sumption, for instance, would be found to be mutually illustrative, and the correlations of these forms of disease being more successfully studied, practical improvement would most unquestionably result. I believe it would be advantageous to dispense altogether with the term *scrofula* (*swine swellings*) from our nomenclature, whether scientific or vernacular. The disinclination which patients often have to admit the existence of the diseased condition which they believe it implies, and the prejudice and disgust which it excites, operate very injuriously to the interests of science by inducing them to withhold or garble the truth, and I have for some time past, with great practical advantage, substituted the term tuberculous for scrofulous whenever I have found it necessary to designate the disease.

Norfolk Crescent, Hyde Park, March 1854.

ON THE USE OF CHLOROFORM AS AN ANÆSTHETIC.

By H. LOWNDES, Esq., Surgeon to the Liverpool Dispensaries, and late House Apothecary to the Liverpool Royal Infirmary.

[Read before the Liverpool Medical and Pathological Society, November 17th, 1853.]

DURING my residence at the Infirmary, as well as since that period, it has happened to me to have to administer chloroform in a very great number of cases. I have paid much attention to its effects; and I have been led, in relation to this subject, to read a little of what has been written on the physiology of respiration.

I will first mention the results at which I have arrived, and which I lay with great deference before this meeting; and will then state, as clearly as I can, the reasons I have to adduce in support of them.

I believe, then, that chloroform may, and often does, prove fatal by its local effect on the peripheral extremities of the branches of the par vagum, and particularly on those of the superior laryngeal nerve; that is to say, by paralyzing that nerve, and thus preventing it from conveying any impression to the medulla oblongata. Without venturing to say that this is its only mode, I think that this is its usual mode of destroying life; and that the only way of preventing this effect is to take care that the chloroform is freely diluted with air.

This differs, I believe, from the theories hitherto adduced to explain the action of chloroform, none of which seem to be very satisfactory.

That chloroform and ether do produce a local insensibility of the nerves to which they are applied, has been proved by Dr. Simpson, who, by keeping a man's hand immersed for a sufficient time in the vapour of chloroform, succeeded in producing at least superficial insensibility. In another experiment, two nerves of a rabbit being laid bare, one was immersed in ether, while the other was exposed to the air; in five minutes, that immersed in the ether was dead to all sensation, while the other retained all its sensibility and power of producing contraction.

I beg now to draw your attention to the physiology of respiration; and the work I shall chiefly refer to is Carpenter's *Physiology*.

The act of respiration is to some extent under the control of the will. Thus, during sleep, or when the attention is strongly absorbed, the inspirations become fewer, and of a prolonged laborious character; whilst the number of inspirations is always increased by merely trying to count one's own breathing. But, that consciousness is not absolutely a necessary link, for a limited period at least, is shown in sleep and in coma, by anencephalous fetuses, and by decapitated animals.

Respiration is essentially a reflex function; and the upper part of the medulla oblongata is believed to be the nervous centre of this action. The motor or efferent nerves are principally the spinal accessory and the phrenic and posterior thoracic, which are both connected with it; and

the chief intercostal nerves, which, Dr. Carpenter thinks, probably have their origin in the grey matter of the medulla oblongata. The excitator or afferent is the par vagum. When this nerve is divided on both sides, the number of inspirations are reduced to one-half. Irritation of its trunk is immediately followed by an act of inspiration. Dr. J. Reid states that, in a kitten a day old, removal of the encephalon reduced the inspirations from a hundred in the minute to forty; and section of the vagus on both sides further reduced them to three or four, and so they continued for some time. As they did not altogether cease then, it would seem to be also influenced by some other power: probably the nerves ramifying on the general surface, and particularly on the face, have this effect; and the fifth nerve appears to be peculiarly concerned in the first inspiration made by the infant. One branch, however, of the par vagum, the inferior laryngeal, appears to be strictly a motor nerve, as it is distributed to the muscles of the larynx, possesses very slight sensibility, and is capable of exciting contraction of the laryngeal muscles when its separated trunk is irritated. The superior laryngeal, on the other hand, appears to be almost solely an afferent nerve, and is principally supplied to the mucous membrane of the larynx. The superior and inferior laryngeal branches then constitute the circle of incident and motor nerves by which the aperture of the glottis is governed; whilst the superior laryngeal nerve also excites the muscles of expiration, so as to cause the violent ejection of a blast of air, by which the irritant, of whatever nature, may be carried off. The mucous membrane of the trachea and bronchi appears also to be endowed with impressibility, so that stimuli applied to it induce expiratory movements; and these evidently operate through the branches of the par vagum distributed upon the membrane.

