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Vacation Lectures

ON

FIBRINOUS DEPOSITION IN THE HEART:

ITS HISTORY AND PRACTICAL MEANING.

DELIVERED AT THE GROSVENOR PLACE SCHOOL OF MEDICINE.

By BENJAMIN W. RICHARDSON, M.A., M.D., M.R.C.P.,
Lecturer on Physiology at the School, and Senior
Physician to the Royal Infirmary for
Diseases of the Chest.

LECTURE IV.

Application to Prognosis and Practice.

GENTLEMEN,—We have been led by our previous studies to the last, but not the least important, part of our subject. We are brought to consider the *cui bono*. Some writers, who would agree with much that I have said, ask whether, supposing it all to be correct, there is any good in the knowledge of the facts? If, say these argumentalists, things are so, the hopelessness of medicine as a practice is broached openly, when it had better be concealed; for treatment there is none, when the diagnosis of fibrinous deposit in the heart is complete.

I demur altogether to this argument; but, as it has met me these ten years, daily I had almost said, I must state it as it is. I urge, on my side, that what seems hopeless knowledge, is ever better than floundering ignorance. I hope now to show that the information I have tried to convey has a threefold value: that it may, in the first place, make prognosis more accurate; in the second place, guard the practitioner against bad practice; and, in the third place, that it may in time lead to suggestions bearing directly on curative treatment. But, as a primary step, let us consider how far, in special cases, the existence of a concretion in the heart determines the course of a disease. From this point of view, fibrinous depositions have three meanings.

i. There is, as we have seen, a class of cases in which the deposition is the sole cause of dissolution. I have given already various illustrations of this variety of disease, when dwelling on acute hyperinosis. Those examples connected with puerperal fever are, *par excellence*, examples of the kind.

ii. In other cases, the deposition of fibrine occurring in the course of an acute disease adds new symptoms to such disease, and renders fatal that which before was dangerous, but not of necessity fatal. In acute pneumonia, in pleurisy, in peritonitis, and, in fine, in all the sthenic inflammations, this effect of deposition may obtain.

iii. In a third set of cases, the deposition of the fibrinous constituent may be the consequence of conditions in themselves necessarily fatal. We have had before us several examples of this nature. We may place advanced phthisis pulmonalis, cholera, deadly injuries producing shock, and poisoning by depressants, in this category.

It is my wish strongly to impress these distinctions on the mind; and I would specially guard myself, as I always have guarded myself, from the assumption that fibrinous deposition is in every case where it occurs the cause of death. I may offer a law on this point, indeed, without a touch of dogma. It is this: Fibrinous deposition occurring in the course of a disease ensures a fatal termination as a general rule: but the disease might be fatal without the occurrent deposition. This premised, let us turn to matters of prognosis and practice.

In all cases of the acute inflammatory type, whether slightly or broadly defined, the practitioner who is conversant with the risks of fibrinous deposition will be on his watch tower. He will remember the symptoms by which the existence of deposit is recognised; he will be guarded in his promises, and careful in his predictions. Most of all, he will be protected from being misled by those rapidly developed changes from severe local disease to suppression of local symptoms, which so often give rise to illusory hopes, and usher in the death.

