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THESES AT THE PARISIAN CONCOURS.

[See page 90.]

ARGUMENTATIONS ON THE THESIS OF M. VELPEAU, BY MM. LISFRANC AND BERARD.

Remarks of M. Lisfranc and Replies of M. Velpeau.

M. LISFRANC.—In page 91 of your thesis, speaking of the doctrines of M. Fleurens, and the manner in which compression of the brain is produced, not merely by the presence of a certain quantity of fluid in the cavity of the skull, but by the indirect influence of the osseous case, reacting on the extraneous body, you say that M. Serres (*Ann. des Hopit.*, t. 1, p. 250) has omitted to take this peculiarity into account, and hence concluded that fluids effused within the skull were incapable of producing compression, and that the trepan, in this respect, was almost useless. I think you have here fallen into an error, and attributed to M. Serres ideas which he never had. M. Serres was perfectly acquainted with the influence of the osseous skull in giving rise to compression, for he always took the precaution of closing the orifice by which he introduced the fluid, and thus placing, as far as was possible, the skull in its natural condition.

M. VELPEAU.—The observations and experiments of Serres all tend to prove that the effects of effusion into the cavity of the skull are not dangerous, and that a considerable quantity of blood may be shed between the membranes, without causing any derangement or trouble in the functions worth noticing. He attributes many of the symptoms of compression to alterations of the cerebral substance, and neglects altogether to take into consideration the reaction of the skull on the effused matter.

M. LISFRANC (*Interrupting*).—But I say yes; for why did he close up the opening which he made into the skull, if it were not for that object?

M. VELPEAU.—Serres closed the opening merely to prevent the issue of the fluid which he injected, not to represent the integrity of the skull, for he nowhere speaks of the resistance offered by the bones as the principal determining cause of compression. Had he done so, he would have agreed with other writers, and it would not have been necessary for M. Fleurens to refute his doctrine in the manner he has done.

M. Lisfranc, after some discussion on this point, attacked the author of the thesis for having spoken too lightly of trepanation as a surgical operation, which he says (p. 143) “presents no difficulty whatever.”

There are few operations in surgery more easy or more simple, and without ignorance or awkwardness the surgeon can make no mistake of consequence." I do not (pursued M. Lisfranc) regard the application of the trepan as so simple or easy an operation as you here represent it to be. While I was employed in giving lessons in operative surgery I had frequent occasion to see the trepan applied by surgeons and physicians who came to me from the provinces, and the mistakes made were much more frequent and serious than you seem to think of: for example, I have more than once seen the dura mater opened.

M. VELPEAU.—I do not say that trepanning does not require some degree of dexterity, but I maintain that it is an easy, a very easy operation. You have here no long or laborious dissection, no very important points to avoid, no arteries to take up, &c.; you have simply to divide a certain quantity of bone. As to the injury of the dura mater, a surgeon must indeed be very awkward to wound it; besides, division of that membrane is not a very grave accident; by no means to be compared with the division of a main artery or nerve, &c. in various operations on the extremities.

M. Lisfranc answered to this—in the short, caustic, almost contemptuous manner which he so often employs—I regard injury of the dura mater as a much more severe accident than you do. M. Lisfranc then referred to the uncertainty of opinion which characterized the thesis, and to some contradictions, even, which required to be rectified; thus in one place, where fissure is spoken of, the author says, "if the fissure be large and the blood escape freely, trepanning may be deferred when the symptoms of compression are not very severe; in opposite circumstances the trepan should be applied to the exclusion of all artificial separation;" but in page 53, M. Velpeau distinguishes some cases of effusion in which the trepan is not absolutely necessary:—"Thus, when the fracture presents some slits gently separated from one another, the interval of the fragments may be increased for the moment, and the issue of the blood, if it still remain fluid, be favored." This, said M. Lisfranc, is a manifest contradiction; in one place you say the trepan should be applied to the exclusion of all separation, and in a subsequent passage you recommend what you have before condemned; the words are clear.

M. VELPEAU.—In the first passage I spoke of the *permanent* separation of fragments as practised by Giraud. In the second I refer to a *temporary* separation, which is quite a different thing, and say that when it is easy to separate the bones for a short time, it is better to do so than to trepan, but if it were necessary to keep the fissure open for several days, I regard it as a bad practice, and would sooner trepan.

M. Lisfranc again pointed out a contradiction. In page 244, you "trepan for all effusions, wherever situated and of whatever nature;" while in page 246, you say the trepan is not indicated when the effusion of blood or pus is diffused. Now I should like to know how you distinguish or are able to tell whether an effusion of blood be diffused or circumscribed, whether pus be infiltrated or collected in an abscess. You may have an effusion covering half the hemisphere, or merely extending for six or eight lines in diameter; here are two cases which we have no means of distinguishing by the symptoms, and in general the two species

of effusion are liable to be confounded by the best surgeons. I have myself seen many cases in which, were we to follow the symptoms given in books, you would have said the effusion was diffused, but on opening the body after death we found it perfectly circumscribed.

