and strychnine. The neural trouble of herpes zoster is wonderfully controlled by quinine and arsenic. An efficacious treatment of gout used to be by quinine and colchicum. But I must close my list of illustrations by a bare reference to bromine and camphor, lobelia and stramonium, belladonna and chloral; and simply add my conviction that in no branch of our therapeutic work is there more legitimate ground for extension than in joining together things of different constitution and yet similar action. And I have not time to do more than express a hope that some here to-day will speak from a larger experience of cucaïne, electricity, heat and cold, blisters, and mineral waters as essential parts of our apparatus for gaining a mastery over pain.

The purely chirurgic part of our therapeutic warfare is so large that a bare enumeration of all the means which the surgeon enjoys would occupy much time and space. But when the surgeon has done his operative work, his medical function begins; and it is decidedly more necessary for him to be a good physician than it is for the physician to be a competent surgeon. Both surgeon and physician may, however, lawfully ask the pharmacologist why, in order to blunt the excess of sensibility technically called pain, we are compelled so often to use what Shakespeare calls "insane roots which take the reason prisoner?" The ideal analgesic is one which, however administered, bars the currents that tell of a starved and injured nerve, or of a nerve poisoned by bad blood, and yet spares the consciousness and the intellectual faculties. As yet such a boon is not fully granted, although cucaïne seems to be a little step on the road; but I venture to think that this is the direction in which our inquiries and experiments should go. Of general anæsthetic vapours we have almost more than enough; and this type of analgesic is the exact opposite to what we are now in special quest of.

For the present, let us make the most of the possessions we have, using them with definite aims, and under the guidance of recognised physiological laws. The want of harmony among medical men in the prescription of the commonest medicines is a reproach to high scientific pretension; and it is becoming more and more inexcusable. There is a touch of sarcasm about this in Charles Kingsley's *Water Babies*, which deserves to be quoted. On a particular case, he says: "All the doctors were called in to make a report; and, of course, every one of them contradicted the other, else what use is there in having men of science?" May not this contradiction be checked by methodical habits of observation and record? A writer of distinction says that there are two species of so-called practical minds. One sort of mind is constructed according to the carpet-bag pattern, where nothing is put in any order, and, to find any article, everything else has always to be turned out. The other is the museum sort of mind, in which every specimen is arranged and labelled in proper sequence, and whatever is wanted is always easily found. Peep into the *penetrabilia* of our surgical friends, and behold all their manual apparatus stored in neat and handy array: splints, sutures, lancets, knives, syringes, probes—everything is in its place, and responds immediately to the master's call. Now would it not be an excellent training of the medical intellect if the physician had his little cupboard, duly furnished with analgesic medicines, arranged in therapeutic groups, and always pure and ready for instant use?

To conclude. The spiritual elements and forces around us are the most gracious anodynes, full of consolation and hope. They deserve our thankful commemoration, and a coarse materialism shall never put them aside. A medical man himself may be a good and powerful anodyne, by reason of his truthfulness, self-sacrifice, and loyal care. Without the analgesic touch of sympathy all our cleverness is but dust and ashes. And if we have what Kingsley calls the Christian "fire in the heart," which prompts us to dare and do our best, we shall regard it as the noblest badge of our craft to lighten the sorrowful burden of pain—the penitential heritage of a sinning and dying world.

AN UNCONSIDERED FACTOR IN THE MOUNTAIN-CURE OF PHTHISIS.

By C. CREIGHTON, M.D.

BESIDES the mechanical action of a rarefied atmosphere on the lungs in phthisis, there seems to be a direct physiological action upon the blood and tissues, which would serve in part to explain the mountain-cure according to one of the theories of the nature of tubercle. The therapeutic action to which I refer is deducible—I am not aware that it has already been deduced—from a research of the late M. Paul Bert on the blood of animals acclimatised to the great altitudes of the Bolivian plateau. The application which that distinguished physio-

logist made of his facts was not to phthisis, but to the peculiar group of functional disturbances known as mountain-sickness, or *mal des montagnes*. But the physiological adaptation to a rarefied atmosphere discovered by him must have a general value, and my object is to show that it is of such a kind as to be of peculiar service to the phthisical.

