

inadequate in the method of conducting the examination. There could be nothing more disastrous to the future prosperity of the University than that the University as a whole—Senate, Court, and General Council—should condone a standard of examination declared to be wholly insufficient by the Medical Council and by the medical press. Dr. Glaister's resolution expressly concurred with the opinion and action of the Medical Council, while Dr. Duncan's amendment was silent on this matter.

Again, it was in my opinion essential, both to the welfare of the University and to the public interest, that the condemned diplomas should be entirely cancelled and erased from the Medical Register. This would have been effected by the procedure proposed by Dr. Glaister, but was held to be unnecessary by Dr. Duncan. The amendment states that the Medical Council "has already dealt with this matter as it considered best in the public interest," but Dr. Duncan knew perfectly well that while the Council stayed further registration of such of the diplomas as had not then been registered, it was expressly stated that it could not delete from the Register any of the twenty-nine diplomas that were already on the Register. So far as is known this could only be effected by an Act of Parliament. However perfect future examinations may be, the new diplomates will be in an invidious position so long as the diplomas of last year are in circulation. I considered it, therefore, best to support the resolution which would clear the University from all trace of the disastrous proceedings of last year. Any Glasgow graduate who reads this letter will, of course, form his own opinion as to which of the courses proposed would have been best alike for the University and the public.

Nov. 5th, 1890.

I am, Sirs, yours truly,

D. C. M'VAIL.

## DEATHS FROM ANÆSTHETICS.

*To the Editors of THE LANCET.*

SIRS,—It would seem from recent reports that fatal accidents from anæsthetics are not becoming less frequent. It is a serious matter if there is a source of danger from them which has never been recognised, for if so fatalities must and will continue to occur. I have long held that there is such a danger, and have lately laid my views before the profession in some detail in a pamphlet entitled "A New Theory of Chloroform Syncope, showing how the Anæsthetic ought to be Administered," and still more recently in a paper read before the Medico-Chirurgical Society of Glasgow. According to the new view, the vapour of chloroform exercises an important action on the pulmonary circulation or on some part of the respiratory tract, and this is attended by reflex influence on the vagus through the medulla, with inhibition of the heart. Now, when any agent directly stimulating the vagus is suddenly removed, great acceleration of cardiac activity is liable to occur, and the danger of syncope in such circumstances is well known. It must be evident that the same will apply to any agent operating through reflex mechanism on the vagus and the heart. Now, observations have shown that when the inhalation of an anæsthetic is left off at an early stage the vapour leaves the lungs in about ten seconds. Experiment has demonstrated that when a cat is made to breathe about 3 per cent. of chloroform vapour for not more than a minute, and is then allowed to breathe fresh air, the pulse, which is at first from 70 to 80, will in ten seconds suddenly rise to 200, and this is liable to be followed after another minute by a lengthened pause in the heart's action, no beat being audible for sixty seconds—i.e., during the third minute from the commencement of the experiment. The theory is mainly founded on this experiment, and it could be shown to explain various facts which have been observed by some recent experimenters. This pause in the action of the heart of the cat is held to be the analogue of the primary syncope in the human subject, although the former seems always to recover from it. If so, it follows that acceleration of the pulse must precede the syncope, and this has actually been observed in several instances in which the pulse has been described as rapid and running. Hence administrators ought to keep up an atmosphere of not less than 2½ per cent. of chloroform vapour without a break until deep anæsthesia is induced, and they must make sure of the means by which this is to be done. If they will not do so, then they ought to watch for any sudden acceleration

of the pulse as the first indication of danger, and for this purpose an observer might apply a long double stethoscope directly to the heart.—I am, Sirs, yours truly,  
Partick, Glasgow, Oct. 28th, 1890. ROBERT KIRK, M.D.

