It is one of the numerous merits of this classification, and therein it shows its truly scientific character, that it helps in the solution of a practical question such as the one under discussion. By bringing the bodily causes of the mental symptoms prominently before our minds, it enables us to see whether we can treat and remove them with the means at our disposal in private houses. It helps our prognosis greatly; so that we can take into account the probable duration of the severe and acute symptoms that often make home-treatment impossible if they continue long. When a case is distinctly incurable from the first, the question of sending him to a lunatic asylum is immensely simplified.

Dr. Skae's Classification.—I shall briefly go over the different groups into which Dr. Skae divides insanity with reference to this question. His first group is congenital insanity, comprising idiocy and the various degrees of imbecility. This class of cases require training rather than medical treatment, and, as a general rule, are not suitable inmates of asylums. At home under the kindly care of relations, in institutions for the training of imbeciles, or in workhouses, such cases are most suitably placed. The next group, that of epileptic insanity, does most undoubtedly often require asylum-treatment, where there is a tendency to great violence and excitement after the fits. In many cases, however, this violence is very transient; and where circumstances allow suitable attendance during these times, the patients may be kept outside, and lead far happier lives than in asylums. If an epileptic, however, have become permanently irritable, with suspicions and sudden impulses to violence, then our duty is clear in the matter. The insanity of masturbation comes next; and I think every effort ought to be made to keep this class out of asylums, at first trying every means of cure before sending them there. The great hope in such cases is to get the bodily health into a thoroughly vigorous condition, and rouse the mind to interest itself in something that is healthfully stimulating. The same observations apply to hysterical insanity in the female, and to the short attacks of mania that sometimes occur at puberty (insanity of pubescence) in both sexes. In the case of puerperal insanity, everything depends on the intensity of the symptoms and the circumstances of the patient. In itself, this form, more than any other, tends to recovery with anything like proper treatment. Among the very rich, almost no cases of puerperal insanity need be sent to asylums; but they must often be sent from home, under the care of proper attendants. Among the very poor, on the contrary, I believe nearly every case would get better much sooner, and more satisfactorily, if sent to asylums. Somewhat the same remarks apply to insanity from prolonged lactation. The cure of this is a mere matter of abundance of good nourishment and stimulants, with some mild sedative to procure sleep. The insanity of pregnancy should only be sent to an asylum when the symptoms are so extremely acute that the patient cannot be managed elsewhere, or where strong suicidal tendency exists. Uterine insanity is so apt to be associated with fixed delusions of a sexual character, that most cases have, sooner or later, to be sent to asylums; and that being so, it is better they should be sent early, when treatment is of some avail. In few kinds of insanity is there such a tendency to think that nothing is wrong, and so an obstinate aversion to treatment of any sort. In an asylum, this treatment can be carried out *volens volens*. Climacteric insanity, or that occurring at the change of life, is usually a very curable affection, and its symptoms are mild in their character, so that resort to an asylum is seldom necessary. In the case of senile insanity, all must depend on the violence of the symptoms and the circumstances of the patient. Among the rich, such cases need never be sent to asylums. Among the poor, no cases are often so much benefited by asylum regimen and treatment. Phthisical insanity unfortunately needs asylum-treatment in most cases. The waywardness, dislike to relatives, irritability and fickleness that characterise it, can be better managed in an institution than in a private house. Traumatic mania, or that caused by violence, is generally best treated in an asylum. Its symptoms are apt to be violent and unmanageable out of one. Delirium tremens and alcoholism should not be sent to asylums, nor the attacks of ephemeral mania that sometimes follow a drunken bout. Dipsomaniacs should not be sent to an asylum. They should, assuredly, for their own sakes, be prevented from having stimulants; but, unquestionably, the law does not at present recognise this as a form of mental alienation that justifies deprivation of liberty. If we send such cases to asylums, we do so at our own risk; and I cannot see that it is the duty of our profession to expose itself to legal penalties in such cases. Mr. Dalrymple's bill will, no doubt, in time, put matters right in this respect. General paralytic insanity, I may say, should always be sent to an asylum. It is utterly hopeless from the first, and can be far better managed in an institution with means and appliances at hand. And there is no object in delaying time when we have made our diagnosis. The patient will merely be squandering his means, and causing infinite pain and annoy-