Again, as I have said, the knowledge of the existence of a fibrinous concretion in the heart may prevent the committal of meddlesome, and, because useless, mischievous practice. In days when blood-letting had its full swing, when everybody was bled once in every disease, and once a day in inflammatory diseases; when even the convulsion of hæmorrhage was met by a recall on the lancet; in those days there arose, amidst all the confusion, this persuasion, that to bleed late in an inflammatory disease was bad practice. If I remember rightly, immortal Mackintosh himself, on every page of whose *Practice of Physic* there is a gaping vein, admits this rule; and the reason of the rule is obvious enough. In the later stages of inflammation, the tendency to fibrinous deposition is increased. The effect of hæmorrhage is to increase the volume of water in the blood, to distribute the fibrine solvent, and to encourage fibrinous deposition. When, therefore, late in the inflammatory state blood is drawn, the balance of the blood constituents, already disturbed, is disturbed the more, and all that is wanting to secure the deposition of fibrine is secured. The rule, therefore, as I say, originated that it was better in inflammatory disorders not to bleed late in the disorder; and this rule, expanded now into wider pretensions, goes pretty nearly to the length of telling us that it is well to avoid bleeding at all stages; not because bleeding is not a grand remedy, but because it is too grand a remedy to be used without a definite knowledge of the time when to use it. I have, nevertheless, seen the rule about late bleeding in an inflammatory disease disobeyed even in our own time; and therefore I enforce it, and give the reason for its enforcement. In a case of acute pneumonia, which came under my observation some years ago, I was overruled to take blood. After the bleeding, the strong man fell like a shot bird. He fell into sleep, and he slept into death. A large fibrinous concretion blocked up the right cavities of his heart. This clot had possibly commenced to form before the venesection; afterwards its rapid increase was determined, and the death was hastened by the means intended for its prevention.

The remarks against the general abstraction of blood in the later stages of inflammatory disease tell with little less force against local blood-letting. Indeed, there is not unfrequently a greater danger in the local than in the general remedy, inasmuch as, owing to its supposed harmlessness, it is more fearlessly resorted to. "A few leeches can do no harm," is the common sentiment: it is an incorrect sentiment, nevertheless, and to be guarded against always, and with special care in cases of acute hyperinosis in the young. Speaking, indeed, of hyperinosis in children, I would, if my voice had any influence, raise it against the abstraction of blood altogether in such examples. There is a form of acute pneumonia often met with in well fed fat children. We put on leeches; the bitten surfaces bleed furiously; but the disease is not stopped. From such a patient, the more the blood taken, the greater the risk incurred; for, whenever the local indications of inflammatory mischief subside, the general indications of deposition are in the ascendant. "Breathless, pale, and bloodless", the end of the patient is at hand.

The same rule which tells against venesection extends to the administration of two classes of medicaments—purgatives and opiates. To reduce the amount of water in the blood, by draining it off through the alimentary canal, is but to increase relatively the amount of fibrine in a blood in which the fibrine is already absolutely increased. And so, once more, to paralyse the heart with opium, during the existence of acute hyperinosis, is to double the danger of deposition by bringing

into play that feeble condition of the circulation which we have described as in itself immediately favourable to fibrinous separation.

There are, further, some points of practice to which I would direct your attention, in relation to one special disease and one particular surgical operation. The disease is croup, the operation tracheotomy. I have shown that, in croup, death may result from two distinct causes—from obstruction in the trachea or larynx, from obstruction on the right side of the heart. Some years ago, led by the observation of these facts, I was forced to protest against what I considered an indiscriminate resort to the operation of tracheotomy as a remedial measure in this disease. My opinions had the misfortune at that time to be opposed with great vigour and ability by my friend Mr. Henry Smith, an English surgeon who has, I believe, performed tracheotomy in more cases of croup than any other operator in this kingdom. I saw, however, no occasion to desert the position I had taken; and late events have confirmed the correctness and safety of that position. Recently, with a candour which is most honourable to his character, Mr. Smith has publicly stated that he believes himself to have been wrong. He admits that there are cases in which death in croup is the result of fibrinous deposition in the heart; and the practical lesson he leaves with his readers is, that, in these cases, the operation were better left undone. With such an important admission before me, I need only plead the seriousness of the question in excuse for some repetition in laying down the following observations for the guidance of the medical man in doubtful cases of the disease.

