

III. Annie R—, aged twenty-one, has been gradually getting pale and weak for two years. Menses very scanty, but not suppressed. No organic disease. Aspect chlorotic; occasional dyspnoea and palpitation; systolic murmur over heart, and venous hum in neck. No fever.

Obs. 42. Five days after admission.

$N=4,130,000$; $G=0.28$.

Obs. 43. Fourteen days after admission.

$N=5,990,000$; $G=0.37$.

The improvement in her appearance and symptoms was striking and rapid. She was treated with Bland's pills (sulphate of iron and bicarbonate of potash).

These few observations quite bear out Duncan's statement as to the lack of agreement between the hypocytosis and the hypochrosis in chlorotic anæmia. The two coexist, but their proportion to each other is variable and uncertain. This want of harmony between them is especially marked during convalescence under iron. The influence of the remedy appears to be exerted in the first place on the number of the corpuscles, while their functional value remains at its original level, or may even sink below it (Obs. 39 to 41). The numerical increase proceeds very rapidly at first, and then more slowly; at this stage the corpuscles begin to grow richer in hæmoglobin. Substantially the same result was arrived at by Dr. Gowers in two very interesting cases of anæmia in young women, in which the corpuscles were repeatedly counted while iron was being administered (*Practitioner*, July, 1878). He points out that the "primary effect of the administration of iron may be an increase in the globules out of all proportion to the increase in the hæmoglobin, and that the rise in the latter may be accompanied by an actual decrease in the number of globules." These results are especially noteworthy in the face of the very different conclusions at which both Hayem and Malassez have arrived on this subject. The former, after asserting that "in chronic anæmia of whatever kind there is always a want of agreement between the number of the coloured elements and the colouring power of the blood, the latter being more invalidated than the former," goes on to lay it down as a general principle that the action of iron is the same in all forms of anæmia, whether curable or incurable; "it always augments the richness of the corpuscles in hæmoglobin, but their number does not follow a parallel course; it varies but little, and not unfrequently undergoes actual diminution." In another place he alludes to a fatal case of progressive anæmia, in which the number of red corpuscles had fallen as low as 414,062 per cubic millimetre, their individual value being also much reduced. Under iron these elements actually grew much richer than in health, attaining the extraordinary value of 1.34; but their number steadily continued to decrease until it reached a point no longer compatible with life. Malassez tells of a case of anæmia (due either to lead-poisoning or to visceral cancer) in which the administration of iron was followed by an increase in both the number and the value of the corpuscles; but their value rose much more quickly than their number. When the medicine was stopped both elements began to sink, but the colouring power of the blood sank faster than its globular richness.

It is hard to reconcile these contradictory results. It may be that there exists a fundamental distinction between different forms of anæmia in their relation to iron, the metal influencing the hypocytosis primarily in one set of cases, while its primary effect is on the hypochrosis in another. But the time is not ripe for even a provisional hypothesis. More facts are needed. A comparative study of the blood in cases of chlorosis, progressive idiopathic anæmia, Addison's disease, and malarial anæmia, would probably throw much light on the question. As regards chlorosis, all that we are at present justified in asserting is, that the normal ratio between the number of the corpuscles and their functional value ceases to be maintained; that while hypocytosis and hypochrosis coexist the latter is always in excess of the former; that under the influence of iron the increase in the number of corpuscles usually precedes the increase in their individual value, though both factors are affected by the remedy. It is interesting to note that the patient's symptoms are always relieved when the number of corpuscles attains its normal level, even though their individual value show no corresponding improvement. This is what might have been expected. The addition of a certain quantity of hæmoglobin to the circulating fluid will clearly be more useful to the organism if it be distributed among a larger than if it be concentrated in a smaller

number of elements. The former arrangement, by offering greater facilities for the transfer of oxygen, will be the more economical of the two.

As regards diagnosis, the numeration of the corpuscles in conjunction with chromometry may be of service in chlorosis in more than one way. It may enable us to recognise the first beginnings of the malady, before its presence is suggested by visible changes in the colour of the skin and mucous membranes. Again, it may serve to distinguish cases of unusual pallor in young women, without any actual lessening either of the corpuscles or the hæmoglobin, from true anæmia. In such cases the routine administration of chalybeates is not merely useless, it is positively injurious, giving rise to headache, digestive disturbance, and other undesirable symptoms.

