

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Retrograde Course of the Emulgent Veins.* By M. FRANCHESCHI.—Since the demonstration by M. Claude Beroard before the Society of Biology, of the existence of a retrograde current of the blood in the emulgent veins, experiments and facts have been multiplied tending to prove the facts discovered by that celebrated physiologist—facts which the novelty of the idea and the contradiction to the known laws of the circulation which they entailed caused to be looked upon with much doubt. The works of Littre, de Robin, de Bérard, &c., leave little doubt now of the accuracy of this theory. We have no intention, however, of multiplying experiments and observations on this important physiological point. Those of M. Francheschi are of considerable interest.

The Italian physiologist opened the abdomens of some rabbits, and having drawn out a loop of intestine, he injected into the mesenteric vein a solution of atropine. At the end of half an hour it was discovered in the urine, although neither then nor afterwards did the animal show any symptoms of poisoning. On the contrary, the same dose injected into the jugular vein of another rabbit put an end to him very quickly before the atropine had time to arrive in the bladder. Is it not evident, then, that in the first instance the poison found its way to the kidneys by the abdominal veins without passing into the general circulation?

Here is another not less interesting experiment: M. Francheschi rendered a rabbit diabetic by the means proposed by M. Beroard, by pricking the central nervous centre. Scarcely had the rabbit shown the signs of glycosuria, when he killed him and collected in one vessel the blood of the emulgent veins, and in another that of the corresponding arteries. On being submitted to the action of a cupro-potassic liquid, the venous blood gave a red precipitate of oxide of copper, while the arterial gave no indication of the saccharine matter. "I could," says M. Francheschi, "multiply proofs that the urine is principally secreted by the venous blood," and he quotes the following case:—

A female, aged 30, rather spare than robust, and enjoying sufficiently good health to undergo the fatigue consequent on her position as a poor servant, was for six years in the habit of drinking never less than four or five gallons of water per day. Every two or three hours she was attacked, whether fatigued or not, whether in winter or summer, with so great thirst, that she was never satisfied unless she filled, and, so to speak, refilled her stomach. She drank at least six or eight glasses each time. At night her usual allowance was two gallons and a half of water, and even this frequently failed to assuage her burning thirst, and she has been obliged to get up to look for more. Scarcely had she drunk when she was relieved by the bladder of colourless, inodorous liquid, which, in appearance, resembled pure water. In fact, on analysis, neither sugar, albumen,

nor any other product could be discovered; and so much did it resemble common water as to be with difficulty recognized as urine.—*Gaz. Médicale de Paris.*

2. *Researches on the Coagulation of the Blood.*—At a meeting of the Medical-Chirurgical Society of Edinburgh (December 16, 1859), JOSEPH LISTER, Esq., communicated some researches on this subject.

He reminded the fellows that in a paper read before them in 1858, he had brought forward facts which seemed to prove that the ammonia theory does not apply to blood within the vessels of a living animal. "The theory," he observed, "asserts that the fluidity of the blood depends upon the presence of a certain amount of free ammonia holding the fibrin in solution, and that coagulation is the necessary result of the escape of the volatile alkali. But it was shown in the paper referred to, that the blood, in man and other mammalia, though coagulating soon after death in the heart and great venous trunks, remains fluid for days in vessels of smaller size, and this under circumstances affording free opportunity for the escape of ammonia; and, on the other hand, that when a portion of a vessel either in an amputated limb or in a living animal is treated in a manner calculated to destroy its vital properties, the blood coagulates in the injured part, but retains its fluidity elsewhere, although there is no greater opportunity for the escape of ammonia in the one case than in the other. A striking instance of the difference between the natural receptacles of the blood and ordinary matter in their relations to the vital fluid happened to come under my notice this morning, in an arm which I amputated last evening at the shoulder-joint, on account of injury inflicted by machinery. On examining the limb, which had lain undisturbed since the operation, I saw that the axillary vein, which was patulous at the part where it had been divided by the knife, contained some blood at a distance of about half an inch from the open orifice; and having squeezed out a few drops, found that it was perfectly fluid, but yielded threads of fibrin when the point of a needle was drawn through it some minutes after emission. The blood had been for upwards of twelve hours freely exposed to the air, but being situated in an uninjured part of a bloodvessel, had remained free from coagulation.