M. VELPEAU.—In circumscribed effusion you have certain local symptoms connected with the point of the brain which is the seat of the injury ; these are generally sufficient to show that the effusion is confined to a small space. When the fluid occupies a larger surface and is diffused, you have paralysis, &c. and all surgeons point out the difference between the two forms of effusion. With respect to the cases to which you have alluded, when a great part of a hemisphere is covered, the fluid is either laid on in a thick or a fine layer ; in the latter case there is no paralysis, no compression ; if the layer be thick, these symptoms of sudden compression are manifested ; this shows how we can distinguish certain forms of effusion. When the fluid occupies only half a hemisphere I regard it as circumscribed, though you do not.

M. Lisfranc did not consider any effusion circumscribed unless it was collected into an abscess (foyer) ; besides, he could not allow an effusion of fluid extending over half a hemisphere to be circumscribed.

Remarks of M. Berard and Replies of M. Velpeau.

The length to which we have already carried our report will permit us to give the remaining argumentations only very briefly indeed.

M. Berard began by asking the candidate what symptoms distinguished fractures with depression from sanguineous tumors under the scalp (*bosses sanguins*, we did not exactly catch the word) ; one requires the trepan, the other not ; he did not find them distinguished in the thesis.

M. Velpeau would ask in reply whether the speaker was unable to distinguish them by the symptoms detailed ; if not, let him state in what respect there was an omission or deficiency, and he should have an answer.

M. Berard, after some remarks on the application of the trepan in cases of depression, recurred to the objection already advanced by M. Lisfranc, on the diagnosis of circumscribed and diffused effusions, which he said M. Velpeau did not distinguish in his thesis sufficiently well.

M. Velpeau did not think he was bound to enter into these particularities, he had merely to treat the symptoms in a general manner ; if he had a patient before him, it would be a different thing ; then he might lay down the distinguishing symptoms.

M. Berard opposed to this that as he had entered into the symptomatology of compression, contusion, commotion, &c. of the brain, he should have equally spoken of the distinguishing symptoms of diffused and circumscribed effusions, particularly as the treatment with regard to the trepan was so different. Besides, said M. Berard, there is a marked indecision of opinion running through your whole thesis : at one page you say one thing ; a few pages further on you differ from yourself ; and if we read on we soon find an opinion quite opposite ; here is a proof :—in page 96 you say—“ Contusion of the brain is a frequent complication of wounds of the head,” and you ask if the trepan may not be applied to

prevent the accident which follows it (*à titre de moyen preventif*) ; in page 114 you recommend the trepan for contusion, when announced by a certain set of symptoms, as dull pain, &c. ; and again, in page 245, you say, "we trepan in contusion of the brain, with symptoms of supuration or paralysis." Here are three different opinions ; first, the trepan to prevent accidents of contusion ; second, you trepan in all cases of contusion, on account of the accident itself ; third, you trepan in contusion only where there is paralysis or suppuration.

M. VELPEAU.—In page 245 I spoke of contusion and the trepan in a general manner ; at page 114 I say we may ask whether the trepan be proper or not when certain symptoms of contusion, such as dull pain, a sense of pressure, &c. are felt.

We need report this debate no further. It yielded nothing more which possesses interest for the English reader. We have still, however, something to add which is necessary to render our record of the concours complete,—a record which is unique of its kind in our own language, and calculated, we believe, to yield much gratification, in the perusal, to the profession. We are enabled to conclude our notice this week, by announcing that after a doubtful contest between M. Velpeau, M. Blandin, and M. Sanson, the first was chosen definitively, and that

The nomination of M. Velpeau to the Chair of Clinical Surgery took place on Wednesday, August the 6th, at five o'clock in the afternoon, M. Velpeau having obtained seven votes, and M. Sanson five.

REMARKS ON FEVERS, WITH CASES.

BY JOSEPH COMSTOCK, M.D. OF LEBANON, CONNECTICUT.

[Communicated for the Boston Medical and Surgical Journal.]

[See page 128.]

ISOLATED cases of fever, and indeed of all other diseases, if they have no general bearing upon the healing art, and if they stand detached from principles, precepts and inferences which may be applied either to other cases or to the prevailing diathesis or epidemic constitution, are of little utility.

To illustrate this subject still further, I will briefly refer to a case which occurred whilst I was residing in the State of Rhode Island, and during the reign of typhous fever there, and which has never been published.

Mrs. C., a lady of distinction, became my patient, Dec. 1814, having been pronounced by her former physician in a state of confirmed hectic. She had night sweats, cough, expectoration, swelled ankles, and a pulse of 140 in a minute. This extreme quickness of pulse, strange as it may seem, was the only symptom which gave me any reason to hope that her hectic was not confirmed. My reasoning was, that this *extremely* quick pulse partook more of the reigning and all-controlling epidemic, than of consumption. She recovered, and in the autumn of the next year became pregnant ; and I have no doubt that the atmospheric cause, which in the village where she lived produced malignant typhus, converted her