The symptoms which mark the cases of croup about to terminate in syncope, the result of cardiac obstruction, are distinct from those arising from obstruction in the air-passages, in the following particulars:—

In the cases of syncope which arise from arrested circulation, the dyspnoea is not caused by obstruction in the larynx, but by the peculiar anxiety and gasping desire to breathe incident to the want of blood in the pulmonic circuit. In this case, therefore, if the stethoscope be carried from the upper part of the windpipe downwards, and over the whole chest, the respiratory murmur is audible, and, it may be, clear throughout, so that the observer is prepared to say that there is here no such deficiency of respiration as will account for the severity of the symptoms. Again, the most common physical pulmonic sign in these cases, is that of emphysema, which is often accompanied, in very young children, by a peculiar prominence in the anterior part of the chest. This emphysema, when present, is strictly diagnostic of fibrinous obstruction, and is altogether subversive of the idea that the cause of the symptoms is an obstruction in the windpipe.

In addition, there are, in these cases, the definite signs which mark the cardiac obstruction. The body is cold, and generally pale, almost marbly, but mostly so at the extreme parts. The lips are slightly blue; the cheeks are occasionally the same. The jugular veins are distended. The pulse is irregular. The body is painfully restless. The heart-beats are feeble, quick, and irregular; the sounds muffled. No real convulsions of the limbs occur, but intense anxiety and constant movement.

In those cases, on the other hand, where the death is really due to apnoea—the effect of obstruction in the air-passages—the symptoms are widely different. In these cases there is some point in the respiratory canal where an obstruction can be detected. The lungs show signs of congestion, but never of emphysema. The difficulty of respiration arises from an absolute inability to fill the chest. From the fact of the obstruction being in the respiratory circuit, such blood as passes through it is not arterialed, and the surface of the body, instead of being pale, as in cases of cardiac obstruction, is generally of a dark hue, with the veins more decidedly turgid. The muscles are not simply restless, but actually convulsed violently, the patient being unconscious of the fact; the heart-sounds are clear, and the motion of the heart, though feeble, is rarely tumultuous.

Lastly, the breathing is the first to stop at death, while, in the former case, the heart takes the precedence in this respect.

If then in any given case the practitioner shall find the symptoms referable purely to obstruction in the trachea or larynx, and the circulation unembarrassed, he will operate with good chance of success, granting that the point of obstruction is not too low, and that no further inflammation succeeds. If, on the contrary, he should diagnose the symptoms of cardiac obstruction, whether or not complicated with constriction in

the windpipe, the operation is worse than useless; it will, of necessity, fail, because there are other fatal influences at work which the knife cannot reach, and the most perfect respiration cannot remove.

These same precautions in reference to croup extend to another disease; I mean to diphtheria. In this disorder, as my friend Dr. Milner Barry of Tunbridge Wells has pointed out, fibrinous deposition in the heart is a frequent cause of dissolution, as frequent, I opine, as the obstruction arising from exudative plasma in the windpipe. In considering the propriety of tracheotomy in diphtheria, the same diagnostic rules would apply as in the analogous disease croup.

So much with regard to the acute forms of hyperinosis. In those states where the increase of fibrine is relative, or where there is tendency to deposition from sheer feebleness of the circulation, the rule against depressant measures is the more strongly to be enforced. I can add nothing further in this direction.

We will move from what may be called negative treatment to our third point—the lesson taught by our knowledge, such as it is, regarding a positive course of proceeding for the prevention of deposition. And here, I think, the first great truth which appears is that, to treat disorders marked by hyperinosis successfully, we must treat them by depressants at their early stages only, and must be prepared to move from the depressant system at much shorter notice than is commonly considered necessary. Before there is positively established in the blood an absolute increase of fibrine, I have no doubt that the conditions leading to such increase may be struck down by the use of rational means for the reduction of the force of the circulation; and so sometimes an acute inflammatory disease may be averted by free venesection. But, as it is rare that the practitioner is called to see disease in this early stage, blood-letting is dangerous to recommend as a general remedy. Whither, then, shall we turn to a remedy? We must turn, certainly, to those remedies which, while they reduce the force of the circulation, possess also a solvent effect on the blood. Is mercury such a remedy? I see no reason, either of a physiological or practical kind, to lead me to an affirmative on this question. In its physiological effects on animal organisms, mercury, carried to salivation, produces a peculiar disintegration of blood, but not a fluidity: in many of the cases of acute hyperinosis, in which after death I found large concretions, I trusted throughout to mercury, and gave it boldly, but with no result. The old calomel and opium pill is, therefore, to my mind, a combination deserving its loss of caste. When such a pill does good, I suspect the opium should have the sole credit, if not a little more.