The incident or afferent nerves, then, chiefly concerned in inducing respiratory movements, are the ramifications of the superior laryngeal on the mucous surface of the larynx, and those of the pulmonary branches of the vagus on the trachea and bronchial tubes.

I wish here to allude to the analogy between the machinery of the act of respiration and that of deglutition. The only important difference is, that the act of deglutition itself is wholly independent of the will, whilst that of respiration, though it can, and often does, proceed independently of the will, is yet, for particular purposes, placed generally under the influence of the will. Deglutition is solely a reflex action; the principal afferent nerve concerned is the glosso-pharyngeal, the principal motor is the pharyngeal branches of the par vagum. This strong analogy leads me to another point. What is the appropriate stimulus to the act of respiration? That is to say, What is it, the influence of which, conveyed to the medulla oblongata, prompts the reflex action of respiration? It has been assigned to two causes. First, it is said, that the sense of venous blood in the lungs requiring oxygen is the exciting cause; and secondly, that the presence of venous blood in the general circulation may be the exciting cause, through the medium of the sympathetic system. But neither want of food in the stomach, nor want of nutritive matter in the general circulation, can cause the reflex act of deglutition; they cause indeed the sense of hunger, but it is the actual presence of a substance in the fauces that is the exciting cause of deglutition; and I believe that it is the presence of air in the larynx and trachea that is the exciting cause of the reflex act of respiration.

It is true that the presence of venous blood in the lungs and in the general circulation will cause a sense of want of air, as the absence of food in the stomach, or the want of nutritive matter in the circulation, causes the sense of hunger and thirst; and when a person is conscious, he will make voluntary efforts to relieve this want of air. But, when a person is in a state of unconsciousness, he cannot make these efforts, any more than he can in that state feed himself. The par vagum, however, still conveys the impression of its proper stimulus, air; and the respiration still goes on, in the same way that the want of consciousness does not at

all imply the loss of the power of deglutition. Let us look at an attack of asthma under this view of the question. From spasmodic contraction of the air-tubes, the air enters the lungs very imperfectly; the want of air in the lungs and system is urgently felt; the patient makes strong voluntary respiratory efforts; he fixes his arms, to give a firm basis for the pectoral muscles; but, in addition to this, he exposes himself to the open window, where he knows, from experience, that the fresh air gives the muscles under the control of the reflex system that stimulus which his own will is giving to the voluntary muscular system.

So powerful is the tendency to the reflex act of respiration, so long as air is present, that no man is recorded to have succeeded in destroying himself by merely holding his breath, although attempts have been made. After a time, convulsive efforts occur, that outweigh all the voluntary efforts at control. There is another argument which presents itself to show that the presence of air, and not that of venous blood in the capillaries, is the exciting cause of respiration. When a child is just born, and the cord not yet tied, why does it breathe? The circulation proceeds in the same manner that it has done *in utero* for some time; but I believe that the presence of air in the throat, aided by the stimulus of the cold air on the skin, particularly the face, gives rise to the first inspiration.

In speaking of the action of chloroform, particularly in the lower animals, I shall mention other facts that tend to show that the presence of air is the appropriate exciting cause of respiration.

If, then, in a state of unconsciousness, respiration depend entirely on an impression conveyed by branches of the par vagum, it follows that the effect on the respiration will be much the same, whether the par vagum be divided in the neck, or whether its peripheral ramifications be completely paralysed. I have already mentioned that, in Dr. J. Reid's experiments, when the par vagum was divided on both sides, the number of respirations was immediately reduced to one-half. In one of a number of experiments with chloroform, carefully made by Mr. Edward R. Bickersteth, as soon as anaesthesia was produced, a small opening was made in the abdominal parietes, and a finger introduced, and placed on the diaphragm. After two minutes, its descent declined from thirty-eight to twenty-five in the minute, and, at the same time, it descended less completely. When animals are made unconscious by the removal of the encephalon, division of the par vagum on both sides instantly brings respiration almost to a stand still. In the same way, Mr. Bickersteth informed me that he destroyed many animals before he succeeded in giving them the chloroform in a sufficiently diluted form: and this is at once explained by supposing that the extremities of the branches of the par vagum, and particularly of the superior laryngeal nerves, become rendered insensible to the presence of air by the too concentrated vapour of chloroform.

