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James ARNOTT

On the Effect of Chloroform upon the Result of Surgical Operations.
Some Correspondence on this subject.

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the dermis by means of a camel's hair pencil, night and morning, the vesicating paper was again used, and cod-liver oil administered. This treatment was pursued for three months, at the end of which time the discharge had disappeared, although the power of hearing was not wholly restored.

Case 8.—*Chronic catarrhal inflammation of the dermis; great inflammation.*—Mrs. A., aged 30, consulted me in August 1856. She was in tolerable health.

History.—Two years previously she experienced at times great irritation and pain in the tube of each ear, followed by discharge: thinks that the affection was caused by a residence in a damp house: it was aggravated by exposure to cold air. Since the above period has had several similar attacks, and the ears are never quite free from itching and discharge. The power of hearing has not been diminished.

Upon *examination* the surface of the dermis in each ear was found to be denuded of epidermis, red, slightly tumefied, and covered by a mucous discharge. Each membrana tympani was healthy. The hearing was natural.

Treatment.—As there was evidently considerable congestion of the meatus, two leeches were applied to the margin of the orifice of each meatus, each ear being syringed with warm water, twice daily. In the course of a week, the congestion having much diminished, a solution of nitrate of silver (gr. 6 ad ʒi.) was applied to the surface of each meatus by means of a camel's hair brush, twice daily. Under this treatment, and the occasional use of the syringe, and warm water, the irritation subsided, and the discharge disappeared. The affection was, however, slightly reproduced by a continuance of damp weather, and by any causes producing a weakened state of health; but it speedily subsided under treatment.

In some cases where chronic catarrhal inflammation is allowed to proceed unchecked, the bone is liable to become diseased. These cases we shall consider when we come to speak of diseases of the brain arising from affections of the ear.

In *ulceration* of the dermoid meatus, the bone is usually diseased; the subject will be treated fully when speaking of the diseases of the bone and brain.

ORIGINAL COMMUNICATIONS.

ON THE EFFECT OF CHLOROFORM UPON THE RESULT OF SURGICAL OPERATIONS.

By JAMES ARNOTT, M.D.

The announcement of "death from chloroform," does not now affect the professional reader in the manner it originally did. He regrets the individual occurrence, but he finds consolation in the reflection that such casualties are rare, and that they are much overbalanced by the favourable influence exerted by chloroform on the results of operations. Though a few may die, he says, from the immediate or direct effects of chloroform, many are saved by its ulterior agency.

He judges of the first circumstance—the number of sudden deaths—by the small number of reports which reach him; of the second, by the statistical investigations that have been instituted to determine the question.

On the first point, however, he is very apt to be deceived. Scarcely a hundred instances of sudden death from chloroform have as yet been reported, but there cannot be a doubt that by far the greater number have been concealed.

In some remarks on this subject, two years ago, it was stated that, although five cases of sudden death from chloroform had taken place in the London Hospitals within the seven months preceding my publication, not one had been reported as occurring in the private practice of London during that period; and the same observation, it was stated, was applicable to a much longer time. Indeed, I am not aware that more than two cases of sudden death from etherization have been published as happening in the private practice of London since its introduction. This evident concealment is neither extraordinary nor reprehensible. What family is there that would not endeavour to the utmost to avoid the horrors of a coroner's inquest, and the consequent newspaper report, where, as happens in the case of death from chloroform, there can be no suspicion of foul play? Nor can we blame the practitioner who would gladly escape from a criticism of his proceedings on such occasions. Useful as such modes of publicity may be on other accounts, they do not, in this instance, promote the advancement of medical knowledge. But many die from the direct effects of chloroform and within a few hours of its administration, whose deaths are attributed to other causes. Dr. Mouat has recently drawn attention to such cases. He speaks of soldiers who were operated upon in the Crimea under chloroform, as gradually sinking under the peculiar state of nausea and depression which follows its use. "Reaction is never thoroughly established, the desire for food never returns, and the patient sinks as it were stealthily, and dies from exhaustion in from twelve to twenty-four hours." "These cases," he adds, "are far more numerous than is generally supposed, and many of them may fairly be termed 'deaths from chloroform,' but are never so returned (a)."

The second source of consolation on such occasions, we have said, is the opinion that the sudden deaths from chloroform are much overbalanced by saving property as respects the results of operations. It has been asserted, in reference to certain amputations, that if five cases were to be killed by the direct agency of chloroform in every hundred operated upon, there would still be a saving of life by its ulterior agency. This is a most important point, about which there should be no doubt. If the position that there is a saving of life upon the whole be incontestable, it ought, for the credit of the profession, to be brought more frequently forward, especially on such occasions as the recent inquest at St. Thomas's Hospital: if it be unfounded, the sooner it is contradicted the better. It is the purpose of this paper to renew the investigation of the subject.

The question whether chloroform saves life as well as prevents pain, can only be determined by statistics. To attempt to form a judgment from individual experience, would be scarcely less erroneous than to estimate the comparative proportion of the two sexes (now ascertained with such wonderful accuracy by the aid of statistics) by counting the males and females in the enquirer's own family. Nor would reasoning from the observation of the sensible effects on the system bring us nearer the truth. The absence of pain may be, and probably is, an advantage as respects the result of an operation, but it may be much over-balanced by the other effects of the anæsthetic; and the beneficial stimulus excited by chloroform may be followed by injurious prostration (b). On the other hand, poisonous and depressing as the drug manifestly is in some cases, it may still possess some hidden virtues conducing to recovery. Whatever its ulterior agency may be, as this produces no peculiar symptoms, it is by results alone that its nature can be determined.

The writer, whose inquiries on this subject have been most influential, is Dr. Simpson of Edinburgh.

About two years after the introduction of etherization, and when Surgeons were beginning to fear that the bad effects of chloroform might not be limited to the very time of its administration, but might seriously affect the results of operations, Dr. Simpson's statistical investigations respecting this point were published. They were prefaced by an ably written and lucid exposition of the value of statistics applied to this enquiry, which, with the manifest trouble he had had in collecting his materials, and the apparent care he had taken in arranging them, insured a ready and confiding reception to his investigations by the Profession. From that time all anxiety about the ulterior effects of chloroform seemed to cease, and Surgeons have continued to employ it without hesitation, under the conviction that, instead of there being a destruction of life as the price paid for the insensibility, a great saving of life is effected by it. The tables in which he condensed the results of his investigations, and which have just been reprinted in his collected works, have been as influential with Surgeons as the Northampton Life Tables have been with Assurance Companies.

As these tables represent the mortality from amputations to have been 29 per cent. immediately before the introduction of

(a) *Medical Times and Gazette*, September, 1856.

(b) The credit of a double advantage claimed for general anæsthesia, of preventing both pain and danger, is justly due to local anæsthesia from cold, and when used for other purposes as well as operations. Mr. Langston Parker, in his recent work on Cancer, speaks of our now being able to use caustic in its cure in consequence of the pain from it being removed by frigorific applications; but the prevention of erysipelas by intense cold is not of less importance, to say nothing of its own direct and efficacious curative agency in malignant diseases.

etherization, and 23 per cent. after its introduction, and as the inference from them is that there has been a saving of life effected by the change to the amount of 6 per cent. (a number doubtless far exceeding that of the sudden deaths which have taken place during the administration of chloroform). It is no wonder, then, that they should have been so often appealed to in discussions upon this subject, and that all theoretical objections to chloroform should have been refuted by them.

Nevertheless, these tables, when closely examined, are found to involve the greatest fallacies; they do not afford a particle of evidence that the introduction of chloroform has lessened the mortality after amputation.

The first, which professes to give the average mortality of thirty British Hospitals, should have shown the number of operations, and their results, at each of these Hospitals during precisely the same period of time; but, instead of this, while the period of observation, as respects the only large healthy hospital inserted in the list, is limited to two years, that of the large unhealthy hospitals of Edinburgh and Glasgow, the excessive mortality of which almost equals that of the Paris hospitals, extends to more than three times this duration. If an equal period of observation be taken to form this average, (excluding two of the small hospitals, one healthy and the other unhealthy, on account of the period of observation respecting them being uncertain), the table, instead of showing a mortality of 29 per cent., would show one of only 24; and, if other large healthy hospitals, like that at Bristol, had been included—such as the Liverpool Royal Infirmary, where (as appears from a published return) the deaths from amputation during three consecutive years were only at the rate of 6 per cent.—the average mortality of the whole would probably have been considerably less than 20 per cent.

The second table involves no miscalculation so palpable as that in the first, but it leads to conclusions equally erroneous. It gives an account of the number of amputations in which ether was administered, with the results; but what the character of the cases was in which it was used—whether the patients were healthy or worn out with disease—we have no means of judging. In all probability the best cases were generally selected, for only a few were returned from each hospital; and it was natural and proper that at first the best cases should be chosen for trial; not only those free from serious organic disease of the vital parts (a class which were long excluded), but those in which the reparative powers were most conspicuous; and a clearer proof that this was the case cannot be adduced than the fact that the etherized cases from the eight London hospitals inserted in this list, show a mortality of more than 10 per cent below that which (as we shall presently see) exists at the present day.

But as the prospect of recovery from amputation is good or bad according to the general health of the patient, and other circumstances, if we could always select our cases, the usual mortality would probably be reduced to less than a half. As it is, all the advantage which the 302 etherized cases appear by the table to have over the non-etherized 618 of the other table (admitting the returns to be correct), does not amount to more than 1 per cent. To prove that there was not actually a loss of life, instead of a gain, from etherization, there should have been, assuming that the cases were generally selected, a much greater difference than this. A percentage of 23 deaths from amputation in the English provincial hospitals, even supposing that every case was etherized, would indicate a great increase of the usual rate of mortality before the introduction of etherization.

Another objection to the reception of this table as an argument in favour of the indiscriminate use of chloroform, is, that it has reference principally to sulphuric ether as the means of producing anæsthesia, for very few operations had been performed under chloroform at the time of its publication. Now, chloroform, whatever other advantages it may possess over ether, has none as regards safety; and, what is of more importance in respect to this table, it has of late years been employed much more boldly than was formerly usual. Patients were then frequently only half intoxicated by the anæsthetic, and the intoxication was kept up but for a short time. A change in this practice had not yet been effected by the singular argument, that, because a patient labouring under convulsions may be kept for a long time under the full influence of chloroform apparently without injury, the same proceeding can be adopted with impunity in the case of a patient exposed to the long-continued danger of a large amputation wound.

We shall now proceed to the consideration of tables of a very different character from the above, as respects their construction, and which disclose facts of a very different import.

Although I had long felt convinced, from reflecting on the evidently poisonous character of chloroform, that the number of sudden deaths produced by it, whether reported or not reported, was by no means the measure of the whole mortality, I was unable to obtain satisfactory evidence of this. It was by statistics alone that this point could be determined, and I had no easy access to the repertories of the necessary facts preserved in hospitals. At last, my attention was directed to the Statistical Reports of Operations which have appeared for several years past in the *Medical Times and Gazette*, by a reference to them in Sir Benjamin Brodie's recently-published paper on Lithotrity. On examination, I found that these reports were all I could have desired. A monthly account is given of the whole of the operations during the last three years. Their accuracy is assured by the circumstantiality with which every case is mentioned, and by the fact, that they were not drawn up with a view to the settlement of any particular question in practice. The reporters of these statistics have been under no conceivable bias; they have been actuated solely by a desire to promote surgical science. If their returns have a fault, it is certainly not the over-statement of the mortality; for, almost every month, a large number of cases are mentioned as being still under treatment; and although the fatal issue of a few of these is afterwards reported, it is probable that other deaths have happened in consequence of the operation, but at too long a period after it to be known to the reporter or to be recorded by him. It might at first sight appear desirable to have reports for a longer period than three years, but were the period now extended, any such comparison as that we are now making between the results of operations, becomes imperfect or impossible by the advancing improvements altering the circumstances.

In the *Medical Times and Gazette* there are separate statistical reports both of the London and Provincial Hospitals; but I shall restrict my attention to the first, for the following reasons. The principal is, that the Hospitals in the provinces are too far apart, and differ from each other in too many circumstances, such as climate, site, and character of the patients frequenting them, to render it possible to form an estimate of their average mortality before etherization was introduced, from the very few published returns of the results of amputations in the Provincial Hospitals at that time. Another reason is that I am not sure that the administration of chloroform has been so universal in operations in the country as it has been for many years past in the metropolis. In London, on the other hand, there are many large Hospitals furnishing the requisite number of facts, and they are all under nearly the same kind of general management, Surgical practice, &c. We have authentic returns also of the mortality after amputations in some of the large London Hospitals before ether was introduced, from which, in consequence of the similarity of circumstances just alluded to, we can construct a sufficiently correct estimate of the general mortality for comparison with the present rate. The following table has been constructed from these returns.

Table I.

Showing the Average Mortality after Amputations of the Thigh, Leg, and Arm, in four London Hospitals before the Introduction of Chloroform.

Hospitals.	Date of Observation.	Reporter.	Primary Amputations.		Secondary Amputations.		Total.	
			Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
University College ..	1835—40	Mr. Potter.	8	3	50	7	58	10
St. Thomas's	1842—47	Mr. South.	20	7	29	6	49	13
University College ..	1841—46	Mr. Cadge.	7	4	38	10	45	14
Bartholomew's	1846	Mr. Haig.	8	1	14	3	22	4
							174	41

Percentage of deaths to cases (taking equal periods of observation), 21·9.

The great diversity which appears in the above Table between the two equal periods of observation at University College Hospital, is a striking illustration of what has been termed a run of good or bad luck in the practice of the same Surgeon, for Mr. Liston was the principal operator at the Hospital during both periods; and it shows, also, how unsafe it would be, unless for a very long period, to rely on any particular Hospital as a standard. The return of deaths from amputations at St. Thomas's is heavy, and I might have been justified in rejecting it as being of too private a nature to have the requisite authority; but, in order to prevent any cavil, or appearance of selection, it is retained; and, for the same reason I have omitted the only other return of amputations which I have been able to find, as respects the London Hospitals: objection may be made to it, because the mortality is much below the usual average. This return is from Guy's Hospital, and is mentioned by Dr. Fenwick in his elaborate paper on the statistics of amputation, in the *Edinburgh Journal of Medical Science* for 1847. The period of observation is from 1843 to 1845; the cases are 36, and the deaths 4, or at the rate of 11 per cent. Were this return added to the others in the table, it would reduce the average of the London mortality to less than 20 per cent, or one fatal result in 5 amputations.

The present mortality of the London Hospitals is shown by the following Tables, into which the several returns in the *Medical Times and Gazette* have been condensed.

Table II.

Showing the Mortality from Amputation of the Thigh, Leg, and Arm, performed under Chloroform in the London Hospitals during Eighteen Months, from June, 1855, to June, 1856, inclusive.

Hospitals.	Primary Amputation.		Secondary Amputation.		Total.	
	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
St, Bartholomew's..	1	..	23	7	24	7
St. Thomas's	4	3	12	3	16	6
Guy's.....	15	10	34	3	49	13
London	11	2	13	4	24	6
St. George's	6	3	15	5	21	8
University College..	3	2	14	3	17	5
King's College.....	1	1	8	3	9	4
Middlesex.....	1	1	5	2	6	3
St. Mary's.....	5	3	12	3	17	6
Westminster	..	..	2	..	2	..
Charing-cross	1	1	9	2	10	3
Metropolitan Free..	..	..	2	..	2	..
Hospital for Sick Children	..	..	1	..	1	..
Seamen's	..	..	5	..	5	..
Marylebone Infirmary	..	..	1	..	1	..
Total.... ...	48	26	156	35	204	61

Table III.

Showing the Mortality from Amputation of the Thigh, Leg, and Arm performed under Chloroform in the London Hospitals during Three Years, from July, 1853, to June, 1856.

	Number of Cases.	Number of Deaths.
First Year..	144	57
Second Year	150	50
Third Year	136	41
Total..	430	148

Average percentage of deaths, 34·4.(a)

(a) Several sudden deaths were reported as happening from chloroform in the London Hospitals during this period, but none from its administration in amputations, though, in the account of cases in the journal, the fatal terminations are occasionally spoken of in such terms as these, "vomiting continued until death," "death from collapse followed," "sank from shock during the performance of a primary amputation," "sank almost immediately after artificial respiration and galvanism had been resorted to without success."