Is antimony deserving of more favour? I believe it is. In a large number of experiments, in which I placed animals under the influence of antimony, I found in every case that the effect of the substance was to maintain a fluid condition of the blood; while common experience confirms that the drug has a power of arresting the force of the circulation. The danger of antimony, indeed, is that it may kill by the very depression which it produces; and its value, to put the opposite, is in proportion to the sthenicity of the disease. Its value, consequently, is limited: add to which, the remedy may be put into the body very much quicker than it can be carried out; so that its administration in excess is a pretty sure way of securing a prolonged convalescence, should the hyperinosis even be checked, and the patient recover.

Salines, what of them and their uses in hyperinosis? In these, I opine, we have not only the oldest, but the best remedies. I speak specially of the vegetable salines. As I have often pointed out, it is a curious fact that the common vegetable acids, citric and acetic especially, act on the blood exactly as the alkalies; they maintain, that is to say, the fluidity of blood. In combination, therefore, these bodies have a rational intention in the treatment of all disorders attended by excess of fibrine, particularly when they are given with a free quantity of water. Given in full and frequent doses (and it signifies, probably, but little which acetate or citrate is chosen) they are, after all, the safest, and, according to our present knowledge, the surest medicines in the diseases under discussion. It is not easy to push them too far; they do not accumulate; they put a gentle check on the circulation; and their transformation in the economy does not apparently diminish the solvent effect produced on the blood by their administration.

The narcotic series of remedies, dangerous as they become in late stages, are invaluable in the early stages of inflammatory disorder. I do not now speak of opium alone, but of the volatile as well as of the stable narcotics. The whole class

have the physiological property of arresting the oxidation of the tissues, and, as a result, of calming that excitement which is the preliminary and necessary step to hyperinosis. The more rapidly acting narcotic is therefore often the best; and if chloroform, as the most rapid of ordinary narcotics, were a practical remedy—a remedy which could be entrusted to other than medical hands for its administration—it were, I believe, the best antiphlogistic known. I have treated croup at its onset with chloroform by inhalation, with a success such as I have seen from no other plan of treatment. But the effects of the drug must be fully carried out; and, unfortunately for practical utility, the effects must also be sustained, often for many hours, and must be watched with an attention which none but an experienced mind can give. Opium, from being more convenient in its application, has the advantage of chloroform as a presentable remedy; but, as it is so much the less rapid in its action, and so much the more permanent in its effects when these are elicited, it is in a physiological sense so much the less valuable as an active remedy against deposition.

External applications in the shape of counterirritants, blisters and sinapisms, whatever may be their influence on the local lesion, can have no direct influence in preventing deposition. If cantharidin applied to the cutaneous surface be absorbed, it may have an effect on the blood; but whether that is for good or for evil it is impossible to say. I do not make these remarks on counterirritants in opposition to them as local remedies for local affections, but simply to indicate that they are not to be considered as of any avail in meeting the more general mischief present in the inflammatory condition.

Presuming that the practitioner is under suspicion, in a case of acute hyperinosis or in a case of an asthenic kind, that deposition of fibrine is recurrent, what course shall he pursue? I know of but one: it is to give up at once all idea of carrying depletion further, and to throw in ammonia freely but with discretion. The carbonate of ammonia is the remedy usually given; but the liquid ammonia is the most direct and most satisfactory remedy: well diluted, it may be given in doses of five, ten, and by increasing it, twenty drops; and it should be given until it shows itself demonstrably and easily in the breath on the application of the hydrochloric acid rod.

It may be given at the same time that quina or the decoction of bark is given; but in most cases I suspect it forms alone the natural remedy. It is a chemical remedy; that is to say, applied for definite intentions.