*To the Editors of THE LANCET.*

SIRS,—When reading the account of a death from methylene I was struck by what appears to me to be the excessive amount of the anæsthetic which had been used—viz., from three to four drachms in four or five minutes. During the last four years I have administered methylene to nearly 200 patients over the age of fifteen either in the Warminster Cottage Hospital or in private, and have seen it administered by others many times, and it has always seemed to me to be the pleasantest anæsthetic both for the patient and administrator. The inhaler I use is Snow's, as modified by the late Mr. Coates of Salisbury; in this instrument the methylene is dropped through the perforated top of a metal globe on to some blotting-paper wrapped in a coil inside, the inspired air passes over this and through a tube into the face-piece, and after being expired passes out through a mica valve placed in the top of the latter; there is another mica valve at the junction of the tube and face-piece, to prevent the expired air returning through the globe. By this means the air is never rebreathed. I now come to Mr. Coates' method of administration. Having adjusted the face-piece, allow the patient to breathe the air alone several times to allay nervousness, and then drop in five minims of methylene; after thirty seconds drop in ten minims, and at the end of the first minute drop in another ten, and after this administer fifteen minims at the end of every minute until the patient is perfectly anæsthetised. I have never seen the patient struggle or seem frightened when administered in this way at the commencement. After the patient is totally under, ten minims can be given from time to time so as to keep the pupil contracted and the conjunctival reflex just abolished. I have generally found that if the contracted pupils are watched, and when seen to dilate five minims are added immediately, the total quantity of anæsthetic used in a prolonged operation is very small. I regret I have not kept carefully the exact quantities used in the several cases, but about six drachms are enough for an excision of the breast and clearing the axilla of glands. The time taken in fully anæsthetising a patient varies from five to seven minutes, using from 85 to 115 minims during that time. When first I gave it I had someone to measure it out and pour it into the globe, but this can be overcome by dropping in so many drops, having previously ascertained the number of drops which from the bottle used correspond to a measured five minims. If anyone could suggest a dropping bottle by which one could let out a known quantity at a time and be able to vary the amount at pleasure, I think it would be a great boon.

From my limited experience of its administration I should say that the advantages of methylene are the following:—  
1. The quiet way in which the patients go off, especially in the case of alcoholic subjects, as compared with ether.  
2. The small amount required if administered as above.  
3. Sickness after coming out of methylene is not so troublesome. If it does come on, it is not until from eight to twelve hours after the operation.  
4. The total absence of any dangerous symptoms during its administration.  
5. The rapidity with which its effects pass off. This is doubly advantageous: first, if by any chance there were any dangerous symptoms produced; and, secondly, the administrator must keep his attention riveted on the patient, or else he will come round before he expects it.  
6. I know of nothing to contraindicate its administration.

I am, Sirs, yours faithfully,

Warminster, Nov. 3rd, 1890.

J. W. JOLLYE.

## ABDOMINAL SURGERY.

*To the Editors of THE LANCET.*

SIRS,—It is very pleasant to see that as time goes on those of us who differed so very greatly ten years ago come into closer and closer approximation in our ideas upon the majority of points. Probably ten years ago Mr. Thornton's address would have given rise to angry correspondence; now it gives rise only to the desire on my part to make slight corrections upon matters in which I think Mr.

Thornton does not take a fair view of the facts, and I think I may limit them to two. First of all, he says that the careful examination of my published tables leads to a conclusion that my results have improved with the general improvement which took place in all abdominal work about fifteen years ago—that is, when antiseptics in some form or other came into common use, and when short ligatures and the drainage-tube became popular. I submit that the facts of the case do not warrant such a conclusion at all. They are as follows: In my first ten cases of ovariectomy I treated the pedicle with the écraseur, and nine of these patients recovered. I was over-persuaded—a result which I have ever since deeply regretted—to take to the clamp, and this I continued to use till 1878. I did not adopt any of the so-called Listerian precautions till 1880, and during a period of three years I employed them diligently, and towards the end with some variations. In my book on “Diseases of the Ovaries” (1886) I have fully described the results of the experiments, and the following table speaks for itself:—

	Per cent. mortality.
Ligature, non-antiseptic (187 cases) .....	3·74
Ligature, antiseptic (52 cases) .....	3·84
Clamps, non-antiseptic (36 cases) .....	25·00
Clamps, antiseptic (26 cases) .....	27·00

Since then my mortality has been steadily diminishing, till now I am able to publish the conclusion of a series of such operations as removal of the appendages for uterine myoma to the extent of 327 consecutive cases, with a mortality of 1·8 per cent., and without the use of useless ceremonies. I submit that these facts do not in the least degree bear out Mr. Thornton's conclusions. In fact, this rather vexed question needs no further discussion, and Mr. Thornton stands alone in his opinion. I am content to accept the decision of the discussion raised recently by Mr. Meredith upon this question, in which it was laid down as a thesis that “the former mortality of ovariectomy was chiefly due to septicæmia, commonly originating in connexion with the use of the clamp, and that the improved results are consequent upon the general adoption of the intra-peritoneal ligature.” When this thesis was discussed there was no kind of dissension from it.

The second point is that in which he applies my name to an operation, and speaks of “Tait's operation” in spite of the continued protests on my part against such a proceeding. It is, in the first place, entirely wrong, and the habit of christening operations by people's names is one open to the objection that it leads to confusion and misunderstanding, as it has done in the present instance. It may be that Mr. Thornton in this particular desires to pay me a compliment; but I assure your readers, and I assure the author of the address, that it is a compliment which I do not in the least degree appreciate.

I am, Sirs, yours truly,  
LAWSON TAIT.

Nov. 1st, 1890.