It appears, by comparing these with the foregoing Table, that the mortality in the London Hospitals has increased since the introduction of etherization from 21 to 34 per cent., or, to vary the expression, instead of amputation being fatal in a less proportion than one in 4 of those operated upon, it now proves fatal in 1 in 3. Is not so enormous a sacrifice of life too high a price to be paid for anæsthesia, even granting that this cannot be otherwise obtained with perfect safety? Is life to be held as nothing when compared to pain?

(To be continued.)

A SINGULAR CASE OF MIDWIFERY.

By FREDERICK HOBSON WARREN, Esq.

On the 30th of May last, I was summoned at half-past one, a.m., to attend Mrs. C., aged 28, in labour of her first child. And I should here state, that, until then, I had never to my knowledge seen my patient, a friend of hers having engaged me for her.

I was informed that pains had been for some hours regularly coming on, and that they were rapidly becoming more severe. On proceeding to make an examination in the usual manner, I was at first puzzled to discover an entrance to the vagina; and it was not until the recurrence of a pain, that I succeeded in introducing my forefinger to a small aperture immediately under the symphysis. Retaining it here, after a short lapse, a pain recurred, and I could feel the membranes borne onwards, the child presenting naturally, and the head very low.

It was, however, at once evident to me that there was something unusual about the external organs of generation, and in this I was confirmed by my patient exclaiming, "Oh, I ought never to have married," at the same time revealing to me the fact that she had sustained a great injury to the lower part of her person, from machinery, when a child ten years of age, and also reminding me, that I had attended her subsequent to her discharge from the Devon and Exeter Hospital, where she had been under the care of Mr. Barnes, for the injuries she had then received.

However, pursuing my examination, I found the labia entirely wanting, and their place seemed occupied by a hard, dense cord on either side, which offered an unyielding impediment to each uterine effort, and which I could trace, stretching upwards above the pubes, which was entirely devoid of hair, and in lieu of the mons, was thinly covered by a cicatrice.

During this, labour continued with increased force and rapidity, and finding the perinæum to distend, I began to think it would be impossible, under such circumstances, that a child could be born without some serious result to the mother. I therefore determined at once to divide these cord-like bands, hoping that thereby the parts would yield sufficiently, and room enough be gained to allow the passage of the child.

A very considerable advantage was immediately gained by this step, and the vaginal orifice lengthened thereby to the additional extent of an inch and a-half. But the uterine efforts now became incessant, and the consequent rapid advance of the child, left me no longer time to deliberate, and my sole attention was directed to the support of the perinæum.

At this time, 3 o'clock, a.m., the membranes gave way, and immediately after, while firmly supporting the perinæum, I felt a sensation in the hollow of my hand, as of a crack, a gush of blood directly followed from the vagina, and the next moment, I felt a tear of the perinæum, and the head of the child protruded *through* it, its body following in a few seconds.

I had now leisure to inspect the part, and found that the rent extended from the front of the rectum throughout to within an inch of the perineal fourchette, which, not torn, looked bruised and dark.

I removed the after-birth through the rent, and the labour was ended at 3.30 a.m.

Nothing occurred to detain me, and I left about an hour after. At 2.30 p.m., I found that my patient had enjoyed some refreshing sleep, and was tranquil and happy.

The perineal aperture had already much contracted, but at the time, I deemed it advisable to bring the surfaces together by the interrupted suture.

9 p.m.—In every particular doing well.

ORIGINAL COMMUNICATIONS.

ON THE EFFECT OF CHLOROFORM UPON THE RESULT OF SURGICAL OPERATIONS.

By JAMES ARNOTT, M.D.

(Concluded from page 414.)

If the above Tables required confirmation, a reference might be made to other statistical statements, which, though the numbers constituting them are too small to have much weight by themselves, may be usefully considered in conjunction with others having a more extended basis. Of this kind are the tables respecting etherization, that were published four years ago in America (*American Journal of Medical Science* for 1852), and certain notices which we have received of the surgery of the Crimean campaign. It is generally understood that the results of amputations were unfavourable during this war; and, though there were several other causes in operation, there can be now no doubt that this want of success may in some degree be attributed to the use of chloroform. Dr. Gordon, who had Medical charge of the Second Division of the army (in which, however, chloroform was not so much used as in the other Divisions), informs us (see the "Report of the Proceedings of the Crimean Medical Society") that the result of amputations was very favourable in the preceding war in India, and before etherization was in use. Of twelve amputations performed in his regiment during the Punjaub campaign only one proved fatal.

Allusion may also be made to a report of amputations performed under etherization, which was published in the *Medical Gazette* some years ago by Dr. Snow; and as its purpose was very different from recommending caution in the administration of chloroform, it may be here referred to; although under other circumstances the fact of its being a private unauthenticated report would, in such an inquiry as the present, render it inadmissible. Betrayed, apparently, by Dr. Simpson's erroneously high estimate of the mortality in British Hospitals before etherization was introduced, Dr. Snow did not hesitate to publish this report, in which the mortality from 55 amputations of the thigh, leg, and arm, amounts to 27 per cent., as a statement favourable to etherization. But when it is considered that none of the amputations in this list were primary, that, at the commencement of the practice there was more selection of cases than has since obtained, and that several of them occurred in private practice—circumstances all highly favourable to recovery—the mortality from them must be deemed quite as high as that indicated by the reports in the *Medical Times and Gazette*. Dr. Snow says he publishes this report in order to dispel the fears with which some Surgeons may be troubled; as well might a recruiting serjeant encourage to enlistment by enumerating our losses at the Redan.

Although I have not, for certain reasons assigned above, made so accurate an examination of the statistics of the results of amputation in the provinces as in London, I have ascertained that they show an equal, if not a greater, comparative increase of mortality. This operation at some of the largest provincial Hospitals did not formerly prove fatal to 15 in 100; the average mortality for the last two years and nine months (the whole period embraced by the returns) is 30 in 100.

I am perfectly aware that the inferences drawn from these tables and numerical statements will not be acceptable to many members of the Profession who have been using chloroform in every important operation; but I am unable to anticipate any valid objection that can be made to them. The point that was supposed established by Dr. Simpson's investigations, it must at once be acknowledged, is not so established; and, although there may be a difference of opinion regarding the exact correctness of the estimate in my first table of the mortality in the London Hospitals before etherization was in use, this is a point of no importance. The great question is, not whether my table makes this estimate two or three per cent. higher or lower than it ought to be, but whether the mortality from amputation in the London Hospitals, after all the improvement of late times, would have amounted steadily during the last three years to more than 30 per cent. or about a third of those operated upon, but for the continued agency of chloroform? If it be asserted that the mortality of the London Hospitals has not increased, then it must follow that the returns we have had of the results of operations in former years have been most erroneous; that the long-boasted superior salubrity of the London over the Paris Hospitals was not authorized by the truth; and that our Surgeons, though well aware of the comparatively trifling mortality after amputation in the country or in well-ventilated Hospitals, whither most of the patients might have been sent, recklessly persisted in this fearful sacrifice of life.

But (it may be argued), admitting that a great increase of mortality has been proved by these tables to have existed during the last three years, it is not a necessary consequence that this has been caused by chloroform. If any other cause has been acting, let it be pointed out. There has been none which could have effected this difference through so long a period but the introduction of chloroform. Other influences may, doubtless, have occasionally acted in unison with it, and increased the mischief; but to a continued cause alone can the continued increase of mortality be attributed. We know that chloroform is a powerful agent, and that it possesses noxious properties; the ulterior effects of which, as well as the immediate, would have been sooner discovered but for the mists raised by erroneous statistics, and the bold, unqualified assertions of its innocuousness by parties whose opportunities of observation gave an authority to their opinions. It would be no satisfactory explanation of the cause of this mortality to allege, that inflammation of the veins, or pus in the blood, has been epidemic or prevalent. We wish to know what caused this prevalence. An epidemic caused by atmospherical or other similar influences does not last for many years over the whole extent of a country; but this epidemic has been co-existent with, and has, doubtless, sprung from, chloroform. Much of the misapprehension existing on this point has arisen from a forgetfulness of the distinction between predisposing and exciting causes. Intemperance (to take, as an example, a predisposing cause, closely allied to, if not identical with, intoxication from chloroform), is not the immediate or peculiar cause of yellow fever or cholera; but the consequences of drunkenness, and particularly the debility or prostration produced by it, render the human frame a prey to exciting causes which its conservative powers would otherwise have enabled it to resist. We know that the majority of diseases prove fatal by some modification of inflammation and its consequences; and that there are a hundred remote and immediate causes of inflammation. Nothing, therefore, could be more erroneous, if any particular inflammation became prevalent, as of the brain, or blood-vessels, in consequence of the prevalence of some particular predisposing cause, than to say, that this cause had nothing to do with the fatality. Chloroform probably acts injuriously as hæmorrhage acts, or by the debility which it produces. The plan for producing general anæsthesia by excessive bleeding, proposed about thirty years ago by Mr. Wardrop, would have often proved fatal from the same cause (a).

The time comprehended in these statistics is three years, and the locality, London. Such a period and field of observation are amply sufficient to determine the question; but if more be required, let the records of other periods and other hospitals be examined, and the same result will appear. The report by Dr. Snow, for instance, was published six or seven years ago. In extending this inquiry, let care be taken that the return shall be from various hospitals of similar character, that they comprise the requisite number of similar cases, and be sufficiently authenticated. And let it be remembered, that

(a) Dangerous as this plan was, it was better received than what may be termed the first systematic proposal of etherization. I allude to Dr. Hickman's suggestion of producing anæsthesia by the inhalation of carbonic acid gas, which was published twenty years before the subject was taken up in America. We may judge from the following extract from an abusive criticism of it in the *Lancet* for February, 1826, how great the change is which has come over our opinions on this subject:—"Does Dr. Hickman suppose, that he would not be laughed to scorn, if he were to recommend a man who was about to have a tooth drawn, to be previously hanged, drowned, or smothered for a few minutes, in order that he may feel no pain during the operation?" When speaking of the introduction of etherization, I may remark, that no scientific truth in connexion with this measure has exerted a more important influence than the great practical discovery by Whately, alluded to in my essay on Therapeutical Inquiry, that, in cases of suspended animation from narcotism, life may be preserved by long-continued artificial respiration. But for this resource, the deaths, numerous as they are, which have occurred from intoxicating or narcotic inhalations, would have been tenfold. Mr. Whately, of Berkhampstead, lately saved a child in imminent danger from chloroform, by respiration, affected in the manner recommended by Ricord, or "bouche à bouche."

if the effect of chloroform administered in healthier hospitals than those of London be investigated, we cannot expect the same proportion between its injurious effects. A patient might inhale chloroform in a pure country atmosphere and escape, who would be killed by it under the contrary circumstances; just as, according to common observation, a person may, if other circumstances be the same, be bled with impunity in the country who would sink under venesection in London.

Before concluding the subject of amputation, I may remark that, of the two kinds of this operation termed primary and secondary, the latter is the best adapted for a subject of comparison in respect to etherization. The first not only varies more in danger, according to the severity of the injury rendering it necessary; but the question of the propriety of having recourse to primary amputation in certain kinds of injury is far from being yet settled. If the Surgeon were invariably to cut off limbs which another and a better Surgeon would endeavour to save (and this observation applies to both primary and secondary amputations), he would soon be enabled to exhibit a much more favourable return of amputations than the usual average; and the adoption of such a system at any particular Hospital would give it a fallacious appearance of advantage over others.

These observations respecting primary amputation apply with equal force to the operation for strangulated hernia. The mortality from this operation is affected principally by the length of time that has elapsed from the date of the strangulation; and if a Surgeon were always to operate immediately on its occurrence his success would be much above the usual average. He would not, however, diminish the mortality from the disease by so rash a proceeding, for assuredly the greater number of strangulated herniæ can be reduced without any dangerous incision, and the several novel modes of effecting this, which I have at different times recommended, particularly that of suddenly contracting the bulk of the protruding part by a short application of intense cold, will still further diminish the necessity for resorting to the knife. I may observe, with respect to the use of chloroform in herniotomy, that from the smallness of the wound it probably causes little increase of danger. In most minor operations, indeed, the principal danger to be apprehended from its use, is that of sudden death from its primary effects. If I may be allowed to judge of the matter from general impressions (for I have not as yet applied the test of careful statistics) I should be disposed to attribute the mischief from the secondary effects of chloroform principally to its predisposing to pyæmia; but unless in certain parts of the body, small operations involving the incision of only small blood-vessels are rarely the cause of this affection.

Although amputation has been generally selected as the operation for comparison in similar inquiries to the present, it is not the best adapted for the purpose. No operation has undergone or is undergoing greater changes and improvements. The substitution of the ligature of arteries for the hot iron, of the double for the single incision, of union by the first instead of the second intention, of heating for cooling dressings, were all great improvements; but, perhaps, none of these will effect so favourable an agency on the result of the operation, as the plan of regulating the temperature of amputation-wounds, and excluding the air from them, lately adopted in Germany by Langenbeck. Changes of this magnitude render such a comparison as that which engages our attention difficult; and if the practice recommended by Langenbeck prove as successful in other hands as, he assures us, it has proved in his own, a comparison of this kind will very soon be no longer possible (b). It is very different as respects the operation of lithotomy, which has, unhappily, remained nearly stationary for a whole century. The slow and painless dilatation of the neck of the bladder by the pressure of a fluid, which was suggested more than twenty years ago, has rarely been properly executed; and Surgeons have judged of the proposal more by these imperfect trials than by the principles of the operation. In consequence, however, of this stationary condition of lithotomy, the average mortality from no other operation has been so well determined by statistics; and, on this account, it is unnecessary to enter into any such detail on the subject as that which was required with reference to amputation. In young persons, or while the parts about the neck of the bladder remain easily distensible, the fatality of lithotomy is comparatively little; and there is, perhaps, as much difference between youth and age in this respect as between amputation of the thigh and forearm. The deaths from lithotomy in adult patients before the introduction of chloroform were, according to our best authorities on the subject, in the proportion of 1 to 4½, or 22 per cent. of those operated upon. What they are now, will appear by the following tables:—

Table IV.

Showing the Mortality from Lithotomy performed on the Adult and under Chloroform in the London Hospitals during the last Year, from July, 1855, to June, 1856.

Hospitals.	Cases.	Deaths.
St. Bartholomew's	1	0
Guy's	3	2
St. Thomas's	2	1
London	1	1
St. George's	2	1
University College	2	1
King's College	3	2
St. Mary's	2	1
Metropolitan	1	0

Total cases, 17; deaths, 9; percentage of deaths, 52·9.

Table V.

Showing the Mortality from Lithotomy performed on Adults (c) *in the London Hospitals during the last Three Years.*

	Cases.	Deaths.
First Year	10	7
Second Year	14	6
Third Year	17	9
Total	41	22

Average percentage of deaths, 53·6.

The fact that, instead of one in four operations proving fatal, the mortality should now be doubled, and amount to one half of those cut for stone, is so remarkable as to render any comment unnecessary.

I must now bring these observations to a close. Desirous to be as brief as possible, I have excluded every statement or reflection not essential to the exposition of the subject. To dispel the delusion that we have sufficient evidence that chloroform saves life, it was only necessary to point out the fallacies in Dr. Simpson's two tables; and to show the present greatly increased rate of mortality from amputations, it was enough to place before the reader the results of those performed in the London Hospitals. That such an increase of mortality was likely to proceed from the administration of chloroform, will not now be denied by any one who dispassionately reflects on the palpable evidence of its poisonous nature and debilitating agency afforded by the sudden deaths, and the lasting prostration so often occasioned by it.

If the truth of this exposure be called in question, on the plea that it is improbable that so great and pernicious a delusion should have existed so long, it is a sufficient answer to say, that, until a considerable time had elapsed, and the test of statistics could be properly applied, there was no possibility

(b) There is an allusion to this improvement at page 28 of the account which I lately published of the recent improvements on the treatment of stricture; and I may here state, that the modification of the current apparatus, called the "water muff," which I recommended ten years ago for regulating the temperature of wounds, diseased joints, etc., is well adapted for congealing a limb previously to amputation. The frigorific solution may either be made to flow over the skin in immediate contact with it, or a thin membrane may be interposed.