"Further, in the opening meeting of last session I demonstrated another important principle, viz: That ordinary solid matter, unlike atmospheric air, induces coagulation of blood in its vicinity when introduced within the living vessels. Having inserted a piece of clean silver-wire for a considerable distance into one of the veins of an amputated sheep's foot, I slit up the vessel after a short time had elapsed; when I exhibited a coagulum extending along the whole length of the foreign body, whereas a mere wound of the vein failed to induce a clot except immediately at the spot where the injury had been inflicted. It was obvious that the introduction of the wire could not affect the amount of ammonia in the blood; and from this and many other facts to which I need not here allude, I was led to the opinion, that as regards what takes place within the living vessels, the ammonia theory might practically be left entirely out of consideration.

"What I have to show this evening will, I think, prove that even for blood outside the body, the ammonia theory, whatever degree of truth it may contain, is very far indeed from representing the whole truth.

"One of the most remarkable circumstances connected with blood that has been shed from the vessels is, that it refuses to coagulate below a temperature of 40° Fahr. or thereabouts. This is explained by Dr. Richardson on the hypothesis, that the low temperature prevents the evolution of ammonia,¹ while the rapidity with which coagulation takes place at high temperatures seems to him satisfactorily accounted for by the increased volatility exhibited by the ammonia under such circumstances. I was myself at first disposed to accept this inter-

¹ For some of these facts see *Philosophical Transactions* for 1859, pp. 673 *et seq.*

² See Dr. Richardson's *Atley Cooper Prize Essay*, p. 303, where a fact is mentioned, indicating that no ammonia was given off at 34° Fahr. from a specimen of blood which had been artificially ammoniated, and which at 96° afforded distinct evidence of evolution of the alkali.

pretation; but subsequent reflection led me to think that, to say the least, it required confirmation. It occurred to me, that if it were true that the fluidity of blood below 40° was due to free ammonia retained in it, coagulation would take place immediately, in spite of the cold, if the alkali were neutralized by the addition of acid, provided the fibrin were not impaired in its coagulating property by the reagent employed. In order to ascertain whether this result would really follow, I poured blood freshly shed from a sheep into vessels surrounded by ice-cold water, and by this means succeeded in keeping some portions of it fluid for a considerable time, and found that it contained liquid notwithstanding the addition of dilute acetic acid in what I supposed must be sufficient quantity to overcome the feeble alkalinity of the blood, while the acidulated specimen retained the property of coagulating very rapidly when raised in temperature. But on attempting to discover whether this blood was really acid in reaction, I found that its red colour entirely vitiated the indications of both litmus and turmeric; and even the serum obtained after contraction of the clot was too much tinged to admit of the satisfactory application of the test paper.

"Being thus baffled in my experiments with the sheep, I had recourse to the horse, in which the red corpuscles subside with peculiar rapidity in the plasma, giving rise to the buffy coat well known to occur in the blood of that animal in the state of health, so that the opportunity would be presented of obtaining liquor sanguinis free from red corpuscles, to which the tests could be applied without risk of fallacy. Accordingly, yesterday afternoon, a horse having been placed at my disposal by my friend Mr. Gamgee of the New Veterinary College, I tied into the right jugular vein one end of a piece of vulcanized India rubber tube, four yards in length, the greater part of which was coiled up in a freezing mixture, and some of the blood, having been allowed to remain for a while in the tube, was shed into vessels standing in ice-cold water. Its temperature on first escaping into the air was 39½ Fahr., and having been since kept in the cold it is still only partially coagulated at the present time (twenty-nine hours after it was shed). At first, however, it appeared as if we were likely to fail, the blood of this horse being a rare exception to the general rule, in exhibiting for a long time an appearance of the 'sily' layer. But after it had stood for about two hours, I succeeded in removing from the surface, by means of a glass tube, a sufficient amount of liquor sanguinis for the performance of an experiment, taking care that the glass into which it was shed, and the tube, were both near the freezing point. To half a drachm of this plasma I now added one minim and a half of moderately dilute acetic acid, which had the effect of rendering it distinctly acid, as indicated by its communicating a red tint to litmus and restoring the colour of turmeric paper which had been reddened by dipping it in the portion of the liquor sanguinis which had not been acidulated. I kept the specimen in ice-cold water till this evening. For a long time it remained perfectly fluid, except the formation of little soft coagulum at the surface, just as in the unacidulated blood; but a few drops placed in a watch-glass and brought into a warmer atmosphere, coagulated in about the same time as the blood that first flowed from the tube, a soft clot forming in about a quarter of an hour. Even at the expiration of twenty-four hours a portion of what remained in the cold was still fluid, though faintly acid, but set into a pretty firm clot on being removed into a warmer situation.

