which he saw in the year 1832, in the person of an old soldier who had been wounded by a bullet in the wars of the Empire—therefore at least seventeen years before. The projectile had lodged apparently under the scapula, after having just grazed the clavicle. The man's health was perfect, except that he was troubled with a continuous buzzing or snoring noise, so loud that it could be heard at a distance of six inches from the neck. It extended over the axilla and side of the neck, and was accompanied of course by a strong thrill. The pulse in the arm was weak, and he said that he felt the cold more in that hand; but otherwise he had nothing to complain of.

It seemed probable in this case that the subclavian was the seat of the injury. In M. Larrey's well-known case the communication was also between the subclavian artery and its vein. But in all the other five cases given by Robert the carotid artery and the jugular vein appeared to have been wounded; although in none of them was any post-mortem examination obtained. Indeed, in none were there any formidable symptoms. All of them had been produced by sword-thrusts, and in one the symptoms had existed for twenty years; yet the patient was in perfect health, nor did the tumour increase even in the accesses of rage to which he was accustomed to yield.

The diagnosis rested in all these cases on the peculiar bruit and thrill, often very perceptible and annoying to the patient when he lay down. There was in most of them a tumour, formed apparently by the dilated vein, for in one case it disappeared altogether when the patient drew a long breath, returning gradually; and M. Robert speaks of dilatation of the whole venous system of that side of the head and face, though his notes do not particularise it in any of the cases. There was no evidence of any aneurismal tumour lying between the artery and veins in any of the cases, nor was there in any of them any alarming symptom which would have justified surgical interference. In one indeed there was at first very great ecchymosis and some amount of dyspnoea; in another (a young man wounded in a duel) extreme agitation and delirium, but bleeding and general treatment relieved these symptoms in both instances.

Somewhat resembling these cases is the one which is described by the late Mr. Porter,* in which a young girl had a "varicose aneurism" from a stab with a pair of scissors seven years previously. The disease affected the external jugular vein, and (as it was conjectured) the internal carotid artery. The swelling "became very large and prominent when pressure was made on the vein below it, so as to interrupt the current of blood, and it then became excessively painful, and exhibited the usual thrilling sensations both to the finger and ear; but when looked at from the mouth a strong and continued pulsation, together with considerable tumefaction, was obvious to every eye." "It was only matter of conjecture," says Mr. Porter, "that a communication had been established between the external jugular vein and the internal carotid artery, with the intermediate existence of a varicose aneurismal sac." No treatment was attempted, nor is anything said to show that the girl suffered any grave inconvenience from the disease.

The only case I have as yet found where, instead of a mere arterio-venous communication, there has been a distinct tumour—a varicose aneurism—as proved by dissection, is recorded in the *Gazette Médicale de Paris*, 1840, by M. Joret, and it will be noticed that the progress of the case was very different from those hitherto quoted—pain in the head, denoting probably venous congestion, being a marked symptom from the commencement, and being gradually succeeded by more definite symptoms of degeneration of the brain, slowly advancing to a fatal result from softening of the cerebral substance. The patient survived the injury (a bullet-wound) more than two years, and after death an aneurismal sac was found communicating with the internal carotid artery near the skull, and the internal jugular vein. The cerebral veins were dilated and the brain-substance softened. The patient had, from a very active man, become epileptic and idiotic before death.

The contrast between the sure, though slow, advance to a fatal termination in the case last quoted, and the harmlessness of those in which the arterio-venous communi-

cation has been uncomplicated by the development of an aneurismal sac, appears to correspond to what we know of the relative gravity of aneurismal varix and varicose aneurism in other regions of the body. The uselessness and impropriety of any interference in those cases of arterio-venous aneurisms in the neck where there are no dangerous symptoms hardly needs any demonstration. It is well stated by M. Robert. But there may be others, more analogous to Joret's case, in which the gradual advance of the symptoms points strongly to the necessity for some interference, if possible, before it is too late to save the patient's intellect or life. In such instances, if the foreign body which caused the wound is lodged, I see no alternative except to cut down on the tumour, open it with all imaginable precautions, having the circulation commanded below, and above also, if the position of the wound allows, and tie both the artery and the vein. The foreign body would of course be removed in the course of the operation. Had the tumour in M. Joret's case been more accessible, this course would doubtless have been recommended. In those cases where there is no evidence of lodgment of the foreign body, it is possible that the combination of direct with indirect pressure might be successfully employed, which Vanzetti has introduced in the treatment of varicose aneurism of the brachial. If the venous thrill and murmur could be suppressed by direct pressure on the tumour, it would be a proof that the venous orifice was controlled; and in that case simultaneous compression of the lower part of the artery might be trusted to complete the cure.