To the Editors of THE LANCET.

SIRS,—I cannot allow Mr. Thornton's statement that he believes the electrical treatment of uterine fibro-myomata to be an utter and complete failure to pass without notice, as I have had to cure, by means of electricity, one case where Mr. Thornton had removed the ovaries. The operation was performed in January, 1887. In March there was some bleeding; in June a flooding was checked by medicine prescribed by Mr. Thornton's assistant; the patient flooded badly during the whole of August, and from that time until the electrical treatment was begun in the end of October there were several lesser hæmorrhages. She was extremely anæmic and breathless, and had had to give up her situation. Since the treatment she has been in constant employment. Mr. Thornton will doubtless say that time was not given for a cure; but the woman had to work, and could not afford to wait for a menopause, and to be worse at the end of nine months than at the beginning was not very encouraging. She has still hæmorrhagic discharges from the uterus, or at least had, when heard of, not long ago; and I therefore think I am justified in saying that Mr. Thornton's surgery has failed to do what he expected—viz., to stop hæmorrhage; while my treatment reduced the amount of bleeding, cured the woman of her symptoms, and gave her back to health, exactly as I said it would do. It is not worth my while to discuss the

arrangement of figures and percentages; but if it be right for Mr. Thornton to compare his most recent results, for an indefinite time, with those of others operated on at different times and in different numbers, surely it would be also right for me to put my mortality of 3 per cent. in my first hundred cases of ovariectomy, alongside the much greater death-rate of Mr. Thornton's first hundred. If Mr. Thornton will look at the matter in this light, I think he will not be so anxious to prove on paper that his results are better than those of anyone else. I suppose that two deaths in 106 means three in 107. Why Mr. Thornton should believe that my father uses clean water and frequent drainage I do not know; perhaps he will explain. Is it not rather unfair that my father's cases of hysterectomy, thirty-eight in number, with three deaths,—results which Mr. Thornton appears not even yet to have attained to,—should be mixed up with cases of removal of the spleen &c., and the whole described as forming a ghastly record? I am, Sirs, your obedient servant,  
Charles-street, W., Nov. 3rd, 1890. SKENE KEITH.

THE COMPARATIVE DEATH-RATE OF TOTAL ABSTAINERS AND MODERATE DRINKERS.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of Oct. 11th you have an article upon Dr. Drysdale's paper on the Comparative Death-rate of Total Abstainers and Moderate Drinkers. I think he is right in bringing before the public the subject, as the paper, said to have emanated from a committee of medical men appointed by the British Medical Association, has been quoted extensively by the daily press. It has come under my personal observation that an impression has been made upon some minds that total abstinence is not favourable to either good health or longevity. I believe that Dr. Owen has done his best to correct the idea that his statistics are sufficient to prove that the total abstainers are worse lives than moderate drinkers. As you say, it cannot be wrong to publish facts, which are striking, and show, not only that total abstinence is consistent with good and vigorous health, but that total abstainers are longer lived than even moderate drinkers. For a long time considerable doubt was thrown upon this idea, and we were told to wait until trustworthy statistics could be produced. I think that there is now an accumulation of facts demonstrating this. To those already referred to in THE LANCET I beg to add some others drawn from the experience of the Sceptre Life Association, which has been in existence about twenty-five years. For the correctness of these statements I can vouch. In this association, from its commencement, the temperance and general sections have been kept distinct. In the general section the greatest care is observed in selecting lives known to be of temperate and sober habits; persons engaged in the liquor traffic are excluded. I will give the experience of the five years ending December, 1888. I may remark that it is in agreement with previous years. Mr. Manlay, F.I.A., in his report, states: “I have compared the actual claims in each section during the past five years with the claims that might have been expected according to the Institute of Actuaries' H. M. Table.” The result is as follows, and will no doubt prove interesting:—

	Expected.	Actual.	Percentage
General section ... ..	569	434	76·27
Temperance section ... ..	249	143	57·42

This experience is amongst 11,227 lives: in the general section 6700, temperance section 4527. In your article you refer to an opinion expressed that total abstainers are not greater eaters than moderate drinkers. I am inclined to think they are. This conclusion is not only the result of my own observation; it is supported by the late Baron Liebig, who expressed his views strongly upon this point. In the examination of many thousands of proposes for life assurance with one fact I have been forcibly impressed, that the total abstainers, as a rule, exhibit a much cleaner condition of tongue, more like that of a young child, as compared with the tongue of a moderate drinker. So much have I been struck with this evidence that now from the condition of the tongue I can generally guess whether the person under examination be a total abstainer or otherwise.

I am, Sirs, yours truly,  
Willesden, N.W., Oct. 21st, 1890. ROBT. BENTHAM, M.D.