Were it only the absence of air that produced this effect, the removal of the chloroform should immediately cause the recommencement of respiratory efforts; whereas air has to be steadily injected for some moments—that is, until the insensible nerves recover their impressibility: and thus Mr. Bickersteth succeeded in suspending respiration, and again restoring it time after time, so long as the heart's action did not cease.

I think, then, that at least one of the modes of death from chloroform is the one I have pointed out. A certain mode of preventing it is the admission of plenty of air with the vapour; and if respiration unfortunately do become suspended, I believe that prompt and persevering artificial respiration affords the only hope of recovery. But artificial respiration affords much less chance of success to man than to the lower animals; for in him the functions of respiration and circulation appear to be more delicately connected. Thus, in a dog or a hare, their prolonged and rapid movements must cause the breath to be suspended for some time, whilst the heart still goes on actively; whilst man is soon obliged to pause for want of breath; and I believe that in him the respiration can be entirely suspended

for a very brief time, without the heart ceasing to beat. In the case of two rabbits killed by chloroform, the action of the heart went on for an hour after the cessation of respiration. On account, also, of this intimate relation between the action of the heart and lungs in man, I believe the pulse is a valuable indication in the administration of chloroform, as the breathing cannot much fail without the pulse falling; and besides, the breathing itself is apt to deceive, as a person may breathe slowly, and yet very fully, or may breathe quickly, and still most imperfectly.

Chloroform is not the only substance that may act in this manner. I believe prussic acid sometimes destroys life in the same way; and a very interesting case, which seems to be of this kind, is mentioned by Liebig. He states, that two drops of anhydrous acid, in four to six ounces of water, produce no effect on a cat when swallowed; the same quantity of acid, placed on the tongue of a cat, causes no perceptible effect, provided the animal be prevented from breathing by stopping its mouth and nostrils; but the animal dies the instant it breathes, because the vapour thus reaches the lungs. The fumes of tobacco also exert a temporary anæsthetic influence on the laryngeal branches of the par vagum. When we enter a room of smokers, we are nearly choked, our throats are so much irritated; but, as soon as we lessen the impressibility of the nerves by the stronger fumes of our own cigar, we feel no further inconvenience.

Another view of this subject may be and has been taken: it has been said that the chloroform gradually absorbed into the system affects first the brain itself, and progressing downwards attacks the medulla oblongata and spinal cord; but if this were the case, the danger would increase in proportion to the quantity of chloroform administered, however gradually this might be effected, and thus Dr. Taylor actually thinks. Speaking of ether, he says: "The continuous exhibition of morphia or strychnia at intervals so short, as not to allow of a recovery from each successive dose, must cause an accumulation in the system, and lead to fatal results. It is so with ether vapour: and experience now points to the propriety of withdrawing its use altogether, in those cases in which the administration of it would require to be prolonged for a long period. A man may breathe a mixture of carbonic acid or sulphuretted hydrogen with air for a few minutes, but he would die if compelled to respire it for half an hour or longer." The answer to all this is, that Dr. Simpson has kept a patient under the influence of chloroform in midwifery practice for thirteen hours. I have, in a case of natural labour, administered it for an hour and a half; and for three quarters of an hour I have kept a patient deeply insensible during a prolonged surgical operation,—the removal of a tumour of the uterus. Moreover, we do not find that in the fatal cases recorded, and to which I shall soon allude more particularly, death did take place after a prolonged state of insensibility, or in general after a large dose. It is to be noticed, that death has occurred generally either just before or just after the commencement of the operation; at a time when the person giving the chloroform is very often hurried, and tempted to keep the saturated lint close over the patient's mouth and nostrils. There are cases of death by chloroform scattered, at long intervals, through the pages of the medical periodicals, but in general few details are given, and it is difficult to draw any conclusion from them. But since I commenced to write this paper, three well marked cases have occurred: one at the Edinburgh Infirmary, another at St. Bartholomew's, and a third at University College Hospital; and the history of the cases, and of the *post mortem* appearances, has been published by careful and experienced observers. To these I beg to draw your attention.