(c) I am not aware that more than one death from the direct effects of chloroform has been reported as occurring in the London hospitals, but the following notice in the statistical return in connexion with one of these cases at Guy's Hospital nearly amounts to such a report:—"The man required an unusual quantity of chloroform. He never rallied well from the operation, and death took place next day."

of detecting it. Chloroform, like the foul air of crowded hospitals, is a secret poisoner, its pernicious effects are rarely cognizable by any peculiar mark; and a reliance on individual experience cannot but lead to the most erroneous conclusions. The light which statistics have thrown on this important question, is only one of the great services they would render practical medicine, if carefully and judiciously employed, and brought, through the assistance of Government, to bear on those collections of facts preserved in the medical records of the hospitals of the country.

That the exposition now made will put a stop to the indiscriminate use of chloroform in operations, is not to be expected; for whatever the opinions of surgeons may be respecting it, or whatever danger may attend its use, patients will now insist upon its being employed, until a perfect substitute can be found. As well might we look for a cessation in the abuse of alcohol, the dangerous anæsthetic for mental pain. But it will be the duty of the Surgeon, instead of urging the administration of chloroform as a means of saving life as well as of preventing pain, to represent to the patient about to submit to amputation or lithotomy, that the endurance of a moderate degree of pain (for by the substitution of local anæsthesia, the pain need never be more than moderate) is a far less evil than to have the hazard of the operation increased to so frightful an extent as to be fatal in three instead of four or five amputations, as formerly, and to render recovery or death after lithotomy, an equal chance.

But it is not necessary that the argument of an increase of the unfavourable results of operations should be insisted upon. Supposing there were no such increase, if there be no sufficient proof of a decrease of these results, as a compensation for the numerous deaths which proceed from the direct and immediate effects of chloroform, the recommendation of its use, especially in the large doses employed of late years, could hardly be justified.

UPON A FORM OF PHAGEDÆNIC ULCERATION OF THE THROAT, NOT SYPHILITIC.

By HOLMES COOTE, F.R.C.S.
Assistant-Surgeon to St. Bartholomew's Hospital.

Patients are occasionally seen in London, in whom there exists a condition of the throat not unlike that which in some parts of the East is regarded as the result of "leprosy." The soft palate and uvula and even the hard palate are destroyed by phagedænic ulceration, which is characterized by a foul surface, deeply eroded, with elevated and everted edges, and thin sparing secretion. The voice becomes thick; the act of swallowing is rendered difficult, and there is an expression of distress in the countenance. In course of time, and under proper treatment, the ulcerative process may come to an end, when morbid adhesions form between the remains of the soft palate and the back of the pharynx, by which the patient's distress in deglutition is considerably increased; add to this that the whole glandular structure of the soft palate is destroyed, so that a most irritating sensation of dryness remains. This condition of the throat is familiar to all surgeons, and when seen in its active stage seems to suggest only two ideas, namely, that it is either the result of cancer or of syphilis. As regards the former disease, I must consider that any obscurity as to the nature of the case would shortly be cleared up, even to the most inexperienced eye. Cancer of the soft palate is a most intractable affection, and does not yield to any known treatment. The history, origin, and progress of the case, the indurated margin and base of the ulcer, and the aspect of the sore are quite different from phagedæna. But, as regards syphilis, a mistake may much more readily occur.

I am aware that many Surgeons will deny the existence of this form of phagedænic ulceration of the throat unconnected with the syphilitic taint; but, I would reply that such a statement is by no means proved, and that of late years there has been a growing faith in the widely spread influence of the syphilitic virus in modifying or developing a multitude of diseases both in children and adults, which is injurious to the cause of free investigation, and in many cases positively opposed to facts. Many years ago I saw in St. Bartholomew's Hospital two boys, of the age respectively of ten and twelve, in no way related, but in both of whom there existed phagedænic ulceration of the fauces and pharynx. No satisfactory history could be obtained to explain the occurrence of this disease on the grounds of its being syphilis; the parents were poor, but apparently healthy, and no particular symptoms had occurred at the time of the children's birth. Are we to believe that the syphilitic poison contracted by the father at least thirteen years before, may, after infecting the unborn child, remain dormant in that child until the age of twelve, and then manifest itself in the tertiary form? I confess such an assertion seems to me to call for a considerable amount of belief; and I am the more inclined to set it aside from observing how frequently suspicion is cast upon those, whom further investigation would completely exonerate. During the past month I have seen two or three children of the ages of twelve, suffering from eczema impetiginoides and strumous periostitis, both of which might have been mistaken for syphilitic rupiæ and nodes. They had indeed been diagnosed as such.

The following case is interesting in connexion with this subject:—

Jane B., aged 20, a pallid, strumous-looking girl, of respectable parents, and born in the country, states that seven years ago her throat became sore. She came to town to enter into service, and became out-patient at an hospital on account of difficulty of swallowing and loss of voice. These inconveniences continuing she was recommended change of air, and she returned to her friends, but subsequently was obliged to enter another hospital, where she was treated with cod-liver oil and the local application of nitrate of silver. It was about four years ago when she left this hospital, apparently in good health, but with considerable damage done to the soft parts, although there was no active ulceration. Since that time she has had frequent advice for return of the soreness of the throat, and has been questioned as to her ever having had syphilis, but she firmly denies having even exposed herself to contagion. During the course of her illness she has had disease of both the forehead and right upper arm, in both of which situations there are large cicatrices. The lining membrane of the soft palate was preternaturally red and vascular, and presented throughout tubercular elevations and numerous wart-like growths: the arch of the palate had been greatly changed in appearance by ulceration, and the remains of the uvula were adherent to the right and left side. In several situations there were spots of ulceration.

Upon first seeing this patient I concluded, without a moment's hesitation, that she was suffering from tertiary syphilis; but she assured me most solemnly to the contrary, adding that the same question had frequently been put to her before. She stated that she had no objection to inform me of anything connected with the disease, but that she had never exposed herself to infection. As I received her evidence with mistrust, I questioned her in private, when she repeated her denial, with evident distress at being disbelieved. She could have had no motive for concealment, for I saw her at the out-patient room of St. Bartholomew's Hospital, where she might have given a false name, or withdrawn from attendance without further inquiry. Under proper treatment, the ulceration, which was of chronic character, healed.

Just one month after the coming of this patient, a respectable-looking man presented himself with an affection of the hard and soft palate, in many respects similar. There were three foul ulcers occupying the hard palate and the velum palati, with elevated edges, surrounding a deep hollow, covered by an adherent secretion. The appearance of these sores was as if a phagedænic ulcer had extended to the bone, which had become denuded and carious, or dead; such, however, to all examination, was not the case. Upon questioning this patient I found that he was married, that he had never had syphilis, but had suffered thirteen years ago from gonorrhœa, which yielded to the usual treatment. This case is still under treatment.

There is in respect to these cases a ready mode of solving the difficulty in diagnosis by totally disbelieving the patient's statement; by affirming that the disease can be none other than syphilis according to all precedent; but I ask whether so ready a solution would be satisfactory? Have we attained that state of knowledge and certainty which will enable us to prejudge our patient's story?

That such a frame of mind is ill-suited to advance medical science might be illustrated by its application to the examination of the leper-huts at Jerusalem. There the believer in

of a moderate knowledge of Greek and Latin, while they altogether neglected to ascertain whether the candidates who came before them were reasonably acquainted with the most important branches of an English education. They seem to have taken this for granted. I am happy to observe, that in the University of Dublin, all candidates for entrance are now examined in English Composition and Arithmetic. Indeed the vast improvements which have been effected in the course of education, in this University, within the last three or four-and-twenty years, indicate the most enlightened and liberal views on the part of those to whom its government is entrusted, and are quite in keeping with the progressive character of the present age.

Next week I shall send you a short abstract of Dr. Neligan's address at the opening of the Dublin School of Medicine, Peter-street, delivered at twelve o'clock to-day, and also an account of the first meeting for the season of the Association of the Members of the College of Physicians, which will be held to-morrow evening.

GENERAL CORRESPONDENCE.

MORTALITY AFTER CHLOROFORM.

[To the Editor of the Medical Times and Gazette.]

Sir,—I have read, with much surprise, in your last two numbers an essay by Dr. Arnott, the object of which is to prove—1. That the mortality after operations in London Hospitals is considerably greater now than it was before the introduction of chloroform; and, 2. That this increase is the effect of the administration of that anæsthetic. Allow me to say a few words on both these points:—1. The figures adduced by Dr. Arnott show (if their correctness be assumed), not that the deaths after amputation have been more numerous in London generally since than before the introduction of chloroform, but that the average of deaths in all the London Hospitals taken promiscuously for the last three years has been higher than it was in four series of cases taken before that time. Your readers will observe that the last three years do not comprise the whole of the time since the introduction of chloroform. I can testify, from personal observation and tables drawn out at the time, that the mortality after amputation has been much greater at my own Hospital (St. George's) during those three years than in the previous year, 1852, so that there is nothing to show that the average of those three years is a fair measure of the proportion of deaths during the eight years in which chloroform has been used. Nor is there any better proof that Dr. Arnott's first table represents accurately the rate of mortality in all the London Hospitals before that time. It consists of four series, one of them that published in South's Chelius, and comprising the results of Mr. South's own practice at St. Thomas's; two out of the other three from University College Hospital, and consisting, as Dr. Arnott intimates, almost exclusively of cases operated on by Mr. Liston; the fourth, a very short series from St. Bartholomew's. I believe, therefore, that this table does not accurately represent the ordinary rates of mortality, and that the facts on which Dr. Arnott bases his conclusion are erroneous. Still less conclusive are the remarks which Dr. Arnott makes on the operation of lithotomy. It is quite sufficient to reply to this part of his paper, that the increased mortality after lithotomy in the adult in the present day is due to the number of the more promising cases subtracted by the invention of lithotrity; and I may, perhaps, be allowed to express my surprise, that any one writing on this subject should have passed over so obvious a consideration. I believe the mortality after lithotomy now is not by any means greater in the class of cases operated on than it was in old times; nor do the figures adduced by Dr. Arnott in the least degree shake that conviction.

2. But if we could admit the facts, should we, therefore, be bound to accept the inferences? I think not, until they are more intelligibly connected together; for at present I am unable to discover in the whole essay any argument which applies directly to the thing which is to be proved. The following sentence embodies the gist of Dr. Arnott's reasoning. Speaking of the prevalence of pyæmia to which the increased mortality after amputation of late years is due, he says, "An epidemic caused by atmospherical or other similar influences does not last for many years over the whole extent of a country; but this epidemic has been co-existent with, and has, doubtless, sprung from, chloroform." The assertion that this epidemic has coincided, either in duration or extent, with the administration of chloroform, is, as far as I know, utterly gratuitous. In the Hospital to which I am Registrar it commenced in 1853, and this was the case also, I believe, at other London Hospitals. You will see by reference to the Tables of Amputations and Compound Fractures which I publish in your Journal from time to time, that the difference between those two years was most marked in both classes of cases; although chloroform was given uniformly in the first and not in the latter; and by reference to our post-mortem records for the same two years I find that (exclusive of those two classes of cases) there were, in 1852, 4 fatal cases of pyæmia, in 1853 14, in only one of which chloroform had been given. So that in St. George's Hospital, at any rate, the epidemic attacked equally those who had and who had not been submitted to the influence of chloroform. In fact, the epidemic of pyæmia showed the greatest analogy to those of erysipelas, phagedæna, etc., to which our London Hospitals have been liable from time to time, as far as we know ever since their foundation. The assertion, therefore, that the increased mortality of the last three years (which, as my tables show, was owing exclusively to pyæmia) had anything to do with the administration of chloroform, is so gross a misstatement, that it would not be dangerous did it not rest on authority so respectable as Dr. Arnott's. I would remark on many other parts of this paper—such as the curiously prejudiced comparison between operations in the Crimea and some in the practice of an Army-Surgeon in India—under circumstances respectively which had nothing in common; but I abstain from a fear of overburdening your space. Allow me, in conclusion, to say, that so far from decrying the anæsthetic agent which it is the object of Dr. Arnott's essay to recommend, I have frequently used it with success, and always borne willing testimony to its great value in minor operations (in the late unfortunate case at St. Thomas's, for instance, its employment would very likely have been quite effectual); but I cannot conceive that any unprejudiced person would, for a moment, advocate its use as a substitute for chloroform in those involving deep tissues or requiring much time for their performance. I am, &c. T. Holmes.

4, Vigo-street, Nov. 4, 1856.

ELASTIC LIGATURES.

[To the Editor of the Medical Times and Gazette.]

Sir,—Being convinced of the superiority of elastic over inelastic ligatures for the purpose of strangulating or obliterating vascular tumours, hæmorrhoids, polypi, syphilitic warts, etc., I am induced to submit the method of their application for the consideration of your readers.

An elastic thread being stretched to its fullest extent, is tied in a single knot, slipped over the body of the tumour, and then securely fixed by a double knot. By its own elasticity it contracts again as quickly as possible, and what was, on its application, a large noose, is thus reduced to the smallest possible size. Strangulation follows, of course, and the tumour dies and falls off in a few days.

By this method a great deal of pain is saved to the patient, the use of the knife is frequently avoided, and even the fashionable écraseur itself is, to a great extent, superseded. Fistulæ in ano may be treated in a similar way, the thread being allowed to cut its own way through the sphincter.

I am, &c.

November 3, 1856. F. A. Nesbitt, M.R.C.S.

POOR-LAW MEDICAL REFORM ASSOCIATION.

[To the Editor of the Medical Times and Gazette.]

Sir,—In the intended application to Parliament, by the Poor-law Medical Officers next Session; I believe their position may be materially strengthened by comparing the payments now made in Unions, with the average cost of patients in Dispensaries; I shall therefore feel obliged, by Professional men connected with these Institutions sending me a copy of the last-printed Reports, or an extract of the particulars of the expenditure; and the number annually under treatment. I shall also be glad if Surgeons connected with Gaols throughout the Kingdom, will send me an account of the amount of their

consisted, however, of sound practical advice to the students, addressed to them in reference to their "duties as students," and their "duties as Practitioners." The lecture was delivered altogether *extempore*, and was listened to with deep attention by a numerous auditory. The students present seemed deeply impressed with the value of the advice they had received.

Mr. Ledwich commenced the regular business of the Session at the "Original School of Medicine," with a lecture which was delivered in the style of flowery eloquence for which the Lecturer is, on such occasions, remarkable. Mr. Ledwich took for his text the words, "Help one another," a simple sentence on the acknowledgment of the influence of which depends, he observed, "the root of all commercial policy, of civil and political government, of social connexion, and the profitable investment of intellectual labcur in those professional pursuits that either minister to the natural reality or acquired habits of the entire community." The address, from its nature, scarcely admits of condensation; it will, however, I understand, be published *in extenso*.

The first meeting for the discussion of subjects connected with practical Medicine of the Association of the College of Physicians of Ireland, took place on the first Wednesday in the present month, when communications were made by Dr. Neligan, and Dr. Henry Kennedy. Dr. Neligan's communication consisted in an account of a singular case in which a human hair had become accidentally knotted around the second toe of an old lady so tightly as to produce strangulation of the ungual phalanx. At the time Dr. Neligan saw the case, the strangulation had existed for three days; and the toe was very much swollen, inflamed and painful. With some difficulty he was enabled to divide the hair with a sharp pointed pair of scissors, when the swelling, and consequently the pain rapidly subsided. In remarking on the case, which he said, he brought before the association, chiefly in consequence of its singularity, he alluded to a paper by Dr. J. Weisse, Director ot the Hospital for children at St. Petersburgh. In it a nearly similar occurrence in young children, was described under the name of dactylostrangalis (strangulatio digitorum).

Dr. Henry Kennedy detailed the case of a young man, aged 18, who was admitted into Sir Patrick Dun's Hospital, labouring under scarlatina, attended with intermission of the pulse; as the disease advanced the intermission increased, and was accompanied by very great irregularity of the heart's action. Both these symptoms subsided *pari passu* with the declension of the disease, and the only cause which Dr. Kennedy could assign for their occurrence was the fact that the patient had, previously to his illness, been an inveterate smoker.