ance to his relatives during the time he is allowed to remain at home. In the case of acute idiopathic insanity, with a strong hereditary predisposition, if the patient be young, I believe it is of much importance to send the case to an asylum. Early treatment of the right kind may save the patient from falling into dementia, which he is likely enough to do under any circumstances. How should the existence of strong hereditary predisposition to insanity affect our view of sending a case to an asylum or keeping him at home? I think it ought to incline us to send him to the asylum, on account of the greater importance of having right treatment early and thoroughly applied in such a case. Hereditary predisposition does not denote incurability, by any means. In fact, such cases are quite as curable as others; but it does indicate a great liability to recurrence of the attack after being cured.

I am not going to enter here into the question of boarding out, in suitable families, the harmlessly demented, weak-minded, and mildly incurable insane. That is a question of expediency. They are working it out in Scotland. The English lunacy law as yet makes no provision for its being carried out under the supervision necessary to its being successful; so we need not discuss it.

THE CONSTANT OCCURRENCE OF SARCINA VENTRICULI (GOODSIR) IN THE BLOOD OF MAN AND THE LOWER ANIMALS: WITH REMARKS ON THE NATURE OF SARCINOUS VOMITING.

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GREAT mystery has prevailed regarding the conditions under which the peculiar organisms *sarcina ventriculi* (Goodsir) appear or are generated, as the phrase is. They have been most commonly observed in cases of chronic disease of the stomach associated with obstinate vomiting of acid, frothy, yeasty matters, of so characteristic a nature that the condition has received the name of sarcinous vomiting. Since the time when these organisms were discovered by Goodsir, and called by him *sarcina ventriculi*, it has generally been believed that the stomach is their natural habitat; and their occurrence elsewhere has been looked upon as a curious pathological fact, and has led to ingenious speculations endeavouring to account for their appearance as secondary to the affection of the stomach. *Sarcinae*, however, have been found by numerous observers in so many different situations, and under such varied conditions, that this explanation is entirely out of the question. Consequently, different species of *sarcinae* have been established, deriving their name from the locality in which they were found, or from some apparently slight difference in size and appearance. They have been found in the urine by Heller, Beale, Mackay, Johnson, Munk, Begbie, etc.; in the faeces and intestine by Bennett, Hasse, Demme, etc.; in cholera-stools and diarrhoea-evacuations by Wedl, Mensonides, Cunningham, etc.; in diseased conditions of the lungs by Virchow, Frerichs, Zenker, etc.; in the peritoneal fluid, in a case of perforation of the stomach, by Zenker; in the cerebral fluid, in a case of tubercular meningitis, by Jenner; and once on the surface of the capsule of the lens, in a case of cataract, by MM. Robin and Sichel. Virchow has seen them in the stomach of a rabbit, and Frerichs in the stomach of a dog the subject of a gastric fistula. Eberth has found them in the caecum and faeces of fowls, and in the stomach of an ape.

Many other instances may have been recorded, but these are sufficient to show in how few respects, apparently, they agree. The results of numerous experiments which I have been making for a different purpose appear to me to warrant the conclusion that organisms which develop into such as have all the characters of *sarcina ventriculi* are a constant, if we cannot call them a normal, occurrence, in the blood of man and the lower animals; and that the name *sarcina sanguinis* would indicate more truly the natural seat of these organisms. In reference to human blood, I find I have been anticipated by Löffler (*Wien. Med. Fahr.*, 1871), who by a different method has succeeded in developing organisms like the *sarcina ventriculi* from the blood of several healthy individuals. That his observations are correct, my own experiments, both in reference to human blood and the blood of various lower animals, will fully show. In the case of rabbits, cats, dogs, and frogs, the special subjects of physiological experiment, I took sufficient precautions to ensure against introduction of organisms from without, by allowing the blood to run directly from the carotid or other large vessel into tubes (previously raised to a white heat), which were then immediately sealed; or into flasks similarly prepared and stopped with cotton-wool. Human blood was introduced into similar tubes

