

has no recollection of any peculiar sensation at the time. This was the only occasion of his spitting pure blood; but the expectoration of smoky mucus, mentioned among his symptoms, commenced about two months before his admission. He has worked up to the present time; but three months ago, after a violent cold, his breathing became short, and has continued so ever since. He has never suffered any pain. His history presented no other symptom worthy of note.

During his residence in the hospital the symptom underwent no change beyond what is mentioned above. He soon grew tired of the confinement; and passed from under our observation.

As the case is not verified by a *post mortem* examination, it does not afford a safe basis for comments on two interesting points in the history of aneurism, suggested by the hæmorrhage and by the dysphagia. I only advance it in connection with the other symptoms especially noticed in an earlier portion of the present communication.

## Original Communications.

### REMARKS ON SYPHILISATION.

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If our memory serve us well, but a year or so ago, Mr. Lee, in a correspondence with Dr. Boeck before that gentleman's visit to this country, threw cold water on the cure effected by syphilisation in a person who returned to England after going through a course of treatment at the hands of the Norwegian professor, surmising that he (the patient) had never had syphilis at all; and the impression pretty generally conveyed was, that persons who went in all innocence to Christiania to be syphilised were treated without due discrimination, and might possibly come away, in regard to having the disease, in a worse condition than that in which they arrived. The same class of objection was put into play at Paris; viz., the denial of the existence of the disease until it was brought into the patient's system by the method of cure; and it was more than once affirmed, that any surviving traces of syphilis were entirely due to the treatment by inoculation as a primal cause. This iteration of the same species of argument in the two countries shows that the springs and sources of prejudice in the human mind are limited, like all things in the kosmos; for it would seem that the illimitable inoculation of the chancre on man is a wholly exceptional and unparalleled fact, of which more anon. But, if the hard sore be not inoculable on a syphilitic soil, and the soft sore be a purely local concern, whence, then, all this pother? for, under accepted views, it would be curious to ascertain what harm could ensue from inoculation of purely local sores. But, to be brief; as well as we can learn at so great a distance, it is the manifestly infecting form of the disease which is prevalent and common in Norway; while the soft chancre, with its frequent accompaniment the suppurating bubo and its readily inoculable virus, is rare there, or at least its incidence seems regulated by fresh arrivals in the ports; and these arrivals vary most capriciously. Hence, at times, to procure matter from such a source is more than difficult, it is impossible. But, ever since the

method of inoculating the hard chancre has been found, the Norwegian physicians employ its secretion, and are thereby rendered independent of any such irregularity of supply as occurred to them when using the pus of the soft chancre. And this, as we believe, is the truth.

And yet it would not seem to be of much use to quote cases and facts to those so well fortified against their appeal. If facts are to be constantly denied, and when they are most convincing remorselessly explained away, where, then, is the sense of observation? Where is the name of science? This reproach was made to Ricord, after writing his *Lettres sur la Syphilis*, that he had made science impossible; that observation was paralysed and tetanised under the benumbing and fatal influence of dogma. The perfecting of dogma seems to have gone on in an increasing ratio ever since that time, until at last it would appear to have reached its acme. To one of the French school, if we say we have inoculated from a hard chancre on the bearer, they point, with a smile, to the very fact of its auto-inoculability as sufficient proof that it was not a chancre of the true indurated type. If we tell them that an infecting sore began in a vesicle or a pustule, they will tell us that it ought to have been a papule, and that a papule is included in the pustule. Dogma is supreme. They cannot conceive that a virulent disease can begin by more than a single elementary lesion; though nosologies (like those of Willan and Rayer) have broken down on that very point. This audacity of assertion has, at least, the merit of being precise, and certainly of being French. If not inspired by truth, it is at least calculated to beget resistance; so that between violent extremes their idea of progress may finally be realised. It might perhaps bring forth ripe fruit enough, if subjected to the mellowing influence of British thought; but, in Henry Lee, we find it interpolated with a certain "obscurantism" of his own. Quoting the excellent pathologist Mr. Simon, he questions whether the limits are strictly defined or definable between ordinary and specific inflammations. Nothing can be more just than the remark of Simon—admirable in point of truth. But let us reason now. Is there any disease with a pedigree like syphilis? As regards exactness, we believe, without a single flaw. If we take away syphilis, where would our notions of contagion be? Where they were in ancient time—involved in thick obscurity. No; in regard to contagion, it is syphilis which has taught us all we know. Mr. Lee confesses that there is great difference between the examples chosen (eczema, etc.) and those specific effects obtained by inoculation of variola, vaccinia, and glanders. He says that, for an inoculation of syphilis to be satisfactory, we ought to get a hard sore in the point inoculated. This observation strikes us with more wonder than all. Is Mr. Lee, then, so experienced in syphilisation as to know that a hard chancre never ensues? But when an indurated chancre is multiple, we know pretty well, or at least it is said so, that the infection was contracted in both, within a little, at the same time, and not one in sequence to another.\* A great master has said that the indurated chancre occurs but once. It was somewhat rash of him, however, to assert so much, as he was liable to be "called to order" by the observation of facts. But, in fine, as the disease unrolls itself, the ribbon is drawn forward from the roll; it does not go back, nor is the pattern often repeated; and this, as it seems to us, is a sufficient reason why in the base of a sore inoculated from a hard chancre there is no indura-

