bladder was very wide, readily admitting the finger. It was so wide, that the knife, in running along the groove, had only divided the mucous membrane. The bladder was much hypertrophied. The ureters were greatly dilated, the right especially, being almost as large as the small intestine. There was some extravasation of blood about the neck of the bladder, and a short distance up its left side. There was nothing about the parts concerned in the operation to, in any way, account for the death. On opening the chest, numerous old tough adhesions were found in the left pleura. Both lungs were congested posteriorly. The pericardium contained an unusual quantity of serum. The left auricle was filled with soft clot. The ventricle was empty. The right ventricle was filled with more consistent clot, extending into the pulmonary artery. The left pulmonary artery was tightly plugged at its first division, by a firm, tough, fibrinous clot.

The stone was elliptical, smooth, long diameter 11-12ths of an inch, short diameter 9-12ths. It weighed 120 grains.

This case is interesting in respect to the condition of the urinary organs, as revealed by the post mortem examination, and as to the cause of death.

There can be no doubt that the dilatation of the urethra and neck of the bladder was caused by the stone being constantly forced forward by the child's straining in micturition, and that the hypertrophy of the bladder and dilatation of the ureters was caused by its blocking up the urethra. When brought to the hospital, the stone was in the bladder as it was at the time of the operation. The slight difficulty in
exposing the groove of the staff seems to have been caused by the thick dilated urethra being pushed in folds against it.

The death from plugging of the pulmonary artery, shows the necessity of carefully examining the heart and vessels when the cause of death from operation or injury is obscure. Of course no case is safe from such a contingency; and in this instance, the lad progressed so favourably after the operation, that there was no reason to anticipate any other than the usual favorable result of lithotomy in children.

Mr. MacGillivray also exhibited a pendulous fibrous tumour he had recently removed from the groin of a man.

**PAPYLOMATOUS GROWTHS.**

Mr. Wilkins exhibited a number of small growths removed by the aid of the laryngoscope from the laryngeal surface of the epiglottis and vocal cords. After removal, the power of deglutition, which had been much impaired, was greatly improved. They came on after an attack of laryngitis, and had been the cause of much suffering to the patient. Mr. W. had removed them in two sittings, and he was going in a few days to excise those which still remained.

The thanks of the meeting were given to the several authors and exhibitors.

---

**THE EARLY HISTORY OF VACCINATION.**

By Alexander Paterson, M.D. Ed.

[Read before the Philosophical Society of Adelaide, August 13, 1872.]

(Continued from page 275.)

Small-pox is a disease of great antiquity. Moore says that it was known among the Chinese 1,100 years before Christ. In the Hindoo mythology, there is said to be a goddess specially devoted to it. Some ingenious authors have attempted to show that the plague which was sent by Moses on the Egyptians, was a virulent form of this disorder attacking both men and cattle, and that the Athenian plague described by Thucydides, and the various epidemics which devastated Rome in its early days, were attributable to the same cause. Livy, describing one of these, says, that man and beast were equally affected, and remarks that ministration on the sick and contact propagated the diseases. But it is mere conjecture, incapable of proof, to suppose that these plagues were due to small-pox. Ahrun, a Christian priest and physician of Alexandria, is celebrated as being the earliest writer who notices small-pox and measles, which he described under one heading, evidently regarding them as the same disease. He lived in the reign of the Emperor Heraclius, A.D. 610-641. Rhazes is the first author whose treatise on small-pox and measles has come down to our times in an entire state. Mohammed Ibn Zacaryia Ar-Rhazi, commonly called Rhazes, was a native of Rai, near Chorasan in Persia, and is supposed to have died about A.H. 320,
A.D. 932. He attained great reputation as a physician, and was a most voluminous writer, having published over 200 treatises on medicine. From the numerous authorities which he quotes, it is evident that small-pox had deeply engaged the attention of the Arabian physicians. Some idea of the frequency of its occurrence in his time, may be gathered from his remark, that scarcely "any one escapes it." The excellence of his treatise made Albert von Haller rank him alone with the Greek fathers of medicine.

Small-pox, however, during the twelve centuries of which we have historical record of its existence, did not always confine its ravages to man. There is evidence to show, that from time to time it attacked cattle with great fury. About the middle of the 18th century, it broke out among the herds in England, and committed great havoc; so serious were its depredations, that His Majesty George III., in his speech from the throne, directed both Houses of Parliament to give it their earnest consideration. It is generally believed to have been the expiring embers of this epidemic, that Jenner found in the dairies of Gloucester, when he first commenced his researches.

The first case of small-pox of which we have historical notice, occurred in the person of Elfrida, the wife of Baldwin the Bald, Earl of Flanders; she recovered. It proved fatal to her grandson Baldwin, who died in early life, in 961. It was about this time probably that the term variola began to be employed in describing the disorder, by writers who used the Latin tongue; the Teutonic or Saxon equivalent was pocca, the modern pouch or pock. Avicenna, about the middle of the 11th century, was the first who minutely described the pustular eruption, and recognised the contagious character of the disease. Sydenham, born A.D. 1624, died A.D. 1689, separated small-pox from measles, which he described as different diseases. By the Arabian physicians, as already observed, they had been classed together, being in fact regarded as one disease. He introduced a great improvement in treatment, by adopting what he called the cooling regimen. So great was his reputation for success in the management of small-pox, that when Mr. Hyde, afterwards the great Earl of Clarendon, and father to the Queen of James II., was suffering from that disease, he was entrusted to his care. In his works, he describes various epidemics which occurred in London, from 1667 to 1672, noticing three kinds, the distinct, confluent, and anomalous. Boerhaave, whose views on treatment were much the same as Sydenham's, so far anticipated Jenner's discovery, as to express the opinion that an antidote might be discovered to prevent the outbreak, or cure the disease when found, and suggested a combination of antimony and mercury. His views were so far realised, though in a totally different and very imperfect way, by the adoption of the practice of inoculation, i.e., the insertion of the matter of true small-pox, introduced into England by Lady Mary Wortley Montague. This lady while living with her husband, who was British ambassador at Constantinople, had seen inoculation practised there, and having become impressed with its value,
on her return to England, had her daughter inoculated by Mr. Maitland. An account of the process had been published in England, by Dr. Simoni of Constantinople, in 1714. It was eagerly adopted by persons of all ranks. The Princess of Wales, with the king's consent, caused her two daughters to be inoculated, but it was soon discovered that it did not possess all the advantages that had been claimed for it. In all cases it produced an eruptive disease, precisely the same as small-pox, which, though in a majority of instances mild, in not a few, caused constitutional symptoms of great severity. Out of 845 persons inoculated during the first eight years after its introduction into England, 17 died, or a proportion of 1 in 50. But the great drawback to it lay in its keeping alive a malignant and infectious disease, by a system of artificial propagation. Vaccination contrasted strongly with this. It produced a local sore, without any general eruption, and it was not infectious, so that the vaccinated, during the progress of vaccination, did not act as centres for the dissemination of disease to those with whom they came in contact. For these reasons, inoculation never made much head-way.