"[Mr. Lister now proceeded to perform a similar experiment before the society. A glass containing some liquor sanguinis of the horse's blood shed twenty-nine hours before, was taken out of the mixture of ice and water in which stood, and the contents were seen to be still to a considerable extent fluid, although acidulated with acetic acid two hours previously. A portion of the liquid was poured into a watch-glass, and, having been shown to be acid by litmus paper, was set aside to coagulate, and about a quarter of an hour later was exhibited as a soft clot. Mr. L. then continued—]

"From these facts it is obvious that the ammonia theory utterly fails to explain the influence of temperature on coagulation. The circumstance that the liquor sanguinis was acid in this experiment is clear proof that it contained no free ammonia whatever; yet the acidulated plasma was affected by cold and heat, just like ordinary blood. It remained fluid near the freezing point, although

the ammonia it originally contained must have entered into combination and lost its reputed power of dissolving the fibrin, and it coagulated when warmed, though the ammonia, fixed by the acid, must have been incapable of evolution. If the author of the ammonia theory were asked to explain why this horse's blood took a quarter of an hour to coagulate, he would no doubt reply that it must have contained a large amount of ammonia, requiring all this time to escape. But we have seen that the acid liquor sanguinis, though possessing no free ammonia at all, took as long to clot. There can therefore I think be little question but that the slowness of coagulation in the horse, compared with the rapidity of the process in the sheep, and the variations met with in the period in the human species, depend not on the amount of ammonia present in the blood, but on differences in its other constituents, and, speaking generally, that the theory which attributes the coagulation of the blood to the escape of ammonia is fallacious."—*Edinburgh Med. Journ.*, December, 1859.

3. *Saccharine Function of the Liver.*—Dr. GEORGE HANLEY, in a paper read before the Royal Society (Feb. 2, 1860), related a number of experiments which he performed, in concert with Prof. Sharpey, in the Physiological Laboratory at University College. The results of these experiments do not in any way countenance the notion that sugar is not produced in the healthy animal body; but, on the contrary, such conclusions as they afford are altogether in favour of the following generally received views upon the subject: 1. Sugar is a normal constituent of the blood of the general circulation. 2. The portal blood of an animal fed on mixed diet contains sugar. 3. The portal blood of a fasting animal, as well as of an animal fed solely on flesh, is devoid of sugar. 4. The livers of healthy dogs contain sugar, whether their diet be animal or vegetable. 5. Under favourable circumstances, and with proper precautions, saccharine matter may be found in the liver of an animal (a dog) after three entire days' rigid fasting. 6. The sugar found in the bodies of animals fed on mixed diet is partly derived directly from the food, partly formed in the liver. 7. The livers of animals restricted to flesh diet possess the power of forming glycogen, which glycogen is, at least in part, transformed into sugar in the liver. 8. As sugar is found in the liver at the moment of death (even when the plan of freezing it has been strictly attended to), its presence cannot properly be ascribed to a post-mortem change, but it is to be regarded as the result of a natural condition.—*Medical Times and Gaz.*, Feb. 11, 1860.