OBSCURER ALLIANCES OF TYPHOID.

By J. P. H. BOILEAU, M.B.,

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IN THE LANCET of the 21st November, 1868, it was stated by the editor, in his review of my article on "Fever in Malta," published in the Army Medical Reports for 1866, that to complete our knowledge of the gastric remittent fever of the Mediterranean, we only wanted pathological information. In Barbadoes a fatal case of fever was treated by me which forcibly recalled to my mind the experience I had had in Malta, and the cases of fatal typhoid recently published in THE LANCET have revived in my mind the whole subject. It is very desirable that the nature of that obscure disease described by me as the "long" fever should be clearly established, and I believe the following case will help to elucidate the subject.

A young sailor of the Royal Navy was admitted into hospital at Barbadoes. A detailed account of his illness would be merely the repetition of a subject already exhausted by Dr. Marston, R.A., Dr. Chartres, 100th Regt., myself, and others who have observed and studied Mediterranean remittent, so similar was the sailor's case to that fever. I will therefore omit my notes up to the thirty-fourth day of his illness, the thirty-second of observation.

Thirty-fourth day.—Progressing favourably; tongue perfectly clean and moist; bowels regularly moved twice daily; he complains only of debility; temperature has been as high as 103.2° F. (evening of ninth day); abdomen as a rule has been hot and dry. I have not once satisfied myself as to the existence of any rose spots. The boy's age is nineteen. His tongue has never had the slightest appearance of the typhoid state, nor has he had typhoid dejections. His temperature for the past three mornings has been 99.2°, and under, but—significant fact—his evening temperature has not been below 100° since the commencement of his illness.

On this day he was to have had a little chicken, so well did he appear to be, and so anxious was he to have some; but the order I countermanded, and was assured that he did not get any. I am particular in recording this matter, because if he had got the chicken we could not have done otherwise than attribute to that solid food the untimely issue of the case. On the following morning his temperature was 103.8°, and on the following evening 104.2°. In two cases of fever I have seen an equally sudden and great elevation of temperature follow the ingestion of jelly.

* Dublin Journal of the Medical Sciences, vol. xvii., 1840, p. 95.

Thirty-sixth day.—He has relapsed a second time (see diagram). He has not diarrhoea; tongue remains clean; he has not any pulmonary complication.

Thirty-seventh day.—As usual, no complaint.

Forty-second day.—With the exception of his temperature he appeared to be progressing favourably until 3 p.m., when he was seized with a sudden and most violent pain in the abdomen. At 5 p.m. his axillary temperature had risen

The upper line shows the 5 p.m. temperature; the lower shows the 9 a.m. temperature.

to 105.2°, pulse 120, and was in great agony; abdomen extremely tender. At 7 p.m. he was considerably relieved, having taken ninety drops of Battley's sedative solution.

I need not further detail the symptoms of peritonitis following perforation. He survived for no less than six days after that fatal event. The autopsy, made six hours after death, showed the peritoneum in a state of universal inflammation, in several places adherent; the cavity containing a quantity of sero-purulent fluid; the intestines healthy, with the exception of the lower portion of the ileum, where there was attenuation and superficial ulceration. One ulcer, having the area of a threepenny piece, had penetrated through the peritoneum, and here there was a scarlet peritonitis, from which, as a centre, the general inflammation extended.

Now, the indisputable facts are these: that in a tropical island I have had one case of fever under my care which so closely resembled that known as the gastric remittent of the Mediterranean that had it been under my care in Malta it would have been described as such; that the subject of this fever died about the fiftieth day of his illness from the effects of a perforating ulcer of the lower fifth of the ileum, and that I did not see any reason to doubt my diagnosis of the case as typhoid to be correct.