I do not know of any better way of illustrating the inestimable value of the boon which Jenner's discovery has conferred on our race, than by referring to the statistics of mortality from small-pox previous to the introduction of vaccination. In Russia, two millions of human beings are said to have perished from this cause in one year. In the same country, Sir Alexander Crichton, physician to the Imperial family, estimated that it is fatal to one in every seven children. It was reckoned by Dr. Lettsom that 210,000 souls annually fell victims to its ravages in Europe. During the last half of the 18th century, i.e., from 1750 to 1800, it was so prevalent in England, that out of every 1000 deaths from all causes 96 were due to small-pox. Among the various German States, the mortality from this cause was 66.5 per 1000 deaths. 40,000 were said to have perished from it annually in Prussia alone. Those who recovered were often in a worse plight than those who died. Some were blind, others deaf; the faces of all were so scarred that every trace of manly or feminine beauty had disappeared, while the constitutions of a large number were hopelessly shattered.

In England, during the first half of the present century, viz., from 1800 to 1850, during which time vaccination has been more or less practised, the mortality has fallen from 96 to 35 per 1000 deaths, and on the Continent of Europe, where vaccination has been made compulsory, to 7.26 per 1000. Rather a strong argument for a compulsory vaccination act, which forces people against their will to use this means of preserving their lives. In England, during three years previous to 1855, viz., from 1852 to 1855, Dr. Farr says, that out of 1000 deaths from all causes, only 7.6 were from small-pox. But the most wonderful testimony is to be found in Lord Kimberley's despatch, on the efficacy of vaccination, dated October 17th, 1870. His Lordship says, "in 1800, Lord Mayo carried through Parliament an Act (for Ireland) providing that the
poor-law medical officers should receive a gratuity of 1s. for every successful vaccination performed. Owing to the impetus given to vaccination by this latter act, the deaths from small-pox in the years 1858 to 1863 did not average more than 1000 per year. On 1st January, 1864, commenced the operation of the Act of 1863, which rendered vaccination compulsory, and which, it should be observed, continued the provision in Lord Mayo's act, whereby the medical officers had been given a pecuniary interest in the efficiency of the system they were called on to administer. The effect of the Act of 1865, in stamping out small-pox, is shown by the following table of deaths resulting from that disease in the years since its outbreak:

<table>
<thead>
<tr>
<th>Year</th>
<th>Deaths</th>
</tr>
</thead>
<tbody>
<tr>
<td>1864</td>
<td>854</td>
</tr>
<tr>
<td>1865</td>
<td>347</td>
</tr>
<tr>
<td>1866</td>
<td>187</td>
</tr>
<tr>
<td>1867</td>
<td>20</td>
</tr>
<tr>
<td>1868</td>
<td>19</td>
</tr>
<tr>
<td>1869</td>
<td>1</td>
</tr>
</tbody>
</table>

The man who died in 1869 was a Swedish sailor who came to Ireland with the disease on him.

It must not be inferred from this that the only case of small-pox in 1869, was the one which resulted in the death of the Swedish sailor; there were doubtless many other cases, but the disease was so far shorn of its virulence, that though it could still attack, it could no longer kill.

Though vaccination efficiently performed is a most effectual protection against an attack of small-pox, it does not protect with mathematical certainty. The very exaggerated expectations which have been entertained on this point have been disappointed, and have, along with other causes, especially carelessness in the performance of the operation and the terrorising idea of syphilis, tended to bring it into disrepute. Before Jenner's time the most effectual preventive was a previous attack of small-pox, but instances were not unfrequent in which that disease had recurred a second and even a third time. Jenner did not claim more for vaccination than had been claimed for a previous attack of small-pox, but he did not ask less. His own words are "dually and efficiently performed, it will protect the constitution from subsequent attacks of small-pox as much as that disease itself will. I never expected it would do more, and it will not I believe do less." But this result was only to be obtained by the operation properly and efficiently performed. The conditions for success were simple: limpid lymph was to be taken from a healthy vaccinifer at a certain stage of the vesicle and inserted into the skin of the person to be vaccinated, where it should pass through certain stages in such a way that the constitution was infected. Experience has borne out the truth of Jenner's expectations. At the Royal Military Asylum, Greenwich, out of 5774 boys admitted, 1950 had on admission marks of small-pox, and 3824 were either vaccinated on admission or bore marks of vaccination. 6.15 per 1000 of those who had scars from small-pox were subsequently attacked with variola, while the ratio of the vaccinated who contracted the disease was 7.06 per 1000. Dr. Balfour in his memoirs (quoted in Reynolds' System of Medicine, p. 497, vol. i), says that
the proportion of men in the British army who suffered from smallpox was 6.6 per 10,000, four-fifths being protected by vaccination, while only one-fifth was protected by previous smallpox. At the commencement of the present century one out of every three boys admitted into the Royal Military Asylum, Greenwich, bore marks of smallpox, while in 1864 the proportion was one in forty. But it is in the modifying influence which vaccination exerts upon varioila rather than in the absolute security which it affords, that its claims on our regard are discernible. Variola after vaccination assumes a mild form, and loses almost entirely its mortal character. The vaccinated who are attacked, are not disfigured, nor are they left blind or deaf. Mr. Marson, of the smallpox hospital in London, during 30 years has had 15,000 cases under his care. Among the unvaccinated the death-rate was 37 per cent., while among the vaccinated it was reduced to 64.

In the following table he exhibits the difference between vaccination efficiently and vaccination imperfectly performed.

<table>
<thead>
<tr>
<th>Classification of Patients affected with Small-pox</th>
<th>Ratio of Deaths per cent.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 Unvaccinated</td>
<td>37 per cent.</td>
</tr>
<tr>
<td>2 Stated to have been vaccinated, but with no cicatrix</td>
<td>23.57</td>
</tr>
<tr>
<td>3 With one cicatrix</td>
<td>7.73</td>
</tr>
<tr>
<td>4 With two cicatrices</td>
<td>4.70</td>
</tr>
<tr>
<td>5 With three cicatrices</td>
<td>1.95</td>
</tr>
<tr>
<td>Four or more</td>
<td>0.55</td>
</tr>
<tr>
<td>A well-marked cicatrix</td>
<td>2.52</td>
</tr>
<tr>
<td>Badly marked</td>
<td>8.82</td>
</tr>
<tr>
<td>Having previously had small-pox</td>
<td>19</td>
</tr>
</tbody>
</table>

From this it is evident that there must be a certain amount of local inflammation before the constitution is properly infected.

There can be little doubt that smallpox is still as contagious and as mortal a disease in our time as ever it was, though vaccination has done much to check its ravages. Among 215 unvaccinated persons who were exposed to the contagion of smallpox by either living under the same roof, or sleeping in the same room with infected people, 200 took the disease and 46 died; of 91 vaccinated persons exposed to the same conditions, only two took the disease, and then only in a mild form.

There is only one other proof of the efficacy of vaccination which I should like to adduce. It is the rarity in our time, of faces scarred with smallpox. At present I can recall to memory only one case of this kind in Adelaide; in Edinburgh 25 years ago I can remember many.