\* See Lee's Description of the Hard Sore. "And if multiple, it is so from the first." Lecture I, On Syphilisation, etc. (*Lancet*, 1866.)

tion, and why it generally has a soft base, differing only, as Mr. Lee has elsewhere said, from the soft chancre, "by being more superficial, not attended with any loss of substance, and not leaving cicatrices", and, therefore, in this respect, "differing materially from those produced by the secretion of an ordinary suppurating sore." (BRITISH MEDICAL JOURNAL, April 22nd, 1865, page 417.)

Mr. Lee cannot rest the point on so narrow a basis as this. The ordinary effect of the inoculation of the hard sore is, except in virgin subjects, a sore with a soft base. The true proof of its success lies in its being the vehicle of a "specific virus"; and Mr. Lee does not in this place go so far as to affirm, in anything like a direct manner, that an ulcer with a soft base, such as we get from inoculation of a hard or indurated sore, carries no infection in its train. He does not as yet impugn the dictum of the master, that the chancre is contained in its secretion. He reserves the point for future discussion, and at present what he does say is this.

"Further investigations as to the dynamical changes by which an inoculable ulcer is developed, are doubtless required, to throw light upon the obscurity which yet hangs about it; but 'I am,' he says, 'tempted to introduce some experiments which appear to bear upon the subject.'" Forthwith he quotes the instance of three scapegrace girls, who inoculated themselves from the stores of Bidekap, and had the luck to escape syphilis. As a simple result, the circumstance is worthy of observation and record. To any one conversant with syphilitisation, this negative fact is far from conclusive as to the general innocuity of inoculated matter; the variety which is found in the quality of venereal matter, as modified by races of men and individuals, perhaps also largely by circumstance, is almost infinite. We have matter which we call superior and inferior\*; and M. Ricord laughs at this distinction in one place, and condones the expression in another. Very often, after being used in long series throughout the hospital, the pus finds a barrier in a single patient, who may accept an inoculation with matter refused by others. In virgin subjects, no doubt it is more or less the same, for we reject the doctrine that "all are equal before syphilis." But there are positive proofs that the pus employed has an infecting character, and that it is the vehicle of a specific virus. This may be found in the case immediately following that which we have already quoted in the treatise of Melchior Robert; it refers to a student, who, as that writer naively expresses it, "wished to verify experimentally on himself, the doctrine of the chancreid, (soft sore) in which I had initiated him, by the bedside." The poor young gentleman paid dearly for his lesson, and was well cured of the trick of experimenting, having had enough of it for that time at least. The case is abbreviated without prejudice to facts.

March 22nd, 1858. A patient with exceedingly large indurated sore on the glans, accompanied by multiple bilateral bubo, had besides an exuberant sore on the lip, also indurated, with submaxillary adenitis. Roseola in patches. Affection dating from two months. On March 25th, Dr. M. Robert inoculated from the penis to the thigh, and again on the 31st, from the pustule produced, and also once more from the penis to the opposite thigh. In both these in-

\* See Rules of Syphilitisation. Communication of M. Auzias-Turenne to the Académie des Sciences, Nov. 17th, 1851. Rule 6: "Not only does pus derived from a person far advanced in syphilitisation show an inferior and decreasing activity when inoculated on that person, but also in a less marked degree when inoculated on another individual wholly untainted by syphilis." Also Rule 35: "The chancres which do not give constitutional syphilis are those produced by a pus of inferior form which are at their first generation: in which case the defect is in the seed."