4. *On Nutrition by Blood in Starvation.*—Experimental researches made on animals subjected to a more or less absolute privation of food have shown that life may be maintained for a certain period at the expense of the substance of the organs, as is proved by the progressive diminution of the weight of the animal suffering from inanition. This mode of nutrition has long been

¹ Since the above communication was made, I have seen for the first time the able essay of Dr. E. Brücke, which competed for the Astley Cooper Prize (see *Med.-Chir. Review*, vol. xix.); and I find that the principle which he advocates—viz., that the fluidity of the blood within the living body depends upon an action of the walls of the vessels upon it—is supported by many facts which he has observed in the chelonian reptile, very similar to what I have made out in mammalia. Thus, he found that the blood remained fluid in the heart of the turtle for days after death, and for several hours after he had blown air through the veins of the neck, so as to make a foamy mixture in the cavities of the organ. He also found, as had been previously ascertained by Virchow and others, that after the introduction of mercury into the heart the blood coagulated about the globules of the metal, but not elsewhere, and thus he regarded as an example of the influence of ordinary matter in inducing coagulation in its vicinity. He also succeeded with the following very striking experiment, which would not answer with mammalia: He drew blood into a cup from the veins of a living turtle, and injected it into the empty heart of another tortoise just killed, and found that the blood remained fluid for several hours in its new situation, instead of coagulating in a few minutes as when retained in a cup.—J. L.

termed *autophagy*; and M. ADELUNG wishes to designate it as *spontaneous autophagy*, as a contrast to the term *artificial autophagy*, which he employs when the animal subjected to inanition is submitted to daily small bleedings, and his blood given him as aliment. He has made a great number of comparative experiments upon two groups of animals, resembling each other in every circumstance as far as possible—one group being abandoned to the effects of inanition, and the other exclusively fed with the blood drawn from the veins of the animals experimented upon. The following are the results deducible:—

1. The absolute privation of food diminishes the production of caloric in all warm-blooded animals. The diminution is nearly uniform during three-fourths of the duration of vital resistance, that is, about 62.1° C. in every twenty-four hours. During the last fourth of the time it decreases very rapidly, and death occurs between the 23° and 24° C.
2. A relative privation of aliment causes a less rapid diminution of the production of caloric, proportionately to the rations.
3. In all animals of warm blood the temperature of the blood cannot descend below 26° C., without death being the consequence.
4. Death from starvation is the result of an arrest of nutrition produced by the progressive diminution of the temperature of the animal—the production and accumulation of a certain quantum of caloric being one of the conditions of nutrition in all animals of this class.
5. Death by starvation would not be the result of the consumption of all the materials which the organism can supply if we could change the condition of cooling which is the consequence of inanition. In fact in the animals who have succumbed from absolute abstinence, the emaciation is on the average four-tenths of the initial weight, while in relative abstinence it may attain six-tenths.
6. The diminution of calorification arises from the inactivity of the gastro-intestinal absorbent system, the temperature of the animal increasing or diminishing according to the degree of the activity of this function, just as the latter is modified by the temperature at which it effects its operation.
7. If we draw from animals subjected to inanition a certain amount of blood, and give it them as an aliment, we find the production of caloric continues, together with the gastro-intestinal activity, the daily loss of temperature being less considerable, and the emaciation becoming more complete, so that it may attain six-tenths of the initial weight.
8. The bleedings and the rations which they supply should be diminished in quantity in proportion to the prolongation of the experiment; and digestion takes place more completely and more rapidly in proportion to such prolongation. In proportion to the frequency of the bleedings, the exhaustion of the organism, the nervous irritation, the diminution of the gastro-intestinal secretions essential to digestion, the monotony of the aliment, the diminution of the temperature, and the preternatural condition of the aliment, the prolongation of this mode of nutrition becomes impossible.
9. The gastro-intestinal activity is indicated by the return of the excretions, the elevation and generalization of the temperature and pulse, an increase of muscular force, and the diminution of nervous phenomena, and of the sensations of hunger and thirst.
10. The calorification does not decrease more than a mean of 62.1° C. in the twenty-four hours.
11. Artificial autophagy allows of excessive emaciation, *i. e.* allows of its being carried to six-tenths in fat subjects, five-tenths in medium subjects, and four-tenths in the young; while the author's and Chossat's experiments show that in spontaneous autophagy it attains only five-tenths in the fat, four-tenths in the medium, and two-tenths in the young.
12. Artificial autophagy thus considerably prolongs life, *viz.* for nearly one-half more than its duration in spontaneous autophagy. The application of the author's views may be made in the case of shipwrecked persons, or others subjected to the horrors of starvation.—*Comptes Rendus*, December, No. 24.