Before closing this paper there are two points to which I feel myself compelled to allude, because they have lately engaged a good deal of public attention. These are: 1st, The propriety of vaccination direct from the cow or heifer. 2nd, The possibility of communicating syphilis by vaccination. With regard to the former, viz., animal vaccination, the evidence is of a very conflicting nature. The opinion of Mr. Ceely, who experimented on the human subject with lymph obtained from the natural cow-pock in the cow, and whose
opinion therefore is entitled to much weight, is to the effect that it possesses no advantage over lymph obtained from the human arm. If it possesses only the negative advantage of being no better than human lymph, it is scarcely worth while to go direct to the cow, because I need scarcely remark that it is a much more difficult matter to deal with a struggling quadruped than with an infant in its mother's arms. On the other hand, according to an article in Harper's Weekly Times, illustrated by an engraving, the practice would appear to have been reduced to a regular system by some of our American cousins. Young heifers are substituted for cows, placed on their backs on an operating table with each leg strapped to an upright post, and the vaccine inoculated from one animal to another by an artificial system. It is only fair to infer that they would not have gone to all this trouble if the system of heifer vaccination had produced michievous results. (Dr. Wm. Stone, Vaccinator-General of the Island of Trinidad Lancet, May 30th, 1868, p. 684), finding that the lymph which he was using was inert and feeble in its operation, successfully inoculated with human lymph from capillary tubes a fine young cow, and from the supply thus obtained vaccinated a number of the finest Portuguese and European children he could lay hands on. The vesicles produced by this lymph were singularly large and full, often the size of a three-penny piece, and were not always limited to the seat of puncture, being found on the shoulders in one case and on the face in another. Dr. Kirkman Finlay used some of the lymph derived from the first vaccinations in children, and reports the results as most satisfactory, without a single failure. Several cases presented on both arms a triple row of numerous pustules. On the other hand, experiments on a large scale on the continent of Europe show that animal lymph is more feeble and uncertain in its action than human lymph. Mr. Trotter, assistant-surgeon of the Coldstream Guards, reported in the Lancet a series of cases operated on with heifer lymph, which presented sores exactly resembling chancres. In Adelaide dairies, so far as I can learn, there is no natural cow-pox. No doubt it could be produced artificially, as in Dr. Stone's case, by inoculation with human lymph. If it were proved by general experience that the lymph obtained in this way suffered no impairment of its virtues on the one hand, and on the other developed no deleterious qualities, such as the chancreoid ulcerations described by Mr. Trotter, and the constitutional symptoms mentioned by other authors, timid parents who dread the possibility of syphilisation for their children, might then have the option of using heifer lymph if they preferred it. Under a compulsory act, I am not prepared to say that the right should not be conceded to parents of insisting on the employment of animal lymph, if, after the contingencies of the operation have been explained to them, they elect to have it. But heifer vaccination can never generally supplant arm to arm vaccination. There is an obvious advantage in fresh drawn lymph. Even if it were reduced to a system, as is said to be done in America; it would be impossible to keep establishments all over the country for this purpose. I do not
deny that it is possible to fill a sufficient number of tubes with vaccine obtained in this way, and distribute them to vaccinators so that a separate tube could be used for each child vaccinated. But until more satisfactory proof than has hitherto been forthcoming, is shown of superiority of animal vaccination, we cannot do better than follow the example of the national vaccine establishments in England, France, and other European countries, by continuing to use the vaccine drawn from the human arm.

With regard to the next question, viz., the possibility of communicating syphilis, I adhere to the opinion that vaccination transmits vaccinia and vaccinia only. In using the term vaccination, I take it for granted that the fundamental laws of the operation are complied with, and I make no allowance for the violation of these laws by ignorant or careless operators. The vaccinator is then at fault and not the vaccination. The rules for the operation are very simple, viz., that clear lymph, unmixed with blood, pus, or any other foreign matter, should be drawn from a true Jennerian vesicle on the 8th day. It is possible that by grossly ignorant people, and a combination of circumstances which are just possible, a pustule of secondary syphilis may be mistaken for a Jennerian vesicle. Let us suppose a case of this kind: A child having latent constitutional syphilis is vaccinated, but the vaccine does not take; on the 8th day, perhaps the 10th or 12th, it is brought back to the vaccinator: the irritation of the punctures on the arm have developed the latent syphilitic taint, and in place of the vaccine vesicle there is a pustule of syphilitic acne which the grossly ignorant operator mistakes for true vaccinia; from this pustule he vaccinates a number of children, aggravating his first error by another of a still more serious nature, by using an admixture of blood and lymph. The children so operated on show symptoms some time after of syphilis, and are triumphantly exhibited as cases of vaccino-syphilis. But this is syphilitic inoculation practised under the name of vaccination. I verily believe that the evidence on which vaccino-syphilis rests has no better foundation than this imaginary case. In one of the cases which occurred on the Continent, where ten families were said to have been syphilised, the operator was a veterinary surgeon.

It is now a well-established physiological fact that the syphilitic poison is capable of being communicated by inoculation of syphilitic blood. It is very difficult, however, so to transplant it; out of 23 operations of this kind performed by skilful Continental physicians only six were successful. Diday, one of the first French Syphilographers quoted by Lancereaux, himself a foremost authority on syphilis, says: "The non-specific morbid products of a syphilitic subject, pus, catarrhal matter, serum of eczema, are not contagious. I inoculated unsuccessfully a healthy young woman with the matter from a pustule of iodic acne taken from a syphilitic patient in the climax of secondary symptoms." Lancereaux goes on to say (Lancereaux on Syphilis, vol. iv, p. 216): "The inoculation by the same experimenter of the serous discharge of eczema (i.e. syphilitic eczema) was equally unsuccessful. Drs. Rollet and
Viennois showed at the Lyons Medical Congress that pure vaccine matter taken from a syphilitic subject transmits the vaccine poison only." I again quote from Lancereaux, vol. iv, p. 240: "The interesting work of Dr. Viennois of Lyons appeared in 1860. After having examined the cases on record (viz., cases of vaccino-syphilis), that author came to the conclusion that it was the blood and not the virus which is the agent for the transmission of syphilis. In a paper published about the same time Rollet arrived at the same conclusion." The writer of the article on vaccination in Reynolds System of Medicine, himself no mean authority, says: "M. Cullerier and other experimenters in France, especially M. Taupin, have taken lymph (i.e., lymph unmixed with blood) on purpose from syphilitic children, have vaccinated healthy children with it and watched the result. In no instance has syphilis been communicated. Heim made similar experiments in Germany with the same result. Other experiments of a different kind were made in the General Hospital at Vienna, under the direction of Professor Sigmund. The matter of syphilis was mixed with the vaccine lymph and inoculation performed. Syphilis and only syphilis resulted. The two diseases were never communicated together." It would appear, granting the truth of the alleged cases of vaccino-syphilis, that blundering ignorance and stupidity sometimes by an accident attain results in the wrong direction, which skilled physiological experimenters fail to attain when seeking them with a fixed purpose.

There is no lack of English authorities. Dr. West and Sir James Paget agree in expressing the opinion that there is no ground for believing that syphilis can be communicated in the vaccine lymph. Mr. Marson of the Small-pox Hospital in London, Mr. Leese, and the National Vaccine Establishment of England, entertain the same view. The experience of South Australian practitioners, so far as I can learn, is to the same effect.

