I think that the time has arrived for pricking this bubble once for all, for the dissemination of this report can only give unnecessary trouble to the writers of the history of medicine. In exposing this fallacy I do not pretend to deny that syphilis may have occurred in Egypt in ancient times; all that I can state is that among many thousands of ancient Egyptian skeletons I have never met with a single case which competent observers will agree to call undoubted syphilis.

I am, Sirs, yours faithfully,
G. ELLIOT SMITH.

Anatomical Department, The School of Medicine, Cairo,
Dec. 7th, 1907.

OBSTETRIC RARITIES IN GENERAL PRACTICE.

To the Editors of THE LANCET.

SIRS,—In making brief reference to the following cases it is simply to draw attention to the somewhat infrequent occurrence of such consecutive rarities. Even in hospital practice I do not think such a sequence is by any means common, but in private practice I should imagine it is extremely unusual. Hence my record of them. To be brief, they were a placenta prævia, a carneous mole, and an hydrancephalocele, the three occurring on consecutive days. In neither case was there any special feature worthy of particular comment, except the fact that the hydrancephalocele was one of precipitate delivery. This, I take it, is most unusual in so much as at least one cranial diameter (in this the suboccipito-bregmatic) was materially increased. When called to the case I found the child lying dead in a "chamber" which was half full of blood. The cord had ruptured and I am of opinion that death was due to loss of blood. In the occipital region was a large tumour which proved on examination to be a hydrancephalocele. On dissection I found the subarachnoid space full of fluid and the greater part of both hemispheres with much dilated ventricles projecting into the sac through an almost circular hole in the supra-occipital. On introducing the finger the tentorium could be felt below.

With regard to the placenta prævia, I think the fact that the placenta, although almost central, was with no great difficulty displaced, allowing of a comparatively easy application of forceps, might also bear passing reference. In this case I had thought of turning, but the desire to hasten the termination of the labour suggested to me immediate instrumental interference. The mother made a good recovery but the child being premature (six and a half months) only lived six hours. The case of carneous mole presented the usual history. I first saw the patient on Oct. 1st, when she was presumably in her fourth month of pregnancy. On that date she had a "show." I saw her again on Dec. 5th, when she had had another "show." She had never felt any movements. Next day something came away, which, as might have been anticipated, proved to be a carneous mole. Possibly I have been somewhat prolix in my remarks concerning the above occurrences, but, as I have before mentioned, I think their rarity as a sequence in private practice warrants their description.

I am, Sirs, yours faithfully,
EDWARD TOWNSEND TUCKEY, M.B. R.U.I.
St. Stephen's-road, Bow, Dec. 10th, 1907.

ANTHRAX-INFECTED CARCASS AS HUMAN FOOD.

To the Editors of THE LANCET.

SIRS,—The following may be of interest as an instance of the carcass of a bullock affected with undoubted anthrax having been eaten apparently without ill-effects; and also of the practice too prevalent in country districts, of a sick animal being slaughtered, as it is said, "to save its life," so that it might be sold for human food.

A man was admitted into the Royal Surrey County Hospital on Nov. 26th suffering from malignant pustule of four days' growth, on the anterior aspect of the left forearm. The pustule was excised and from it a smear was taken for direct examination and aerobic cultures in broth and on serum were made, both methods revealing the presence of anthrax bacilli. The man reluctantly gave a history of having assisted a butcher in dressing a bullock which had been found to be very ill on the morning of Nov. 9th

and which had been hurriedly slaughtered by a neighbouring cattle-dealer as it lay in the shed. A butcher who is in a large way of business in a neighbouring district was sent for to dress the animal, and he bought the whole of the carcass. He remarked that the spleen was about eight times the normal size and left it on the premises for the owner to examine. This spleen was subsequently unearthed by the county veterinary inspector who found the anthrax organism in smear preparations. The butcher denied noticing that the blood was dark in colour and asserted that the animal was well nourished and that the flesh appeared perfectly sound. He first cooked a piece of the meat for his own table and finding nothing abnormal in the taste proceeded to sell the remainder. No case of illness has been reported as attributable to the meat, but it was found impossible to obtain a list of the customers to whom it had been sold. As previously mentioned, the pustule was first noticed exactly a fortnight after the handling of the carcass. It subsequently transpired that another man who assisted the butcher at the same time and who had cut his thumb the day before and injured the other thumb during the handling of the carcass developed symptoms of septicæmia a week later—i.e., on Nov. 16th—and died on Nov. 19th.

It is anomalous that the only way the matter could be brought home to any of the parties concerned would be by prosecution of the owner by the County Council for failing to notify the occurrence of anthrax on his farm. He, of course, pleads ignorance of the nature of the illness; even then there remains the fact of the animal, whilst obviously moribund, being slaughtered for human food. The local authority can only deal with unsound meat by having it actually seized and condemned. As all the meat in this case had been eaten before the facts came to light no punishment, except that of publicity, seems possible.

