My attention was first called to them by Dr. Wade, and I am indebted to him for kindly allowing me to make reference to them here. A number of these cases were treated by Dr. Wade with weak alkaline solutions, as recommended by him in a recent number of the *Practitioner* (April 1877). The rapid improvement was very remarkable; but what I wish especially to refer to is the changes that took place in the circulatory system. The arterial tension soon diminished, and the reduplication gradually became less, until at last a stage was reached when it required some care to determine positively whether or not it was present at all. If the auricular contraction were the cause of this reduplicate sound in these cases, then it is necessary to suppose that, as the arterial tension passed off, the auricular systole was followed more and more quickly by that of the ventricle, till at length the time between the two systoles was reduced to nothing. But it is known that the one occurs about one-tenth of a second before the other in a heart beating sixty to the minute.

CHEIRO-POMPHOLYX.

By JONATHAN HUTCHINSON, F.R.C.S.,
Senior Surgeon to the London Hospital, etc.

I HAD certainly hoped that all personal discussion between Dr. Tilbury Fox and myself as to "priority of observation" was long ago at an end. The facts were very fully put before the profession, and might well have been left to its decision. After the expiration of a year, however, the subject is reopened, and Mr. Tweedy has been induced to forward to the JOURNAL his record of a conversation with me which occurred as long ago as 1873. The publication of statements made in conversation is always of questionable propriety, and especially so when the interval is long. Memories are treacherous; and, where the parties concerned differ, it is impossible to decide. Now, although I do not dispute Mr. Tweedy's verbal accuracy, my memory as regards the general bearing of what passed on that occasion is different from his. His letter appears to me to imply (although in this I may be mistaken) that at the date referred to I asked him about the disease in question, it being at the time comparatively new to me. The conversation took place at my desk at the Moorfields Ophthalmic Hospital. I cannot be quite certain, but I believe that a case which was present suggested it. I described to Mr. Tweedy, as I had done a hundred times before to others, the peculiar disease which I then named "relapsing bullous eruption on the hands", and for which I then proposed that of cheiro-pompholyx. I was just as familiar with the features of that disease then as I am now; and, if I asked Mr. Tweedy, as he says I did, if he knew it, the question was not put in the search of information respecting it. Mr. Tweedy suggested to me that it was the same disease which Dr. Tilbury Fox called dysidrosis; and, after a remark or two as to the etymology of that word, I said that my malady had nothing whatever to do with sweating. Mr. Tweedy said nothing in defence of the dysidrosis theory. On the contrary, I quite understood that he did not support it; and there, so far as Mr. Tweedy and myself were concerned, the matter ended. I subsequently, as I have previously stated, received a courteous letter from Dr. Fox, inviting me to read his writings on dysidrosis. This, I regret to say, I never did.

Let me just add that it was the theory involved in the name dysidrosis which prevented me from ever realising the possibility that Dr. Fox had really described the same malady which had so much interested me. I felt so confident that my "relapsing bullous eruption on the hands" had nothing to do with impeded sweating, and that so definite and wide a term as "dysidrosis" must comprise much more than this curiously local malady, that I never gave the subject the attention which it ought to have claimed. May I suggest, further, that this constitutes a strong objection to the use of the term? I have no desire to pre-judge the question as to the anatomical site of the fluid in the sago-grain. It is possible, after all, that that fluid is sweat. We must wait for evidence. But meanwhile it is not, I submit, convenient to designate the disease as if its nature were established, nor to use a term which is etymologically applicable to much more in exclusive reference to one local and clinically very peculiar form. If future investigation should show that in cheiro-pompholyx the initial lesion is, as Dr. Fox and Mr. Tweedy think, a distended sudoriparous duct, still it will remain only one variety of dysidrosis. Names based upon a single physiological hypothesis are seldom appropriate to maladies which present us with a complex group of clinical peculiarities. When it is wished to recognise such groups of symptoms by a name, it seems to me much better to use one which is comparatively meaningless. As such, I still prefer the one which I proposed; and, as a further reason for continuing to

use it, may state that I still agree with Dr. Robinson of New York, Dr. Thin, and others, in believing that cheiro-pompholyx has nothing to do with dysidrosis.