from a slight incision in the arm or hand, previously carefully cleaned. The blood was then placed under a temperature of 100 deg. F.; and in every instance, at the end of a week or ten days, and in some cases examined first at the end of two months, immense numbers of beautifully formed sarcinæ were found. In the frog's blood, as well as in human blood and the others, the sarcinæ, as to size and otherwise, presented exactly the characters of sarcina ventriculi. Blood taken from the arm of a patient at the height of enteric fever gave the same results. No other organisms were observed. In all these specimens, there was entire absence of putrefactive changes. This would of itself sufficiently establish the fact of freedom from external contamination.

Though the same precautions against contamination could not be taken with the blood of the sheep and ox, sarcinæ were developed in a similar manner. With the occurrence of putrefaction, however, they appeared to become disintegrated, or at least were less easily recognised. I have never observed fully formed sarcinæ in the circulating blood, but have constantly found numerous refractive granules, single or in pairs. From these, as has been observed also by Lortorfer, the sarcinæ are developed. Their previously doubtful nature is thus satisfactorily explained.

Though a special examination and cultivation of the blood of every animal would be necessary in order to say with perfect certainty that they exist in every case, sufficient grounds have been shown that they only require to be looked for.

True sarcinæ have never been found otherwise than in the bodies of animals or their excreta. The real source of sarcinæ, therefore, in the various circumstances in which they occur, is naturally to be looked for in the blood itself. That they do not occur in mere transudate liquids, the entire absence of them in hydrocele fluid cultivated in a similar manner would seem to show. It is probable, therefore, that their evolution is due to direct egress of the germs from the vessels with the blood itself. This explanation is quite in accordance with the facts of the cases related, in so far as they have been fully recorded. In the stomach, they are usually found with perforating ulcer or other diseased condition of that viscus. Their occurrence in the urine has always been associated with disease of the kidneys or the bladder. In the cerebral fluid, in Jenner's case, they occurred along with blood-corpuscles. Their occurrence in gangrenous cavities can easily be explained in a like manner.

In all cases there have been favourable conditions in hollow viscera or cavities for the development of the sarcinous germs, directly carried to their surface by effused blood.

The blood in which the sarcinæ had developed to an enormous extent always retained its alkalinity. There was no sign of putrefaction or fermentation, and no gaseous evolution. Sarcinæ directly introduced into a tube containing boiled Pasteur's fluid standing over mercury, and under conditions favourable to fermentation, caused no such phenomenon.

The above facts will serve to throw light on the nature of sarcinous vomiting. Sarcinæ neither generate acids in organic fluids, nor is their growth accompanied by the evolution of gases. Constantly, however, in the characteristic sarcinous vomit, there are, along with sarcinæ, immense numbers of torulæ and bacteria. So constant is this association, that some have attempted to establish a developmental relation between sarcina and torula. This my own experiments entirely negative. Bacteria and fungi are the great, and, I may say, the only, cause of putrefaction and fermentation.

Sarcinous vomiting is usually associated with pyloric obstruction. We can thus see how ulcer of the stomach or catarrh, slow digestion, and hindrance to the exit of the food from the stomach, offer all the conditions requisite for rapid fermentation through the agency of the organisms mentioned. The consequent formation of irritating acid and gaseous products naturally leads to the ejection of these by vomiting, and along with them the sarcinæ, which, however, are merely in accidental, and not in causal, relation. The beneficial effects of antizymotics, such as sulphurous acid, chlorine, etc., are entirely due to their fatal influence on the fungi, which are the agents of fermentation.

The constant presence of sarcinæ in the blood is a point of great importance in reference to the whole question of contagion and the nature of disease-germs. It throws especial light on a much-discussed subject—viz., the nature of the vaccine particles which are constantly to be seen in fresh lymph. In numerous experiments made by Dr. Burdon Sanderson and myself in reference to vaccine, we found, on cultivation of fresh lymph in suitable organic fluids, that the particles multiplied, and assumed, in many instances, a form resembling sarcina. I have now not the slightest doubt of the nature of these particles, and regard them as the ordinary *sarcina sanguinis*. The conditions of vaccination are just such as would readily account for the presence of these organisms in the developed vesicle. It is more than probable that sarcinæ,

if looked for, will be found in most pathological fluids. The subject only requires further experimentation.

