

lead us to believe, though in a sense inherited, is so only from the local disposition of certain parts. Practically, therefore, it begins locally, and may and must be thought of in its first occurrence in the system as a *local disease*."

Finally, whilst disinclined to join issue with Sir James Paget upon every point, his statement that we have not yet found a method for either the prevention or the cure of cancer cannot be allowed to pass unchallenged, for instances in which the disease has been cured are familiar to most practitioners; and though there is no common method applicable for the prevention of all kinds of cancer, there are recognised principles to be adapted to many of its varieties. As examples, let me cite the premonitory induration of the tongue or lips in elderly subjects caused by irritation from the stumps of teeth, or by smoking, which will often spontaneously resolve upon removal of the exciting cause. Leucoma or psoriasis of the tongue, dependent upon local irritation coupled with syphilitic taint, may be resolved by antisiphilitic treatment sedulously carried out. Where cancer is presumed to be about to commence in the scrotum of a sweep, change of employment and cleanliness should be insisted upon, and suitable local treatment employed. *Cessante causa, cessat effectus*.

I am, Sirs, yours faithfully,

CHARLES E. JENNINGS, F.R.C.S. Eng.

Upper Brook-street, Grosvenor-square, W., Nov. 19th, 1887.

"THE NATURE OF SMALL-POX."

To the Editors of THE LANCET.

SIRS,—The fact that Mr. Birdwood has had almost unequalled advantages for acquiring a special knowledge of small-pox naturally directs attention to his theory. Mr. Birdwood asks us to believe that the pocks in small-pox develop from spores adhering to the skin of the patient who has been exposed to a spore-laden atmosphere. And he asks us to believe this on the strength of two statements.

The first statement is that this hypothesis accounts for the distribution of the pocks. Those who have examined many cases of small-pox in the unvaccinated must have been struck by the copiousness of the eruption on the back as compared with the abdomen. And this difference is much more striking than the difference between the number of pocks on the exposed parts and on the back. But, according to Mr. Birdwood's theory, in the race for spores the face and hands should be first, and the rest nowhere.

The second statement is that "on the unprotected the number of pocks does depend on the circumstances of exposure." That would, if proved, be a very interesting fact. But much definite evidence must be brought forward before anyone can effect so revolutionary a change in the general belief. Few would positively deny that it is conceivable an unprotected person might be more seriously attacked by small-pox after nursing a hæmorrhagic case than after passing a discrete case in the street, but the general effect of small-pox experience on the minds of medical men is to bring about a strong belief directly opposed to Mr. Birdwood's. Instances of the severest confluent small-pox where the patient and his friends cannot even guess the source of infection are very common in small-pox hospitals, and everyone who has seen families brought into small-pox wards, sometimes in groups, must have noticed the remarkable variations in the severity of the different cases from one household. It is highly probable that unknown personal "idiosyncrasies" have far more to do with the character of the attack than have the circumstances of exposure.

Having thus seen objections to the acceptance of Mr. Birdwood's two statements in support of his theory, one may look at the arguments against it, and in favour of that which is usually accepted.

1. Small-pox, scarlet fever, typhus, measles, and chicken-pox all begin with illness followed by eruption. As regards the *copiousness* of the eruption: scarlet fever rather prefers the neck, chest, and abdomen; typhus the sides of the chest and abdomen; measles the back; and chicken-pox the chest and back. Why should small-pox be classed by itself because it prefers the face, wrists, and back? The theory that the poison is spread by the circulation does not explain this preference, but neither does it explain the selections of the others. In this respect these maladies are all on a par, and one can find no reason for removing small-pox from acute febrile diseases.

2. In small-pox the initial fever is severe for two or three days before the pocks appear. Until an hour or two before the actual eruption no unusual sensation whatever is felt in the skin. Why then attribute so striking a general fever to an as yet non-existent irritation in the skin? The common theory keeps the cart behind the horse, and does not attempt to make the pocks account for a fever which precedes them.

3. In many cases of small-pox the line of pressure of a garter, waistband, brace, &c., is traced out by an extra number of pocks. The circulation theory explains this naturally, because a congestion occurs in such places when the pressure is removed, and it is easy to suppose that there would therefore be a great pock-formation. But on the spore-adhering theory these are the very parts where pocks should be fewest.

4. In many cases of hæmorrhagic small-pox the pocks are few, ill-developed, and never get beyond the early papular stage, whilst over nearly the whole surface of the body violent redness, rapidly becoming purple, shows the extraordinary skin hæmorrhage that takes place. In such cases the post-mortem examination reveals that singular distribution of internal hæmorrhages which is peculiar to small-pox, and of which the hæmorrhage into the substance of the wall of the pelvis of the kidney, not merely, as usually believed, into the cavity of the pelvis of the kidney, is the most striking example. To explain such occurrences on his theory, Mr. Birdwood must suppose absorption and subsequent circulation of the organisms, thus tacking the old theory on as an occasional necessary adjunct of the new.