I am, Sirs, yours faithfully,

R. W. C. PIERCE,
Guildford, Dec. 14th, 1907.

Medical Officer of Health.

A CASE OF SELF-INDUCED(?) ABORTION.

To the Editors of THE LANCET.

SIRS,—Dr. A. B. M. Thomson in THE LANCET of Sept. 7th asks under this title if it is possible for a woman to introduce into her uterus without causing injury to the vaginal walls or as a straight rigid rod four inches in length. During the last 12 months I have had two cases of the kind. In one a girl introduced a bone crochet needle five inches long into the uterus and successfully induced abortion at three months. I removed the needle and she aborted the next day and did well. In another case a married woman introduced a large hair-pin, four inches long, points upward, into the uterus and lost it. I dilated, removed the hair-pin, and curetted. She was six weeks pregnant. A week later she developed acute pelvic peritonitis and had to be opened and drained through the abdominal wall and Douglas's pouch and eventually did well.—I am, Sirs, yours faithfully,

Dubbo, Oct. 22nd, 1907.

WILLIAM DAISH, M.D.

THE OPHTHALMOMETER AND ITS LIMITATIONS.

To the Editors of THE LANCET.

SIRS,—In the interesting annotation under the above heading which appeared in THE LANCET of Dec. 7th there are some statements which seem to require amplification.

First, as to the claim that the ophthalmometer "was designed by Helmholtz for measuring the refracting surfaces of the eye," I would point out that, many years before, the curvature of the living cornea had been ascertained by the immortal Thomas Young, and described in his classical work "On the Mechanism of the Eye," published in the Philosophical Transactions for 1801. He measured the diameter and height of his own cornea, viewing it in a mirror, and from the chord and sagitta so obtained he calculated the radius of the corneal sphere to be $\frac{1}{100}$ ths of an inch (7.87 millimetres). His striking accuracy is shown by the fact that it is the mean of later investigations which have limited the variations to between 7.2 and 8.4 millimetres. But although Young's work suffices for our knowledge of the cardinal points of the dioptric system the procedure could not be employed clinically.

Modern ophthalmometry is based on the physical properties

of convex mirrors. As is generally known, in these the image is erect, diminished, and virtual, being placed behind the surface. The cornea acts in the same way, and, *en passant*, let me point out that the Hebrew name for the pupil (*ischon*, a little man) and our own word pupil (Latin, *pupilla*, diminutive of *pupa*, a doll) owe their origin in all probability to the tiny erect image of themselves seen in the plane of the iris by the ancient philosophers. In ophthalmometry an object, or two luminous points (*mires*) demarcating the extent of the object, is placed before and at a known distance from the eye. The corneal image is then magnified and examined by means of an astronomical telescope. The size of the image is measured directly, or by the method of "doubling." Knowing these quantities, it is easy to calculate the fourth by the rule that "the object is to the image as twice their distance apart is to the required radius."

Now, in 1840, there appeared in Oken's *Isis*, Jahrg., S. 886, an article by Kohlrausch, entitled "Ueber die Messung des Radius der Vorderfläche der Hornhaut am lebenden menschlichen Auge." He also had obtained a radius of 7.87 millimetres by means of an apparatus consisting of an astronomical telescope of short focus on either side of which was a candle. The focal plane of the eyepiece bore two parallel spider threads which could be relatively moved by means of a screw. The threads were made to bisect the images and so the observation was completed. Curiously enough, his work has never been generally known, although it was described by Schiötz in the "Dictionnaire Encyclopédique des Sciences Médicales" in 1881, republished in Javal's "Mémoires d'Ophthalmometrie" in 1890, while in that magnificent work, the "Encyclopédie Française d'Ophthalmologie," 1904, Sulzer, in the chapter on Ophthalmometry in the third volume expresses himself as follows: "Les ophthalmomètres actuellement en usage sont des modifications de l'instrument de Kohlrausch."

It was not until 1854 that Helmholtz applied to the instrument the plane-parallel plates, already used in the heliometer for measuring the heavenly bodies. Great a step as it unquestionably was in the evolution of the ophthalmometer, Helmholtz did no more than graft upon it a principle long used elsewhere. The discoveries of the illustrious scientist are so abundant and fruitful that in seeking material wherewith to weave a wreath in his memory there is no need to pluck the hard-won laurels of others.

