those other changes of the skin, bronzing, induration, and ulcer, were in much more constant correspondence than with varicosity.

Now, in 2 cases external varicosity existed alone: none of the sub-aponeurotic structures were in anywise diseased or disturbed. In the other 7, with external varicosity there were other complications, but no other disease of the skin. These complications were as follows:—In 1 there were beaded strings of coagulum in the varicose veins; in 2 there was dilatation of the intra-muscular veins; in 3 the deep trunk veins were dilated; in 5 there were coagula in the deep trunk veins, in 2 instances obstructive; whilst in 2 others the arteries were slightly diseased.

But this statement does not represent the condition of the parts logically. It only shows that, *with superficial varicosity, there are other serious lesions affecting both arteries and veins, deep and superficial*, which cannot be ignored in discussing the relation to it of associated diseases.

Now the several conditions of the various vessels in these cases were differently combined; and it is to these especial and individual groupings that the greatest amount of importance must be attached. Although coagula were found in the trunk veins in 5 out of the 9 cases, these were, as I have said, *obstructive* in 2 only; and of these cases in 1 only did the obstruction affect the deep trunk veins, the obstructed vein being in the other case but an *intra muscular branch*. In 1 only was there *thrombose obstruction of the deep venous trunks with superficial varicosity*; and, be it further noticed, in this, as in the other thrombose case, as well as in 1 beside, in which an old clot in the deep veins had ceased to be obstructive, there was co-ordinate dilatation of other of the trunk comital veins.

It appears, then, that in no one of these cases in which varicosity existed alone, was there coincident obstruction of deep trunk veins, although there were those especial signs of derangement of these vessels and their contents in most instances, and of the arteries as well in 2, as would lead to the conclusion that the general circulation had been subject to very considerable and long-standing embarrassment.

In relation to ulcer the bearing of these remarks is this: that varicosity may exist with mere *embarrassment of the whole vascular system of the limb, and yet without ulceration*.

I shall now go a step further, and give a summary of the condition of the vessels of the leg in 10 cases, of which varicosity was associated with bronzing and induration of the skin in 1 case, and with ulcer and discoloration in the other 9. The former I shall not further consider. It arose from capillary congestion, and was associated with obstructive cardiac disease and great dilatation of the deep venous trunks.

In 3 of these coagula were found in the trunk of the *external saphenous vein*. (I may here explain that, from a large number of dissections, I have come to the conviction that the anatomy of that vessel, as given in works, is, in one most important point, incorrect. We have all, I think, been taught that this vein takes its course obliquely along the outside and back of the leg to the popliteal space, where it penetrates the fascia to reach the popliteal vein. In a large number of dissections that I have made for the purpose of clearing up this point, I have not found this course taken by the vein in a single instance. It invariably pierces the deep aponeurosis below the calf of the leg, immediately behind the junction of the gastrocnemius, and completes its course below the aponeurosis, and sometimes even within one of the bellies of the muscle itself. It was in portions of the external saphena adjoining this foramen that these clots were found.) In 6 there was obstruction of the deep trunk veins by coagula. In 3 there was dilatation of the deep trunk veins. And in 6 there was arterial disease, more or less severe.

I must again collate these cases, so as to present the distinct groupings of the several morbid phenomena.

In 1 case the external saphenous vein was obstructed, with disease of the anterior and posterior tibial arteries. In 2 other instances, in which the external saphenous veins were obstructed, there was also obstruction of the deep trunk veins; with, in 1 of these, dilatation of deep trunks, and arterial incompetency. In 3 others there was deep trunk obstruction, with like arterial incompetency; with, in 1 of these also, dilatation of deep trunk veins.

(It will be understood that in speaking of dilatation and obstruction of deep trunk veins in the same leg I am speaking of different conditions of two comital veins.)

In one case, already referred to, in which a very large proportion of the integument of the leg had been eaten away by deep ulceration, all of the main arteries were found coated with earthy matter, and had become, as it were, functionally

obsolete. In this case, which Mr. Hopgood injected with wax for me, I was interested to observe the *unusual vascularity* of the remaining tissues as far as the arterial system was concerned. The diseased vessels sent off an unusual number of healthy branches, which supplied the tissues which the ulcer had spared, and these anastomosed freely with each other in the course of their ramifications.

Now the general conclusion from the second series of dissections is that, *unlike the former*, venous obstruction, especially of trunk veins, superficial and deep, was, in severe cases, found to be associated as well with arterial incompetency. Contrasting this with the conclusion arrived at from the details of the first series, and remembering that there was no ulcer in the former, but ulceration with bronzing and induration in all the latter cases but one, and that an unimportant one, it is impossible, I think, to avoid the further inference that *ulceration is not a direct consequence of varicosity, but of other conditions of the venous system, with which varicosity is not unfrequently a complication, but without which neither one of these affections of the skin is met with*—conditions which involve obstruction of the trunk veins, deep and superficial, either from impediments on the venous side or incompetency on the arterial, or from both causes combined.