stances the result was positive; in both inoculations from the penis, ulcers ensued. From the inoculation first made, viz., on the 25th, when it was yet a pustule, one of Robert's pupils inoculated himself on the thigh. The pustule from which he inoculated was on its sixth day of progress, dating from the day of its inoculation. Four days subsequently to this rash proceeding, which dated from March 31st, the young gentleman had a pustule which subsequently spread to a large sore with induration of well marked character, commencing twenty-three days from his inoculation. The sore spread and had nearly closed before another week; but in three weeks time, viz., May 22nd, it had opened afresh, with another sore in its neighbourhood. In both of these, the infiltration of their base was considerable, and they were highly indurated: the inguino-crural region shewed a perpendicular line of multiple bubo. On June 10th, the original inoculation had the dimensions of a five franc piece, and was very hard; there were present multiple bubo, post-cervical adenitis, ulceration of the throat, papular syphilide, etc. At the time of the experiment, this young man had a simple chancre with bubo. He appears to have recovered with a fixed and settled persuasion in his mind, a conviction which he was not timid in expressing, that a chancreid taken from an indurated sore is capable of producing an infecting chancre ("jurant qu'on ne l'y prendrait plus et avec la persuasion intime que le chancreid né du chancre induré peut très bien donner le chancre infectant.")

Now how do the French dualists manage to get over such a case as this? "The chancre with multiple adenitis, from which the inoculation was taken," say they, "must have been a chancre mixte.\* The inoculations on the thighs were perforce simple chancres. Those of the pupil, the subject of the experiment, were also simple, or typical soft chancres. How then came the infection? Some little goutlet of blood must have complicated the inoculation. The blood from syphilitics, Pelizzari proved to be contagious. And this ingredient was the vehicle of the infection. Such is the interpretation they give to it. But, when we consider the great difficulties which attend the inoculation of syphilitic blood, the large proportion of avowed failures, the doubt which hangs about the fact as yet, the *besoin de cause* or hard up condition which suggested the argument, or afterthought (had it not better be called?), we must say of the explanation that it is a little too much for our coarse English wit. The lesson in our eyes is precious, as showing the danger of infection from inoculable matter, and also as proving how hard a Frenchman will fight when he sets his back against a wall.

Now, whether there be a "natural way" of contracting syphilis, or not (a thing we take leave to doubt, for, regardless of human respects, this demon enters in as he may, intent only on his cruel work), we yet hold it for certain and precise, that the consecutive symptoms of the disease give a surer proof of its presence than can be deduced from any induration of the primary sore, such as Mr. Lee makes the lever of his argument to annihilate the evidence of facts like those which we have had before us, and to controvert all that is true, or resembling truth, in syphilitisation. He places in the very front of his battle this induration of the primary sore, as constituting the anatomical sign, the only reliable and positive sign of the specific action of the virus; and advances the doctrine, that an individual cannot be subjected to it a second time. We do not concede to

\* M. Nodet, however, says it was a simple chancre, and adds: "Admire the choice clouding of Melchior Robert." Certainly never did effrontery surpass that of M. Nodet. (*Etudes Cliniques*, etc. Louis Nodet. Paris: 1864.)

the induration of the sore, even when accompanied by its appropriate bubo, the same amount of importance as a means of diagnosis attributed to it by Mr. Lee; for we have to consider that this hardness is frequently wanting in the primary infecting sore; and often, when it is present, it cannot, he says, be detected. We do not find in it that description of character which corresponds to the botanical peculiarities by which one plant is known from another, as, for instance, a rose from a myrtle; which stamp of character in plants is required to be present and discernible, or at least not fugitive or fortuitous. We perceive, however, in the argument before us, two elements concerned: first, a virus which is extraordinary, of exceeding potency and penetrative character; and this may be likened to the seed. The soil is the human body, which becomes vitally affected; and this affection, or reaction of the body, we call physiological, or better, pathological (as to "dynamical", we know not what it is, unless when a man drags in a strange word head over heels). Now this affection of the body is in an onward series, from hour to hour and day to day, without a halt as yet; and it dates from the very beginning of the infection. Nor is this series like a chain with equal links, or even as a flat alluvial plain; but rather as a gradation of steps, or as the uneven ground of a mountain ascent, the soil varying as we advance. And the product of the soil will vary too; for the product of the later period differs from that of the period which precedes, and which soon is left behind; and this is why an inoculation from a hard sore results in a sore without induration; though in direct succession to the former, it carries the virus still.

