In the light of your comments it is evident that the fact that I have made no attempt to establish in any degree the relative effect that different causes have in determining the size of the child will lead the reader to suppose that I do not consider the usually accepted ones as potent. This is very far from being the case and the omission to lay stress on these is due to the fact that opportunity to sift such figures as I have accumulated has hitherto been lacking. It is very far from my intention to put forward this law of parallel growth as the only factor determining the size of the child; it is undoubtedly only one of several, but it is one which has hitherto escaped attention but yet merits further investigation, and it is partly for this very reason that I have published the figures, for being no longer in a position to carry on this investigation myself I wish to bring it to the notice of others for confirmation or refutation. You further criticise my paper in two directions—first, as regards facts and, secondly, as regards the inferences to be drawn from them. The facts you object to, first, on the score of the insufficiency of the measurements. The cases are 111 in number and certainly before such a law as I have stated is allowed generally the figures available will have to be much greater; at the same time I think that the very regularity of the parallelism between the size of the conjugate and that of the child is a warranty that further numbers will confirm those given.

You are disposed to think that my figures are not accurate and that accurate figures could only be obtained either by examination on the cadaver or under anæsthesia. Undoubtedly measurements on the cadaver would be most useful, but their accumulation would take very much longer than the accumulation of clinical figures. I do not consider that the condition of anæsthesia would make results more accurate because the recti have usually little influence ten days after delivery in the ease with which the promontory is reached. The muscles are usually still separated and the hand can sink between them without serious resistance. You are further of opinion that it is impossible clinically to estimate the size of the conjugate diameter of the pelvis accurately within a quarter of an inch. This is probably true in any individual case but I think that by taking many measurements the results will be accurate in the average. Even if this conclusion is not correct, it will yet be found that the figures for successive increase of half an inch in the conjugate diameter, concerning the general accuracy of which there can scarcely be any question, are just as striking and as strong evidence of the law which I have put forward.

My conclusions are drawn from averages. That the parallelism of the child and the pelvis is one of average is constantly insisted on in all the tables accompanying the paper and it will not necessarily hold in individual cases except within wide limits. In order to avoid making the paper too long no lists of actual figures were given. As an example of the differences in these I may say that taking the Bengalis with a pelvis estimated at between four and four and a quarter inches in the conjugate diameter the weight of the children varied from four pounds 14 ounces to eight pounds six ounces. This in itself is sufficient proof that other factors than that of the size of the pelvis are concerned in regulating the size of the child and that the size of the latter can only be estimated within wide limits from that of the former. I do not wish to claim that the law which I have enunciated is the only or even the chief law by which the size of the child is determined; but subject to confirmation or refutation I hold that it is a factor, a practically unrecognised factor, and one the relative place of which among the others requires investigation which I am not in a position to carry out myself.

Your objection that normally sized children may be born through a congenitally contracted pelvis need not, I think, invalidate the general law. Other congenital factors may have been stronger and have resulted in the production of a normally sized child. The consideration of the relative influence of these various factors in determining the size of the child, so far as I have figures on which to base conclusions, I have intentionally omitted from my paper and my apparent dogmatism has been due not to any want of appreciation of the importance of these but partly to the wish to emphasise the new factor and partly to the hope that I should find opportunity in the future to sift the evidence satisfactorily.

I am, Sirs, yours faithfully,

CLAYTON LANE, M.D. Lond.,
Captain, I.M.S.

Medical College, Calcutta, Oct. 20th, 1903.

THE M.D. DEGREE OF BRUSSELS.

To the Editors of THE LANCET.

SIRS,—If Dr. N. Leonard will refer the question of the value of the M.D. Brux. to the proper official quarter in Brussels he will find that my statement in THE LANCET of Oct. 17th, p. 1119, is absolutely correct—viz., that the degree does *not* confer the right to practise in Belgium and that the examination is quite a different one from that which the Belgians themselves undergo. A few foreign medical men were formerly authorised to practise a *speciality* in Brussels irrespective of the particular qualifications they held—though similar favours are not now granted. This, I presume, is what Dr. Leonard refers to in his last paragraph in your issue of Oct. 24th. I think if these facts were more widely known fewer Englishmen would be ready to take a degree which is valueless even in the country where it is obtained.

I am, Sirs, yours faithfully,
Ludgate-hill, E.C., Nov. 5th, 1903. P. A. NIGHTINGALE.

SIMPLE CYST OF THE LIVER.

To the Editors of THE LANCET.

SIRS,—Attention having been called to the rarity of simple cysts of the liver in the paper by Mr. Alban Doran in THE LANCET of Oct. 31st, p. 1218, I think it worth while to refer very briefly to one case of my own. I knew at the time I operated on the case that it was a rare one but after reading Mr. Doran's paper it seems to me desirable that it should be recorded.

A woman, aged 63 years, came under my care in hospital in November, 1900, with a smooth round swelling in exactly the position of a distended gall-bladder. She had no symptoms and had noticed the swelling (which was visible) accidentally two months before I saw her. On opening the abdomen I found the whole liver enlarged so that the lower border of the liver reached nearly to the umbilicus, and projecting from it a bluish translucent cyst of the size of an orange. There was no liver tissue on its anterior aspect but it did not quite extend through the substance of the liver below—i.e., it lay above the quadrate lobe. Its wall was very thin, white, and glistening and it contained straw-coloured fluid. It did not in the least resemble hydatid cyst. The gall-bladder was hidden away under adherent colon and omentum. I stitched the cyst wall to the parietal peritoneum and drained it and she made a good recovery.

I am, Sirs, yours faithfully,
Bristol, Nov. 6th, 1903. CHARLES A. MORTON.

ROBERT STEPHENSON AND THE PUBLIC HEALTH.

To the Editors of THE LANCET.

SIRS,—In these days when we think as little of getting into a train for a short or long journey as we do of eating our breakfast or going to church on Sunday it is perhaps somewhat difficult to cast our minds back to the time when George Stephenson constructed and carried through the first railway for passenger and goods traffic that the world has ever known. In the surveying of "The Stockton and Darlington Railway" George Stephenson was greatly assisted by his only son Robert, to whom England owes much as a pioneer in railway enterprise, after having paid due homage to his truly illustrious father. The centenary of Robert Stephenson has recently occurred, for he was born on Oct. 16th, 1803, and in these days when centenary anniversaries are taken as fitting occasions for the recognition of lives that have been spent in the service of their country it seems unjust that a life so well spent and of such practical service not only to Great Britain but to the whole world should be passed by without comment of any description.

When we consider how dependant we are on our railways and other engineering projects, how the health of dwellers in towns is often speedily restored at perhaps a minimum of time and expense by a quick journey to the country or seaside for rest and change of air and scene, when we think also how the congested state of our towns can at any time be relieved and how at the same time the ambitious villager can satisfy his curiosity about town-life and progress by journeying from one place to another, the general public and the medical profession with it can only recall with gratitude the names of those benefactors who have helped, even indirectly, to increase the standard of health in our country