The first meeting, for the present session, of the Royal Irish Academy, was held last evening at their house in Dawson-street, the Rev. J. H. Todd, D.D., President, in the chair. Mr. Wilde gave a most interesting account of a MS. of Dr. Willoughby, one of the original Fellows of the King and Queen's College of Physicians, written in 1690, on the subjects of "the Bills of Mortality and Increase of People in Dublin, the Distempers, Air, and Climate of this Kingdom, also of Medicines, Physic, and Surgeons, and Apothecaries." Your readers are aware that Mr. Wilde has devoted much time and attention to the subject of Medical biography, and in particular to the history of Irish Medicine, and that we are indebted to him for an account of the Dublin Philosophical Society, established in the year 1684, for the most part by Medical men; Willoughby, the author of the MS., being its first Director. The present communication will be considered by the learned in the Profession as a most valuable contribution to Medical literature at a period when it is most defective.

GENERAL CORRESPONDENCE.

THE SALE OF POISONS.

[To the Editor of the Medical Times and Gazette.]

SIR,—Much has been said lately about the indiscriminate sale of poisons and drugs; angular bottles, with other things, having been proposed as a safeguard to the public; why not strike at the root of the evil at once, and give the same power to the London Apothecaries' Company which is given to the Apothecaries' Hall of Ireland? By looking at an advertisement from that body in your last number you will see that, "According to act of Parliament, no person can be taken or employed as an apprentice, assistant, or shopman to an Apothecary, and no person can open a shop or act in the art and mystery of an Apothecary within the kingdom of Ireland until such person shall have been examined and obtained the certificate of the Court of Examiners of the Apothecaries' Hall of Dublin." "It is also provided, that if any one shall take or employ any person as an apprentice, assistant, or shopman, or shall open a shop or wareroom for the retail of medicine, or practise the art and mystery of an Apothecary within the kingdom of Ireland, without such person having obtained the proper certificate for the purpose, so such person offending shall for every s .ch offence forfeit the sum of twenty pounds, to be recovered by the Apothecaries' Hall of Dublin." Now, why not give the Apothecaries' Company of London the same power, and let the Medical Practitioners support them? Here you strike at the root of the evil at once, and give the power of dispensing life and death to a class who may be trusted, and not, as it is at present, to those who have undergone no examination as to their knowledge of drugs and dispensing, although they have usurped the place of the legitimate Apothecary or Druggist, and with it all the gains. In the proposed new Medical Bill, I would suggest that the Apothecaries, conjointly with the Physicians, should form an examining body, and be empowered to examine all Chemists and Druggists, and that all assistants or shopmen should be compelled to take out a licence under a penalty. Perhaps some of my Medical brethren of more standing and experience may join in my views. A proposition should come from some of us, as it is evident the public will require some laws to restrain the indiscriminate and reckless sale of poisons, and the ignorance and carelessness in dispensing medicine by unqualified and ignorant persons. The Apothecaries' Company of London, although much vilified, has done more to raise the standard of education for the Medical Profession than any other body. Give them, then, the same power by Act of Parliament, as the Irish Apothecaries' Company, to sweep away the abuses which have crept in, and the public and Medical Profession will have no longer any cause of complaint.

I am, &c.

AN APOTHECARY AND SURGEON.

Worthing, November 8, 1856.

ON THE EFFECT OF CHLOROFORM UPON THE RESULTS OF SURGICAL OPERATIONS.

[To the Editor of the Medical Times and Gazette.]

SIR,—Mr. Holmes' office, as Registrar to St. George's Hospital, gives authority to the statements and opinions contained in his letter in the last number of the *Medical Times and Gazette;* and the reply to which he is entitled on this account will give me, besides, an opportunity of briefly noticing some points omitted in my recently-published paper on chloroform.

To commence with his facts, or statements. He sees nothing surprising in the circumstance that lithotomy should prove fatal in half of the adult cases operated upon in the London Hospitals, but is only surprised that I should not know (what he asserts to be) the reason of this great mortality, namely, that the more favourable stone cases are withdrawn, in order to be the subjects of lithotrity. I confess I had not examined the statistical returns with respect to this point, as the knowledge that Sir B. Brodie had, in his recent paper, referred to them for the purpose of showing the greater mortality of lithotomy as compared with lithotrity; and that Surgeons do not, as a rule, select the best cases for the latter operation, rendered such an examination unnecessary. On examining them since, I find that the cases of lithotrity at the London Hospitals have been too few sensibly to affect the calculation which I have made; and that the best cases have not been selected. Two or three are mentioned as cases in which the prostate was much diseased; and in the only adult case of lithotrity, occurring in the third or last year (October, 1855), the patient was 74 years of age, and "of so feeble a condition that it was not thought advisable to perform lithotomy."

So much for Mr. Holmes' facts; his opinions are not more correct. He finds fault with the shortness of the period of three years, from which I have deduced the present rate of mortality in the London Hospitals, and reminds the reader that this is not the whole time that has elapsed since the introduction of chloroform. Every reader knows this, though,

perhaps, he may think that it has not been universally employed in large doses, for a much longer period; and those who have read my paper are aware that I was limited to three years, because my statistical returns had the same limit. Though I am of opinion that this period is sufficient to determine the point, considering the wide field of observation furnished by the London Hospitals, I should be glad to have it extended, in order that every one may be convinced. On this account, I was pleased to fall in, a few days since, with an accurate return of amputations performed at the Oxford Hospital, for many years before and since the introduction of chloroform. It is drawn up by Mr. Hussey, and published in a recent volume of the "Transactions of the Provincial Association;" and exhibits a great increase of the rate of mortality between the period of the introduction of chloroform and the date of the return in 1852, or the commencement of the data from which the tables I have published are constructed.

Mr. Holmes considers it a mark of prejudice, my attaching any importance to the statement by a Military Surgeon of his great success in amputation before the introduction of chloroform; but he omits to mention that I had prefaced my notice of this report with the observation, that such limited statements were only of value when considered in conjunction with others of wider range. Had he remembered this axiom in statistics, he would not have been so ready to conclude, because the amputations at St. George's were more favourable in 1852 than in 1853, that the difference could only be accounted for by the breaking out of a new or unheard-of disease,—an epidemic pyæmia. A reference to the returns of the three years' mortality would have shown, that quite as great a difference occurred in other hospitals during that period; and shown also, that, instead of its being an erroneous procedure to estimate the general mortality by taking the average of various hospitals of similar character "promiscuously," it is, under the circumstances, the only rational, or, indeed, possible, mode of forming such an estimate.

When Mr. Holmes speaks of the co-existence of pyæmia in cases of fracture in which chloroform had not been exhibited with cases of amputation in which it had, as an argument against the position that chloroform may act injuriously by predisposing to pyæmia as well as other fatal affections, he forgets that pyæmia may have many causes, both predisposing and exciting; and (in reference to an illustration already used), that though drunkenness predisposes to cholera, this disease often attacks the temperate.

A reference need not be made to other remarks of less importance, because they have already been anticipated and replied to in my paper.

The important question is, what has caused this long-continued, widely-extended, and great increase of mortality after some of the severer operations.

If it be called an epidemic mortality, what epidemic, in the common sense of the term, lasts so long and continues over so great an extent of country? Cannot a sufficient reason be assigned for the fatal effect in the individual cases, without having recourse to a generally operating epidemic cause? Suppose that in lieu of chloroform, it had become the practice to bleed to syncope as an anæsthetic before amputation, according to the suggestion of Mr. Wardrop, would the mortality resulting from it have been attributed to an epidemic anæmia?

I believe that chloroform, in the large doses in which it has been generally administered of late years, acts as large bleedings would act; and that it is impossible for an unprejudiced person to witness its prostrating effect, the long-continued sickness, the suspended animation, and the sudden death so often caused by it, without acknowledging that it has all the appearances, at least, of a powerful and a pernicious agent. Whoever witnesses and reflects on these effects, and remembers the large number of sudden and uncontradicted deaths from chloroform, requires no proof of its ulterior fatal agency to convince him of the necessity of a change of practice in respect to it.

We cannot, after the examination of the evidence which has been adduced, be longer led astray by the often-repeated assertion that there is a saving of life upon the whole; and it would be absurd to suppose that the cases of sudden death from chloroform constitute the full measure of the mortality. How few even of these are generally known or reported! Hardly any in private practice, and now, even in Hospitals, they are concealed. It is well known to the frequenters of the London Hospitals, that, in the same week in which the recent death from chloroform at St. Thomas's occurred, another took place in another Hospital, but which did not become the subject of judicial inquiry. Humanity and the character of the Profession demand that the whole subject should be investigated anew.

I am, &c.

London, Nov. 8. James Arnott.

REPORTS OF SOCIETIES.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

November 11, 1856.

Cæsar Hawkins, Esq., President, in the chair.

A case of true elephantiasis, by Jonathan Toogood, Esq., communicated by Sir B. Brodie, was read by the Secretary. The subject was a young woman, aged 22. Both legs and feet were involved in the disease, which also extended a considerable way up the thighs. There was a diminished secretion of urine, which was albuminous, every other function being natural. The patient recovered after the application of a steam bath every night to the lower extremities; fresh squills three times a day, and the recumbent posture.

The President, who considered the case one of true elephantiasis, mentioned that in a case of Barbadoes leg which came under his care, and in which he had amputated the limb, there was an alteration of cellular tissue pervading every portion of the limb, not confined to the integuments, but extending down to the bone, and affecting the periosteum.

Mr. Fergusson's paper on

THE TREATMENT OF ANEURISM BY MANIPULATION

was then read.

The author explained this term to mean peculiar forcible squeezing of the aneurismal tumour, with the intention of breaking up the fibrin supposed to be within; so that, being displaced, it might possibly block up the distal end of the tumour, or the artery leading from it. After sketching the various means whereby Nature is supposed to bring about occasional spontaneous cures, cases having come under the author's observations, in which spontaneous cures had seemingly been caused by displaced fibrin, the author proceeded to show that, while Surgeons had in some degree followed the dictates of Nature, as gathered by experience in their attempts at cure, they had not, as far as his knowledge went, attempted to imitate the actual displacements of fibrin by any active interference on their part. He then explained how he had for many years entertained the idea that a cure by such a plan might possibly be effected. After many years watching for a case, in which for want of a better plan, such a one as he indicated might be used, a case of aneurism of the right subclavian artery, between and outside the scaleni, came under the author's notice in February, 1852, wherein, appreciating all the known dangers of the usual mode of treatment, he resolved to try this plan:—The flat point of the thumb was laid on the aneurism, which was about the size of a hen's egg, and, when the sac was emptied of fluid blood, the inner surfaces and supposed contents were rubbed against each other. The pulse, which had been carefully examined, was immediately arrested in all the vessels below the aneurism, and the patient became faint and giddy. In six or seven hours the pulsations returned, but the author repeated the manipulations the next day with a similar but not lasting effect on the circulation in the arms; for it was not till seven or eight days that circulation could be readily detected in the arteries. The tumour gradually diminished in size and in force; a pulsation, and various indications, particularly the gradual enlargement of a branch of the subclavian artery, at the root of the neck; the supra-scapula, or the transversalis colli, gave every hope that a cure was in progress. After seven months, at which date the tumour was much diminished, the patient had a severe feverish attack, accompanied with excruciating pain in the tumour, and died after a few days' illness. On dissection, it was found that the axillary artery was blocked up, and that

tended to have been placed within inverted commas. Whatever there was of original merit in Dr. Brinton's communication I have fully acknowledged; but I did not think it necessary to give him credit for facts previously published by myself and others; but I never entertained the idea of charging him with plagiarism, notwithstanding the close similitude which some of his passages bear to mine published six years previously. I know, and so must also Dr. Brinton, that Medical men pursuing the same investigations, and using the same means to arrive at a similar object, often will, nay, even must, express their ideas in nearly identical language; and I feel confident that these charges have not emanated from Dr. Brinton, who being himself a practical Physician, can comprehend the difficulties attending original investigations on disease.

In the course of lectures I am at present engaged on, and which are chiefly intended for Students, I think it a duty to them, and my own right, to adopt (with due acknowledgment) whatever of value has been published on the subject, and I have, therefore, availed myself of the writings of Budd, Cruveilhier, Rokitansky, Taylor and Watson, as also of the Proceedings of the Dublin Pathological Society. I trust this explanation will satisfy you and your readers that I am guiltless of "plagiarism or piracy;" and therefore, consistently with your statements (in which I fully concur) as to the "morality of the Medical press," I can hardly doubt but that you will readily give equal prominence and publicity to my refutation as to those unfounded charges, which obviously, under an erroneous impression, you have so publicly made against me.

I am, &c.

CATHCART LEES.

17, Lower Fitzwilliam-street, Dublin.
December 1, 1856.

ON THE EFFECT OF CHLOROFORM UPON THE RESULTS OF OPERATIONS.

[To the Editor of the Medical Times and Gazette.]

SIR,—In the fourth paragraph of my paper on chloroform, published on the 25th ult., the opinion is expressed, that the concealment of deaths from the administration of this agent is, for the reasons there assigned, "neither extraordinary nor reprehensible;" and, though this remark refers particularly to private practice, the same reasons obviously apply to such deaths occurring in hospitals, for the relatives of the poor who die from chloroform have feelings as well as the rich, and should not be needlessly afflicted. After having expressed these opinions, it is with no little surprise that I find myself charged, in the last number of this Journal, with casting an imputation on the Profession, by asserting that there has been such a concealment. I conceive that a Practitioner is no more called upon to divulge accidents from the use of chloroform than those proceeding from mercury, opium, or other powerful drugs; and surely his concealment of these has never been considered dishonourable. He has it always in his power to make his professional brethren acquainted with such occurrences at a convenient time, if the information be useful to science, but he should confine the communication to them.

It is of importance, however, in judging of the propriety of employing chloroform indiscriminately, to know that the reported cases of sudden death from it do not constitute the whole number that have occurred. My opinion on this point is by no means singular. At a meeting of the Edinburgh Medico-Chirurgical Society, on the 5th of last March, and on the occasion of a discussion respecting a death from chloroform, Dr. Gairdner, one of the most distinguished of its members, stated, that "within the last three weeks, he had been informed of three distinct cases of reputed death from chloroform, which had been the talk of a neighbourhood (not in Edinburgh), but which would probably never be noticed in any more public way." But what Medical man is there who does not know several such cases? How rarely do we hear of death by chloroform or ether in France? Not once in a year; yet etherization is as much in use there as here, and we cannot admit that their plan of administering anæsthetics is superior to ours. The reason, doubtless, is, that French Practitioners do not consider themselves bound to divulge these more than other accidents occurring in their practice.

I shall now proceed to notice Mr. Holmes's second letter; but, as I have already, in the paper referred to and in my first letter, answered most of his reiterated objections, I shall confine myself to what he says respecting the rate of mortality from lithotomy. He is not satisfied with the reasons which I adduced for believing that the cases in the Hospitals, upon which lithotomy has been performed during the last three years, were of the ordinary description, and not only those bad rejected ones deemed unfit for lithotrity. But, if the facts, that only one case of lithotrity in the adult occurred during the last year in the London Hospitals, that that case was pronounced unfit for lithotomy, and that several in the former years were also bad cases for lithotomy,—if these facts fail to convince him, what hope can I entertain of conviction from other evidence? Will it serve any purpose to inform him, that of the few that were lithotritized during this period, nearly a third died; showing that the cases could not have been what are termed good ones, as this mortality is of higher rate than that arising formerly from lithotomy? Does he not know that lithotrity has as yet obtained but little favour with Surgeons, and that this was one of the reasons assigned by Sir Benjamin Brodie for publishing his recent paper, recommending the operation, in which he refers to these very statistics as a support to his argument? As a professed logician, Mr. Holmes ought not, so openly, to shift his ground as he does in this discussion. The question is not, as he states, whether it is, or is not, the case, that the best subjects are reserved for lithotrity, although the proper answer to this question is very different from that he supposes; but, whether the cases in the Hospitals, during the three years, were not ordinary cases of stone in the adult. The obvious truths that every case of disease has peculiarities affecting the prognosis, and that large numbers are better than small for such calculations, it was surely unnecessary to state; but the advantage of statistics, or of forming an estimate from a great many examples is, that these differences or peculiarities being thus balanced, become of little or no importance. As to Mr. Holmes's new question, it may be true that, amongst those Surgeons who practice lithotrity, the best cases may often be selected; but Mr. Holmes must surely know that the cases to which this operation has been deemed peculiarly adapted are not the best, but the worst cases for lithotomy, or those of very old men. If he will refer to Mr. Erichsen's "System of Surgery," he will find statistical returns, showing the extraordinary success of lithotrity performed on septuagenarians and octogenarians, few of whom would have probably survived lithotomy.