VIVISECTION, AND ITS USE IN THERAPEUTICS AND HYGIENE.¹

BY DR. CH. RICHET (PARIS).

IT is often of advantage to reopen questions which are practically settled; for although with the majority of medical men the utility of vivisection is thoroughly admitted, yet by a section of the public, and especially the English public, it is held to be not only cruel but useless. We shall not consider the question of cruelty, but simply discuss the advantages that have been gained for man by animal vivisection. We desire to judge vivisection by its practical results. Science has different ends from medicine. Medicine aims at curing disease; physiology at learning function. The former seeks to allay pain or ward off death; the latter to unravel the innumerable mysteries of nature. Because a discovery cannot be turned to direct profit in the art of healing, it is not the less important. It is as if a mathematician, before solving a problem, were to ask himself what gain he would have by knowing its solution. Someone has wittily said that the composition of a new dish is of more value than the discovery of a star. But such witticism must not be seriously taken. The end of science is truth, and it is quite legitimate to conclude that vivisection is justified if by it we know more of the function of an organ. Apart from this, let us see what are its useful and practical results. Have we gained by experiment the means of curing one or two diseases of man or of assuaging pain? If this can be answered in the affirmative, then vivisection is absolutely lawful. The partisans of *humanity* in science should acknowledge that the pain of man is a greater ill than that of an animal, and that we should not hesitate to inflict suffering on dogs or frogs if thereby we can save the life of one human being or relieve his sufferings.

The first place must be given to the discovery of the *circulation* of the blood. Galen, who made many experiments on animals, was the first to demonstrate that the arteries contained blood. He laid bare the artery of a living animal, and intercepted the blood-flow in its course by means of two ligatures placed a short distance apart, and then, opening the artery, found blood, and not air, as Eristratus taught, filling the vessel in the included portion. Harvey, taking up and working out the grand idea of Michael Servetus, established by irrefutable experiments the fact of the circulation, and he begins (chap. i. and ii.) by speaking of the experiments he had made on animals. He opened the chest of living animals, cut into the pericardium and saw the heart contract, in warm as well as in cold-blooded animals—serpents, frogs, toads, snails, crabs, &c. He also observed what takes place in the arteries and veins going to the heart. These admirable experiments established the theory of the circulation, which is now uncontested and incontestable. It was not by observations on the anatomy of the veins or the examination of cardiac ectopia (as in the case of Count Montgomery), but by experiments on dogs, frogs, serpents, and fish, that he discovered the way in which the blood goes

¹ This is a full and careful summary of a paper contributed specially to our columns by M. Richet.

to and returns from the heart. It would be absurd to discuss the question that this discovery is without importance in therapeutics. There is no physician or surgeon who does not avail himself of the knowledge of this fact in observing morbid phenomena, and in applying his remedies.

A certain number of people die from loss of blood—on the battle-field, after operations, after parturition, and to save them transfusion is necessary. Lower in 1664 first showed that this could be done with impunity. His experiments on the dog were repeated by Denis, and the results he gained by vivisection led him to make attempts on man. It has taken its place in therapeutics, and has saved many lives. It will save many more. Experiment alone will teach us precisely what quantity of blood is necessary and what is harmful; and if over-sensitiveness forbids animal suffering for this end, then the experiments would have to be made on human beings.

One of the most poisonous of gases is carbonic oxide. It is evolved by burning charcoal in a confined space. The mode of death from its inhalation has been known only through vivisection. Orla, Portal, and Claude Bernard have shown that carbonic oxide combines with the red corpuscles and renders them unfit to absorb oxygen; and the therapeutical bearing of this is, the avoidance of conditions leading to its inhalation, and the attempt at prevention of death in one poisoned by it by means of artificial respiration, the inhalation of oxygen, and transfusion of blood.

In *respiration* great discoveries were made through vivisection by Lavoisier, Regnault, Reiset, Claude Bernard, and Paul Bert, who have in succession shown that death may take place, either from deficiency of oxygen or excess of carbonic acid. The amount of carbonic acid in the atmosphere that is fatal has been estimated. Are such researches without value for the life of man? Next to man himself, the question can only be solved by experiments on animals. All that we know in hygiene of the quantity of air necessary to support life is the result of experiments on dogs and rabbits. Sometimes a precise knowledge of the conditions of respiration has served to prevent men from perishing. Paul Bert has shown that an animal suddenly transferred from compressed air to the open dies rapidly from the sudden liberation of gases in the blood. Men who work under water at great depths should therefore be brought up very slowly, and similarly certain accidents to aeronauts may be avoided by the inhalation of oxygen.