5. *On the frequent Occurrence of Phosphate of Lime in the Crystalline Form in Human Urine.*—By ARTHUR HILL HASSALL. It is commonly stated by writers on the chemistry and pathology of the urine, that phosphate of lime never occurs in the renal excretion in the crystalline form, but always presents itself as a granular amorphous deposit.

The author has shown in this communication that deposits of phosphate of lime, in well-marked and highly characteristic forms, are of frequent occurrence

in human urine, very much more so indeed than the amorphous deposits of that salt, which are comparatively rare and exceptional.

It follows, therefore, that the statements hitherto advanced, of the absence of crystallized phosphate of lime from the urine of man, are erroneous.

From the frequency of their occurrence, it is singular that the true nature of these crystals should have been so long overlooked. This, the author considers, cannot have arisen from the crystals themselves, at least in some of their various modifications, not having been observed, but rather from their having been confounded with those of the phosphate of ammonia and magnesia, from which, however, they differ as much in form as in composition.

The author considers the occurrence of deposits of phosphate of lime to be of deeper pathological significance than those of phosphate of magnesia or phosphate of ammonia and ammonia. While the greater part of the phosphoric acid of these latter phosphates and all their magnesia are derived from the ingesta, there is in the animal organism in the bones several pounds' weight of phosphate of lime, from which, in some cases, and in certain maladies and conditions of the system, the deposits of that substance encountered in the urine are doubtless obtained.

The communication was illustrated by a series of drawings, exhibiting the several varieties in the form and grouping of these crystals observed by the author.—*Proceedings of Royal Society.*

MATERIA MEDICA AND PHARMACY.

6. *On the Chemical Composition and the Medical Employment of the Oils from the Liver of the Cod, the Skate, and the Dog Fish.*—M. DENEVIER has made a report to the Academy of Medicine of Paris, on a memoir by Dr. DELATTRE on this subject.

M. Delattre resides at Dieppe, and has therefore abundant opportunities of obtaining a perfectly pure oil, but up to the present time the purest oils have been procured in contact with the air. M. Delattre, however, has devised an apparatus for isolating the oil from the influence of the atmosphere. This object is effected by expelling the atmospheric air from the vessels in which the oil is extracted from the livers, and replacing the air by carbonic acid. By this process the operator avoids the formation of the oleic, sulphuric, and phosphoric acids, which would otherwise be formed. M. Delattre having thus obtained pure specimens of oil, he made twelve analyses of each kind, and he tabulates the quantitative and qualitative results, from which it appears that all the oils contain a very large proportion of oleine, with some margarine, and some very small quantities of chlorine, iodine, bromine, sulphur, and phosphorus. M. Delattre also ascertained that the iodine, bromine, chlorine, phosphorus, and sulphur are not in combination with the potassium and sodium, as was formerly supposed, but are in a free state. Another important fact was ascertained by MM. Delattre and Girardin—namely, that in the spring of the year cod-liver oil does not contain a particle of iodine. It is also ascertained that the livers do not yield an equal quantity of oil at all periods of the year; that the quantity increases from June to November, and then diminishes from November to March, when it is at its minimum. In comparing the chemical composition of the oils from the cod and the skate, it is found that the proportion of iodine is less by half in the latter oil, and that that of sulphur is less by a fourth; but on the contrary, that the proportion of phosphorus is greater by about a third. As to the dog-fish oil, it is richer in phosphorus and iodine than cod-liver oil, and contains rather less bromine and sulphur. The increase of iodine is double the loss of the bromine. Compared with the skate-oil, it contains two-and-a-half times more iodine, and only a fifth less of phosphorus. Chemically, therefore, it is richer in inorganic elements than the cod and skate oils, except as to the proportion of phosphorus in the latter. M. Delattre has extended his researches