But, while I thus recommend ammonia as a remedial measure which has to me proved of most value in the cases under description, and as a remedy which I have often brought into use in the early stages of acute disease, and always with advantage, I warn you that, in prescribing it, its effects must be carefully watched. Carried to an extreme, it produces the very opposite condition to hyperinosis; and then it is no longer a remedy—it is a poison. In one case, recorded in my essay on the *Coagulation of the Blood*, in which five grains of bicarbonate of ammonia were given every two hours, the remedy acted, I fear, as a poison to a certain extent. The patient was sinking, and in some measure comatose, when the administration began; all the excreting processes being much diminished. After death, the odour of ammonia in the blood was most distinct. The blood was dark and thick, but uncoagulated. I collected half an ounce from the right side of the heart, and exposed it to the air for three days: it underwent feeble coagulation, but its true plastic quality was clearly destroyed. The corpuscles, as is common in such blood, were disintegrated, and modified in character; there were some stellate, others oval, others many-sided, others collapsed, as indicated in the subjoined sketch; but not one was of the natural appearance.

A, B, C, E, F, G, Corpuscles modified by the administration of the alkali.
D, Corpuscles healthy, before the administration of the alkali.

Let me, then, renew this precaution relating to the action of

ammonia, and extend it to the other alkalies. Indeed, the effects of potassa and soda, carried to the extent of producing the same changes, are more serious than those produced by ammonia; for the volatile alkali, being volatile, can escape more quickly from the system than the fixed alkalies, after its withdrawal; it holds, in fact, as a physical agent administrable to man, the same relation to soda and potassa as chloroform holds to opium or belladonna.

In those extreme cases where the fact of absolute deposition is too clearly demonstrated, what, then, shall be done? Alas! I can at this moment point out no satisfactory solution of this question. The day will come when the veil shall fall from our or our successors' vision; but now we can but grope our way, and pray for the light. I for one know, however, the difficulties in the way; and this knowledge, perchance, may be useful to some one in the future. I will therefore sketch them forth as matter for thought, and aids to future work.

It will occur to you all, that the proceeding to be first considered is the removal of the fibrinous deposit by causing its dissolution. This is a definite proposition; the difficulties in the way are equally definite. In the first place, we possess no agent having a sufficiently rapid solvent action on separated fibrine. When fibrine is as yet in solution in the blood, it is the easiest matter in the world to keep it fluid; the addition of a very small quantity of alkali, say two parts of alkali to a thousand of blood, is sufficient for this purpose. But, when once fibrine has separated, its resolution is a difficult matter. True, it is not less soluble than before; but time is destroyed in the process. If, for instance, a solution be made containing so much as four grains of alkali to the thousand grains of a blood containing two grains of fibrine, a period of at least six hours must elapse before such fibrine would be redissolved. But such an amount of alkali thrown into the mass of blood as would bring that fluid altogether to the alkalinity thus required, would be in itself impracticable, because fatal. Under such alkalinity, the blood-corpuscles would be entirely dissolved.

Again, when deposit is the fact, the administration of alkalies by the mouth is in no way a direct means of effecting solution of the obstructing body. Medicines introduced in this way, granting that they are taken up into the blood at all, which is in truth doubtful, are but slowly carried through the heart, and are brought into contact with the concreted mass at a single point. From such limited exposure we can expect no result.

The administration of ammonia by inhalation in combination with the vapour of water, seems at first a reasonable suggestion. I have tried it both in disease and in experimental work. It fails. Ammonia is not absorbed easily by the lungs; and, as absorbed, it is never present in the blood, passing stroke by stroke through the heart, so as to produce any effect on a deposit of fibrine in this organ.