What Mr. Holmes exactly means when he presumes to accuse me of bringing a charge of rashness against the Hospital Surgeons, because I affirm that chloroform has increased the mortality from lithotomy, it is difficult to say; but this I know, that although Surgeons may be sorry to find that that which they considered a great and unmixed boon, does not deserve this high character, they cannot for a moment consider the publication of my inquiries on the subject as any imputation on their judgment or skill. Do Mr. Bellingham, and the other distinguished Dublin Surgeons, who have shown that the ligature of arteries in aneurism is more fatal than compression, or Mr. Syme, who has shown that excision of certain joints is preferable to amputation, thereby cast imputations on the Profession?

Instead of following Mr. Holmes through other details, in which his reasoning is equally incorrect, I will occupy what remains of the little space I can venture upon, with a few additional observations respecting the question of the cause of the admitted great increase of mortality since the introduction of chloroform. Mr. Holmes mis-states my argument on this point. I have not said that chloroform must be the cause, only because no other cause can be pointed out. I have adduced chloroform as a *vera causa*. I have enumerated the symptoms or effects produced by it, manifesting its poisonous or pernicious character, and I have given what I think will, in the present state of our knowledge, be deemed a sufficiently satisfactory explanation of its mode of action. Can a better explanation be given of the action of foul air, an admitted powerfully predisposing cause of fatal disease after operations?

As regards the "epidemic pyæmia" which has been deemed the cause of this increased mortality by Mr. Holmes, or any other similar influence, it may be stated, in addition to other objections, that the circumstance of there having been an increase of mortality only after the severer operations, militates

strongly against such an idea. If an epidemic influence prevailed, how was it that the wounds from excision of tumours, etc., escaped the infection? It is not so when hospital gangrene or erysipelas prevails. I have explained in my paper why chloroform only proves fatal in minor operations by its primary effects. Mr. Holmes admits that pyæmia existed before the introduction of chloroform, though its nature was unknown; and he must be aware that other diseases have appeared to be unusually prevalent only because means have been discovered of detecting them. After the discovery of auscultation by Laennec, we had what was, often in jest, called an "epidemic heart disease." To allege that the increased mortality has proceeded from an imaginary epidemic, (though, no doubt, it may, as I have said, have been at times aggravated by general influences,) when we have so sufficient a cause as chloroform before our eyes, is hardly a greater mistake than it would have been to attribute to an epidemic the excessive mortality which prevailed among Dr. Sangrado's patients in Valladolid.

I trust that this correspondence will now cease. I have no objection to defend any of the positions I have laid down connected with this important inquiry; but I am unwilling to waste my time and unprofitably occupy the valuable columns of a Medical journal by repeated replies to repeated objections. I am, &c.

London, Nov. 29, 1856. JAMES ARNOTT.

UNIVERSITY OF LONDON EXAMINATIONS.

[To the Editor of the Medical Times and Gazette.]

SIR,—The "UNDERGRADUATE" who comes forward as the champion of the present high standard of requirements demanded by the University of London has, I think, somewhat misapprehended the purport of your remarks. I did not understand that you wished the examination lowered to the "grinders'" level. There is a very wide difference between the highest standard that need to be imposed upon the average of undergraduates, and the tests that can easily be attained by the help of a "grinder." A wide margin would be left for the incompetent; while those who had proved their competency would have obtained a degree that confers dignity. To those who aim still higher, the honours of the University are open.

The "Undergraduate," however, has altogether lost sight of a large class of men who in after-life may desire to acquire the honour of a degree from the University of London. These are now effectually excluded from all chance thereof, if they have any engagements demanding the greater share of their time and attention. By elevating the standard so that prizemen or *élites* only can reach it, these men are compelled to seek their degree at some inferior University. It must be borne in mind that the class to whom I refer includes not merely routine Practitioners, who, having grown grey, would like a dignified relaxation from the duties of general practice, but it embraces many who hold the most distinguished positions in the Profession, to be classed with whom is alone and of itself an honour.

Ambitious of its degrees, I should equally with the "Undergraduate" regret that the University of London should lose the respect and confidence of the public and the Profession. Such a result could not, however, I conceive, follow from placing these within reach of experienced Practitioners, as well as of the *élites* of the schools. Neither could there be any injustice towards the existing graduates to admit as their equals men whose attainments are attested by an examination adapted to their position; probably, also, by the attainment of prizes in early life, and by subsequent years of hard-earned experience.

The University of London was intended, I should suppose, to be useful as well as ornamental. It is all very well to pride ourselves on the "lustre," and so forth, of its graduates, but we must not forget that it was established in order that the inhabitants or residents of London and its neighbourhoods should enjoy the opportunity of obtaining degrees compatibly with the prosecution of studies, for which it was perfectly clear that London offered far greater facilities than the older Universities.

For all that has yet been shown to the contrary, I still submit that it would greatly extend the usefulness and credit of the University of London were a scale of examination adapted to the opportunities and acquirements of men of some standing in the Profession, who have already passed the ordeals of other examinations.

There are many who would have prided themselves on such an M.D., who having perforce gone northward, keep the *alma-matership* of their M.D. as quiet as possible, being half-ashamed thereof, seeing how easily these are gained. The principal credit of some Universities has been reflected by graduates who have earned their reputations where they cannot obtain their degrees. I am, &c.

F.R.C.S., L.A.S.

INDEPENDENT POSITION OF POOR-LAW MEDICAL OFFICERS.

[To the Editor of the Medical Times and Gazette.]

"The fact that the vacancy has been caused by what appears to be the unjust dismissal of Mr. Waldegrave."

SIR,—The above remark in your editorial article of Saturday, November 29, 1856, appears to me to embody an error with regard to the power of Boards of Guardians over the Medical Officers of Unions.

By Article 191, Consolidated Order, the Medical Officer holds his appointment "till he die, resign, or become disqualified," or be removed by the Commissioners, not by the Guardians; they have only the power of suspending a Medical Officer; but that suspension is required to be forthwith reported to the Poor-Law Board, and it is only right to infer that they would interfere in a case of injustice; but it generally happens that the Surgeon, bullied and disgusted, and perhaps intimidated, and not understanding the strength of his position, sends in his resignation, which the Poor-Law Board accept, and do not interfere to reverse.

It is painful to the feelings, and very trying to the temper,—I speak from experience,—to bear patiently the indignities and petty tyranny which some of these gentry seek to inflict upon us. I say some, for many of them are Guardians of the Poor by deed as well as by name; but I believe that, as Professional men, we need only be careful that our duties are well performed, both by ourselves and representatives; and, to act simply with firmness and moderation, in order to stand perfectly secure in our position, and ensure a victory over the attempts of our enemies.

Again: the stipend of a Medical Officer is, in fact, a compact between him and the Guardians, submitted to and accepted by the Poor-Law Commissioners; for them to interfere would be, virtually, to destroy the principle of free trade. In cases of insufficient remuneration the proper course appears to me to be, to submit the appeal against it not only to the Guardians but also to the Poor-Law Commissioners, who, although they "regard not man," may yet be influenced by external pressure to procure some alteration, for the sake of their own convenience. Although a subscriber to Mr. Griffin's movement, I have more confidence in this individual representation and assertion of right, than in an appeal to Parliament; we had better put our own shoulders to the wheel than call upon Jupiter; and it appears that we have more power than we think for.

A POOR-LAW MEDICAL OFFICER.

RICORD ON SYPHILIS AND MERCURY.

[To the Editor of the Medical Times and Gazette.]

SIR,—My attention having been drawn to an original communication in the October number of the *British and Foreign Medico-Chirurgical Review* on Infecting and Non-Infecting Syphilitic Sores, by Mr. Henry Lee, and finding in it a passage containing a gross perversion of facts, enunciated as well by Mr. Langston Parker as by myself, I feel called on at the earliest opportunity to expose it.

At page 500 occurs the following passage:—

"In England opinions have been as varied as in France. Thus, in the third edition of Mr. Langston Parker's work, where he speaks of the circumstances which particularly indicate the use of mercury in primary syphilis, the Author includes 'all sores which have yielded a characteristic pustule by inoculation.' The indication for the employment of mercury, Mr. Langston Parker says, is still more pressing if the primary sore be accompanied by bubo (p. 15). In support of this opinion he quotes the following as from M. Ricord: 'In

BOOK NEWS.

The third edition of Dr. Mackenzie's *Outlines of Ophthalmology* contains the excellent introductory discourse on the causes which have rendered the eye a separate object of medical study, a large portion of which we published in a recent number. The *Outlines* themselves are a sort of programme of a most complete Course of Lectures on the Eye and its Diseases, and will be found invaluable to students, to show them what they have to learn, and to lecturers, to remind them what they have to teach. — Mr. Savory has just completed the fifth edition of his well-known *Compendium of Domestic Medicine*. It is the best book of the kind in the language.—Mr. Snape has published *A Letter to the Committee of Visitors of the Surrey Lunatic Asylum*, in which he enters into a full explanation of the circumstances in reference to the case of Daniel Dolley, who died in the Asylum, and whose death has given occasion to so much injury and pecuniary loss to Mr. Snape. It is stated in this letter that the treatment of insanity by the shower-bath and by tartar emetic has been attended in many cases with great success, and the death of the man Dolley is attributed to fatty degeneration of the heart. The injustice of the Lunacy Commissioners in receiving secret and anonymous evidence is very properly exposed, and the conduct of Dr. Diamond, the colleague of Mr. Snape at the Surrey Asylum, is rather severely criticised. This letter ought to be read by all who wish to hear Mr. Snape's defence to the charges lately made against him.—Dr. Alfred Taylor has presented in a separate form his article on *Poisoning by Strychnia*, which appeared in the last number of the Guy's Hospital Reports, and which we have already noticed in this journal. In its present shape, Dr. Taylor's version of the late Palmer trial is an important contribution to forensic medicine and toxicology.—In the second edition of Mr. Piesse's *Art of Perfumery* those interested in these matters will find a variety of useful receipts for the preparation of perfumes, medicated soap, pomades and oils, hair dyes, depilatories, tooth-powders, and mouth washes.—Mr. Lizars's *Supplement* to his *Anatomical Plates* disappoints us on the whole. There is a want of freedom of drawing throughout, and the general effect is stiff and formal. In some of the plates the details are accurate, but in others the appearances are very different from anything we ever saw in the dissecting room. — *Matter, its Forms and Governing Laws*, is the title of a little book which will be found very useful to those who have neglected the study of Natural Science and wish to begin it.

GENERAL CORRESPONDENCE.

DR. BRINTON AND DR. LEES.

[To the Editor of the Medical Times and Gazette.]

Sir,—I have no wish to add one word to your remarks of last week respecting Dr. Lees and myself. You have now laid before your readers two sets of parallel passages; and it remains for them to decide whether, in either or both of these, the resemblance may be referred to a casual use of similar language in describing similar facts, or whether that resemblance is sufficiently close and extensive to imply a transcript, such as no man of sense could misinterpret, and no man of honour could justify.

One remark in Dr. Lees's letter, however, I feel bound to notice, since my answer to it will also explain my conduct in reference to other authorities on the Gastric Ulcer. He complains that, while I claim to have made a toilsome search through the literature of the subject, I have left unnoticed his contributions to it.

My defence is simple. I have not acknowledged them, because I owe nothing to them—probably never read them.

It was my object to make my Essay a study from nature; and, in introducing it, I explicitly stated my materials to consist of cases either reported by others or observed by myself. The numbers of these cases made it impossible for me to quote their sources, save where many were derived from a single observer. The same reason obliged me to lay aside a project I had entertained of publishing a detailed table of cases, like one I some years ago contributed to the "Transactions of the Pathological Society" (On Intra-cranial Aneurisms, vol. iii. p. 47). And in conducting my inquiry, I soon fell into a practice of eliminating from it all but the precise materials I was in quest of; so that, for example, in glancing over a memoir or lecture on the subject, nothing arrested me save the well-known *indicia* of a narrative or report.

I think many of your readers will so far appreciate the accuracy of this statement as to see that it expresses the only way in which research of this kind can possibly be rendered compatible with the absorbing duties of active professional life; and I could pledge myself, if necessary, to collect, from the records thus brought together, every detail stated in my Essay, and to substantiate, from independent sources, every word which my own general descriptions may seem to have borrowed from Dr. Lees, or any other author.

Nothing is further from my intention than to disparage the description of other and earlier writers. I chose an independent (and, as I thought, undervalued) mode of research, such as enabled me to dispense with them; but only by an amount of labour far exceeding that which would have been requisite merely to verify and adopt them; and I gladly seize this opportunity of pointing out that, in pursuance of this plan (which, by confirming such descriptions, does more to accord it them than the most servile imitation), I did not spare my own feelings, where impartiality demanded their sacrifice. So far as I can recollect, my Essay nowhere alludes to the honoured name of my teacher and former colleague, Dr. Budd, who has not only done more than any one in this country to call due attention to this disease (as shown by his admirable treatise on the Diseases of the Stomach, reprinted from your columns); but who has long had the strongest personal claims on my gratitude and esteem.

I will only add to these (unavoidably egotistic) remarks, that I am sure I shall ultimately be acquitted of all wish to injure other authors, either by plagiarism, by detraction, or by silence. I should be glad, indeed, to believe that the standard of conventional morality in this respect was a higher one than that which I endeavour to keep before me. Even in this very Essay, I think that my citations, if examined by the above light, will be found as just as any reasonable amount of care could have made them; and with what accuracy I should like to fulfil this important duty of authorship may, I think, be inferred from some of my earlier writings; for example, from my Essay on the Stomach ("Cyclopædia of Anatomy," Supplement) where (page 328) I can point to a single foot-note, which (if I recollect right) cost me weeks of continuous research.

I am, &c.

Brook-street, Grosvenor-square, December 8, 1856. William Brinton.

ALLEGED MORTALITY FROM CHLOROFORM.

[To the Editor of the Medical Times and Gazette.]

Sir,—I quite coincide in Dr. Arnott's wish that the correspondence between us on the subject of chloroform may cease. My object in the present discussion was to draw the attention of your readers to some of the weak points of Dr. Arnott's theory; and so to prevent its exercising an influence on Surgical practice which would not fail, in my opinion, to be injurious. I had no hope of convincing Dr. Arnott, who had evidently (though doubtless unconsciously to himself,) started from a foregone conclusion. Before closing the correspondence, however, I must be allowed, in a very few words, to protest against some of the unfair assumptions to which Dr. Arnott's logical difficulties have driven him in his last letter.

He assumes that the mortality "has been increased only after the severe operations." I have endeavoured to convince him that pyæmia has been prevalent not only after severe operations, but after trifling operations, after other minor injuries, sores, etc., and even as an idiopathic disease. He does not seem to be aware of the nature of his own charge against modern Surgeons. It is, that a course of treatment introduced by them has more than doubled the mortality after lithotomy and other operations. Surely then they have been guilty of rashness in persisting in it for nine years. To compare this with an imaginary accusation against Mr. Bellingham's or Mr. Syme's contemporaries for not anticipating those gentlemen's inventions is puerile. When Dr. Arnott says that he has enumerated the symptoms or effects produced by chloroform, it is difficult to believe that he refers to his present paper, in which nothing of the sort is attempted; and as to explaining

its mode of action, he has merely asserted that this is analogous to that of bleeding, an assertion very improbable *à priori*, quite unsupported by proof, and which would still leave it altogether unexplained how either predisposed to pyæmia. With reference to the lithotomy question, if Dr. Arnott had been able to answer the plain question which I put, from the ascertained practice of modern Surgeons, he would have seen that I had not shifted my ground; and if he had had sufficient interest in the matter to have procured the details of the few cases he cited, which are, I believe, perfectly accessible, he would, I am convinced, have seen that no chloroform theory was necessary to establish a "*vera causa*."