In the *Comptes Rendus* of the Paris Academy of Sciences (vol. xciv, 1882, p. 805), M. Paul Bert communicates a short paper "Sur la Richesse en Hémoglobine du Sang des Animaux Vivant sur les Hauts Lieux," of which the argument and facts are as follows: Jourdanet's theory of the symptoms of *mal des montagnes* (hurried breathing, quick pulse, palpitation, giddiness and swooning, troubled sleep, nausea, muscular pains, and fatigue) is, that the oxygen in the blood is suddenly diminished, owing to the individual having moved into a rarefied atmosphere, or an atmosphere where the tension of that gas in the air is much lower than at the sea-level. That theory accords with the well-known physiological fact that the oxygen loosely held in combination with hæmoglobin, forming the compound of oxy-hæmoglobin, is parted with in a vacuum, or under low barometric pressure. The *mal des montagnes* seldom lasts long in men or animals removed to great elevations; they become acclimatised; and the adaptation, according to the theory, consists in the amount of hæmoglobin held by the blood being increased. The tendency to the loss of oxygen loosely held in combination as oxy-hæmoglobin would be made up for by an increase in the capital stock of the hæmoglobin itself.

In order to put that hypothesis to the test of fact, M. Paul Bert resolved to examine the blood of animals acclimatised to great altitudes. A discovery made by a former assistant in his laboratory, M. Joylet, now professor at Bordeaux, showed that decomposition of the blood sent from a distance to be examined in Paris would not matter. This fact was that the hæmoglobin in the blood was not affected by the putridity of the latter. M. Joylet found that putrid blood, when shaken, took up the same amount of oxygen as fresh blood. Accordingly, M. Paul Bert sent out to Bolivia for samples of the blood of various animals; and, after waiting a considerable time, he received from a French resident of La Paz, a town some 12,000 feet above the sea, samples of the blood of seven species of animals, mostly herbivorous, including llamas, sheep, and others. When the specimens of blood were shaken up in air, they absorbed oxygen in various ratios, from 16.2 to 21.6 cubic centimetres per 100 cubic centimetres. As the maximum amount of oxygen taken up by the blood of herbivorous mammals at the ordinary elevations in France is 10 to 12 cubic centimetres per 100 cubic centimetres, it followed that the corresponding animals born at or acclimatised to an elevation of 12,000 feet, had a capacity for oxygen in their blood half as great again as at the sea-level. Such is the argument of the paper in the *Comptes Rendus*, and I do not profess to say whether it is sound at every point. Jourdanet's hypothesis that mountain-sickness depends on a loss of oxygen from the blood, in consequence of the lowered tension of that gas in the air, with the complementary theory that the overcoming of the sickness after a few days or weeks is an acclimatisation to the rarefied atmosphere due to an adaptation in the amount of hæmoglobin, is thus held to be confirmed.

Apart altogether from the question of mountain-sickness, and with reference even to elevations where that group of symptoms is not manifested, there would always be an adaptation in the oxygen-carrying capacity of the blood to a rarefied atmosphere. The effect of going to reside a good many thousand feet above the sea-level will be to increase, by way of natural adaptation, the amount of hæmoglobin in the blood. The loss of oxygen from oxy-hæmoglobin, which takes place under low atmospheric pressure, would be met by providing a larger capital amount of the hæmoglobin to work with. An adaptive increase of hæmoglobin at great elevations may thus be turned to account as a contingent advantage in the treatment of constitutional disease.

Chlorosis is the disease where the contingent advantage is most obvious. By whatever means the hæmoglobin in the blood may be increased, a chlorotic person will be benefited; and it is in a rarefied atmosphere that the hæmoglobin has a natural inducement to increase. Of course, it does not follow that the adaptive powers of the organism are always equal to the call made upon them by circumstances; but there are cases of anæmia (and chlorosis) where benefit has actually been got from high altitudes. The Engadine is frequented by those who suffer merely from anæmia, as well as by those who are truly tuberculous.