In *digestion* the utility of experiment is manifest. There are two methods by which we may learn the conditions of gastric digestion, and collect its secretion—viz., by observation of gastric fistulæ produced by chance in man, and by artificial fistulæ in animals. The first has only been possible in three or four instances, but the study of the effect of food on the gastric secretion in dogs and cats has been largely observed. When we think of the common occurrence of dyspepsia, of the relief given by pepsin, acids, and alkaline waters, we should remember that this treatment is due to the suggestion of physiologists who have studied the effects of such substances on the gastric juice, and the relief so obtained is not bought too dearly at the expense of the mutilation of these animals.

Remarkable experiments have added to our knowledge of *nutrition*. Dogs and cats have been submitted to varied alimentation, and the quantity of food necessary for man has thereby been deduced. It has been proved that on a diet of gelatin alone animals die; that when the food contains no nitrogen they waste and die; that fat, sugar, and starch taken alone are insufficient to support life. Certainly an animal dying from inanition for want of proper or sufficient food may excite compassion, but I should hold this pain sufficiently recompensed if I had been able to find the quantity of aliment necessary to maintain the health of one of our own kind. From such experiments the normal diet of adult man may be computed, and it is on these facts of the physiologist that the daily rations of our soldiers and sailors are computed. The advantages gained far outweigh the disadvantages.

As to the *nervous system*, how many experiments and how much pain have been inflicted on animals! But how much suffering has been spared to man! The great discovery of Charles Bell and Majendie is due to experiment. No one can dispute the importance of this discovery in therapeutics. Therapeutics is the art of relieving as well as of healing. Often the physician is powerless to give a remedy for a disease. Cancer, rabies, typhus, are plagues which cannot be directly combated; but pain can be assuaged; and how

should we succeed in this if we knew not that the nerve-cords were the seat of pain. Before the experiments of Majendie and Legallois we did not know that the nerve was the agent for transmitting painful impressions from the periphery to the centre. It is now so notorious that one is almost ashamed to think of former ignorance. In obstinate neuralgias nerve-section leads to nearly immediate relief. Like the circulation of the blood, the duality of nerve-function comes into nearly all treatment of diseases; and it is impossible to think that we could relieve our patients if we were in ignorance of these facts as surely as we can in knowledge of them. The experiments of Fritzsche, Hitzig, and Ferrier in proving the existence of portions of the cerebral cortex as centres of movement have been already put to account by surgeons in the relief of paralysis due to pressure of certain convolutions. Thanks to the scientific analysis of those vivisectioners, we can pass from the effect to the cause, and assign to paralysis a central lesion at a well determined spot, so that trephining at this spot may cause the paralysis to disappear.

Of the most undoubted methods of treatment of nervous or muscular affections electricity stands foremost. Galvani—the discoverer of dynamic electricity—experimented on frogs; and since his time numberless *savants* have not scrupled to make martyrs of innocent animals for the like gain. They have taught us to estimate the effect of the electric current on nerve and muscle. The muscle, whose nerve is divided, rapidly becomes fatty and incapable of function. Electricity prevents this change. In former days patients whose motor nerves were destroyed by disease or injury were condemned to remain permanent paralytics. Frequent galvanisation is now applied to prevent this paralysis—keeping the muscle alive until the nerve regains its function. In a very large number of nervous diseases—neuralgia, paraplegia, ataxia, hemiplegia—excellent results follow electrical treatment. The numerous patients relieved by this admirable therapeutic agent have no call to speak ill of such vivisectioners as Galvani, Aldini, Volta, Majendie, Marshall Hall, Remak, Du Bois Reymond, and many others, since it is to their discoveries that the relief of their ills is owing. Would Galvani have made his discoveries had he refrained from dissecting frogs? Would the electric current have been applied to atrophied limbs if it had not been found that the action of this current in dogs was salutary and not dangerous?