Sarcinæ still remain as mysterious as ever. What is their true nature? Are they parasites, or are they a normal constituent of the blood? are questions on which one might speculate, but which I reserve rather for experimental solution.

CLINICAL MEMORANDA.

MOLLUSCUM CONTAGIOSUM.

WITH reference to the view taken by Mr. Balmanno Squire, and reported by him in the JOURNAL of the 13th instant, as to the origin of this disease, I would remark that M. Hardy (*Leçons sur les Maladies de la Peau*, deuxième partie, p. 96) anticipated his parasitic theory, and described branched tubes containing spores of a cryptogamic plant, which, he believed, were constantly to be found in tumours of molluscum. I have never myself seen any such structures; nor, from the numerous examinations of the contents of molluscum that I have made, am I led to believe that any such are to be found (On the Molluscum Contagiosum of Bateman, *St. Bartholomew's Hospital Reports*, vol. iv, 1868). I may state further that, in the most recent accounts of the contents of these growths given by Virchow (*Die Krankhaften Geschwülste*, Band i, p. 221) and by Dr. Hilton Fagge (*Guy's Hospital Reports*, 1870), no mention is made of any vegetable parasitic elements.

Mr. Squire has, I presume, only committed a *lapsus penne* in describing, in the same communication, the *Microsporion Audouini* as the fungus met with in favus.

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January 20th, 1872.

MOLLUSCUM CONTAGIOSUM.

MR. SQUIRE will be glad to learn that his explanation of the contagiousness of this disease is supported by the authority of M. Hardy. In his well known *Leçons sur les Maladies de la Peau* the French dermatologist, after describing the microscopic appearances he discovered, including mycelium, adds:—"Nous pouvons du reste nous expliquer facilement ce mode de contagion à l'aide des spores du cryptogame que nous avons découvert dans cette affection, et nous aurions pu même à ce titre ranger l'acné varioliform [the name given by Bazin to this disease] dans la classe des maladies parasitaires: si nous ne l'avons pas fait, c'est seulement pour ne pas scinder la description des affections des follicules sebacées." (Deuxième Partie, pp. 99, 101. Paris, 1863.)—P. S.

SIMULTANEOUS DISLOCATION OF BOTH ENDS OF THE CLAVICLE.

A GIRL, aged 13, rapidly growing, of lax tissues, and of a consumptive family, but who had always had good health, while washing the back of her neck with her left hand one morning in September, felt something give way in the shoulder of the same side. I found dislocation forwards of the sternal end of the clavicle and partial luxation upwards of the acromial one. There was very little pain. Both extremities of the bone were easily replaced by drawing the shoulder backwards and downwards, but the double deformity was reproduced immediately the shoulder was liberated. A pad was applied under a figure-of-8 bandage over the sternal end, and the arm was placed in a sling as a temporary measure. To a strap, fastening round the chest, a strap bearing a truss-pad was attached in such a manner that the pad kept the sternal end of the clavicle reduced, the other end of the strap passing over the shoulder and diagonally across the back to the horizontal strap: the wearing of a sling kept the acromial end in its natural position. The patient soon afterwards returned to school at a distance: she is now at home, and I have found the sling has been discontinued some time, that the straps have stretched and are useless, and that the ends of the bone are as mobile as, but not more than, they were when I first saw the patient, but that the sternal end does not become luxated unless the arm is raised, when it nearly always starts forwards. I have modified a hernia-truss so that the anterior pad keeps the sternal luxation reduced in all positions of the arm, while the band passes under the left axilla and the other pad lies between the right scapula and the spine; but this slips out of place. If any reader of this note will suggest a better (non-operative) treatment, I shall feel much indebted. Mr. Erichsen has kindly written me—"There are three cases on record of simultaneous dislocation of both ends of the clavicle":