They resemble one another in the fact that in each case death occurred immediately before the moment at which the operation would have commenced. In one case a very small quantity of chloroform had been given, stated to be only one drachm and forty minims. In another about an ounce had been given; and in the third the quantity is not mentioned. (I may mention that in a fourth case, which

occurred at University Hospital in March 1853, it was stated that not more than a drachm had been given.) In two of these cases death almost immediately supervened on the cessation of violent struggling. In the third there is not stated to have been any struggling; but the countenance assumed a dusky hue, which was the precursor of death. The patients were all three in the prime of life, two of them particularly healthy subjects. After death in one case considerable fatty degeneration of the heart was found; in the other two this organ was perfectly healthy. In one case the heart was collapsed and empty; in another case collapsed, but containing a small quantity of fluid in its cavities. In all the cases the lungs were healthy, and in two it is particularly remarked that they were crepitant throughout, and not much engorged; nor was there noticeable congestion of the brain in any case. The blood was remarked in two of the cases to be unusually fluid, and in one when exposed to the air in an open vessel did not coagulate, nor lose its dark purple colour.

In only one of the cases does artificial respiration seem to have had a thorough trial, viz., in that at St. Bartholomew's, and the pulse had previously been noted to be weak and fluttering, so we may presume that the heart's action had failed beyond recovery by this means. Cold affusion and injections of brandy into the rectum were tried in one case; tracheotomy was performed in all the cases, and artificial respiration tried in this manner but without effect. In the case at Edinburgh, Dr. Dunsmure asserts that the respiration actually proceeded after the pulse had ceased to be felt, whilst in the other cases the breathing and the pulse seem to have been almost simultaneously affected.

The *post mortem* appearances I have mentioned are not inconsistent with the manner in which I have attempted to explain the mode of death. We do not find either the frothy congestion of the lungs, or the congestion of the brain, that are found in cases of sudden suffocation which have been accompanied by violent efforts to get breath, the heart's action going on for some moments after the ingress of air has been prevented; nor do we find the ventricles of the heart full of blood, as we might expect if death had resulted from syncope or paralysis of the heart, to which it has been ascribed by some. But the appearances are quite consistent with the supposition, that the action of the lungs has gradually failed, that of the heart becoming weaker in unison with them, the patient being unconscious of the want of air, and therefore not exerting the voluntary respiratory muscles; whilst the par vagum is becoming insensible to its natural stimulus—the presence of air, and no longer excites to action the respiratory muscles, which are under the influence of the reflex system.

Before leaving this subject I may observe, that if this theory be a correct one, tracheotomy must be injurious rather than otherwise. I believe that when the tongue is well pulled forwards, air can be easily blown into the lungs; and the superior laryngeal nerve thus receiving its appropriate stimulus, will probably soon recover its sensibility. For several cases in which artificial respiration, adopted instantly on the occurrence of alarming symptoms, has proved successful, I beg to refer you to Mr. Edward R. Bickersteth's paper on Chloroform, published in the *Edinburgh Monthly Journal* for September 1853. I have myself seen two cases in which I have no doubt life was saved by this means.

I have now endeavoured to prove that one mode of death by chloroform is by paralysis of the incident nerves concerned in the reflex act of respiration. That in general, and particularly in the cases I have noticed, death does not occur from the poison affecting the heart or the medulla oblongata by absorption, and that it is not the result of mere absence of air. But I cannot deny the probability of death happening in other modes under certain circumstances. Mr. E. R. Bickersteth has brought forward cases to prove that the shock of an operation may occasionally prove fatal, whether a patient be insensible or not, but I have never myself seen any case where any noticeable change followed the first incision; while many of the deaths

that have occurred just before the intended incision, if the patients had lived a few moments longer, might have been reasonably but fallaciously used as illustrations of death from shock.

In the cases of narcotism in children, it does appear as if the poison acted by absorption, and I shall speak of its action in them in a few moments; but to illustrate how it may be absorbed in excess, I will mention an experiment, one of those before referred to.

Mr. E. R. Bickersteth having given chloroform to a cat and produced insensibility, opened the trachea and inserted a tube. The thorax was then opened, and the pericardium exposed; the diaphragm and heart now acted regularly; the lungs were now inflated from an elastic bag with air saturated with chloroform; soon the diaphragm ceased to act. The heart's action now began slowly to get weaker, until it nearly stopped; pure air was then substituted for the chloroform, and the lungs again inflated, when the heart's action rapidly became strong and natural. The chloroform was then again injected: the diaphragm, which had again begun to act, soon ceased, and the heart's action nearly failed again. In the same way in the human subject, we can conceive that, after the chloroform has produced its proper effect, an excess of it may yet be given so carefully and gradually, as not to paralyse the branches of the par vagum, and yet to cause death by becoming absorbed. But it must require great want of judgment on the part of the administrator, to go on giving it long after its anæsthetic effect is produced.