I leave the question between us with confidence to the decision of your readers, satisfied that they will require different facts and reasonings from those adduced by Dr. Arnott, before consenting to give up the most brilliant discovery of Modern Medical Science. I am, &c. T. Holmes.

4, Vigo-street, December 8, 1856.

ROYAL MEDICAL BENEVOLENT COLLEGE —INCREASED CHARGE FOR EXHIBITIONERS.

[To the Editor of the Medical Times and Gazette.]

Sir,—As it has been ascertained that the next meeting of the Council of the Royal Medical Benevolent College will no be held until Wednesday, the 7th of January, may I reques you will be kind enough to give space for this letter in your Journal, as it will much assist the cause. The memorial requesting the Council to convene an Extraordinary General Meeting will be presented on that day. A deputation will attend for that purpose, and also to hold a conference with the Council as to the best course to be pursued in order to bring the matter at issue, if possible, to an amicable termination. It is hoped that the deputation will succeed in obtaining such a statement of accounts, and of the calculations on which the supposed necessity for the increased charge is based, as will enable all parties to form an impartial judgment upon the question. The 117 signatures I enclose have already been appended to the memorial. I am, &c.

Wm. Frank Wagstaffe (Pro Committee).

Walcot-place West, Kennington,
10th December, 1856.

ON THE DIAGNOSIS OF PRIMARY SYPHILIS REQUIRING MERCURIAL TREATMENT.

[To the Editor of the Medical Times and Gazette.]

Sir,—The last number of the *Medical Times and Gazette* contains a letter from Mr. Stapleton of Dublin, in which he offers some comments upon an article of mine in the October number of the *British and Foreign Quarterly Review*. Mr. Stapleton complains that the *facts* which he has enunciated have not been fairly represented in the article in question. The quotations therein contained as illustrating Mr. Langston Parker's *opinions* (and to these only my observations refer) are, however, correctly given; and if Mr. Stapleton's meaning is not clear, the fault does not rest with me.

At page 15 of Mr. Langston Parker's work on syphilitic diseases, he says there are several circumstances which particularly indicate the use of mercury in primary syphilis; and under the fourth head, Mr. Parker includes all primary sores which have yielded a characteristic pustule by inoculation. "The indication," he says, "for the employment of mercury is still more pressing if the primary sore be accompanied by bubo."

By the unqualified term "bubo" I understand "a swelling in the lymphatic glands from acute or chronic inflammation." Mr. Stapleton says that Mr. Langston Parker here means, "multiple indolent bubo, consequent upon indurated chancre." If this should prove to be the case, it is very desirable that those who read Mr. Parker's widely circulated, and in many respects very valuable work, should be made acquainted with the true meaning of the word; for I have good reason to know that both surgeons and pupils are often in the habit of prescribing mercury for primary syphilitic affections, accompanied by inflammatory bubo, and believe that by so doing they are following Mr. Langston Parker's injunctions.

My answer to Mr. Stapleton, however, is that what he represents as Mr. Parker's meaning appears, upon the face of it, simply not to be the fact. Let us hear the description of a bubo treated by mercury, at p. 68 of Mr. Parker's work on the "Treatment of Syphilis." After a trifling primary affection, in this case, it appeared probable that the bubo would suppurate. The "glands were as large as a turkey's egg, red at the summit, exceedingly painful and tender, and from a feeling of fluctuation given to the finger, it approved, probably, that matter had already formed." Mr. Parker recommended that this patient should use the mercurial baths every other day, and rub in every night half a scruple of mercury. Does this description in any way accord with the multiple indolent bubo mentioned by Mr. Stapleton?

Again, in all three editions of Mr. Langston Parker's work, he states that it is of great importance that a bubo should not be allowed to suppurate; and that, "unless especially contra-indicated, mercury may be employed to assist the resolution of the tumour." In the face of such assertions as these, upon what authority, I would ask, does Mr. Stapleton say that by the term "bubo," Mr. Langston Parker means "the multiple indolent bubo consequent upon indurated chancre"? The importance of the subject alone leads me to repeat, in opposition (as it appears to me) to the deservedly high authority of Mr. Parker, that neither the production of a characteristic pustule by inoculation, nor the existence of bubo, is a valid reason for the administration of a mercurial course in primary syphilis.

Some expressions are used by Mr. Stapleton little adapted for scientific inquiry. It must always be regretted that such should be introduced into Professional discussions, and I must be excused if I refrain from further noticing them. I have, therefore, confined my observations to the points of scientific interest referred to in Mr. Stapleton's letter, and these have now, I believe, been fully answered.

I am, &c. Henry Lee.

13, Dover-street, Piccadilly, Dec. 8, 1856.

THORACIC ANEURISM AND TUBERCLE.

[To the Editor of the Medical Times and Gazette.]

Sir,—In a paper on thoracic aneurism, lately published in the *Medical Times and Gazette*, by Dr. Fuller, I find the following words: "This symptom," alluding to feeble respiration, "may of course be complicated by the presence of tubercular disease of the lung, *but the co-existence of such diseases with aneurism is rare.*"

In the able work of Dr. Stokes, "On the Diseases of the Heart and Aorta," I find the following words (page 578): "*The morbid condition which most often accompanies aneurism, (thoracic) is that of tubercle.*" He embodies this opinion, moreover, in his 38th proposition. Thus: "*That of the general morbid conditions which accompany aneurism, tubercular phthisis is most common.*"

Being somewhat interested on this point, I should wish to know which of these opinionsto adopt. I am, &c.

Stanhope T. Speer.

Eton House, Cheltenham, December 5, 1856.

REPORTS OF SOCIETIES.

THE PATHOLOGICAL SOCIETY.

December 2, 1856.

Mr. Arnott, President, in the chair.

After some preliminary business,

Mr. Hutchinson gave, on behalf of Dr. Wilkes and himself, a report of their examination of Dr. Gibbs's specimen of

CANCEROUS SUPRA-RENAL CAPSULE.

The mass submitted to them for examination was about the size of an egg, and consisted throughout of very vascular medullary cancer, in various stages of growth and degeneration. Under the microscope, cells of most characteristic features were abundant. No portion of healthy supra-renal gland could be found in the mass. The tuberous mass in the liver was also of similar character.

Dr. Gibbs stated that he had, since showing the specimen, himself examined the lumbar glands of the same case, and had found them to be cancerous. He adverted to the fact

circumstances supposed to be connected with the prevalence of the disease.

Dr. Jeannerett thought some comparison ought to be made of the circumstance under which cholera spreads at sea and on land. One of the most singular phenomena connected with the disease was the rapidity with which patients occasionally recovered (sometimes in a few minutes), notwithstanding the depressing influences to which they had been subjected.

Colonel Sykes adverted to the varying and often diametrically opposite circumstances under which cholera appeared, and the consequent difficulty of arriving at correct conclusions.

Dr. Greenhow stated that, according to an account published by the Admiralty, when the *Britannia* put out to sea, the severity of the outbreak decreased; and that it was not till the port-holes were closed that the atmosphere became pestiferous, which was partly due to the excretions of the patients.

Mr. Tucker cautioned the Society from inferring from certain parts of the paper that fermented cider was other than a good prophylactic in cases of cholera.

The Society then adjourned to November.

CRIMEAN MEDICAL AND SURGICAL SOCIETY.

April 19, 1856.

Sir John Hall, M.D., K.C.B., in the Chair.

The proceedings of the last meeting having been read by the Secretary, Dr. Mouat, C.B., Deputy Inspector-General, made the following observations on some points connected with the use of chloroform in military practice. The author commenced by stating, "The subject of the administration of chloroform in the well-known shock or depression following severe gun-shot injuries, is one, from its nature and the peculiar interest it possesses at the present time, that requires no apology for its introduction, and appears to me to be a fit and proper one for discussion in this Society. The Profession at home naturally look to the Medical Officers of this Army to contribute their mite of practical experience towards the settlement of this important and disputed question; but I much fear they will be somewhat disappointed in the results and conclusions arrived at. Great and grave doubts are beginning to arise in the minds of some unprejudiced practical Surgeons and thinking observers at home and abroad, as to the indiscriminate use of this powerful anæsthetic; so tempting to the sufferer, yet at times so fraught with danger and uncertain in its results, that I defy the most strenuous advocates for its employment to say, *à priori*, what its results may prove in any given case; in other words, to say distinctly what fixed laws it invariably follows, if any. The fatal cases, unfortunately, from the simple extraction of a tooth, or removal of a finger, to the more formidable amputation at the hip-joint, leaves no doubt as to its occasional melancholy result. Dr. Simpson's cases are in a great measure confined to its administration in parturition—a simple process of nature; therefore his great experience does not apply strictly to the subject now under consideration. Dr. Snow's practice—at least a very large proportion of it—I have been informed, has occurred in Dental Surgery; at all events, not in gunshot wounds; and no one, I am sure, will attempt to compare the shock of the extraction of a tooth, or ordinary Surgical operation, to the amputation of a limb close to the trunk. The first case in which I saw chloroform administered in this war was one peculiarly adapted to test this question. It was on the day of the memorable and bloody battle of Inkermann, and the patient was an officer, 29 years of age; the injury was a compound comminuted fracture of the femur near its neck, with injury to the blood-vessels and nerves; much blood had been lost on the field. I need hardly say, after this explanation, what the operation was. Several hours were allowed to elapse after the receipt of the wound, and reaction, with the aid of stimulants, had taken place; the patient was in great pain, most anxious and urgent that the operation should be performed, and earnestly stipulated for the administration of chloroform. At the request of the operator, and after a deliberate consultation had been held, I administered the chloroform, which was measured, and amounted to about 2 drachms. He was rapidly and easily affected, and was neither sick nor convulsed. The operation was performed by Mr. Wyatt with great skill, and the loss of blood was inconsiderable; but I regret to say the sufferer died somewhat suddenly after, notwithstanding all attempts at artificial respiration, the cold douche, etc. Some persons present entertained the opinion that he perished from the effects of chloroform. My own opinion was, that he died under the combined influence of shock and the depressing effects of the chloroform inhalation. It is worthy of remark, that the patient had previously been put slightly under the influence of chloroform, in order to examine the extent of injury, as he would not submit without; and no harm resulted. This is case No. 1, and cannot, therefore, be explained under the convenient term of idiosyncrasy. Great weight is, undoubtedly, due to the opinion of so experienced and talented an operator as Professor Syme; but I much doubt if even this distinguished Surgeon has had any experience in injuries of the peculiar nature we are now about to discuss (notwithstanding the opinion of most civil Surgeons as to there being no essential difference between gunshot and railway injuries). I am fully aware that I am standing on dangerous ground in provoking many antagonists, to say nothing of public opinion, for this appears to have become a popular as well as a professional subject. No one can yet have forgotten the storm of abuse, indignation, and misrepresentation, with which a certain memorable departmental order on this matter was received. Even the Sebastopol Committee, in their anxiety to saddle somebody with something, actually went out of their way in their endeavour to cast unmerited odium on the Medical department of the Army on this very subject: and yet what did this famous order, after all, amount to? A wise and humane caution from an old and experienced officer in the field, to his younger professional brethren, most of them entering for the first time on new and trying duties, requiring all the resources of our art, backed by the wisdom of experience; for, after all, experience must be our schoolmaster. We were bound to respect such an opinion, coming from such a source. I am not about to enter into the question as to whether that caution might not have been more carefully and judiciously worded; for, no doubt, it would have been differently expressed, if intended for the perusal of a popular, instead of a professional public. The introduction of chloroform was, no doubt, one of the proudest discoveries and greatest blessings of the age. It has often contributed to sustain the fortitude of many a brave warrior, and given confidence to many a timid operating Surgeon; and anything calculated, in the remotest degree, to lessen the horrors or soften the terrors of war, cannot but be considered a boon to suffering humanity; hence chloroform must claim almost universal approbation. It does not, however, follow that such an agent should be used indiscriminately, or as a matter of routine, as is too frequently done; but its employment should be withheld, when it can be dispensed with, and whenever it is intended to be employed, too much caution and care cannot be exercised in its administration. Most of the fatal cases have, doubtless, been sought out, and brought before the public. A reference to statistics is here not in my power, but I have been informed by one Medical gentleman, Dr. Rooke, that, for some time, about four years ago, he kept an account of fatal cases, and in a short period collected no less than forty. The statistics of this subject are very imperfect. Many cases may have been hushed up, but by far the larger number have sunk silently into their graves. It is that peculiar state of nausea and depression following its use, in which perfect reaction is never thoroughly established, the desire for food never returns, and the patient sinks, as it were, stealthily, and dies from exhaustion in from twelve to twenty-four hours. These cases are far more numerous than is generally supposed, and many of them may fairly be termed "deaths from chloroform," but are never so returned. I can at this moment bring two or three such to my recollection. Medical men are naturally anxious to avoid the kind of publicity which must attach to fatal cases of operation under chloroform, and therefore only return such as actually die on the table. If the patient survives only half an hour, it is very easy to say he died from the effects of the operation; this obvious source of fallacy ought candidly to be taken [illegible] sideration, and I think will go some way in accounting for uniform success of certain Practitioners. To conclude illustration of this subject from the actual cases occ

the surgery of this war, the next instance to which I shall refer was a case of destructive injury to the bone and soft parts of the thigh. The left leg of a soldier was carried away by a round shot during the second bombardment; the knee-joint was smashed, and the femur fractured high up; the muscles and integuments of the opposite thigh were likewise lacerated. The patient was a remarkably fine, muscular young man, and in perfect health. The shock had been great, from the extensive injuries and loss of blood, and perfect reaction had been established. He was calm, collected, and anxious for the operation under the influence of chloroform, which was administered carefully, and in the usual manner, by Dr. Bleckly, of the 14th Regiment, and he was easily influenced. I then proceeded to amputate the thigh at its upper third, by the flap operation. About the average quantity of blood was lost, and it was necessary to apply the saw pretty close to the trochanter. The patient regained his senses for a short time, complained of præcordial oppression and abdominal pain, with great restlessness. He never rallied fairly, and died within an hour, never having been removed from the table; this is case No. 2. I was kindly assisted in this operation by Deputy Inspector-General Taylor, who compressed the femoral artery at the groin, and who, I think it fair to state, considers, that the chloroform enabled the patient to go through the operation. For his opinion, founded, as it is, on the experience of two previous campaigns, I entertain the highest respect; but it appears to me to be a fair illustration of the shock of a severe gun-shot injury, in which the depressing after-effects of the chloroform were not rallied from. As an instance of what may be done in such cases without chloroform, I may here state, that Dr. Gordon, Deputy Inspector-General of 2nd Division, successfully removed the thigh by amputation at its middle third, and the arm at the shoulder-joint on the same subject in succession; and I have the authority of Deputy Inspectors-General Taylor and Gordon (both officers of great practical experience in India,) for stating, that in the campaign on the Sutlej, amputations of the thigh proved more successful without chloroform, than they have done in this campaign under its influence. Dr. Taylor, however, is of opinion that this result may be due to other causes. With regard to the stimulating effects of chloroform upon patients about to sink from the exhaustion of disease, as above quoted, this is a totally different state from the shock occasioned by severe gun-shot injury, with or without loss of blood. But I can adduce an instance of secondary amputation of the thigh, in which the patient was much exhausted after twenty-four days' suffering from a wound implicating the knee-joint: in this instance, there was an unsuccessful attempt at conservative surgery, at the earnest solicitation of the patient, who nearly lost his life from the inhalation of half-a-drachm of chloroform administered by myself; the operation was ultimately performed without chloroform, and with but little suffering. As this case, Sergeant Bennett of the 30th Regiment, was seen by several Medical Officers here, I shall not enter into any further detail. Let me, however, here at once inform the Society, that it is not the intention of this paper to treat on the merits of chloroform generally; they are admitted; and to quote the words of a public commentator on this subject, "Chloroform is so priceless a boon to mankind, that we should all endeavour to ascertain with the utmost watchfulness the nature of the difficulties and dangers which beset its use;" it is only to such that I refer; and I will, therefore, repeat the real question at issue:—

1. Is the administration of chloroform, in the severe depression consequent on large gun-shot injuries, fraught with danger?