5. When formerly small-pox virus was intentionally placed in a patient's skin, the process was followed in a day or two by the appearance of one or more papules. The patient then wandered into no spore-laden atmosphere, but awaited events in his room, and about eight days later a general crop of papules appeared. Are we to suppose that the primary papule used its central depression, like a volcano its crater, to pour forth spores which the patient then rearrested and developed into his general eruption? It is hardly necessary to suggest other points, such as the occurrence and locality of initial rashes. So great are the objections to Mr. Birdwood's theory, that one cannot but continue to class small-pox with the other acute febrile diseases, which it resembles in so many points; and also to confess, with Sir James Paget, that "we cannot tell why small-pox is especially manifested on the skin."

I am, Sirs, yours truly,

JAMES STRUTHERS, Jun., M.B., C.M. Aberd.

Lewisham, Nov. 1887.

NEPHRECTOMY.

To the Editors of THE LANCET.

SIRS,—I regret that I was prevented from attending the last meeting of the Royal Medical and Chirurgical Society to discuss Mr. Warrington Haward's interesting case of nephrectomy, as I had promised. It would seem from the youth of the patient, and from the fact that one kidney was healthy, the case was well selected and one eminently favourable for surgical interference; and those who have Mr. Haward's acquaintance know that in trusting to his manipulative skill the patient had secured one safeguard against the accidents inseparable from surgery. The ill result must therefore, I think, be attributed to the operation chosen. Two reasons are given for the choice of the abdominal operation in preference to the lumbar: (1) the mobility of the kidney; (2) its size. The first of these operators may for the future entirely disregard. The mobility of the kidney, so far from necessitating an abdominal operation, renders the lumbar operation much more easy to perform. In support of this, I may mention that two years and a half ago I removed through the loin a kidney full of stones, which dropped so low in the iliac region that the fingers could be pressed in between it and the ribs; and when the patient lay on the opposite side it fell across the umbilicus. This was the easiest nephrectomy I have yet performed. The second reason I am unable to gauge, as no measurements are given; but of this I am certain, that very much larger tumours may be removed by the loin than surgeons have been inclined to attempt, if they will adopt the oblique crucial incision I have always advocated. This consists of an oblique incision parallel with the last rib, and a finger's breadth below it, followed by a

vertical, corresponding to the margin of the quadratus lumborum, extending from the upper edge of the last rib to the iliac crest. Tumours containing fluid may be always reduced in size by tapping before being removed, and a large suppurating kidney should be drained before nephrectomy is undertaken. Experience has shown that the ureter, however thickened, causes no trouble, and may be left in the wound secured with a simple catgut ligature. The stitching of this to the surface must add a danger both immediate and remote. One speaker referred to a kidney so adherent that he thought it would be impossible to remove it from the loin. I believe a kidney, however adherent, may always be so removed, if the capsule be left behind. There is a patient leaving Guy's Hospital to-day quite well, whose left kidney I removed about five weeks ago. It had been suppurating for nine years, and was closely adherent to the colon and surrounding structures. To have removed it together with the capsule would have been certain death, but it was stripped out without much difficulty from within the capsule. I have now removed five kidneys in succession through the loin without a death, and this success depends not on the operator, but on the operation (which, speaking generally, should almost always be lumbar), and on a proper selection of cases suitable.

I am, Sirs, your obedient servant,

R. CLEMENT LUCAS, B.S., F.R.C.S.,

Senior Assistant Surgeon to Guy's Hospital.

Finsbury-square, Nov. 28th, 1887.

CHIAN TURPENTINE AND CANCER.

To the Editors of THE LANCET.

SIRS,—It is now some seven years ago since Professor Clay gave to the profession, through the medium of your journal, the welcome intelligence that at last a specific cure for cancer had been found in Chian turpentine; and I think it may without fear of contradiction be asserted that no remedy has had a fairer trial at the hands of his *confrères* or more grievously disappointed them. It is true that Mr. Clay endeavoured to get over the discrepancy by alleging the impurity of the drug used, but doubtless many like myself still continued their observations with the genuine article as supplied by Southall of Birmingham, to find the result still the same.