With regard to the cases in which chloroform should be given, I think the danger from it when carefully administered is very slight, that we may obey the dictates of humanity, and give it in every case where the pain to be averted would be either very severe or very prolonged. I do not think we should give it to relieve slight and momentary pain, except in peculiar cases. From what I know of its effects, I should always give it in natural labour; but my limits do not allow me to enter now on this debatable ground, though should no one more conversant with this kind of practice bring the question before the society, I may venture to do so at some future time.

I think that any disease of the heart, or known tendency to apoplexy, or serious disease of the lungs, should prevent us from administering chloroform, though I have given it for severe operations in more than one case where the lungs have been much affected with tubercular disease, without any bad effect. In dislocations it is useful not only as preventing pain, but as completely relaxing the muscles; for this relaxation almost invariably is the result of very deep anæsthesia.

Lithotomy, operations about the eye, and others of a delicate nature, require the patient to be very deeply narcotised; but for operations in general, it is worth remembering that consciousness to pain appears to be lost some time before either complete relaxation of the muscles takes place, or before deep coma comes on. An useful symptom of the very deep coma is the upturned state of the eyes.

I will just mention one case from notes that I have of several to illustrate the different stages. On July 24, 1853, chloroform was administered at Erskine Street Hospital to a young man, for the removal of a tumour from the orbit. This patient, in passing to a state of unconsciousness, underwent extreme excitement in the cerebral circulation, not manifested by any violent muscular movements, but by violent congestion of the face, and turgescence of the capillaries of the conjunctiva, accompanied by very fully dilated pupils. There must be danger in this state, although it is the first stage, and hardly removed from consciousness; the danger here to be apprehended is apoplexy. In this patient, stertorous breathing accompanied the above-mentioned symptoms; and I have always observed, that when stertorous breathing does occur, it does so at a point intermediate between the first stage of unconsciousness and that of profound insensibility. More chloroform was given; the breathing became quiet, and the pulse small: this is the stage of most danger, when a small overdose or want of

sufficient air may stop the respiration. I have seen two cases, both occurring in elderly females, in whom suffocation nearly occurred from the tongue falling back into the fauces. The breathing was suspended in both cases, and the simple pulling forward of the tongue was followed by a long inspiration. This was pointed out to me by Mr. Edward R. Bickersteth.

In young children, the administration of chloroform is an anxious duty, and requires the greatest care. Very little should be put on the fold of lint, and it should be very carefully held two or three inches from the mouth and nose. No attempt should be made to get them very quickly under the influence. They seem to be peculiarly susceptible of its poisonous influence, and these are cases in which I believe it really does kill by absorption, and by thus producing syncope; for often after the operation is over, and the chloroform removed, young children fall into a state of very deep insensibility, and we are alarmed to find the pulse falling, and the colour deserting the face. In these cases, I have found that raising the feet to a higher level than the chest, together with the application of warmth to the body, have the best effect; and the access of cool air to the face and lungs is essential.

That vomiting should be a frequent accompaniment of insensibility from chloroform need not surprise us, when we consider the close sympathy established by the pneumogastric nerve between the lungs and the stomach, which seems, in a great measure, to explain the effects of ipecacuanha, squills, etc.; also the effect of various states of the stomach in inducing fits of asthma. Vomiting is easily prevented by prohibiting the taking of any food for three or four hours previous to the operation.

I shall be very glad if my drawing attention to this subject shall tend to cause greater care to be taken in the administration of chloroform; as otherwise I feel convinced, that deaths from it will be of no unfrequent occurrence, and will strengthen the prejudices that some still retain against its use.

1, Caxton Terrace, St. James's Road, Liverpool, March 1854.

THE NEGLECT OF THE USE OF BLEEDING IN THE TREATMENT OF SOME OF THE Milder AILMENTS.

By J. A. HINGESTON, Esq.