This source of danger is said to have been first pointed out in England by Mosely, in 1805. There are now on record 258 cases of a Continental origin, occurring in 19 separate instances, contributed by 19 authors. A large portion of these was in Italy. Including the 11 contributed lately by Mr. Jonathan Hutchinson, 4 by Mr. Warren Tay, 2 by Dr. N. J. Hayden (Medical Times and Gazette, March 29, 1862), the number is brought up to 275. Out of the millions annually vaccinated during the 74 years vaccination has been in existence, the number to whom this accident—granting that vaccination is to blame—is small, and it may be safely affirmed that small as it is the gaps in the chain of evidence by which it has been attempted to fix the blame on vaccination are so numerous, that for this purpose they are utterly unreliable. In the strongest cases there can be no doubt that pure lymph was not used, but an admixture of blood and lymph. In others it is not made out that the affection was really syphilitic, or there is no clear proof that the vaccinated had not latent secondary syphilis. In reference to Messrs. Hutchinson and Tay's cases, published in the Lancet last year, I cannot do better than quote the Lancet itself. An impression has been left on
men’s minds from the late discussion on the subject, that this great English authority countenanced the idea that vaccino-syphilis had been proved by these cases. How far this is in accordance with fact, the following extract will show (Lancet, July 8, 1871):—

“Mr. Tay’s cases, though warranting a certain amount of suspicion, are far indeed from affording proof of vaccino-syphilis. As to Mr. Hutchinson’s cases, we are abundantly justified in saying that so far, their value as proofs is absolutely nil. Upon the whole we consider that very grave errors have been committed by nearly every one concerned in bringing these cases forward. The enquiry in its crude early stages was one that was not fit to be undertaken by a miscellaneous body of medical men like the Fellows of the Medico-Chirurgical Society, very few of whom could be supposed capable of deciding, in the brief period of a debate, the precise value of evidence on an intricate question of pathology, or of recalling (even if they had ever studied) the whole previous history of the vaccino-syphilis controversy. We repeat what we said before, that it is a discreditable exhibition when prominent members of a great Society like the Medico-Chirurgical actually declare that so and so is syphilised, on the simple evidence of his having an indurated sore on the arm which had reopened after healing up. Why, that is a common characteristic of unusually irritant lymph of any kind, and it is a specially frequent and unwelcome result of that same clumsy and discredited heifer vaccination, which our sage contemporary (The Medical Times and Gazette) seeks to bring into fashion again. Readers of the Lancet have very recently had an example of the latter fact brought before them by Mr. Trotter, Assistant-Surgeon to the Coldstream Guards, a series of patients having had sores exactly resembling chancre after vaccination from the cow.”

The Lancet then goes on to show that the danger, even if proved, is only a microscopic one, and one which the reasonable care which every respectable practitioner will exercise will altogether set aside. Further, it asserts, “there is not the shadow of proof that vaccine lymph itself ever infects.” The article winds up by expressing the opinion that for the future practitioners will observe the old rule, not to employ lymph which contains a trace of blood or any foreign matter, and to avoid taking it from a child that is either unhealthy in itself or in its family history.

Contrast for a single moment the advantages with the risks of vaccination. Its advantages are these: To a large proportion it offers immunity from a loathsome disease, for the remainder it divests that disease of its most disastrous consequences. The vaccinated who contract small-pox are left with faces undisfigured and constitutions unimpaired. Small-pox, when it did not kill, left its victims, some blind, others deaf, nearly all with scarred visages, many with shattered constitutions. Its risks are infinitesimal: granting the worst proved, they are in the ratio of say in round numbers, 300 accidents to hundreds of millions of successes. Why there is more risk in the most ordinary actions of every day life than there is in this. Walking in the street, travelling in railway
carriages, riding, driving, even eating a meal is more dangerous. I have known men to be choked while eating their food, but I have never heard this urged as an argument why they should not dine; nor is it necessary to pass an Act of Parliament making it compulsory on men to dine. The mere supposition is a reductio ad absurdum. Yet I believe if we had the statistics of accidents from dining for the last 74 years, the time during which vaccination has been in existence, it would be found that dining is a much more perilous business than vaccination.

I have no doubt, however, that there are minds both in and out of the profession to which Messrs. Hutchinson's and Tay's cases will carry conviction, as the alleged cases of vaccino-syphilis on the Continent carried conviction there. For myself, I prefer to give my faith to the results of the experiments of Cullerier and Taupin in France, and Heim in Germany, from which every source of fallacy was excluded, and which establish different conclusions. In medicine, there is a wide field for difference of opinion. Vaccino-syphilis, whether it be a fact or not, has raised a note of warning which should not be disregarded, and it will achieve a good result if it leads professional men to be doubly careful in their selection of vaccinifiers, and to observe with increased fidelity the few simple rules for the performance of the operation.

A CASE OF STRANGULATED UMBILICAL HERNIA,
In which the constriction was found two inches below the neck of the sac.

By H. T. WHITTELL, M.D.,
Hon. Medical Officer Adelaide Hospital.

[Read at a Meeting of the South Australian Medical Association, 15th August, 1872.]

Although cases of umbilical hernia are frequently met with in practice, it is a comparatively rare event to find them presenting symptoms of strangulation. In the course of a practice of twenty-five years, two such cases only have come under my notice. The first of these was that of a lady, who had been the subject of irreducible hernia for several years. The tumour had suddenly increased in size, had become painful, and all the usual symptoms of strangulation were present. Stercoraceous vomiting had been going on for about twenty-four hours, and the bowels had not been open for three or four days. In this case chloroform was given, and while the patient was under its influence, an attempt was made to reduce the swelling. There was no perceptible effect produced by the taxis, but it was judged expedient to wait a few hours before proceeding to operate. Fortunately the pain and vomiting did not recur after the patient had recovered from the effects of the chloroform, and on the following day the bowels were freely opened.