But that we may not be judged of as slight and inconsiderate, as never giving a thought to the natural form of things, but weaving cobweb dreams out of the films of fancy, we will note down in this place one of those axioms which were the first recorded observations in this path of discovery. The 49th rule or axiom of Auzias-Turenne, in his letter to the Academy of Sciences, Nov. 1850, runs as follows:—"When the chancres made upon an ape have a few days interval between each, the induration does not always show itself in the seat of the first chancre, but not seldom we find it in the second chancre or in the third. But the occurrence of the induration is wholly referable in point of time to the existence of the first chancre, so as to accompany the second or third in the first day of their development. A pustule of inoculation in one of our patients might, by induration, be followed in the same way."

Let the vain theories of Langlebert go. We see the narrow basis on which rests the artificial system of the French; viz., on infection by the syphilitic blood, a *Deus ex machina* they are always calling to their aid. Yet, when we remember how insignificant in number are the cases of success from this source in a large body of experimental results—and never are they positive, but when the blood has been applied over a very large extent of surface—we are justified in refusing this mode of explanation, not only in the case before us, but also in the cases of vaccination at Rivalta and elsewhere, as quoted by Mr. Lee in his work on syphilis—cases once more brought into the field of discussion by the experiments of William Boeck with the vaccine matter of syphilitic infants mingled with their blood. In all such difficulties, there is a straightforward way of looking at a thing, which should take precedence of what is far-fetched and subtle; and we say that there is a vast difference, and one which cannot but be discerned, in the force and quality of syphilitic matter; and that it appears to us that the quality

of the matter at Rivalta was exceptional, and that its power was intensified by repeated transmissions in a virgin soil. This difference is fully intelligible in a disorder so many times called Protean; and this variety in the matter also serves to explain why the girls in Bidentkap's *clinique* were not affected by the disease, and it explains other circumstances not otherwise easy to be understood.

When the dogma of the non-contagiousness of secondaries fell, and the year 1859 witnessed its last parting sigh—it is now, as M. Ricord once said of syphilisation, thoroughly dead (*bien morte*)—the exclusive and almost fetish worship of the chancre was by the same act, and at the same hour, brought to an end. M. Ricord put his hand to the deed which authorised his dethronement.\* No longer he reigned a magnificent king. To recur to the language of the worthy Velpeau, used long before, from henceforth his school became identified with the setting, and ours, which is the school of observation, moves onward with the rising sun.†

\* In this year (1858), as a result of many scandals, by direction of the minister, a committee of the Academy of Medicine was appointed to decide on the fact of contagion by secondaries; and, being a member of the committee appointed, M. Ricord had to record his renunciation of his long cherished doctrine, on the 24th May, 1859.

† See speech of M. Velpeau, on the debate on Contagion of Secondaries, in Académie de Médecine, 12th October, 1852.

## FRACTURE OF THE HUMERUS BY MUSCULAR ACTION.

By CHARLES TERRY, Esq., Newport Pagnell.

THE patient, J. C., came to me about three months ago. He is sixteen years of age, strong, tolerably robust, and showing the very reverse of a rickety diathesis. He had just been trying how far he could throw a cricket-ball; and, whilst he was in the act of throwing, his arm dropped. He exclaimed, "My arm is broken," and came to me. On examination, I found a fracture at the junction of the upper with the middle third of the humerus, attended with perfect inability to move the arm, slight displacement, great pain, and considerable crepitus. Had there not been a witness, who corroborated this account of the accident, I should have been inclined to doubt my patient's statement. The fracture was put up in the ordinary way, and is now well. The callus can be plainly felt, shewing the point where the fracture occurred.

This case I have considered worthy of being recorded, as some surgical authorities do not refer to muscular action as a cause of fracture of the humerus. In other standard works, however, such as those of Mr. Erichsen and the late Professor Miller, sudden contraction of the muscles of the arm is mentioned as producing fracture in some cases. Mr. Erichsen, indeed, in his *Science and Art of Surgery*, says that fractures of the shaft of the humerus "are more frequently the result of muscular action than are those of any other bone."

OCURRENCE OF ERGOT ON DIFFERENT PLANTS. Dr. Kühn has observed the ergot to occur on twenty-eight graminaceous and five cyperaceous plants growing in different positions, and on soils of opposite qualities. He comes to the conclusion that moist bad soil and low position have little to do with its development. The fungus (*Claviceps purpurea*, Tulasne) produces in from twenty to thirty capita upwards of a million spores, which readily germinate. He reared the ergot from spores placed in flower-pots. (*Year-Book of Pharmacy*.)