It appears to me that, since the publication of Dr. Robinson's able paper, the proof of the matter is not difficult to come at. If Dr. Fox can demonstrate that there is a bullous eruption on the hands, the fluid of which, in the earliest stage, is acid and does not coagulate, many of us will believe that *dysidrosis extremitatum* is a reality.

OBSTETRIC MEMORANDA.

CONGENITAL UMBILICAL HERNIA.

I ATTENDED Mrs. McK., aged 44, in her eleventh confinement, on November 9th, 1875, when she was delivered of a small female child. It appeared to be healthy and otherwise well formed, with the exception of a small conical hernial projection into the umbilical cord. The convolutions of the bowel could be distinctly seen through the cord. The child died on the sixth day, from what I considered to be peritonitis. Whilst Mrs. McK. was sitting before the fire shortly prior to the birth of the child, the waters suddenly escaped, and she remarked to me on my arrival as to their great quantity.

This deformity is one which we should expect to be of comparative frequency, when we consider the development of the fetus from an embryological point of view. I think this will be borne out by notices of cases in the various authorities on deformities. Vrolik has an illustration of this deformity in his beautiful Atlas (*Tabula ad illustrandam Embryogenesin*), and A. Förster, in his short but complete work (*Die Missbildungen der Menschen*), has two diagrams. J. F. Meckel (*Handbuch der pathologischen Anatomie*) describes a number of cases, in some of which the liver, stomach, and part of the intestines were contained in the hernial sac.

According to M. Vrolik (*Cyclopædia of Anatomy*), this deformity is caused by an arrest of development at that stage in which a part of the viscera is contained in the sheath of the umbilical cord. He also states that the viscera lying in it are always in an imperfect condition. He mentions that there were only four cases in which the life of the malformed child lasted for any time after birth, in the case of Van der Voort for eight, and in that of Ribko for twelve months.

According to some authorities, excess of the liquor amnii is a cause of deformity; but whether it be or not, it is impossible to judge from one case. Its *modus operandi* may be conceived from its pressing on the abdominal walls, and thereby protruding the viscera. It will likewise be seen that the mother was a multipara, and, according to J. G. St. Hilaire (*Histoire des Anomalies*, tome iii), deformities occur more frequently in them than in primiparæ; and, in my own short experience, 90 per cent. have been in multiparæ, the other 10 per cent. occurring in the somewhat exceptional case of an unmarried primipara.

WILLIAM SNEDDON, M.D., Beith, N.B.

SUDDEN DEATH THREE WEEKS AFTER DELIVERY FROM THROMBOSIS OF PELVIC VEINS, WITH EMBOLISM AND OBSTRUCTION OF PULMONARY VESSELS.

S. W., AGED 27, a fair somewhat delicate woman, living about two miles from my residence, was delivered of her third child on November 24th. The labour was natural and the placenta readily detached. She progressed satisfactorily, and was seen up to the tenth day. On December 19th, between 9 and 10 A.M., I was summoned in haste to see her; but on arrival found her dead. On inquiry, it appeared that, on December 16th, she had complained of pains, supposed to be rheumatic, in the joints, especially the left hip and in the legs; she was said to have been feverish and to have sweated a good deal. She then had cough and shortness of breath. On the morning of her death, on coming downstairs, she was seized with sudden and intense dyspnoea, complained of pain in the region of the heart, and shortly expired.

I made a *post mortem* examination on the 21st, and found both lungs, especially the upper lobes, much congested, with here and there extravasations of dark coagulated blood in their substance. The left lung was bound down by old adhesions. The branches of the pulmonary artery were filled with reddish-brown friable coagulum. In the right ventricle was a cylindrical partly decolourised firm yet friable clot. The veins of the broad ligament, on the left side, and the trunks of the internal and common iliac veins of the same side, were filled with a coagulum of similar character. In the trunk of the right ovarian vein, just before its junction with the inferior vena cava, was a coagulum of the same character as those before mentioned. The vena