I think then that, pathologically, the doctrine of the “*varicose ulcer*” does not hold water; that, to reiterate my conclusions, ulceration, when it exists with varicosity, but without other complication, is a coincidence, and not a consequence of the vein disease; that, when associated with induration and bronzing of the skin, it is the direct result of serious obstruction of the venous trunks, and this alone, whether associated with varicosity or not. The converse cannot, however, be alleged—namely, that serious embarrassment to the circulation through the trunk veins is *invariably* followed by these affections of the skin. The anomaly, which does not admit of explanation here, does not invalidate the foregoing conclusions.

(To be concluded.)

ON THERAPEUTICS AND ON THE NATURAL HISTORY OF DISEASE.

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AN eclipse of faith is spreading over the face of English medicine, and there is a tendency to disparage the large amount of therapeutical knowledge that has been accumulated; but those who regret this tendency will remember that medicine is not bounded by our line of coast. If some of our most eminent men have no longer faith in medicines, there is no want of that faith on the continent. With regard to France I have watched the variations of medical belief, during the last thirty years, and it will be useful to compare these changes with those progressing in England during the same period. Broussais published in 1816 the first edition of his “*Examen des Doctrines Médicales*,” and by four editions of this work, and by his numerous contributions to the medical press, all burning with passionate eloquence, the doctrine of inflammation, as the cause of all diseases, spread like wildfire; but it almost died out as fast, for although the last edition of his work was published in 1829, when I began the study of medicine in 1834 gastritis was already the standing joke of medical students, and as Broussais had been most virulent in his attacks upon the *polypharmacie* of former ages, when the profession lost faith in bleeding and leeches, they had only expectation to fall back upon. While this was going on, the South of France was somewhat preserved from the new doctrines by the traditional antagonism of the school of Montpellier to that of Paris, and the East by the fact of the school of Strasburg being half German. Even in Paris many sensible men never believed in Broussais, kept alive the good old doctrines and therapeutics, and propagated them in the *Revue Médicale*, which was founded in 1820 by Recamier and Cayol; Michel and Bayles, I believe, founded the *Bulletin de Thérapeutique* in 1831. The most influential conservative leader was, however, Recamier, for he was the teacher of Trousseau and Pidoux, who have done more than any others to spread sound therapeutical knowledge in France, by three editions of their work “*On Therapeutics*,” of which

the first edition appeared in 1836; by their numerous contributions to medical literature, and by the teaching of Trousseau, when, after a magnificent *concours*, about 1835, he took, and held for many years, the chair of Therapeutics in the Paris school.

After taking a Paris degree in 1839, I came to London to walk the hospitals, passing at once from an atmosphere of doubt, scepticism, and therapeutical inaction, to a land where the medical mind was reposing in the undisturbed conviction that it had brought therapeutics to perfection. Had not exorbitant doses of medicine been utterly repudiated? Were there not three national Pharmacopœias to be chosen from, besides the special Pharmacopœia of each large hospital? Had not all this positive pharmaceutical lore culminated in the just published work of Pereira—to be henceforth the bible of British practice? Every medical man I spoke to shrugged his shoulders at the miserable ignorance of therapeutics in the French school; and as I walked round the hospitals I never ceased admiring the wonderful confidence with which drugs were being administered. But even when all seemed so safely settled, there were thinkers already chafing against a system which ignored entirely the all-healing powers of nature, and they found an exponent in Sir John Forbes, who in 1845 published his article on "Homœopathy, Allopathy, and Young Physic." Since then the choice medical spirits of the two countries have been marching in opposite directions: the French gradually acquiring a steadily increasing faith in remedies and adopting our notions, Graves finding an enthusiastic admirer and translator in Trousseau; while we have now lived to see what must disturb the peace of Pereira in the blest abode—we have actually lived to see physicians, in metropolitan hospitals, treat acute rheumatism by mint-water, and boast of having done so. Now that we are all alive to the fact that the operations of nature cure alike the patients of quacks and of orthodox men, the cry is, "the natural history of diseases!" Very good; but it will take considerable time to write afresh the natural history of the most frequent diseases in our latitude, and what is to become of the patients in the meantime? Are we to tell them that we are uncertain whether medicines are useful, useless, or pernicious?

"The natural history of diseases" is a very good cry. It rang in my ears when I was a medical student in Paris. Many wanted to begin medicine afresh, as they had to begin everything else, in the year 1 of the Republic, one and indivisible. It was the reproach of wise men to Louis that he ignored the legacy left us by the great of bygone ages, and was going to begin again to restore medicine, by his own individual efforts. Louis was a quiet gentleman, without a professorial platform; he had neither the eloquence nor the audacity of a successful innovator. He persuaded some that the *numeric method* would save medicine; but he made no *school*, and his efforts did not in the least check the growing confidence in therapeutics.