EVER since the publication of Dr. Marshall Hall's excellent remarks on the danger of excessive blood-letting, many years ago, there seems to have prevailed among English practitioners a reverential horror of the loss of blood. Diseases are treated as successfully in the present day without the loss of blood, as they used to be formerly with the almost unrestrained use of the lancet. This decided change of practice is brought forward as a triumphant proof of the improvement in the treatment of disease, whereby a cure is effected without having recourse to those remedies which drain off the force of the vital fluid. It is affirmed, that the general health suffers considerably less under this milder mode of management than it did under the more heroic practice of our predecessors.

Granting that there is a great deal of sound truth and reason in the foregoing remarks, I, for one, cannot give my entire consent to the conclusion legitimately deducible from them. I was born and bred in the practice of medicine. I can look back upon an interval of thirty-five years at least, during which period this decided change of practice has taken place. I can remember the time when bleeding was, as it is styled, "the order of the day"; and I own, that on calling to mind many cases in which this practice was adopted, it appears to me, at this distance of time and at my present mature age, to have been sometimes both unnecessary and, perhaps to a certain degree, injurious. It was, however, the practice then in vogue, just as the non-bleeding practice is the one most in vogue now. It received the

common consent of the best practitioners; and, in this sense alone, it was certainly entitled to our respect.

What is most approved of for the time being, is upon the whole the best. The character of disease has altered. Nervous diseases prevail now in the same manner as the phlogistic maladies prevailed up to the year 1832. The latter demanded frequent loss of blood, whereas the former are rendered worse by bleeding, but are cured by tonics and a generous diet. It is probable that many nervous diseases were overlooked during the phlogistic period, just the same as that phlogistic diseases are overlooked by the practitioners of the present day. There is nothing to congratulate ourselves upon; for, if we act prudently in withholding the lancet now, our predecessors acted quite as prudently in making use of it formerly.

Taking this view of disease, we must allow that a great deal may be said in favour of both kinds of practice. Truth generally lies half way between the disputants. What I am contending for is, that we are disposed to look too much on one side of the question only, and are apt to lay aside the use of the lancet in the ordinary course of practice, to the prejudice, as it appears to me, of the beneficial and successful treatment of many of the milder ailments.

I have heard practitioners boast of never carrying their lancets with them, and of being able to treat almost every illness that comes before them without ever being forced to have recourse to it. In short, they declare that their lancets are next to useless. Without questioning their ability in the management of disease by means of remedies much less obnoxious than that of blood-letting, we must at least be permitted to declare that such a boast as this is not merely an idle one, but it also shows that they are wilfully resolved to exclude as far as they can one of the grand agents in the practice of medicine.

To bleed properly, requires a good deal of time. It delays the routineer in the progress of his round. It cannot be dismissed in a few minutes. It imposes the delay of an hour or more. Some time must be spent in watching the flow of the blood, and its immediate effects on the constitution. If the patient should faint, the delay is still longer. I am inclined to think that the trouble of bleeding is one of the chief reasons for omitting it, whenever this can be done with safety.

The use of blood-letting is very great on many more occasions than those of acute inflammation, in which it is absolutely unavoidable. Many diseases, or even ailments, are rendered much more manageable by premising the general treatment with a single venesection. It is not the quantity lost—it is the effect produced. A large quantity is not requisite. A small quantity will frequently relieve in the most unequivocal manner. A teacupful, for instance, or only half as much, taken away in a full stream, will ameliorate all the symptoms, afford a refreshing sleep, cause the skin to perspire, and the bowels to relax, increase the appetite, and give a feeling of relief and ease which no other remedy or any mode of diet is able to supply. This happy result is more particularly the case, when the pulse has been found contracted, suppressed, or jerking. It will occasionally decide the cure, and scarcely any medicine is needed afterwards. Moreover, if we are in doubt as to the propriety of blood-letting, a small quantity taken away in a deep cup, and allowed to stand apart for twelve hours or so, will afford us an opportunity of inspecting the quality of the blood, and of informing ourselves of the exact state of the constitutional diathesis.

In all cases of local fulness of blood, bleeding ought not to be omitted, either by leeches or the lancet. In cases of debility even, it is excellent practice to take away blood, and to give steel at the same time. The shock sustained from a violent blow or concussion, hereditary plethora, venous torpidity, visceral congestions, etc., are each of them relieved, if not cured, by a judicious loss of blood. Sedentary habits, a good deal of in-door occupation, and the close confinement of certain trades, lead to many of those passive derangements of the health, which yield to venesection,