In the second case an operation was necessary, and it is to this case that I wish more particularly to direct attention.
On the evening of the 22nd June, 1872, I was requested by Mr. Elphick to see with him a patient (Mrs. R.) residing at Hindmarsh, who was suffering from intestinal obstruction, consequent on a strangulated umbilical hernia. We went to the patient's house and prepared to operate, if necessary; but on our entrance into her room, the patient exclaimed in a cheerful tone, "I am quite well now; I have no pain, and am better than I have been all the week." The patient was a stout, healthy-looking woman, and about forty-one years old. She was the mother of nine children, and stated that nine years ago, during the labour with her fifth child, a swelling about the size of an egg suddenly appeared at the navel. She had never been able to reduce it, and had not worn any kind of support. The swelling had gradually become larger, but she had never felt any inconvenience from it until six days before (14th June), when not feeling well she took a dose of oil which operated once or twice during the day. Being advanced about four and a-half months in pregnancy, she did not think much of feeling a little poorly. On the following afternoon (15th June), she was seized with severe burning pain on the right side of the abdomen, and she began to vomit yellow matter smelling like rotten eggs. The pain and vomiting came on in paroxysms. There was no action of the bowels, and the attacks became more and more frequent. On the 19th June, she had attacks of hiccough, but these subsided during the day. The symptoms becoming more and more alarming, Mr. Elphick was called in. He found the patient suffering as I have described, and saw a quantity of stercoraceous matter, which had recently been vomited. A little time before we visited the patient, she passed a few hard lumps of faeces; but at the side of the bed there was a chamber vessel half full of stercoraceous matter, which she had vomited during the afternoon. At this time there were no general symptoms to cause alarm. The pulse was natural, the temperature normal, and the manner of the patient almost jocular. On examining the abdomen, I found it tender, especially in the umbilical region. At the umbilicus there was a large pendulous tumour about five inches in length, broad at the base, and irregularly nodulated on the surface. Most of the nodules appeared on manipulation to contain omentum, but one, at the lowest part of the tumour, felt as if the skin and investing structure were not thicker than a piece of ordinary writing paper, and gave the impression that it contained a small knuckle of intestine. The colour of the skin was rather dark, and at the lower part of the tumour there was a small bleb filled with black fluid. This was subsequently accounted for by the patient explaining that the hot flannels she had applied had caught fire, and the intensity of her pain had been such that she did not at first feel the burning. Mr. Elphick and I agreed that the seeming improvement was deceptive, and that in all probability the symptoms would return in the course of one or two hours. This was explained to the patient and she was advised that an operation would probably be required. As her means were limited, and her own home unfit for the treatment of such a case, I offered to admit her into the Adelaide Hospital.
heard no more of the case until two days afterwards, when I was applied to for an order for admission. I then learned from Mr. Elphick that the symptoms had all returned soon after we left, but that the patient had steadily refused to have anything more done. During the afternoon of June 22nd, she was admitted to the Hope ward, and from the notes of the house surgeon (Mr. Ellison), I find that on admission she complained of a dragging pain in the epigastrium and of bilious vomiting. Her countenance was anxious, the pulse 122, the temperature 96°. The tumour was in nearly the same state as I have described. The bleb had burst, and there was a black circular patch of integument in its place. The patient vomited stercoraceous matter several times after admission. Having obtained the advice and assistance of my colleagues, we proceeded to operate. Chloroform having been administered, an incision about two inches long was made in the median line and over the neck of the tumour. There was a considerable layer of fat at the upper part of the incision, but this was much thinner at the lower part. Some of the superficial layers having been divided in the usual way, I came unexpectedly on a mass of tissue about the size of a small egg, and having the appearance of omentum. This was a little to the right of the incision, but readily protruded through it. On examination it was found that this mass formed one of the nodules already described; but that the greater part of the hernia, as well as that which we believed contained intestine, was still covered with a firm layer of tissue. Owing to the quantity of fat, it was found necessary to extend the incision before proceeding further. This having been done, I divided the deeper tissues, carefully searching after the division of each layer for any constricting edge which could be divided without opening the sac. At last having reached what we all agreed to be the sac, and still finding no constriction, I reluctantly made an opening into it sufficiently large to admit my finger. A portion of the small intestine descending into the lower part of the tumour could now be seen through the opening. On passing my finger upward, I was surprised to find no constriction; I could move the finger about in any direction, and I felt several pocket-like openings leading from the inside of the sac into the nodules seen on the surface. I passed my finger into two of these by the side of what felt like omentum. I then tried to find my way downward into the pouch to which the portion of intestine seen through the opening appeared to descend. Here I met with a constriction about half way between the neck of the sac and the base of the pendulous tumour; I could feel that the intestine passed through and was tightly grasped by this constriction. Having passed down the hernia knife over my finger, I freely divided the constricting tissue, and after gentle pressure from the outside I felt the intestine glide upward into the abdominal cavity. There was very little bleeding, and this soon ceased. The opening was closed with metallic sutures and a firm compress was applied. On the following morning the nurse reported that the patient had passed a restless night, and had vomited once or twice, but there had been three copious motions.
Dr. Whittell on a Case of Hernia.

from the bowels. For a day or two after the operation there was slight abdominal tenderness, but the patient was remarkably cheerful, and there were no serious constitutional symptoms. From Mr. Ellison's notes I find, that on the third day the pulse rose to 112, but on the same day the temperature was only 99.8°. This was the highest point reached during the treatment. We found it necessary to remove the sutures early, in consequence of a bagging underneath the integument, caused by a collection of rather offensive sero-purulent matter; but the wound healed readily, and the patient was discharged well on the 21st July.

The interest in this case centres in the peculiar situation of the constriction. Instead of being found in the usual position, at the edge of the umbilical aperture, it was at least two inches lower down towards the inferior part of the protrusion. Sir Astley Cooper, in his large plates of hernia (No. 9), represents a peculiar form of umbilical hernia which he dissected after the patient's death, and which appears to have been of the same kind as that I have just described. He represents his case as one in which the sac had been partially absorbed, and openings had been thus formed through which omentum and probably intestine had found their way. Thus in reality there were several secondary herniae (if I may so describe them) proceeding from the larger one. In my case, one only of these openings in the sac gave passage to intestine, and it was at the edge of this opening that the constriction was found. The practical lesson taught by such a case is, that we should not rest satisfied with merely dividing any supposed constriction at the neck of the sac, but that we should also search for any other constriction that may exist in another part of the tumour. With all due reverence to authority, I think also that the possibility of there being such a constriction tells somewhat against the recommendation of Mr. T. Bryant, who in a recently published clinical lecture instructs us never to attempt to return an umbilical hernia, but to "simply cut down on the neck of the sac and leave the rest to nature." The reason given for this advice is that "umbilical herniae are almost always chronic, slow in formation, and exist for many years before strangulation takes place." * It is doubtless sound advice to interfere as little as possible with the deeper structures implicated in umbilical hernia; but I think that in all cases it is desirable to make a cautious attempt to return the intestine, and thus to assure ourselves that all constriction is removed.

Since I operated on this patient, the question has often occurred to me, would it be possible to form an opinion from the appearance of the tumour as to the probabilities of openings existing in the sac, and of the constriction being found at one of such openings. It is obvious that if this could have been done, a very simple operation would have enabled me to reach the constriction from below. I am inclined to think that the peculiar nodulated appearance of the tumour, and the thinness of that part of it which proved to be the

* Medical Times, 20th April, 1872.
pouch in which the intestine was lodged, may serve as diagnostic
guides; and if by chance another case similar in appearance fall in
my way, I shall be tempted to commence my incision very low
down, and to work my way upward to the seat of stricture.

The length of time that elapsed between the first manifestation of
symptoms of strangulation and the operation for relief, is a notice-
able feature in the case. It is clear that, owing to circumstances
over which her medical attendant had no control, this patient suffered
from constriction seven days before the operation, and during three
of these stercoraceous vomiting had been going on. Yet the
intestine, although a little reddened, had a comparatively healthy
appearance, and quickly resumed its natural action when the stricture
was divided. The probable explanation of this is, that although
the constriction was sufficiently light to arrest the passage of the
contents of the intestine, it did not completely arrest the circulation
of blood in the coats of the intestine itself. Sir Astley Cooper
remarked that, in his experience, the symptoms caused by strangu-
lated umbilical hernia are less urgent than those caused by other
forms of herniae.

It is a common experience that cases of umbilical hernia, in which
the sac is opened, seldom recover. In this case the sac was opened
of necessity; yet the patient (although pregnant) recovered without
a single bad symptom. I attribute this partly to the robust health
of the patient, and to her cheerful disposition; but chiefly to the
assiduous care exercised by Mr. Ellison (the house surgeon who had
charge of the case) in the various dressings that were required.

Australian Medical Journal.
OCTOBER, 1872.

THE NEW MEDICAL BOARD.

The Government, in order to escape the difficulties of
their position, at last determined to anticipate the provisions
of the amended Medical Act, and submit to the profession
the determination of the question, who should compose the
Medical Board. Accordingly, circulars were issued request-
ing the return of eight names, of which five should be those
of gentlemen residing in the City of Melbourne. The
result, as announced on the 21st, was as follows: Dr. Barker,
Dr. Day, Mr. Gillbee, Dr. Motherwell, Dr. Robertson,
Mr. Thomson, Dr. Tracy, and Dr. Youl. Dr. McCrea, as
Chief Medical Officer, will be a member ex officio, and will
also be chairman.

