disease which shall suffice to carry conviction. Moreover, our space is limited.—ED. L.

"THE EAST-END MOTHERS' HOME."

To the Editors of THE LANCET.

SIRS,—You ask questions as to the East-end Mothers' Home which I ask leave to answer: 1. "At what period are patients discharged after confinement?" Fourteen days.—2. "Were any patients discharged who could not be fairly classed as quite well?" No.—3. "Were any patients who were seriously ill sent to their homes?" None.—4. "Or to some general hospital?" One suffering from erysipelas was removed to the London Hospital.—5. "Did any such cases die or only recover after a longer or shorter illness of more or less gravity?" In case above-mentioned, recovered.

I am, Sirs, yours faithfully,

London, E., March 18th, 1895.

M. C. CORNER.

ON CERTAIN PSYCHOLOGICAL PHENOMENA ACCOMPANYING THE ADMINISTRATION OF ANÆSTHETICS.

To the Editors of THE LANCET.

SIRS,—In the year 1800 Humphry Davy, then twenty years of age, experimented with nitrous oxide and suggested its use in surgical operations as a means of preventing pain, and in his biography we read that the impressions he received while under the influence of the gas induced him to exclaim to Dr. Kinglake, "Nothing exists but thought. The universe is composed of impressions, ideas, pleasures and pains." In the year 1874, while inhaling chloroform as a relief to the agony of passing a small renal calculus, I suddenly experienced the extraordinary impression that my spiritual being stood visibly outside my body, regarding that deserted body lying on the bed. Shortly afterwards I called on three different professional chloroformists and asked them if any of their subjects had ever experienced sensations like my own? In reply one gentleman said, "I have often heard patients express similar ideas." Another said, "I myself have experienced on three occasions, when under chloroform, exactly similar sensations." And the third gentleman said, "My patients have often said that they experienced no pain, but felt as if they saw with their inner eye all I was doing during the operations." I was told of a patient who said after anæsthesia, "I thought I had got at the bottom of the secrets of nature." And a dentist told me that many of his patients had experienced similar sensations to those I described.

Now, it is well known that those who have been rescued from drowning have often described their sensations while drowning as being pleasurable and their visions as transcendental. Similarly in the case of anæsthetics. The startling and significant fact is this: that while the body is as if dead under anæsthetics the imagination becomes active and at times exalted. Or, as another friend said to me, "When under chloroform the Platonic ideas came to me that Matter was only phenomenal, while the only reality was that which underlay Matter—viz., its spiritual substance." The object of this letter is to bring before the medical profession these psychological experiences when under anæsthetics; and as in Great Britain there are probably not fewer than 1000 individuals, including midwifery cases, daily placed under anæsthetics there should be an almost unlimited store of evidence to draw from, and thus most important facts may be arrived at as to the construction of an experimental psychology.

I am, Sirs, your obedient servant,

Wimbledon, Feb. 28th, 1895.

GEORGE WYLD, M.D. Edin.

passed out, the "manometer pressure indicated four inches negative pressure of water with half an inch respiratory oscillation," and air passed into the pleural cavity from the outside. On the second occasion, three days later, "the manometer registered a positive pressure of four inches and a half of water, with a respiratory oscillation of an inch and a half," and ninety-seven ounces of fluid along with much air were removed. Dr. West regards the existence of a negative pressure of four inches of water with an increasing effusion as "almost inexplicable"; and according to the teaching of most of the text-books I think it may be regarded as not almost but altogether inexplicable. That teaching is to the effect that when any fluid is effused into the pleural cavity it at once exerts pressure on the lung, the heart, and other surrounding tissues, and consequently one is led to expect that when an opening is made into the chest the fluid will rush out. That this teaching is not in accordance with clinical facts has been known since the time of Trousseau, but, so far as I am aware, the problems remained unsolved until Dr. Douglas Powell¹ ascertained and explained the effects of lung elasticity. In health there is a (potential) negative pressure in the pleural cavity, and on the occurrence of effusion this is maintained until the elasticity of the lung is exhausted by its gradual retraction, when the pressure of the fluid becomes positive. Two distinct stages are associated with pleural effusion, the first when the pressure of the fluid is negative and it is held in suspense by the elasticity of the lungs, and the second when the pressure of the fluid is positive, the lung elasticity having been exhausted. In the first stage the introduction of a trocar will not lead to the evacuation of the fluid, but to the entrance of air from the outside, if an opportunity is presented; while in the second stage fluid will flow freely as long as a positive pressure exists. These facts I have frequently verified at the bedside, and Dr. West's case supplies confirmatory evidence. In testing them one further point must be noted—namely, that the patient must be breathing quietly and regularly, for if he cough or hold his breath fluid will be driven through the trocar, whatever the intra-pleural pressure may be, owing to the forcible elevation of the diaphragm and the passage of air from the sound into the retracted lung. As the respiratory oscillation in Dr. West's case was only half an inch during the first paracentesis this disturbing factor evidently did not come into play.—I am, Sirs, yours faithfully,

G. A. SUTHERLAND.

Old Cavendish-street, W., March 18th, 1895.

"THE RAM, THE BULL, AND THE HEAVENLY TWINS."

To the Editors of THE LANCET.

SIRS,—Referring to your Annotation with the above title, which appeared last week, I think the old theory as regards twin calves has been fairly established by modern experience—viz., that if the twins are of different sex the female is sterile though the oestrus and sexual instinct are generally present. Bewick, in his "History of Quadrupeds," 1800, mentions this as a well-authenticated fact and calls the female twin a "freemartin"—a term still used in many parts of the country. Stockbreeders who accept the theory are often unwilling to bring a sterile animal to maturity, whilst many to whom it is unknown overlook the matter altogether, so that extensive evidence is difficult to obtain. Two cases only have come under my own observation, and in each case the female twin proved sterile; but I have repeatedly seen twin calves of the same sex which have eventually both proved fertile. That no such law holds good as regards sheep or mankind is a matter of common knowledge.

I am, Sirs, yours faithfully,

Ballachulish, N.B., March 18th, 1895.

W. D. ANDERSON.

"PLEURITIC EFFUSION WITH NEGATIVE PRESSURE IN THE PLEURA."

To the Editors of THE LANCET.

SIRS,—In the case under the care of Dr. Samuel West, published in THE LANCET of March 16th, there are several points of interest, but I wish to refer only to the one described under the above heading. Fluid was undoubtedly present in the chest, and paracentesis was performed twice with an ordinary trocar and long tube. On the first occasion no fluid

"THE REMUNERATION OF THE RESIDENT OFFICERS IN ST. BARTHOLOMEW'S HOSPITAL."

To the Editors of THE LANCET.

SIRS,—I do not quite understand the point at issue under this head, but as one who enjoyed the rare experience of a two years' service as house surgeon at this hospital (my

¹ Lung Elasticity in Health and Disease. Transactions of the Royal Medical and Chirurgical Society, vol. lix., 1876, p. 165.