"The natural history of diseases." A most laudable pursuit, repeatedly followed at different periods of history; but why talk of history to those who have so much to study that they cannot even read the history of their own art? I only profess to draw attention to what occurred in France after the collapse of Broussais, and I can aver that generally in the Paris hospitals *expectation* was the order of the day. Diet, simple tisanes, lavements (simple or medicated), castor oil,—such was the general plan of treatment; sirop de diacode, if the patient did not sleep; leeches to cure local inflammation; moderate bleeding, if it were acute. Of course there might be found a Bouillaud attempting to *jugulate* pneumonia and typhoid fever, and some less known man treating typhoid fever by saline purgatives; but those who want to know the natural history of diseases will find it recorded in Andral's Clinique, and in the works of Chomel, Louis, and many other writers of the same time.

"The natural history of diseases!" I have insisted over and over again that the right way of studying diseases of women was to do so by the light of the natural history of menstruation, showing the way in one of my works. If for thirty years of her life woman is bled every month moderately, and often exorbitantly, with impunity, that should at least teach the present generation of medical men that it is absurd to be afraid of damaging a patient's constitution by taking away ten ounces of blood in cases of inflammation. The perfect innocuity of vomiting, in most cases of pregnancy, will some day teach medical practitioners how foolish it was to give up the use of emetics in many cases of dyspepsia and biliary derangement. If I take the liberty of pressing on others to hasten as much as possible the process by which they are attempting to make up their minds as to the extent

of the powers of nature in the cure of disease, it is for the sake of our patients and for the credit of our art. It is no consolation to men of moderate opinions to know that when the pendulum of the human mind has violently swung in one direction it will assuredly, after a certain lapse of years, swing back with equal momentum in the opposite direction. There is so little rest between extremes; we have only just escaped from the reign of exorbitant doses and overstringent pharmaceutical formalism, and we are now menaced with a rapid return to that slough of expectant medicine which, centuries ago, was rightly stigmatised as "a meditation on death." Only a few years back and we were outrageously frightened by the bugbear of inflammation, and already our utility is often paralysed by the bugbear of debility.

Grosvenor-street, April, 1868.

ON IMPROVED METHODS OF EXTRACTION OF THE CATARACT.

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No. I.

My object in these papers is, to submit to the profession the results of 96 cases of cataract, which I have extracted by a new method; to compare this method with the ordinary flap extraction, and with the other procedures lately introduced by the German ophthalmologists. For a period of five years I tried these various methods in a considerable number of cases, in the public institutions with which I am connected, as well as in my private practice; but during the last three years I have adopted the procedure which I call "modified subconjunctival extraction," and which I find highly encouraging in its results.

In order to avoid a hasty generalisation, and to arrive at a satisfactory conclusion, I have not restricted my observations to the experience obtained within the scope of my own practice, but have frequently visited the different Ophthalmic Hospitals in this country and on the continent, with the view of seeing as many patients as possible at different stages; and it is only after a long study and comparison of the relative advantages and disadvantages of the different methods that I venture now to recommend my modification to others for trial.

To give a connected view of the whole subject, it will be best to submit a short outline of the history of the operation of extraction, and to trace the different stages of its development. It will thus be interesting to see how we have progressed step by step in our course of improvement; how we have turned a hazardous operation—an operation fraught with anxiety to the surgeon and risk to the patient—into one of comparative ease and security.

The history of extraction of cataract can, according to Rhazes, be traced as far back as the end of the first century, when it was performed by Antyllus, and at a later period by Lathyrion. Ali-Abas, in the tenth century, knew also of the operation, and later, Abulkasem, having found it dangerous, tried to make the cataract disappear by suction, a method generally employed by the Persians. But if the operation ever was performed at that remote period, it had fallen into oblivion, and was practically unknown till 1707, when Saint-Yves, in the presence of Méry, made an incision in the cornea, and by means of a curette extracted a cataract, which, after depression, had risen up again, and passed through the pupil into the anterior chamber. The following year Pourfour du Petit performed the same operation upon a priest in the presence of Saint-Yves and Méry. The latter submitted an account of these operations to the Académie des Sciences,* in which he proposed to extract a cataract through a corneal incision, not only when dislocated into the anterior chamber, but also when lying *in situ* behind the iris. As the author, however, never performed the operation which he so ingeniously suggested, his advice was fruitless, and nothing further was made known on the subject until the hint was reduced to practice, and the whole particulars of the process published by Daviel, a French surgeon, in 1745.† To Daviel, therefore, belongs the credit of having obtained that new conquest to science, in systematising

* Mémoires de l'Académie des Sciences, 1703, p. 310.

† Daviel: Sur une Nouvelle Méthode de Guérir la Cataract par l'Extraction du Crystallin (Mémoire de l'Académie de Chirurgie, t. ii., p. 336, 4to ed., 1763).