2. Are we justified, in a moral point of view, in giving a dangerous remedy for such trifling operations as the removal of a finger or toe, or the extraction of teeth, or bullets lying near the surface?

I think any candid, conscientious observer, who has had the misfortune to meet with such a case as that of Private Martin Humphrey, 62nd Regiment (to recur to the surgery of the war), who died suddenly while under the influence of chloroform for the amputation of a finger, will unhesitatingly a[illegible]wer. We are scarcely justified in trifling, or, in unimpor[illegible]es, in resorting to an anæsthetic that may so sud[illegible]ive a fellow-creature of life, while we possess so [illegible]eady, and so efficient a means of producing local as that described by Dr. James Arnott in the *Medical Times and Gazette* for July, August, and September, 1854. Whatever objections may be urged against this simple and humane means of producing anæsthesia, no fatal result can occur from its use. In all superficial operations, says Dr. Arnott, which constitute the immense majority, cold is superior to chloroform in the circumstances of safety, ease of application, and the saving of time and trouble, certainty of producing anæsthesia, and, lastly, of preventing subsequent inflammation. Anæsthesia will, no doubt, be henceforth a required element of every operation; but chloroform, fortunately, is not the only mode of producing it; and I trust there is a time coming when we may be able to restrict its use to such cases as cannot be affected by other means. The propriety of using an anæsthetic which occasionally destroys life in simple cases is questionable; while we possess one free from danger, easy of application, requiring no assistance, and what is invaluable in military practice, "saving of time." Anæsthesia from cold may be complete in a minute. There is yet another agent of great power and efficacy in a certain class of cases, which it has been the custom of the Profession to sneer at, but which Dr. Esdaile's reports and returns place beyond a doubt. I allude to mesmerism. It is not, however, applicable to military practice. One more case, in illustration of the occasional ill effects of chloroform in the shock following loss of blood. Stephen Newing, a private of the 97th Regiment, aged 21, was wounded on the 8th of September in the attack on the Redan, by a musket-ball through the left fore-arm, producing compound fracture of the ulna; he had likewise a flesh-wound of the right thigh, and a graze of the belly, both by musket-balls. He experienced a slight attack of diarrhœa on the 25th, up to which period he had been doing well: this was of no great severity. On the 30th, he had slight bilious vomiting, shortly after which it was discovered that hæmorrhage had taken place to a considerable extent from the ulnar artery, in all probability induced by the exertion of vomiting. He had lost about 2 pounds of blood before it was discovered. Upon a careful examination of the limb, it was discovered that about three inches of the ulna were dead, and the wrist-joint appeared to be involved. Amputation was therefore decided on, and was performed in about two hours after the discovery of the hæmorrhage; so that the man might, in a certain sense, be said to be under shock. Chloroform to the extent of one drachm only was administered, the operation was rapidly executed, and an unusually small quantity of blood lost. He did not, however, rally from the effects of the chloroform, vomited a fluid of the appearance of coffee grounds, and gradually sank. Artificial respiration, the cold douche, etc., failed to restore animation. Staff-Surgeon Matthews remarks, that he never at any time recovered from the effects of the chloroform insensibility; and, although the immediate cause of death was, doubtless, by loss of blood, or the direct action of chloroform on the brain, or asphyxia, still he is strongly inclined to believe that, had chloroform not been employed, he would not have lost the man. In such cases, the ebbing powers of life are just as likely to be suddenly arrested, as stimulated to fresh vitality; for chloroform does not prevent syncope; indeed, it is one of the modes in which it destroys life. I have not seen that supporting power attributed to it; and here I differ from Dr. Snow, who thinks accidents from chloroform are to be prevented by care in the administration, not in the selection of cases. In confirmation of Dr. Matthews' view of the case, a precisely similar one happened to me in 1849, in which I had to amputate the forearm for secondary hæmorrhage after gun-shot injury, implicating the wrist-joint. The patient, in this instance, Private Hugh Swift, of the 13th Light Dragoons, shot himself through the wrist, while in a fit of intoxication. An attempt was made to save the limb; secondary hæmorrhage suddenly took place on the eighth day after the receipt of the injury, at midnight; and, before it was discovered, he had lost a considerable quantity of blood. A tourniquet was immediately applied; but such was his state of exhaustion, that the propriety of amputation was questionable. He had, however, rallied sufficiently in about two hours, under powerful stimulants; and the amputation was performed without chloroform, and apparently without pain; but such was his condition, that he could not be removed from the table for some hours. He ultimately recovered. Chloroform, I think, under these circumstances would have killed him. In conclusion, I may state, that I was present at every operation under chloroform performed at the General Hospital in camp

during the siege, and several in the 3rd Division; that in many instances I administered the chloroform myself, generally on a piece of lint, the patient always being in a recumbent position, and in no instance was organic disease detected; and I most reluctantly come to the conclusion, 1. That there are states of shock, or depression from loss of blood, following extensive injury, such as the loss of a thigh high up, or the arm at the axilla, in which chloroform may destroy life in various ways. 2. There are likewise cases in which, as I have stated, the patient never fairly rallies, but sinks gradually without any effort at reaction; these cases are never returned as deaths from chloroform. 3. I cannot subscribe to the kind of argument sometimes used to justify its indiscriminate use, viz.:—that the invariable absence of pain to the patient and advantage to the Surgeon, fully counterbalance the risk of an occasional fatal termination. In trifling injuries life is too precious to be thus trifled with; it is opposed to all moral laws; nor can the opinions of hosts of authors, dead or living, make it right in such cases.

(To be continued.)

MR. MARSON'S PETITION TO THE HOUSE OF COMMONS ON THE VACCINATION BILL, 1856.

To the Honourable the Commons of the United Kingdom of Great Britain and Ireland, in Parliament assembled,

THE Petition of James Furness Marson respectfully showeth that:—

Your Petitioner is a duly qualified Medical man, and has been for upwards of twenty years the Resident-Surgeon of the Small-pox and Vaccination Hospital, London.

Your Petitioner has attended, in the time abovementioned, and has kept accurate notes of, nearly 9000 cases of small-pox.

Your Petitioner has also, in the same period, vaccinated upwards of 40,000 persons, and he is desirous of stating that he has never seen any evil results traceable to vaccination, with the exception of a single instance, in which measles occurred at the same time, and four or five examples of rather severely sore arms, arising from lymph recently taken from the cow. He has never seen other disease communicated with the vaccine disease, nor does he believe in the popular reports that they ever are so communicated. If such results were really true, as stated, and formed part of judiciously conducted vaccination, they must have come under the observation of your Petitioner in vaccinating upwards of 40,000 persons. Vaccination is performed generally at an early age; parents are unwilling to believe that there is anything constitutionally wrong in their offspring; and, when other diseases follow, vaccination gets blamed for what is really and truly due to other causes, as may be seen in those who have never been subjected to vaccination.

Your Petitioner has had, and has still, great cause to regret the suffering, disfigurement, and mortality produced by small-pox, nearly the whole of which might be, in his opinion, prevented by carefully conducted vaccination.

The mortality from small-pox in the unvaccinated, of cases taken generally, is 35 per cent.; but of children under 5 years of age, it is 50 per cent. And of those who recover, a great many suffer permanent disfiguration; some loss of sight, and others have their general health greatly damaged.

The mortality, on the contrary, among the vaccinated, attacked by small-pox, is 7 per cent., taken generally. But among what may be characterised as the badly-vaccinated it is 15 per cent. Among those, on the other hand, who may be considered to have been well-vaccinated,—that is to say, who have four or more good vaccine cicatrices,—the mortality is less than 1 per cent. Thus, the necessity of carefully conducted vaccination is rendered strikingly manifest by the mortality of 15 per cent. in the badly-vaccinated, and only ¾ of 1 per cent. in the well-vaccinated, when attacked by small-pox. In the badly-vaccinated who recover from small-pox there is a large amount of suffering, and permanent disfigurement, that might be prevented, and which should be looked upon as a very serious evil, more especially to the female sex.

Among children under 14 years of age who have been vaccinated, it hardly ever proves fatal.

It should, however, be remembered that no authorised system of vaccination has been established in England. All persons—Medical men, clergymen, amateurs, druggists, old women, midwives, etc.—are allowed to vaccinate in any way he or she may think proper, and the persons operated on are considered to have been vaccinated. The consequence of this carelessness and want of arrangement is, that there has been, and is, a great deal of very inefficient, almost useless, vaccination performed in England.

These facts have been ascertained by your petitioner from a minute examination and classification of the cases of small-pox that have come under his notice at the Small-Pox and Vaccination Hospital, London; which cases were individually carefully recorded in the Hospital register at the time of their occurrence, and the general result of which he has embodied and communicated to the Profession, in the XXXVIth Volume of the Transactions of the Royal Medical and Chirurgical Society of London.

As an example of what can be done by efficient vaccination, your Petitioner begs to state, that not one of the nurses or servants of the Small-Pox Hospital has had small-pox for the last twenty years. They have all been either vaccinated or re-vaccinated, on coming to live at the Hospital.

Nearly all the junior members of the educated classes in this country have, for many years past, been vaccinated, almost without exception. What the educated classes have adopted by choice, after mature deliberation, can hardly, by the most perverted reasoning, be considered improper for the lowest and uneducated class to conform to; it is, in fact, what the latter themselves would most likely also adopt, by choice, if they were educated, and a little higher in the scale of society.

As the law now stands, it can hardly be considered just, individually, to the lower classes themselves. They are not obliged to be vaccinated, but they are not allowed to be inoculated, lest they should propagate small-pox, as they undoubtedly would by inoculation; so that if they do not choose to adopt vaccination, they are left to take small-pox in the natural way; this they almost certainly do take, sooner or later, and small pox in the unvaccinated is one of the most fatal diseases in this country, destroying, as was before stated, 35 per cent. of those attacked by it.

Your Petitioner having had frequent opportunities of judging from the vaccine cicatrices on the arms of seamen from Denmark and Sweden, who have become patients of the Small-Pox Hospital, of the efficient way in which they had been vaccinated, and who, having taken small-pox, have had it, almost invariably, in the lightest form. This, your Petitioner believes, was entirely due to their having been so efficiently vaccinated, and, therefore, he ventures to recommend that similar arrangements to those in force in Denmark and Sweden, for conducting vaccination should be adopted in England.

The many foreigners admitted, in a series of years, as patients at the Small-pox Hospital, have enabled your petitioner to observe that vaccination, is, as a rule, much better performed abroad than in England, that in fact it is far less satisfactorily performed in England than in any other country in Europe, owing, most likely, to a total absence of organization of the subject in this country.

Your petitioner, therefore, fully convinced himself, from ample opportunities of judging for a period of 20 years, of the great good conferred on mankind by vaccination, when judiciously and carefully carried out, and knowing also, from ample experience in vaccination, of no objections to it, except unfounded prejudice, earnestly entreats your Honourable House to pass the Vaccination Bill now before Parliament, in the full conviction that it will be, if provision be made for its due administration, a most beneficial act of the Legislature, for those who, from their carelessness, prejudices, and ignorance, or from their early age are unable, on this subject, to take proper care of themselves. And your petitioner, as in duty bound, will ever pray. Signed, F. MARSON,

Member of the Royal College of Surgeons of England, and Licentiate of the Society of Apothecaries, London.

May 26, 1856.

MEMORIAL HOSPITAL AT CONSTANTINOPLE.—The Sultan has made a grant of land for the construction of a hospital near Constantinople, to be called the Hospital of Peace, to be open to all nations, and to be attended by Sisters of Charity.

variably been resorted to, agreeably to the decided recommendation of my friends Mr. Cline and Sir Astley Cooper. It seems, therefore, to me only a becoming diffidence to hesitate to adopt a practice when not corresponding with such great authority.

I beg to narrate a case which has recently occurred to me. A thatcher, 52 years old, residing at Shenfield an adjoining parish, fell with great force from his cart. I was with him in less than an hour. He was expressing a sense of great pain in his hip, the limb was shortened and immoveable, the toe resting on its fellow, and though a healthy, not being a muscular man, the head of the femur was clearly traceable upon the dorsum ilii, leaving no doubt as to the nature of the injury. Not having the necessary "armamenta" with me, I at once adopted the plan, first and so successfully recommended by Mr. Cock, reported in the *Medical Times and Gazette*, June 1855. The man lying on the bed, on his back, I placed his knee across my shoulder, flexed the limb upon the pelvis, bending it a little outwards, an assistant rotating it at the same moment; in less than a minute the reduction was effected with a loud snap. Some tumefaction remains, but otherwise my patient is quite as well as can be expected.

I am, &c. CORNELIUS BUTLER, F.R.C.S.

Brentwood, Essex, Aug. 30, 1856.

POISONING BY STRYCHNIA.

[To the Editor of the Medical Times and Gazette.]

SIR.—In your number of the 15th instant, there is a case related of poisoning by strychnia, in which, after having fully, and apparently carefully, detailed the various symptoms produced by that poison, the author continues, "The nature of the case being no longer doubtful, we were not, however, thereby better enabled to relieve the unfortunate sufferer; we knew of no antidote. . . . At length he yielded, and with some apparent difficulty he swallowed forty grains of ipecacuanha powder, which had been mixed with some warm water; but no vomiting ensued."

Never having had an opportunity of witnessing the effects produced by an overdose of strychnia, on the human subject, it is not my intention to offer any observation upon the phenomena caused by its administration, or to make any remarks on its modus operandi.

Without, however, entering into any controversy as to its primary action on the stomachic nerves, or its previous absorption into the circulation, and its subsequent action on the anterior or motor branches of the spinal chord, it may perhaps be worth inquiry whether, in a case of such moment, where the life of a fellow-being was at stake, it would not have been more judicious to have attempted to clear the poison from the stomach at once by means of a direct tonic emetic (as the sulphate of zinc or copper), which excites vomiting almost immediately after its exhibition; instead of giving ipecacuanha, which, not to speak of the great danger incurred by the unwonted activity taken on by the absorbent system, arising from the increased action of the heart and arteries, requires, together with the vegetable emetics in general, a much longer time for its operation.

It might, perhaps, also have been well to have given the tincture of iodine, which is said to be an antidote.

In all cases of emergency, and especially in cases of poisoning, the great importance and the high value of time should never be lost sight of. At the same time, we should bear in mind, how necessary it is to have a clear and thorough acquaintance with the action of medicinal agents, in order to reason correctly on their effects, and thereby prevent those errors in practice, which in many cases, perhaps by a due foreknowledge of their uses, might have been avoided, and which, in after life, cannot but tend to excite feelings of regret.

I am, &c., J. S. LOWDELL.

Park Road, Stockwell, August 27, 1856.

CHOLERA AT STOCKHOLM.—There had been no new case of cholera within the past week, and it is hoped that the epidemic has totally ceased.

SIR JOHN ROSS.—Many of our readers will hear with regret that this gallant Arctic Voyager died on Saturday last, aged 76.

REPORTS OF SOCIETIES.

CRIMEAN MEDICAL AND SURGICAL SOCIETY.

APRIL 19, 1856.

Sir JOHN HALL, M.D., K.C.B., in the Chair.

(Concluded from page 227.)

Dr. Taylor, C.B. considered that the paper which had just been read must disturb the confidence with which we had been in the habit of administering chloroform in all cases of operation. If he understood Dr. Mouat's paper, the author conceived that there were secondary as well as primary or immediate risks attending its use. The cases adduced did not seem to him to bear out that view. What had been regarded in these instances as secondary fatal effects of the anæsthetic agent appeared to him (Dr. Taylor) rather as the consequences of severe operations, superadded to extensive injuries. He had not heard of direct fatal effects from chloroform in any of the severer forms of injury of gunshot wounds; on the other hand, he could not say that in this class of cases, he was convinced of any supporting power in the use of chloroform, and this, he believed, was the question mooted by Mr. Guthrie. As there was a remote risk of fatal effects from chloroform, of the primary or immediate kind, Dr. Taylor did not think it advisable to administer chloroform for the minor operations, as the extraction of bullets, teeth, and the amputation of lacerated fingers. It was, too, just in this class of cases that fatal consequences had been more frequently found to occur. The caution not generally insisted on by writers on the administration of chloroform, was the necessity of the fully recumbent position. Dr. Snow did not appear to have noticed this point; but Mr. Syme particularly dwells upon the necessity of it in all cases, however trifling their natnre. It was curious that one fatal case in this Army occurred, in which chloroform had been administered to the patient in a sitting position, or, rather, merely reclining. Dr. Taylor thought that abundance of fresh air was a great essential; and he had observed during this war, that the chloroform took effect more benignantly in cases operated on in the open air.