This effect of the appeal to the profession must be
pronounced as, on the whole, very satisfactory. More
especially is it gratifying, as showing the utter powerles-
ness of that small but loud-mouthed section, which has so long proclaimed its self-appraised importance. There was, we believe, a very general feeling that it was proper to re-elect those members of the old board whose resignation raised the other question upon what principle the board, in any case, should be appointed. The thanks of the profession are clearly due to them for the spirited and practical protest they made, against the outrage of letting political or private reasons operate in constituting a board, which has in its keeping, to a considerable extent, the common interests of the brotherhood of medicine in this colony. With the exception of Dr. Howitt, they have all been reappointed, and the only reason why Dr. Howitt was not included, was simply the understood belief that, by reason of infirmity, he had retired from all active duty whatever. Of the new members, there is everything to be said in their favour. They all stand well in their relations to their medical brethren. The election of Dr. Day is peculiarly gratifying, not only because he is personally highly deserving of the distinction, but in that it is an excellent rebuke administered to that very confident gentleman living in the same town with himself, whose curiously acquired popularity has sometimes been mistaken for professional success.

Altogether, the board is fitly chosen; and if they will now devote themselves to the task of getting the amended Medical Act through the legislature, so as to become law as speedily as possible, they will furnish still greater reasons for congratulation that they have been elected to represent the profession.

A LETTER FROM HOME.


I have not much that is new or exciting to tell you on the present occasion, but first of all I must say a word or two on the subject which mainly engaged me last time, viz., chronic Bright's Disease, as expounded by Sir William Gull and Dr. Sutton. The conversazione at the Royal Medical and Chirurgical Society came on, and the mal-specimens were shown, one on one side of the table, the other on the other. From much experience in exhibitions of this kind, Dr. George Johnson had carefully selected typical specimens, which were placed side by side in such a fashion as to carry conviction to the mind, whilst Dr. Sutton's were arranged in such a higgledy-piggledy fashion, that no two specimens could be compared. Moreover, by the mode of preparation adopted, the various tissues had been so soaked and swollen that one was hardly recognizable
from the other. By this plan of preparation too, the specimens had become so transparent, that the transition from one kind of tissue to another was not easy to follow, and to crown all, they were illuminated by a flood of light such as would speedily have destroyed any ordinary eyes. The verdict was not doubtful, save from the gentlemen concerned there has not been a dissentient voice; the specimens submitted by Sir William Gull, and Dr. Sutton, were badly prepared and wrongly interpreted. In short, the thing did not admit of a doubt, the swelling of a tissue gave rise to the hyaline fibroid appearance, and in some of the specimens muscular fibres were supposed by the author of the paper, to be fibres of connective tissue. The fiasco was dreadful; nevertheless, Gull undaunted has been lecturing on typhoid fever at Guy's Hospital, figuring a rising and a setting sun on the blackboard, the more clearly to illustrate one of his favourite notions as to the liability of the earliest and latest days of man's life, to similar accidents and diseases. I am told the Guy's staff do not like it.

Presently we shall have the General Medical Council meeting again, to discuss the various conjoint schemes as they are called, so as to ensure uniformity of qualification throughout the three kingdoms. Here in London, they have succeeded in framing a scheme which includes all English licensing bodies, save the Apothecaries' Hall and the University of London. These pretend, at all events, to be eager to join in it, but are prevented by charter. They have applied to parliament to sanction their combining with the other bodies, to constitute a single examining board for England; but in Ireland and Scotland, no such headway has been made. In point of fact, any union seems impossible. The Queen's University in Ireland, and the University of Edinburgh, refuse to join, and with these left out, the whole scheme would, according to the well-known illustration, resemble the play of Hamlet, with the part of Hamlet left out. Moreover, there is a rising spirit abroad that it would be dangerous, and the proposals already before the public are not encouraging; thus we have the rotten old system of the College of Surgeons, of combining the examinerships in Anatomy and Physiology in the same person still retained, and the component parts of the examining board seem by no means equally balanced.

There is besides a dread lest the new "one portal" should be pitched too high. That would inevitably tend to increase the number of irregular practitioners, especially practising chemists, who are already only too plentiful. Since the Pharmacy Act was passed, chemists have raised the price of their drugs very much, and as a consequence, people dread taking a prescription to be made up. Besides, people here in London have always had a great tendency to seek advice and medicine in the same quarter. So now, as there are fewer men than of old who keep open shop, the public tend more and more to apply to the uninstructed chemist for advice and medicine too. What the remedy for this is I know not; educating the people may do it, but I greatly doubt it.
A report of a commission, on Habitual Drunkards has just been presented to the House of Commons. Its chairman, Mr. Dalrymple, is a medical man, and I am sorry for it. A moderate measure might have some good, but any measure founded on this preposterous report will only damage the whole affair. But this you will see by the newspaper files which accompany the report itself, and the comments of the Times on them.

I am, &c.,

C. J.

CORRESPONDENCE.

THE PATHOLOGY OF ASTHMA.

To the Editor of the Australian Medical Journal.

Sir,—Will you permit me to make a few remarks on Dr. Jamieson’s paper, in the April number of your Journal, on the Pathology of Spasmodic Asthma. The views he there supports, and which are those originally introduced by Laennec, are fully stated in the Medical Times and Gazette for June 8th and 22nd, by Dr. C. J. B. Williams, who is a believer in them. He there mentions also that Bree and others had believed that all the respiratory muscles were affected with spasm, whereas it is the fact of some of these muscles being affected, while the others are performing a compensatory action, that I believe produces in some cases the symptoms observed; and this also includes the parallelism between cases of spasmodic asthma and tetanus, since in the latter disease the muscles are all affected, and therefore very little air can enter the chest, or in severe cases none at all, and of course wheezing cannot occur. I have not yet had an opportunity of reading the cases quoted from Niemeyer by Dr. Jamieson, but must thank him for drawing my attention to them. Dr. Williams states with reference to “Tubercles and other consolidations of the Lungs:” “In proportion as they prevent the entrance of air into one portion of the lung, the air is drawn with more force into the adjoining air cells, and causes their permanent dilatation.” What is true of tubercles, would be true also of any condition which allowed entrance of air into only a portion of the lungs, such for instance as finity of outer thoracic walls with excessive diaphragmatic action, the relation also between the air and its passages being thus suddenly altered, wheezing would be produced.

Dr. Jamieson states with regard to the case of Mrs. B——, “It is not necessary to assume more than that the woman suffered from brachial and intercostal neuralgia, the tenderness over the vertebrae being explained by the fact that here are situated some of the ‘points douloureux,’ the detection of which was so much insisted on by Valleix in cases of neuralgia.” With all respect for the authority quoted, I still think that a morbid condition of the spine is as likely to be attended by a neuralgia of the nerves coming off from the diseased spot, and to be accompanied by tenderness of the spot itself, as that neuralgia should be associated with “points doulou-
reux," and in support of this opinion I will quote from Hilton on Rest and Pain, p. 82:

"If a patient complains of pain on the surface of the body it must be expressed by the nerve which resides there, there is no other structure that can express it, and somewhere in the course of its distribution, between its peripheral termination and its central, spinal, or cerebral attachment, the precise cause of the pain expressed on the surface must be situated."