The second point refers to the discrepancy between ophthalmometric and total astigmatism being referred to the lens. Beyond the patient and laborious investigations of my friend and teacher Dr. Tscherning of the Sorbonne I know of none which can claim to deal with this matter from actual observation. He found that lenticular astigmatism played but a small part and that the main portion can be attributed to three separate agencies: (a) The greater refractivity of the cornea as compared with the aqueous humour which changes an astigmatism "with the rule" of the first surface to one "against the rule" of the second, because the second must act as a negative lens. (b) The ophthalmometer measures the power of the cylinder which, *in contact with* the cornea, would correct first surface error. Obviously, this lens cannot be worn there and when it is transferred to the trial frame we introduce an "error of separation" which increases directly with both the power of the cylinder and its distance from the cornea. The error may reach several dioptres but it can be computed. The fact that it is seldom thought of must be my excuse for drawing attention to it. (c) The fact, also but little known, that the refraction is not the same throughout the pupillary space. I am not now alluding to the visual and paracentral zones of the cornea, but to the total measurements made by means of a luminous point. At the Sorbonne it has been found practically impossible to get theoretically perfect correction.

When, therefore, we hear of discrepancies we must bear in mind that the standard to which they are referred is at best an approximation.

I am, Sirs, yours faithfully,

WILLIAM ETTLES, M.D. Aberd., F.R.C.S. Edin.

Weymouth-street, W., Dec. 9th, 1907.

* * We are well aware that the radius of the cornea had been measured before v. Helmholtz invented the ophthalmometer. As Dr. Ettles admits, the credit of applying the principle of doubling the image to the problem of measuring the curvatures of the refracting surfaces of the

eye belongs to v. Helmholtz. Since this is the fundamental principle of the ophthalmometer and since its application enabled v. Helmholtz not only to confirm previous observations but also to widen the field of investigation in a notable degree we consider that we were amply justified in stating that the instrument "was designed by v. Helmholtz for measuring the refracting surfaces of the eye." We go farther and have no hesitation in affirming that v. Helmholtz was to all intents and purposes the inventor of the ophthalmometer, just as Tscherning is rightly regarded as the inventor of the ophthalmophacometer. It must have been obvious to readers of our annotation that we did not imply that lenticular astigmatism is the sole "limitation" in the use of the ophthalmometer. Dr. Ettles will doubtless remember that Thomas Young, besides measuring the curvature of his cornea, also measured his own astigmatism and proved that it was lenticular and not corneal. Lenticular astigmatism is one factor in the problem; the others which Dr. Ettles has mentioned emphasise the limitations of the instrument for clinical use.—ED. L.

ACUTE PULMONARY OEDEMA.

To the Editors of THE LANCET.

SIRS,—The striking clinical phenomena described by Dr. Leonard Williams in his paper on Acute Pulmonary Oedema in THE LANCET of Dec. 7th are certainly as worthy of detailed treatment in text-books of medicine as some other evidences of disordered function which, for lack of more complete knowledge of their essential nature, have been allotted a special place in a symptomatic nomenclature. I gather, however, that Dr. Williams does not so much insist that the syndrome which he has so graphically and tersely described should go to swell such evidences of our ignorance as are so eloquently expressed by such terms as tachycardia, bradycardia, and Graves's disease, as that an acute and swamping oedema or bronchorrhoea should be described in more detail in text-books in connexion with the various pathological states with which it is associated and of which it may be a very grave and urgent symptom.

As a mode of death or of threatened death in heart disease it is well worthy of special notice, and in all cases, I imagine, there can be little doubt, as Sir James Barr suggests in THE LANCET of Dec. 14th, that it is an expression of cardiac failure. Dr. Williams would seem, in his paper, to exclude syncope as the cause of the excessive bronchorrhoea which he describes. The oedema was, evidently from the conditions found post mortem, transient though fatal. It is my belief, however, that had Dr. Williams been present when the patient first cried out in the alarm of "air hunger" he would probably have found that the initial event in the case was threatened death in cardiac diastole. That the heart recovered itself somewhat afterwards is, of course, in accordance with what is frequently witnessed, but that it was severe enough and lasted long enough, so to impede the pulmonary circuit as to induce the symptoms he describes, I am convinced from my own experience. The aggravation of the symptoms, moreover, on recumbency and the rapidly supervening unconsciousness of the patient appear to me to support this view of the cause of the phenomena for the orthopneic position sought for by the patient under these circumstances is an instinctive attempt to moderate the inflow into an already surcharged right heart.

Aortic regurgitant lesions are, we know, those most frequently associated with sudden death in heart disease, and the physics of the circulation in these cases seem to indicate caution in the use of some instrumental methods to determine blood pressure. Personally, I never feel comfortable in my own mind in applying, for example, the Riva-Rocci instrument under these circumstances without securing sufficient rest for the patient beforehand. I do not expect this view to be shared by all, but the strangling of a considerable portion of a limb even for a short time in aortic regurgitation is, to my mind, a procedure not to be undertaken without precaution. It is, moreover, one which is usually a novelty to, and sometimes excites, a patient. There are also other conditions not primarily cardiac in which a swamping bronchorrhoea is witnessed. Your correspondents (Dec. 14th) have referred to some of these. I also have witnessed this state when pleural effusions have been