From my experience in Malta, I was led to a conclusion which I expressed in my paper on "Fever" in the Army Medical Reports for 1866—viz., that quinine might be banished from the island without much harm. Since that time I have had many opportunities of observing the nature and course of typical, malarial, periodic fevers, demonstratively amenable to quinine. Remarks on those cases were published in a medical publication on July 14th, 1869. I have also had the opportunity, during a residence of more than three years in the West Indian Islands, of observing a large number of cases of fever in their climate. I may, therefore, claim to be now in a better position to judge of the nature of the fevers I observed in Malta six years ago than I was then. And I cannot but conclude that if the *gastric remittent* is not the *typhoid* with which we are familiar in this country, it must be some variety of it; and, if this is the case, it illustrates the truth and the necessity of the warning given to us by the Professor of Military Medicine in the Army Medical School at Netley, whom I well remember cautioning us not to adopt the evil practice of calling fevers by local names. As to the fever being in any way malarial, I must still maintain the view I have hitherto held—viz., that it is not so in the slightest degree. And I now attach a greater force to the words of the editor of THE LANCET than I did when first I read them. His words were these:—"After reading Dr. Boileau's contribution we confess to having strong doubts as to whether some of his cases were not in reality examples of enteric fever."

It follows that in the treatment of the long gastric remittent fever of the Mediterranean we must ever bear in mind the complications which may arise in *enteric* fever. We should not deceive ourselves by the idea that we have a fever to deal with which is amenable to quinine. We should be scrupulously careful in the administration of diet; and not fail to mark the temperature of the patient at least every evening, and by its indication to "govern ourselves accordingly." I need scarcely say that I fully endorse Dr.

Latham's opinion, that it is only after the evening temperature has remained on at least two successive days below 99° F. that we can be sure the ulcers have healed, and that solid food may be given without risk. I may add that I do not remember ever having seen a relapse after such a condition.

As a result of many thousands of observations on the temperature of the white man in various climes, with shade temperatures as high as 105° F. and as low as 20° F. below zero, I have been led to the conclusion that the normal axillary temperature of the healthy adult is between 98° and 99° F.: in fact, it seems to me that this temperature is as valuable and as constant a characteristic of his human identity as is the structure of his bones. The fact may be already established; but as the idea that human temperature is one degree higher in the tropics than elsewhere has been widely entertained and supported by high authority, I here mention the fact that I have observed.

Templemore.

A CONTRIBUTION TO THE SURGERY OF NATURAL AMPUTATION AND SPONTANEOUS FRACTURE.

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(Continued from p. 225.)

Among the more specific, diathetic, or constitutional causes which lead to the degeneration here contemplated—for degeneration it must necessarily be—are lues venerea, struma, cancer, and scurvy, and of these cancer and syphilis are by far the most frequent causes. Whether that ill-defined or less understood condition called *mollities ossium* is due to, or in any way associated with either of these, it is not here necessary to inquire, but its causative influence is unquestionable, and, where it obtains, the whole skeleton is subjected to its agency. I have read of cases which would appear to be due to the long-continued pressure or disuse of a limb, and such mild growths as enchondroma, exostosis, &c., have been known to produce it. The convulsive struggles of epileptic, hydrophobic, or delirious patients have occasionally tended in the same direction, and one of the cases noted below was clearly due to the irritation of chloroform. But cases sometimes occur without any apparent reason, and for which it is not always possible to find an adequate explanation; for, as Mr. Paget observes, "to degenerate and die is as normal as to be developed and live." The expansion of growth and the full strength of manhood are not more natural than the decay and feebleness of timely old age: not more natural, because not more in accordance with constant laws, as observed in ordinary conditions. As the development of the whole being, and of every element of its tissue, is according to certain laws, so is the whole process regulated by which all that has life will, as of its own working, cease to live." Such is undoubtedly the case; and though bone is, for obvious reasons, less liable to the degenerative process than other more