Henle also thinks that the inter-costal neuralgia so common in women, is due to pressure upon the inter-costal nerves by the vena azygos, and refers the greater frequency of it on the left side to the greater extent of this vein. A similar pressure on the motor part of the nerve would probably be accompanied by an affection of the muscular apparatus of the chest. In the absence of post-mortems, I cannot offer an opinion as to the exact pathological condition, but agree with Dr. Jamieson that it is very unlikely to be caries, since this could not exist for so long a time without going to much greater lengths, but that the spine and inter-costal nerves are implicated in many cases I am still convinced.

With regard to the results of treatment, Mrs. B. had suffered more or less for seven years, and severely for two; she had been under the care of various medical men for the asthma, and also for her general health; had taken tonics of every description, including cod-liver oil; she had had children before, whom she had weaned without benefit to herself, and before taking the six weeks' rest was getting worse. Since that time she has not had a single attack of asthma; her health is better than it has been for five years, and there is merely the slightest possible amount of chronic wheezing, the result probably of the frequent severe attacks. There is no neuralgia. It can scarcely be supposed that the cod-liver oil did, on this occasion in six weeks, what it had previously failed to do in three months. Morris C——, who was case 4 in my paper, afterwards remembered to have had a blow on the back before his first illness, and two years before coming to me. He has since been at the hospital with tenderness in the dorsal and lumbar regions of the spine, and severe sciatica in the right leg, which was absent when lying horizontally; but by merely raising the head from the pillow, and thereby stretching the spinal cord, the sciatica was produced. After a few weeks' rest, whereby the sciatica was relieved and the attacks of asthma prevented, he went out and again had an asthmatical attack, although the treatment in every other way was the same as while here, viz., Quinine, &c. He has since been kept lying down for three months, and went out on June 16th well, and has continued so till the present date.

The other cases I have not been able to follow up. With regard to the deficiency of movement in the thoracic walls, I can only repeat the result of my observations in these and some other cases; the only accurate way of determining is to pass a tape round the chest and compare the expansion during an attack with that observed at other times.
A lady who has been subject to asthma for 7 years, and in whom there was tenderness of the fifth dorsal vertebra, has been lying down for two months; the attacks had been occurring every fortnight, but since the commencement of the rest treatment, she has had only two; being pregnant and usually suffering severely at this time, she considers herself benefited, but as the attacks have not ceased, and her health, which at first improved, had fallen off slightly, she wished to discontinue it, and has accordingly done so. She feels less tenderness in the back and has quite lost a "liver pain," which was almost constant. She took antispasmodics during the attacks; no medicine at other times. The fact of change of residence effecting great improvement and even a cure in some cases, is difficult of explanation on the theory of spinal irritation, and points to an analogy between these cases and those of hay asthma, but one element not to be forgotten is, that change of scene is generally associated with alteration of other conditions which may materially affect the result. The fact of generally unhealthy localities being advantageous in some cases of asthma, as stated by Walsh, is very curious.

The action of antispasmodics in cutting short an attack is undoubted, but the benefit derived is very evanescent in most cases, and although Dr. Williams refers the effect entirely to their direct action on the muscles of the bronchi, they are in many instances drugs whose action on the spinal cord is admitted.

Case 5 in my paper I gave merely for what it was worth, while case 6 and those depending on aortic aneurism were inserted merely for the purpose of showing that "spasmodic asthma" may depend on more than one cause; in fact, that the term is a generic one in the same way as "Fever."

It is easy to fall back upon authorities, but the results of the present treatment in asthmatical cases are not. I think, sufficiently good to justify us in abstaining from all further investigation into the pathology and therapeutics of this distressing complaint.

CHARLES SMITH,
Clunes Hospital.

Sept., 1872.

TASMANIA NOT THE SOURCE OF DIPHTHERIA.

To the Editor of the Australian Medical Journal.

Sir,—I have carefully read through the Report of the Royal Commission on Diphtheria in Victoria, together with all the appended evidence. I read with surprise: "It would appear, moreover, from the evidence of Messrs. Crooke and Lempriere, that the disease was in Tasmania at an earlier date than that on which it made its appearance in this colony. . . . . Judging analogously from this circumstance, and from the other evidence before us, and having had ample proof of the contagious nature of the disease, we are of opinion that it was most probably introduced into this colony from Tasmania."
How the Commissioners could have deduced such a conclusion from the vague, general, hearsay, and contradictory evidence of Messrs. Crooke and Lempriere I cannot understand.

In page 7 Mr. Crooke says: "I must say that I never saw diphtheria until I came to Victoria." In page 9, to the question: "There was no diphtheria in the colony till 1858 or 1859?" he replied, "in 1857 I had the first case." In page 12, he says, "I may mention also that they lost also two children in Tasmania of scarlet-fever, and I believe that both of these children died of diphtheria." In page 10, Mr. Crooke states: "I was the first medical man in Australia to commence the use of quinine in scarlet-fever. In 1854 we had it in horrible intensity in Hobart Town. There were 900 children lost by scarlet-fever in four months; I had but three deaths in the largest practice in Hobart Town."

The mistake as to the year 1854 instead of 1853 is of no moment, but the misstatement of other facts in the above citation is of great importance. The registration district of Hobart Town at that time embraced the south-west banks of the Derwent up to eleven miles north of Hobart Town and all the country to the south-west of Hobart Town, including Bruni Island, the Huon River, and the settlements to Recherché Bay. All the deaths from all causes at all ages entered in the registry for 1853 during the scarlatina epidemic alluded to, were 994; of these, however, 230 only were registered as from scarlatina, and a considerable portion of these were adults and married persons.

In an article of mine in your Journal, April 1858, I stated: "From scarlatina three deaths were recorded in 1855. After the great floods in February and March, 1854, only a few scattered cases of the disease arose. In the great epidemic of 1853, long to be remembered by almost every family in Hobart Town, 230 out of the 994 total deaths of the year were caused by this disease; many of them were adults," &c. Question 189, in the Diphtheria Report, says: "You think we had diphtheria, but it was not recognised," to which Mr. Crooke answers: "I think so. I know I had cases in Tasmania in a condition which I can now recognise as the diphtheritic condition, from what I have seen in this colony. I never saw diphtheria in its true form until I came here." In question 307, he was asked: "Have you any idea how it was introduced into the colony?" (i.e. Victoria) to which he answered "not the slightest."

Mr. Lempriere, in question 1481, page 44, was asked: "Have you formed any idea how it first originated in the country?" (i.e. of Victoria) to which he replied: "No. I remember when I was practising in Tasmania we had an epidemic, a very severe one, two or three in a family died, and all the dogs died of distemper." He was then asked: "Was the poison introduced from without or generated in the colony?" He answered: "I think it must have been generated in the colony, but it is remarkable that the dogs got the distemper at the time the diphtheria came." The next question
was: "Did the distemper come into Tasmania with the first case of diphtheria?" to which he answered: "Yes." Question 1491 asks: "In what year was it that you first observed diphtheria and distemper in Hobart Town?" Mr. Lempriere replied: "I think it was about twenty years ago?" on which it was observed by the inquirer: "That was long before it came here;" to which he answered: "It was in 1851 or 1852, and it raged over there very much in 1856 I think."

In an article of mine on the epidemic diseases of Tasmania, published in the transactions of the Epidemiological Society of London, in 1863, I remark: "In 1851-2, influenza was epidemic throughout "this and the other Australian Colonies, and animals suffered "considerably, particularly dogs, which died in great numbers."