Experimental pathology is a relatively new science (it dates from Hunter). Without doubt it has a great future before it, so long as we do not fear to make animals suffer for the sake of man. There are many examples of this. *Uremia*, due to retained urinary secretion, has been studied on dogs whose kidneys have been removed or ureters tied. The knowledge of the fact that the phenomena of certain nephritis are due to the accumulation of urinary materials in the blood has led to the correct treatment of these affections (as bleeding, baths, milk régime). The treatment of sympathetic ophthalmia by section of the ciliary nerves of the diseased side has been shown to be advantageous by experiment, and the results yielded by experiments on dogs and rabbits have been applied to patients. Ophthalmologists have been led to correct treatment of cataract, to iridectomy, by experiments on animals. The experiments on the formation of callus, on pseudarthrosis, on osseous grafts, on regeneration of bone by periosteum, are of chief importance in surgery, and although the subject has not yet been fully worked out, the results of experiment, combined with those of clinical observation, are such as to encourage its continued study by vivisection. Trophic muscular changes due to section or wounds of nerve-trunk are explicable since we have known the precise function of nerves to the parts they supply. The famous experiment of Claude Bernard on the great sympathetic and the rabbit's ear has led to the establishment of the vaso-motor theory—a theory which plays so large a part in the medicine and surgery of the present day. The treatment of the majority of skin diseases rests as much on a knowledge of the cutaneous circulation as on the empirical methods of the older physicians. Brown-Séquard, who has made so many painful experiments on dogs and guinea-pigs, has given to medical science most useful ideas relating to epilepsy and tetanus. We know now that a peripheral excitation of a nerve may lead to excitation of the spinal cord, and to convulsions or generalised epileptic attacks. The treatment of such peripheral epilepsies is thus indicated, and the epileptic seizure has been stayed by suppressing the nervous excitation proceeding

from the altered peripheral nerve. Experiments made upon wounds of the chest have led to the adoption of their correct treatment (e.g., by occlusion of the wound).

The researches still to be made are numberless. A certain number of sensitive people think these studies of no interest, because they do not lead immediately to practical results. For them etiology, pathogeny, pathological anatomy, and physiology, even diagnosis and prognosis, are of small importance. They only know one thing, and that is the therapeutical outcome, and they condemn experiments which do not at once yield this.

But even on this ground experiment has been useful. Therapeutics has and does profit by experiment. It is true that the discovery of some valuable remedies has been a matter of chance—e.g., quinine, mercury, opium, even chloroform; but we only know certainly the action of these drugs from an analysis of their physiological properties, their action on the heart, lungs, and liver, on secretion, on nutrition. At any rate, we do not wish to experiment on man, at the risk of poisoning him, when animals can be employed. A certain number of drugs of recent discovery have been made known to us only through the physiologist. The discoveries of Hunter and Majendie established the laws of absorption, and a drug can only be safely given if it is known how it is absorbed. The properties of chloral, the use of which has become so common, were first demonstrated in animals. The combination of chloroform and morphia, in increasing the anæsthetic effects of each singly, was first shown by Liebreich, and to Claude Bernard and Nussbaum we owe the experiments by which this double medication has been introduced into medical science. Apomorphia, pilocarpin, and a large number of alkaloids extracted from poisonous plants, have replaced the empirical employment of aqueous or alcoholic extracts. Is it not a gain to replace old pharmacopœial preparations by those which are chemically defined, whose action on organs and functions is well known? Experimental therapeutics is a science with a future before it, promising still greater benefits than it has already given.

In legal medicine, in toxicology, the study of the causes of death from poisons, and the means to combat these by appropriate antagonistic poisons, it is necessary to have recourse to experiment. If in an empty protest of humanity we deprive *savants* of the right to submit living animals to experiment, we shall go back beyond the days of Galen, who sought on pigs, birds, and monkeys to learn more of the functions of the human organism. If all those who have been relieved—verily made to live again—by modern medicine and surgery could speak, they would confound those who load vivisection with calumny, and they would hold that their own life and sufferings weighed more in the balance than the sufferings of those animals which have been sacrificed in laboratories to the lasting benefit of man.

HUMAN AND ANIMAL VARIOLÆ: A STUDY IN COMPARATIVE PATHOLOGY.

By GEORGE FLEMING, F.R.C.V.S.,
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(Continued from p. 248.)