It is, however, with phthisis or a tuberculous diathesis that I am here concerned. How does an increased capacity of the blood for oxygen bear upon the therapeutics of that disease? In the first place, it is admitted that there is something antagonistic between high alti-

tudes and phthisis. In the populous towns on the Bolivian plateau, and elsewhere in the Andes, phthisis is practically unknown, although the ordinary exciting causes, such as industrial occupation, sedentary habits, and the like, are the same as at lower levels. It is significant that Professor Hirsch, in treating of this matter (*Geographical and Historical Pathology*, iii, p. 211, English translation), seems to say that the immunity is due to an almost universal or ubiquitous tendency being counteracted. Although Dr. Hirsch accepts Paul Bert's hæmoglobin theory for the *mal des montagnes*, and the acclimatisation thereto (vol. ii, 505), he does not go beyond a mechanical explanation for the mountain-cure in phthisis. Living in a rarefied atmosphere, he thinks, will lead to a sort of pulmonary gymnastics; one must inspire deeper or oftener to cover the requirements for oxygen; and that will lead to a more vigorous development of the breathing organs, and a greater power of resistance on their part to noxious influences from without (*op. cit.*, iii, 212). But it may be questioned whether any such vigorous development of the organs, or more active exercise of their mechanism, does, as a matter of fact, show itself very decidedly in the residents at great altitudes in South America or elsewhere. The only evidence that I know of is the rather vague statement of Archibald Smith for Peru: "In the Indian of the plateaux, the lungs are capacious, bearing a due relation to the tenuity of the air which he inspires" (*Edinburgh Medical and Surgical Journal*, 1842, p. 356).

It may be useful, therefore, to try how a physiological adaptation will work, rather than a mechanical.

The pathological view of the tuberculous diathesis, to which I referred at the beginning, is based upon a certain interpretation of the structural facts of the disease. Although the condition is not one of anæmia in the ordinary sense of the term, yet it may be said to have deficient power of tissue-vascularisation as its most conspicuous feature. According to my reading of the facts, suggested by analogous normal processes in the placenta, of which I gave an account some time ago (*Journal of Anatomy and Physiology*, January, 1879), the caseous decay, the giant cells, and the other pathognomonic characters of tubercle, are explained as being due to growth, renewal, or overgrowth of tissue in circumstances where the power of vascularity is inadequate. I should be going too far afield for my present purpose if I were to discuss the interesting question of how the human body has acquired, or continues to acquire, that peculiar morbid type of tissue-formation, namely a growth redundant or excessive, without being properly vascularised. But whatever view we take of the remote antecedents of the tubercular diathesis and infection, most persons will admit that overgrowth without vascularity—or, perhaps, overgrowth because of non-vascularity—is characteristic of tubercular tissue.

If we go beyond tubercular tissue actually formed, and speak of the tubercular diathesis, inherited or acquired, which favours the local formation of it, we should say that the diathesis meant feeble vascularisation, just as we might speak of a person's innervation being constitutionally feeble.

But the power of vessel-making is at bottom bound up with the power of blood-making. Whatever helps the one will help the other. It is thus conceivable that a general stimulus to the greater production of hæmoglobin in the blood, such as a rarefied atmosphere brings, would directly meet that ingrained or constitutional defect which we know as the tubercular diathesis, assuming, of course, that the individual has any power of adaptation left. That is a somewhat vague position in which to leave this curative effect of altitudes; but it would clearly be hazardous to trace a more particular relation between increased capacity for oxygen in the blood, and that increased power of vascularisation in a redundant tissue, which we have good reason for identifying as the means of cure, or the way to safe repair or obsolescence, in cases where the tuberculous diathesis has actually come to expression. In cases where there is merely the tendency or the constitutional taint, inherited or acquired, the same sort of explanation would serve for the preventive benefits of mountain air. The more complete the adaptation of the blood to the rarefied atmosphere, the more permanently would the tubercular diathesis be held in check. The more naturally or imperceptibly the increased capacity for oxygen (greater richness in hæmoglobin) is called forth by the general environment of the patient, the more fundamentally will the disease be counteracted.

