

the dignified class to which he has been elected by refusing to do as much.

Meantime let me point out that Sir Spencer Wells has not made the slightest attempt to justify the conclusions he indicates that this was a case of neurotic ailment, and not one of incurable pyo-salpinx. Even if his own estimate were correct, if the case were a mere neurotic, why did he amputate the cervix in 1879? If the poor woman was suffering then from suppuration of the Fallopian tubes, and I think the evidence in my possession will prove that she was, a more unjustifiable operation, a more "deplorable and disastrous failure," a more "discreditable" result (to use Sir Spencer Wells' own words) cannot well be imagined.

In any case, Sir Spencer Wells, and he alone, was answerable for the operation which I performed in 1888, and this is proved by a series of telegrams which I have carefully preserved and which Sir Spencer Wells has apparently forgotten. To refresh his memory I publish the last of them: "Mrs. M. is very ill, and in my opinion the case is urgent. Wells has been communicated with and declines to advise. We have decided to operate this afternoon unless you come, in which case it shall be deferred till your arrival." If an operation in this case was unnecessary in Sir Spencer Wells' opinion, why did he not intervene to stop it, as he could have done, and as, if his statements are accurate, he ought to have done. I am quite prepared to submit evidence which will show conclusively that if any discreditable blunder has been committed the responsibility of the same is to be awarded to Sir Spencer Wells. At the same time I am satisfied that the right thing was done and that time will amply justify this conclusion.

I am, Sirs, yours truly,

Birmingham, Jan. 31st, 1891.

LAWSON TAIT.

### "FUCHSINE BODIES" IN TUBERCULOSIS OF LUNG.

*To the Editors of THE LANCET.*

SIRS,—Whatever may be the significance of Dr. Russell's fuchsine bodies in cancer, there can be no doubt that similar bodies occur in genuine tuberculosis of lung. During the Christmas vacation I was trying a method of staining sections of phthisical lung tissue which had been embedded in paraffin by a process which I need not here detail. Suffice it to say that I used fuchsine, to which a little carbolic acid had been added, as the primary stain, and methyl blue as the second, or contrast stain, when, to my surprise, I found in some of the cells which had been detached from the walls of the alveoli a red spot (more rarely two or three such spots) which occupied the centre of a clear transparent area on one side, but outside of this clear hyaline-looking area was seen the nucleus of the cell stained of a faint blue colour. I compared these spots with some of Dr. Russell's figures,<sup>1</sup> and could not detect any difference except that the fuchsine bodies in any section were not so deeply stained. I thereupon cut a few more sections from the paraffin block ( $\frac{1}{100}$  mm. in thickness), and, having fixed each section to a cover glass with Schällibaum's fixative, I heated and dissolved away the paraffin in toluol (turpentine, benzol, or xylol will do quite as well) in the usual manner. I then proceeded to stain with Neelsen's fuchsine—which only differs from Dr. Russell's fuchsine in containing more carbolic acid (5 per cent. plus a little alcohol)—just as one does with a dried film of sputum on a cover glass. Of course I did not pass the lung tissue through the flame, nor did I at any stage use any acid decolourising agent, but simply, after the removal of the paraffin with toluol, place the cover glasses first into absolute alcohol for a few seconds and then into hot Neelsen for about ten minutes; after this I washed the cover glasses in water, placed them again in absolute alcohol from one-half to three-quarters of a minute, and then into a solution of iodine green, to which I added a little carbolic acid, according to Dr. Russell's directions, with this exception, that I did not actually weigh out the iodine green and carbolic acid after staining with the latter mixture for a little over five minutes. I proceeded very much in the same way that Dr. Russell has recommended, and mounted in xylol balsam. On examining some of my sections I found the following appearances:—

1. Most of the nuclei of the cells were of a dark colour, looking more blue than green in ordinary gas light.
2. In addition, a few of the cells contained bright-red fuchsine

bodies, as a rule a fuchsine body to a cell, the fuchsine body being surrounded by a clear transparent area, and what appeared to be the nucleus of the cell was pushed away to one side of it. 3. Portions of the walls of bloodvessels presented here and there a similar red colouration. 4. And last, but not least, there were seen large numbers of tubercle bacilli, diagnosed easily enough by their slender forms and characteristic arrangement.

I am, Sirs, yours faithfully,

GEO CARRINGTON PURVIS, M D., B.Sc.

Edinburgh, Jan. 22nd, 1891.

### DEATH DURING CHLOROFORM ANÆSTHESIA.

*To the Editors of THE LANCET.*

SIRS,—As deaths from anaesthetics are always of some interest, the following case may be worthy of record. On Dec. 7th, after consultation with Dr. Berry and Mr. Watson of Queenstown, Dr. Murray made an exploratory incision over the right hypochondrium of a lad aged seventeen years. Chloroform was administered by Mr. Watson on a small wire and lint mask, and was measured by drop bottle. The total quantity used from first to last was six drachms. Soon after the administration was begun there was a marked diminution in the number of pulse beats. Gradually, however, the pulse became stronger, and his breathing regular. In about twenty minutes the patient's face suddenly became pallid, the pulse stopped at the wrist, and the breathing became irregular and gasping. Respiration continued spasmodically for fully a minute, and then ceased entirely. Artificial respiration by Silvester's method was immediately begun, and continued for over half an hour. Ether hypodermically, inhalation of nitrite of amyl, and electricity were tried, but without success. Death seemed to have been instantaneous from reflex paralysis of the heart. His heart had been auscultated previously by several medical men, and by the anaesthetist prior to the administration, and nothing abnormal had been detected. For six months the lad had suffered from attacks of colic of uncertain origin, for which morphia had been used hypodermically and in increasing doses. No necropsy was allowed.

We are, Sirs, yours faithfully,

JOHN K. MURRAY.

WM. B BERRY.

Whittlesea, Cape Colony, Jan. 8th, 1891.

L. WATSON.

### THE ROYAL COMMISSION ON VACCINATION.

*To the Editors of THE LANCET.*

SIRS,—In your zeal for vaccination you have allowed yourselves to state some curious things, when speaking of my evidence before the Royal Commission, in your issue of Jan. 24th. For instance, for the history of the last century, statistically, as regards small-pox, I used Dr. Guy's paper. And you must know that he was not an authority to treat as "untrustworthy." Yet you say, so I read it, that these statistics of Dr. Guy are "untrustworthy." Your words are: "He [Mr. Wheeler] occupied several hours with inferences drawn from statistics of a past century and of distant countries, the former of which, at least, have been shown to be untrustworthy." Then you say that I forgot to name the best authorities who are agreed that in epidemic years small-pox is not diminished by vaccination or greatly affected by it. On page 62 of the third report you will find that there are names given; one only I will quote here—viz, the authority that I have already named, Dr. Guy, said that in epidemic years the barrier of vaccination was broken down, and the more and the less susceptible fell victims to the small-pox. Your own columns, after the great epidemic of 1871-2, were used to enforce the susceptibility of the vaccinated to the disease in epidemic years. My endeavour was to be clear in the statement of my claims and arguments, and I may have totally failed to be so, it is true; but when I had specially summarised my claims, it is a little hard, do you not think, that this summary should be entirely set aside by you for a statement of my case that is at what seems to me total variance with all the tenour of my evidence? Your six points—which, without "violence," you say may present my thesis—are opposed to it. Thus you say in No 6 that human interference with small-pox has been made by vaccination. It must be perfectly clear to anyone who has done me the honour to read my evidence that that is a proposition that

<sup>1</sup> Brit. Med. Jour., Dec. 13th, 1890.