WHAT we might call the *casual evidence* as to the non-identity between human variola and cow-pox, and their being two distinct diseases of a group, is still further increased by other facts which may be mentioned. The individuality of contagious maladies is perhaps never more strongly marked than when we find two affecting the same person or animal, and running their course concurrently, the various phases of the one being passed through entirely independently of the evolution of the other. Such instances are far from infrequent in the practice of the physician and veterinary surgeon, and from my own experience I could relate several. But I refrain from doing so, as additional testimony of this kind is not required at this period of the nineteenth century, I hope. But in the discussion now undergoing consideration, such evidence should be borne in mind when we mention the fact, that cases are recorded in which people already infected with small-pox, and who had

been vaccinated when so infected, have had the eruptions of the two diseases apparent at the same time, each preserving its special characters, undergoing its different changes, and terminating exactly as if it were totally independent of the other, and had not the slightest influence in modifying or checking its course. And more than this, Hallé is reported to have employed the lymph from vaccine vesicles developed under these conditions, to vaccinate healthy children, and has only produced the pock of vaccinia—never small-pox. In one of Chauveau's experiments, in which a horse was inoculated with the virus of human small-pox, which gave rise to local effects, and was also inoculated with vaccine lymph, evidence of the two morbid actions operating coincidentally was undeniable, and afforded one more proof that a certain lapse of time must ensue before the preservative influence of vaccination comes into play.

However strong and abundant clinical and casual evidence may be, it is not always completely convincing, unless supported by experimental demonstration, when it is then irresistible. This evidence is also fortunately at hand, and in a most exhaustive and complete form; and it is very remarkable that with the exception of one speaker at the Conference (Dr. Cameron), its existence does not seem to have been known to those present; at least this may be inferred from the circumstance that it was never alluded to, save in the one instance mentioned. That my surmise is not very wide of the truth may be taken for granted, when we read of one gentleman saying, in reference to the experiments conducted by my friend, Professor Chauveau, of the Lyons Veterinary School, and briefly summarised by Dr. Cameron, that "it seemed strange that men should, at the present time, go abroad to France, to the futile and speculative experiments of a Frenchman, and take possession of the inferences he entertained." Such language betokens either ignorance of, or an utter failure to appreciate, the value of the results of these experiments; but then this speaker asserted that he had successfully inoculated a large number of cows with small-pox virus, and produced vaccinia, which gave him abundant vaccine lymph!

In France, as in some other European countries, the origin of cow-pox had long been a fruitful subject for discussion and speculation, and more than once it had provoked warm debates at the Paris Academy of Medicine. In 1863, M. Bouley, then director of the Alfort Veterinary School, had re-discovered at Paris the "horse-pox" of Dr. Loy of Pickering—in reality, the "grease" of Jenner, Sacco, and others, who believed in the equine origin of cow-pox. But M. Depaul, who had also with Bouley studied this *vaccinogenous* disease of the horse, was inclined to follow the example of Baron and his followers, and to conclude that it and all other eruptive disorders occurring in animals, and possessing analogous characters, were nothing more nor less than human variola. So it was that he included sheep-pox, and the aphthous disorder of animals known as foot-and-mouth disease, in the list of disorders having their common source in small-pox. The influence which he and Bouley possessed divided the Academy into two camps, and the result was recourse to experimentation, which happily led to the conclusion that this equine eruption was capable of producing cow-pox in the cow and vaccination in mankind. But the burning question as to the identity of small-pox and the diverse varioliform affections of animals remained as undecided as before; no new facts of a reliable kind were brought forward, and there was nothing to talk about save the well-known contradictory circumstances which had been so often employed before, but which were brought into debate with all the oratorical ability and scientific ardour for which that renowned body is remarkable. Viewing it as a purely experimental matter, as a question which could only be decided by true facts, the Society of Medical Sciences of Lyons appointed a commission to carry out this inquiry. The commission was composed of Drs. Boudet, Delore, Dupuis, Gailleton, Horand, Lortet, Meynet, and Viennois, and the veterinary professor Chauveau. The latter was the President of the Commission, and on him chiefly devolved the task of experimenting on the animals. A man more competent for the onerous duty imposed upon him could not be found in Europe; his reputation in the world of medical science stands very high as a most accomplished physiologist and experimental pathologist, the results of whose experiments may be received as absolutely trustworthy. Drs. Viennois and Meynet were secretaries, and the three drew up the final report, which was presented in 1865. On March 5th, 1866, the French Academy of Sciences