How Mr. Lempriere could have confounded that epidemic with diphtheria, I cannot understand; there was no epidemic of any disease in 1856 or 1857, but in 1858, in the first four months, there was an epidemic of diarrhoea, on which I contributed a paper to your Journal. In 1859, no epidemic of any kind prevailed, but in 1860, in the months of July, August, September, and October, another epidemic influenza occurred, which I related in your Journal. Diphtheria never was known in Tasmania until January 1859, when two cases occurred almost simultaneously in two inland districts fifty miles apart, the origin of which was never traced, as I have stated in my article "On the climate and vital statistics of Tasmania for fifteen years," recently printed by the Tasmanian Government for the Royal Society of Tasmania. The same statement was made in my paper to the Epidemiological Society before alluded to. Dr. Moore of New Norfolk gave an account of the outbreak of diphtheria in his district in your Journal of July, 1859. It commenced in Hobart Town shortly afterwards, by the introduction of two children from a family near Oatlands.

Since I have read the report of the Victorian Commission, I have enquired from those medical practitioners who were resident in Hobart Town in 1851-2-3 and since, whether they ever saw diphtheria until 1859, and they agree with me that it was never known here until 1859. We all know that it had been existing previously in Victoria, and the general impression amongst us is, that it was imported into this colony from Victoria, but certainly not exported from this island to Victoria. While I was enabled to trace communication in all the cases which subsequently occurred to those brought from Oatlands, I have never been able to trace from whence it came to Oatlands and New Norfolk. I believe it to be a strictly contagious disease, communicated by germs from the persons or clothing of the sufferers.

Mr. Thomson's article was written before the commission's report was published, but I think the following passage in it, "it is well " known that an epidemic of diphtheria was at its height in England " during 1857-8, and that passengers were arriving in ships from " British ports almost every week, &c.," indicates the most probable source of the introduction of the disease into Victoria.
Except in 1859, diphtheria has never prevailed epidemically in any part of Tasmania, though in every year since, some cases have occurred, with occasional deaths. It will be seen in Table D of my paper on the "Climate and Vital Statistics of Tasmania," that in the years 1868, 1869, 1870, 1871, there occurred respectively, 11, 14, 15, and 5 deaths. Taking the average of the first three years, and comparing it with the diphtheria mortality in the same years in Victoria in proportion to population, the Victorian death-rate from this disease was nearly five times as much as the Tasmanian death-rate. From what I have stated, it is evident that the falling off in the deaths last year was remarkably great, being only one-third of the previous year, 1870.

I am, Sir, your obedient Servant,

Hobart Town, Tasmania, E. SWARBRECK HALL.
September 18th, 1872.

LOCAL TOPICS.

The following Circular has lately been issued to the profession:—
"Central Board of Health, Melbourne, 3rd October, 1872.—Sir,—In the recently published report of the Royal Commission on Diphtheria, a mode of treating the disease, by keeping the room of the patient constantly filled with the fumes of burning sulphur, is very strongly and prominently advocated, and instances are brought forward in the report, from the evidence of medical men of high standing, which show that such treatment has been very successful in some of the worst cases. Those instances, however, though seemingly very conclusive, are but few, and it is considered desirable therefore that the mode of treatment recommended by the commission should have a more extended trial, in order that its value may be fairly tested in the treatment of this hitherto most fatal and intractable disease. The Central Board of Health, with this object in view, are desirous of soliciting the assistance and co-operation of the medical profession in the colony in an investigation into the merits of this remedy, and I am accordingly desired to request that, when cases of diphtheria come under your care, you will be good enough to give the proposed treatment a fair trial. The plan advocated by the commission is to keep the apartment in which the patient is placed, constantly filled with an atmosphere of the fumes of burning sulphur, as dense as the patient can bear, and to continue this night and day till some decisive effect is produced on the disease. I am desired also to request that, as soon as possible after you have fairly tested this mode of treatment in any case, you will be good enough to communicate the result of the trial to the Central Board of Health, stating:—The age and sex of the patient; the parts to which the diphtheritic membrane had extended; the time during which the apartment was kept filled with the fumes of burning sulphur, and the density of the fumigation; and the effect of the treatment. I am to add that the Central Board of Health trust the gravity of the disease, and the desirability of finding an efficient remedy for its treatment, will be sufficient excuse for asking you to take this trouble.

T. R. Wilson, Secretary."

The Hamilton Spectator of the 23rd ult., commenting upon some disturbances then going on in the hospital-committee, says: "That a poor aged patient with a broken arm, and supposed to be suffering also from fractured ribs, should have been allowed to lie a whole week in the hospital without the nature of the latter injuries, if any, being definitely ascertained, seems almost incredible; but that a case of ascertained injury, such as the broken arm, should be left to look after itself, through disputes amongst the medical officers, is more incredible still."
Dr. Ramsey, resident Surgeon of the Alfred Hospital, resigned his appointment on the 25th ult., and on the 16th inst., Mr. Eustace Greenaway was appointed in his place.

The City Council of Sandhurst, on the 27th ult., voted Dr. J. P. Murray a letter of thanks, and the sum of £50, in recognition of his services as health-officer during the small-pox outbreak.

An utterly absurd charge of writing an anonymous letter addressed to the Committee of the Melbourne Hospital, has lately been brought by Mr. Hart, a member of the Committee, against Dr. Hinchcliff one of the resident staff. The only grounds for the charge appear to have been some fancied resemblance between Dr. Hinchcliff's handwriting and that in the letter. Dr. Hinchcliff has indignantly denied the imputation and has also made a statutory declaration to the same effect, but although a sub-committee has reported that there are no grounds for the charge Mr. Hart persists in not accepting the decision of the sub-committee. The matter is yet undergoing investigation.

Dr. Schomburgk, director of the Adelaide Botanic Garden, read an interesting paper on "Poisonous Plants" before the Philosophical Society of Adelaide on the 17th ult. The paper was accompanied by specimens grown in the garden over which Dr. Schomburgk has superintendence.

The Central Board of Health in a circular dated October 8, issued to the local boards throughout the Colony, recommend the re-vaccination of all persons over 14 years of age.

The case of Dr. John Richard Peele, which has been before the public and the profession for some months, was finally determined on the 16th inst. by his acquittal at the Albury Criminal Sessions. Dr. Peele, it will be remembered, in July last, prescribed for a man named Hendry, who was suffering from incipient delirium tremens, a six ounce mixture, containing eighteen grains of acetate of morphia—the dose, two tablespoonsfuls every two hours. After taking two doses, the patient became comatose, and, after some interval, died. A coroner's jury returned a verdict exculpating Dr. Peele, but he was subsequently proceeded against at the instance of the Crown, on a charge of manslaughter. The trial, as we have remarked, took place on the 16th inst. After the evidence had been taken the jury retired, and in about an hour returned to ask if a medical man were, in law, responsible for the proper application of his prescriptions. Having been answered in the negative, they brought in a verdict of not guilty.

BIRTH.


DEATH.

STARKE.—On the 7th inst., at Inglewood, of diphtheria, Winifred Hayden Starke, eldest child of Hayden Starke, M.D., aged two years and seven months.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr. Jamieson, Mr. MacGillivray, Mr. Wilkins, Mr. Gillbee, Dr. Alfred Shaw, Dr. Paterson, Mr. Thomson, C. J., Dr. Silver, the Registrar of the Royal College of Physicians, the Rev. Hugh Croskery, L.R.C.S.I. (Jamaica).