SUCCESSFUL VACCINATION.—Mr. F. W. Lewis, Surgeon and Public Vaccinator for the No. 1 District of the Llandoverly Union, has received the government grant for successful vaccination. This is the second time in succession that Mr. Lewis has received the grant.

THE POSITION AND THE VALUE OF THE OPERATION OF INTERNAL URETHROTOMY.¹

By G. BUCKSTON BROWNE, M.R.C.S.

FROM the earliest times strictures in the anterior portion of the male urethra, when proving tough and undilatable, have been subjected to treatment by the knife or by trocar or cannula, but the real history of the operation of internal urethrotomy is comprised within the last sixty years. Modifications of the trocar and cannula were devised earlier, but it was not until 1827 that Mr. Stafford, of London, brought forward his urethrotomes, which, to me, appear to be the prototypes of all subsequent instruments. In France and in England, during the next few years, various instruments were invented, but all appear to have been used with fear and trembling; small incisions were made to aid the introduction of gradually larger bougies, and the operation of internal urethrotomy was more the handmaiden of interrupted dilatation than an independent operation, and, as a natural consequence, it languished. In 1851, Professor Syme, of Edinburgh, gave a great impetus to the surgery of stricture. He taught earnestly that to ensure a good result from an incision into a stricture, the stricture must be freely divided, and this be accomplished by external urethrotomy upon a grooved staff. Coming after Syme, Sir Henry Thompson fully accepted his teaching, but not his practice, which he thought unnecessarily severe. He soon satisfied himself that a stricture could be thoroughly divided from the inside with greater ease and less risk to the patient than from the outside, and he has unquestionably, during the last twenty years, established internal urethrotomy upon a scientific and surgical foundation.

During the last twelve years, I have had unusual facilities for ascertaining the opinion of surgeons from all parts of the world on the subject of internal urethrotomy; and I have found that there are still some who never practise the operation, and others who view it with suspicion and distrust. I think this opinion arises from two causes: (1) the possibility of very serious consequences after internal urethrotomy in unpractised hands; (2) the indiscriminate employment of the operation by many of its advocates, and their neglect of trial of simpler and safer methods of stricture-treatment.

Now, with your kind permission to-night, I wish to show that, while in the treatment of stricture internal urethrotomy should be the exception and not the rule, still there are certain cases where it is unquestionably the only remedy, that it affords us really brilliant results in otherwise miserable and, perhaps, hopeless cases of disease, and that, with certain precautions, it may be made as safe as almost any operation in surgery.

In the first place, I would briefly state what I mean by the treatment of urethral stricture by internal urethrotomy. I mean the free division—no scarifications, no nicks, no multiple incisions—but one bold free stroke of the knife through all the fibres of the stricture in the floor of the urethra, since almost invariably the induration is most marked there. I maintain that this can only be done by an instrument which becomes practically a long knife in the operator's hand, and which is entirely under the control of that hand, subject to no mechanical restrictions whatever, and cutting, much or little, when and where, just as the surgeon's tactile sense informs him is necessary. When making an accurate incision into any part immediately under the eye, or, for instance, in carving wood, we instinctively cut towards ourselves, or else from left to right, the hand thus being most appreciative; so in cutting a stricture I prefer to cut from behind forwards, or from left to right, and the instrument which permits of this, and at the same time is simply a knife and nothing more, is the urethrotome usually credited to Civiale, and always recommended and used by Sir Henry Thompson. The blade is protruded beyond the stricture, and then drawn forwards, the stricture divided, the blade then sheathed, and the instrument withdrawn. All kinds of mechanisms have been devised whereby a knife sliding in a groove is driven through the fibres of a stricture, but I can as readily conceive the tendo Achillis being properly divided by a similar mechanism, as I can a urethral stricture. Imagine something being put under the tendo Achillis, and distended until the tendon is tightly stretched, and then a knife running in a groove on this machine, passed under the tendon; would any practical surgeon expect the tendon to be properly divided? If all tendons were mathematic-

¹ Read before the Harveian Society.