The injection of alkaline solution into the veins may seem a reasonable project. It is worthy of trial in extreme cases, but I fear it would not succeed. If solution of clot could be determined by this process, I am afraid there would also be determined universal dissolution of the walls of the blood-corpuscles; and an animal without corpuscles in his blood is no longer alive. The first microscopist of blood little knew upon what he had dropped when he beheld these little invisibles. If we could or should at any time light on a chemical body which will dissolve fibrine on immediate contact, without destroying the blood-cells or the coats of the vessels, then we should have a remedy on which we might set our hopes. The difficulties in which the question is involved, by means of this antithesis of argument, will be readily understood.

The observations I have made above are, however, intended to bear entirely on cases of a kind where death is threatened within minutes or hours: in that series of cases where the symptoms extend over a long period, such cases as that recorded by Dr. Sayer and Mr. McNab, a course of treatment by a fixed alkali might possibly be productive of real good. Such treatment is based on philosophical and sound argument.

Returning to extreme cases: would it be possible, when the concretion is on the right side of the heart, to remove it by operation? I will relate an experiment bearing on this point. In one case, where a cat died in oxygen gas, I took the animal out of the box immediately after she seemed to cease breathing, laid bare the right external jugular vein low down in the neck, pushed through it a crochet-needle, passed that into the heart, and brought up to the point of insertion a separated mass of fibrine, which had before obviously filled the auricular cavity. In cases of dying in the human subject, where all is hopeless,

I see no harm in doing a similar operation; the diagnosis of concretion on the right side being clear.

Before I conclude, I have to touch for a moment on a special question which has been specially asked of me. It is this. What are the conditions leading to hyperinosis? To this question I give frankly the answer, that it is at present unanswerable. To approach it even requires a new and extended inquiry into the whole subject of the oxygenation of animals—a point to which I may at some future day be enabled to call the attention of the profession, with a knowledge yet to be learned, and aspirations yet to be realised! *Valete.*

Lectures

ON

THE HISTORY OF MEDICINE.

DELIVERED AT

THE GROSVENOR PLACE SCHOOL OF MEDICINE.

BY

ALEXANDER HENRY, M.D.

LECTURE II.

Origin of Medicine. Difficulties of its Investigation. General Characters of the Early Stages of Medicine. Medical System of the Ancient Egyptians. Grecian Legends. Apollo. The Cabiri. Orpheus. Incantations. Melampus. Chiron. Heroes of the Homeric Poems. Æsculapius: Examination of the Myth concerning him. Machaon and Podalirius. The Æsculapian Temples and Symbols. Practice of the Asclepiads. Summary.

In the present course of lectures, I intend to confine my observations principally to the history of medicine as it existed in early ages among the Greeks. This limitation is imposed by the necessities of the case; for the subject is so vast, that a much more extended course would be required to develop the whole history of medicine in a manner which should be at all instructive to you as hearers, or creditable to me as lecturer—even if we rigorously confined our examination to the course of that great medical stream which has steadily flowed down from the earliest Grecian records to our own days. It would be interesting, too, to trace the progress of the parallel but distinct streams in which medicine has flowed in other nations, in India, for example; but this must also be deferred.

In entering on the history of any science, the first question which naturally occurs is—What was its origin? What were the circumstances that first led men to its cultivation? When we apply these questions to medicine, we find ourselves at once unable to answer them in any other way than by conjecture; for the origin of medicine reaches even beyond the period of fable. Yet we may make such conjectures as shall have an air of probability, by keeping in view the parts of man's mental constitution which would be most likely to be called into action in originating a system of healing disease and injury.

The origin of medicine was doubtless connected with the love of health and life, which is instinctive in all. Disease or injury impaired the power of man to act as he had before acted, and produced a feeling of discomfort; still further, disease or injury produced death. The first intimation, then, that these misfortunes were not altogether irremediable, but that means existed by the application of which certain of them might be relieved, would, it may be well imagined, be received with delight; and the lesson taught would be carefully stored up for future application. For, not only would the instinctive feeling of self-preservation be prominent, but also the godlike feeling of compassion for the sufferings of another, the affection arising from the ties of kindred, and perhaps some share of selfishness, would all, apart or together, induce men to seize on every means of relieving disease, and, if possible, of obviating death. The intellectual process was doubtless often one of observation and inference, in its simplest form. A circumstance, ordinary or accidental, was noted, in which a certain cause was followed by a certain beneficial effect; and then the inference was drawn, that the application of the same cause should, in similar cases, produce a similar effect. Let us suppose two or three illustrative instances—highly probable, though purely imaginary.