Notwithstanding the almost unanimous condemnation of the treatment, I find in a later paper of Mr. Clay's (THE LANCET, vol. ii. 1881, p. 1033) these words: "An enlarged experience, however, has confirmed the statements made in my original paper, and I have now the satisfaction of being able to declare that I have nothing to withdraw or to qualify as regards the statements I then made as the result of observation as to the effects of Chian turpentine in uterine cancer." From time to time he has favoured his professional brethren with repeated cures of cancer by this remedy, and even so recently as in your last week's issue three more examples are given. But what about the failures? In the interest of the public at large, such claims as Mr. Clay makes for Chian turpentine ought not to pass unchallenged by those who differ from him. Unfortunately, examples of cancerous disease are only too common upon whom this remedy (supplied, if necessary, by his own chemist) might be tested by a tribunal in whom the profession at large would have confidence, and the doubt once and for all resolved. If this drug came out of the ordeal triumphantly, then I feel sure there would not be a single dissident to Mr. Clay occupying a position not inferior to Jenner or Harvey as one of the greatest benefactors of our species; but if, on the contrary, it is wholly useless as a remedy, then let it drop into a well-merited, and not too premature, oblivion.

I am, Sirs, yours truly,

Nottingham, Nov. 21st, 1887.

GEORGE ELDER.

THE FELLOWSHIP OF THE ROYAL COLLEGE OF SURGEONS.

To the Editors of THE LANCET.

SIRS,—With your permission I should like to say a few words in reply to Mr. Holmes' sweeping condemnation of the power possessed by the Council of Surgeons of electing to the Fellowship Members of twenty years' standing, two in each year. If as many as that, or more, should present

claims arising from the work done during that twenty years, which claims are open to the investigation and criticism of the Council, I think it would be out of all reason to reject them on the ground of there having been no examination. How many of the Fellows who have gained their position in the only way Mr. Holmes would recognise, and have not been engaged in teaching or lecturing, would be able to retain it if, after twenty years of active practice, they were asked to undergo a second, similar to that which gave them their qualification? In my own case, the twenty years were spent in active and successful practice as a hospital surgeon, and though so many things practically useless were probably forgotten that an examination would have been difficult to pass, I yet believed that on assuming the position of consulting surgeon to the same institution I should not be asking too much in seeking to be recognised as a Fellow. So, I presume, thought the six gentlemen who, as already Fellows, signed my application. I believe in my own year mine was the only application, and I do not think these have ever been very numerous. But if they were to seek the honour, and were to present the same testimonials, I do not think the status of the Fellows would be so lowered by granting it, as Mr. Holmes seems to think.

I am, Sirs, your obedient servant,

Dec. 1st, 1887.

J. H. GRAMSHAW, F.R.C.S. (by election).

PHARMACEUTICAL WEIGHTS AND MEASURES.

To the Editors of THE LANCET.

SIRS,—Since the introduction of the *cental* system (which, it may be surmised, is a step towards the future decimal system) into the Pharmacopœia, our weights and measures for surgery use have become troublesomely obsolete, so much so that it is with difficulty that the general practitioner who dispenses his own medicines can now do so without great waste of valuable time and much mental labour, in having to reduce his drachms and ounces to minims or grains. Solutions that were formerly prepared as so much per *drachm* or *ounce*, have become now so much per *cent*. It is a matter of surprise that the vendors of our weights and measures have not been sufficiently alive to the wants and requirements of the times, and sufficiently enterprising to supply a demand which is now seriously felt, for weights marked in grains, and measures in minims, in grades of tens and hundreds, instead of the old-fashioned and now virtually obsolete scruples and drachms, and fluid drachms with their halves. I would suggest that weights be now made according to the following gradation: 1, 2, 3, 4, 5, 10, 15, 20, 25, 30, 40, 50, 60, 100, 250, and 500; that minim measures be graduated on the left hand in drachms and half-drachms, and on the right hand be marked in grades of 10 minims up to 200, the first 100 having intermediate or 5-minim lines; that ounce measures be graduated on the left in ounces and half-ounces, the first half-ounce being subdivided into drachms, the second into two drachms; and on the right in 25, 50, and 100 minims; and that half-pint measures be graduated on the left in half-ounces and ounces, and on the right in grades of 100 minims. Some recognition of the old system, it is obvious, should be retained, and, although only recommended above for fluid measures, the requirements being greater, might also be adopted for the new weights; for instance, the 20 gr. weight might be stamped on the obverse side with the old symbol, $\mathfrak{D}\text{j}$; the 30 gr., $\mathfrak{Z}\text{ss}$; the 40 gr., $\mathfrak{D}\text{ij}$., and the 60 gr., $\mathfrak{Z}\text{j}$.

Were the above weights and measures possible to be obtained, and supplied through the ordinary channels, the labour and mental calculation saved to the busy practitioner would be immense.

I am, Sirs, yours truly,

Dovercourt, Nov. 1887.

W. W. HARDWICKE.

THE PREVENTION OF PUERPERAL SEPTICÆMIA.

To the Editors of THE LANCET.

SIRS,—An ounce of fact is worth a ton of theory, gaseous or sagacious. Here is a single fact. It occurred in connexion with the case of extra-uterine gestation—a note regarding which appeared a fortnight ago, and also to-day in your columns. There could be no doubt as to "septic