Dr. Gordon, C.B., entirely concurred in the views expressed by the author, and seldom employed chloroform in severe cases. He thought that considerable risk was entailed. The case he remembered of an officer, which proved fatal in five hours, and another, in which the operation was necessarily stopped, chloroform was employed in the dose of two drachms. The Medical Officers of the Division generally agreed with him in the inadvisability of employing chloroform in serious cases. He produced some statistics, and considered that in many of his cases the men would have died if chloroform had been used.

Dr. Sall confessed himself a great advocate for the use of chloroform; and was well acquainted with its inventor. He had never seen a fatal case under its use in Edinburgh, where he had seen it employed for many severe operations; also at Glasgow. A statistical table would be of advantage. As to the use of chloroform during this war, much, of course, depended on its purity; and the manner of using it is most important. He would have no hesitation in using it in any case, even if organic disease should exist.

Mr. Wyatt considered that chloroform was a powerful agent for good or evil, according as its use was confined to particular cases adapted to its use; but how could these be ascertained beforehand? He must premise his observations by saying that he believed there were certain general conditions of system when chloroform was a very doubtful auxiliary to Surgical practice. He more particularly alluded to those severe cases of injury requiring prompt and immediate amputation, after an action in the field, when it was most important that a speedy reaction should be fully and promptly established. From what he had seen, he must confess that the result of his experience during this war led him to hesitate before admitting himself an advocate for the indiscriminate use of chloroform, and feeling conscientiously that there was a certain amount of risk attending its use, he never took upon himself the initiative

of recommending it to a patient. He had witnessed, both in civil and military practice, some unfortunate results, which must undoubtedly be attributed to its use, and even where it had been administered by skilful people; but, of course, unless positive causes for the prohibition of its use exist, it is the duty of the Surgeon to consider the patient's importunities to be released from pain, but, until convinced to the contrary, he should still hold the opinion that it was not a remedy to be used indiscriminately in the severer injuries on the field. The remarks of Dr. Taylor respecting position, and the necessity of a sufficiency of atmospheric air, were most true and practical. Considering that deaths from pyæmia during this war had been more than usually frequent after amputations, he would throw out the suggestion, whether the frequent—he would almost say, the indiscriminate—use of chloroform could in any way be connected with the results stated.

Dr. M'Leod would be sorry if the statements which had been made on the use of anæsthesia in Military Surgery should go forth to the world; as he believed them to be neither correct, nor in unison with the opinions of the majority of Army Surgeons. He did not think there was anything peculiar in gunshot wounds, which made the exhibition of chloroform in them not as admissible as after the severe machinery and railway accidents of civil life, where its use was universally allowed to be so beneficial. Dr. Mouat had fallen into an error in saying, that Dr. Simpson's experience and the statistics he furnishes refer solely to obstetric medicine. Dr. Simpson published a paper in answer to the question, whether anæsthesia increases or diminishes the mortality of surgical operations, basing his deductions on the results of amputations of the thigh, leg, and arm, in thirty of the chief British Hospitals, and he has shown, that the lives of 6 persons in every 100 submitted to these operations have been directly saved by the use of anæsthesia. In the case of thigh amputations alone, he also shows that a large diminution of the mortality has resulted. Dr. M'Leod did not think that death was justly due to the chloroform in the few cases recorded by Dr. Mouat, who had spoken of an immediate and remote danger as following its use. The immediate danger he considered, in almost every case, arose from the real administration of it; and as to the remote, he could not see the logic of the deduction, which ascribed death 24 or 36 hours after the performance of a severe operation under chloroform to the use of that drug. We saw sometimes such fatal results without chloroform, which, in his opinion, would often enable an operation necessary to the prolongation of life to be performed, which otherwise could not be attempted. He considered that the respiration was the great guide to the administration of chloroform, and he showed the propriety of observing how death threatened, in order to ward off the danger of an over-dose. As to its being a cause of pyæmia, the idea was not only new but remarkable.

Dr. Williams thought they had wandered from the subject in considering the general use of chloroform. He had never seen a case where any bad results had arisen; and he must confess, that he had never heard any paper read with greater regret, giving, as he must suppose, the opinion of so conscientious and experienced a member as Dr. Mouat; but until he saw its effects, as explained by the author, he must reserve any opinion on the subject of the present discussion.

Dr. Burke, 3rd Buffs, had alluded to this subject formerly, and could speak in a most satisfactory manner of chloroform. One case only of amputation at the shoulder-joint proved fatal after its use, but the severe nature of the operation caused this patient's death. He certainly would not recommend the use of chloroform in minor operations. He had served in the Sutlej campaign, where the operations were disheartening, that was, before chloroform was known; and he thinks that the operations would have been more successful under the use of chloroform. In an excision requiring prolonged time for its completion, he considers it most useful, and in none more than that of the elbow.

Dr. Bone, 17th Regiment, saw the three first cases in Edinburgh when chloroform was first used. He considered that it was always most important to examine the chest previously, to ascertain if any organic disease existed. He had kept a woman under its influence for nine hours, and felt convinced that many lives after the reception of gunshot wounds were saved entirely by the use of chloroform. He mentioned a case of severe gunshot wound of the thigh, where it was most successfully employed. A case of tetanus under his care, has been kept for three weeks under the use of chloroform occasionally for nine hours at a time, and the man recovered.

Dr. Gordon alluded to the Sutlej campaigns, where the operations were generally very successful. Out of twelve operations performed in his regiment, one only was fatal. In the 24th Regiment, during the Punjaub campaign, the general success of all the operations was very great. The returns of operations performed in the 2nd Division showed that fifty-three had taken place under chloroform, and forty-three without its employment, for the quarter ending September 30.

Dr. Crawford would beg to return to the real objects of the discussion, and to say, that it not being the custom of military Surgeons to operate during shock, the objections to the use of chloroform at that period fell to the ground.

Dr. Jessop alluded to a letter of Dr. Kidd in the *Medical Times and Gazette* of April 5, showing the safety of chloroform in gunshot wounds. Dr. Robinson had performed twelve capital operations under the influence of chloroform, with only one immediately fatal result; and he had very frequently used it himself in minor operations.

Dr. Crawford thought that the heading of Dr. Mouat's paper should be changed, or it would lead the Medical public to suppose that they operated under shock.

Dr. Thornton has used chloroform many times, and seen it employed many times, without any fatal result. He did not think that pyæmia is ever induced; but that the special fatal termination alluded to by Mr. Wyatt depended more on the general scorbutic constitution of the men.

Dr. Mouat begged to reply. He believed that it was the custom of all debating societies to adhere strictly to the question at issue. In the present instance, just what he had anticipated had occurred. The real question had been lost sight of, and had merged into that of the administration of chloroform generally. Some members of the Society had quite misapprehended his meaning, in supposing he could be guilty of departing from such an obvious acknowledged maxim as non-interference during the immediate shock of a severe wound. He was much obliged to Dr. M'Leod and others for correcting any errors he might have fallen into, in the absence of books and authorities. The inference to be drawn from Dr. M'Leod's lecture was, that we did not know how to administer chloroform, and he must, therefore, again refer him to the paper, which distinctly stated that it was administered in the usual way—in the recumbent posture, and with all the necessary precautions enjoined. The subject of the primary and secondary fatal cases had been alluded to, and he thought that the first part of the question could not be disputed: patients did sometimes die under the influence of chloroform; he had adduced three or four cases which could not be contradicted; he had likewise introduced what he considered a somewhat new view of the subject, which had not been fairly replied to; he alluded to those cases in which patients "never fairly rallied;" in which there was a continuous state of nausea, and the desire for food never returned, the patient sinking gradually, without any perfect reaction having been established. He considered that many such cases might fairly be termed "deaths from chloroform," although it might be difficult to separate and distinguish such cases. From what he had seen during the war, he felt no doubt of their existence. The real difference of opinion between himself and those gentlemen who had advocated opposite views was not so great as at first sight appeared. He was an advocate for the administration of chloroform, but not for its indiscriminate use. He was much obliged to Dr. Burke for kindly furnishing him with another case of death from chloroform, in support of his views; and was glad to find that he agreed with him in the propriety of not administering it in trivial accidents and injuries. The Society likewise appeared to be with him on this point; indeed, plainly speaking and independent of other considerations, it was impossible to administer chloroform in every case. After a general action, for instance, the loss of time—in each case unavoidable—would quite upset the notion that it could be always employed. Every one who had witnessed the numerous operations at the General Hospital, must have been struck with the length of time it was often necessary to keep patients on the table, to enable them to rally from the chloroform insensibility. He concluded by stating that it was impossible to follow every gentleman who had spoken on this subject, many had so entirely wandered away from it; but if he had omitted to answer any

material question, on being reminded of it, he should be perfectly ready to do so.

Dr. Jessop thought that the amputation of a finger was a painful process, and required the administration of chloroform equally with amputation of the leg, and should by no means be considered one of the minor operations.

Dr. Hume proposed that the meeting should be adjourned, when other members being present, the subject might be again discussed.

Dr. Rogers proposed, and Dr. Williams seconded, a vote of thanks to the author.

APOTHECARIES' HALL OF IRELAND.

THE following Circular has been addressed to their Licentiates by the above Corporation :—

Apothecaries' Hall, Dublin, August 29th, 1856.

Dear Sir,—A demand having been made by the Government for an authentic list of the qualified Apothecaries in Ireland, for the purpose of enforcing the law against all persons who are found acting in that capacity without "the Licence" of the Hall, the Governor and Council transmit herewith a copy of their last published List, and request that you will be so good as to assist in its correction, by returning to their Secretary the names, residences, titles, and appointments, *in full*, of all the persons who are practising as Apothecaries in your district, in order that your return may be compared with the Registry, and duly certified for publication.

The Governor and Council desire to avail themselves of the present occasion earnestly to impress on their Licentiates the important duty of making use of every precaution in keeping and vending Poisonous Agents, so that all mistakes and casualties from these sources may be guarded against; and they therefore recommend, that all agents of this description be kept distinct and apart from other medicines, each having, in addition to its special label, the word "poison" or "*cave*" affixed to it; and also, that no one of them be sold to the public, except as ingredients in Medical prescriptions, without a knowledge of the purchaser, and without obtaining his or her signature and address in a book to be kept for that purpose.

The Governor and Council wish, at the same time, to direct the attention of their members to the subject of the adulteration of medicines, as revealed in the late Parliamentary inquiry, and to urge upon them the necessity of submitting to careful analysis such commercial articles as are liable to be sophisticated; so that the purity and efficiency of all drugs and preparations used in dispensing be secured; and with the request, that the particulars of this communication be made known to all Apothecaries in your neighbourhood.

I remain, dear Sir, your very obedient servant,
CHARLES HENRY LEET, Secretary.

MEDICAL NEWS.

APOTHECARIES' HALL.—Names of gentlemen who passed their examination in the science and practice of Medicine, and received certificates to practise, on Thursday, August 28, 1856 :—

DAVIES, J. W., Blackwood, near Newport, Montgomerys.
HEY, WILLIAM, Borrowash, near Derby.
OLIVEY, HUGH PENGILLY, Myton, near Falmouth.
MEADOWS, ALFRED, Ipswich.
RICHARDS, DAVID, Llandovery, S.W.

DEATHS.

SPROAT.—Aug. 28, at Rockville, near Kirkcudbright, N.B., Dr. John Sproat, for many years in practice in Melbourne, Australia.

TOMLINSON.—Aug. 27, at Foley-place, Regent-street, James Francis Tomlinson, Esq., Surgeon, Maldon, Essex, aged 62 years.

M. DE HUMBOLDT, it is expected, will preside at the Congress of Naturalists and Physicians which is to meet at Vienna from the 16th to the 23rd of this month. Upwards of 4000 names are already given in, as signifying attendance.

DEATH OF MR. YARRELL.—Mr. Edward Jesse, noticing this melancholy event, which occurred suddenly at Great Yarmouth, on the 1st instant, writes :—"Mr. Yarrell, as is well known, was one of our best naturalists. This was shown in his beautiful works on British birds and British fishes, and in several valuable and interesting papers in the Transactions of the Linnæan and Zoological Societies." Mr. Yarrell, as an ichthyologist, in conjunction with Mr. Jesse, solved the problem which had perplexed naturalists from the days of Pliny, as to the history of the eel. He clearly proved that they were oviparous, had scales, and bred for the most part in the brackish water at the mouths of the rivers. Mr. Yarrell was in his 72nd year.

THE honorary degree of M.D. has just been conferred by the University of New York, U.S., upon Dr. Stanhope Templeman Speer, of Cheltenham.

SMALL-POX IN SCOTLAND.—This disease is on the increase in Aberdeen, Edinburgh, and Paisley, having caused 10 per cent. of the deaths in Aberdeen, 5½ per cent. in Edinburgh, and 5 per cent. in Paisley.

INFANTILE MORTALITY.—Mr. Wilson, the Registrar of the Sub-District of St. John, Marylebone, writes to the Registrar-General as follows :—"Four of the cases in my present return are deaths of infants not more than four months old, from want of breastmilk, marasmus, and diarrhœa; and I have frequently met with cases of a similar nature in the course of my duty as Registrar. They relate to children who have been put out to nurse at a very early age, and have died shortly afterwards. In all the instances of this week and most previous ones the parents are represented to be domestic servants; in one case, though married, they lived with different families, and passed for unmarried. Sometimes the child is put away that the mother may go as wet nurse, at others that she may return to service. There are in most neighbourhoods (particularly suburban ones) poor people who make it an occupation to take charge of such children, their object of course being to derive a profit from the small weekly allowance given with them. These payments, as may be supposed, are often made irregularly; the infant, in some cases which I have known, is entirely abandoned, and, after much suffering, is taken to the workhouse; in others being deprived of natural nourishment, and ill supplied with any substitute, it becomes the victim of some infantile disease which is assigned in the Medical Certificate as the *primary*, when in reality it is the *secondary*, cause of death. I believe that this fatal termination in some cases which I have noticed was the result anticipated, and even desired, by the parent; but in others I am certain it could be avoided, if some means existed by which such children could be provided for, with proper care and at a moderate expense. My opinion on this matter is shared by the other Registrars of this District; and a case which has appeared in the newspapers for this week, and which I beg to enclose, convinces me that fatal results from such causes are not uncommon elsewhere."

MORTALITY IN SCOTLAND.—The following is the Monthly Return of Births and Deaths in eight principal towns of Scotland for July :—During the month of July there were registered in the eight principal towns of Scotland 2658 births, of which 1371 were males, and 1287 females; 1706 deaths, of which 838 were males, and 868 females. Allowing for increase of population, this would give at the rate of one birth annually in every 27, and one death in every 41. Of the 1706 deaths, 806, or 47 per cent., were under five years of age. The proportion of deaths under five years of age in the different towns was, in Perth, 27 per cent.; in Aberdeen, 34; in Edinburgh, Paisley, and Greenock, each 42; in Leith, 50; in Glasgow, 51, and in Dundee, 58. The deaths from the zymotic class of diseases amounted, in the eight towns, to 478; thus constituting 28 per cent. of the total mortality. The proportion of deaths from that class of diseases in the eight towns was, in Perth, only 13 per cent. of the total mortality; in Glasgow, 24 per cent; in Aberdeen and Paisley, each 25; in Edinburgh, 28; in Dundee, 32; in Greenock, 34; and in Leith, 44 per cent.

MORTALITY NOTABILIA.—The present return shows a continued improvement in the public health. In the three previous weeks the deaths registered in London were 1232, 1250, and 1122; last week they were 1081. The present rate of mortality is very near the average rate of corresponding weeks in ten previous years. The deaths caused by diarrhœa,