A man lies tossing in violent convulsions, with large, almost bursting, blood-vessels, starting eyes, and other fearful symptoms. The bystanders are perplexed: they see the danger, but know not how to meet it. At last, blood bursts from the nostrils of the patient, and he is relieved. A man among those standing by draws rapidly the inference that the loss of more blood would be followed by more relief. He therefore makes a gash into some part of the body, so as to draw more blood; and this act is observed to bring back, as it were, life to the patient.

A man eats a portion of some vegetable substance, perhaps more than once, and finds himself fearfully purged. At another time, he is seized with symptoms of a febrile character; a free action of the bowels sets in, and the symptoms are relieved. The same symptoms may again occur in him, or in another man; but they remain intense, for the bowels are constipated. What more natural, than that it should immediately occur to him to use some of that same herb which had before purged him? He takes it, or gives it to his sick friend; relief follows; and a remedy is established.

In such simple manner, doubtless, were the effects of various remedial measures made known to man; and illustrative instances might readily be multiplied.

While we have the subject of the origin of medicine before us, it is interesting to notice the opinion put forth by Hippocrates, in his treatise *Περὶ ἀρχαῖς ἰατρικῆς*, *On Ancient Medicine*. Medicine is there made to be, as it were, a corollary to dietetics.

"The art of medicine," he says, "would not have been invented at first, nor would it have been made a subject of investigation (for there would have been no need of it), if, when men are indisposed, the same food and other articles of regimen which they eat and drink when in good health were proper for them, and if no others were preferable to these. But now necessity itself made medicine to be sought out and discovered by men, since the same things, when administered to the sick, which agreed with them when in good health neither did nor do agree with them." Hippocrates argues further, that this process was the same as that by which man arrived at the knowledge of the ways of preparing food so as to render it wholesome. And he includes this dietetic discovery also under the name of medicine; "since it was discovered for the health of man, for his nourishment and safety, as a substitute for that kind of diet by which pains, diseases, and deaths, were occasioned." These speculations are most ingenious and interesting; and for the full account of them, I must refer you to the Hippocratic treatise from which I have quoted them.

Observation of the effects of certain herbs on animals might also have given hints as to certain remedies. The fable of Melampus, to which I shall afterwards refer, points to this.

The illustrations I have given will sufficiently suggest the manner in which the art of healing may be supposed to have arisen.

In entering on an examination of the first notices we find of the history of medicine, several great difficulties at once oppose themselves to us. In the first place, medical history, like other history, is in its beginning so enshrouded in fable and mythology that it is difficult to always discern the truth. Again, it is very probable that we do not possess all the works of the ancient writers on medicine; and the narratives of the lives and writings of many have failed to reach us. Yet, notwithstanding these and other difficulties which stand in our way, we may, principally from the writings of the poets and historians, form some idea of what was the state of medical knowledge in the mythical or prehistorical period.

In the primitive state of medicine in all countries where we have the earliest account of its practice, there are certain common characters, among which I will enumerate the following:—

1. Disease is popularly looked on as the direct effect of the anger of offended deities, and removable only by propitiation.

2. The origin of medicine for the relief of disease is also attributed to the deities; partly, no doubt, from the idea that the means of removing an evil lay with the producers of the evil; and partly from the exercise of a feeling of honour and gratitude on the part of the people towards those whose medical knowledge had proved the most extensive and most beneficial. Kühn, in his essay *De Philosophia ante Hippocratem Medicinæ Cultoribus*, supposes that the knowledge of the uses of medicines, having been found out piecemeal and principally by accident, in the way which has been imagined in the former