Mountain Sickness.

By Prof. JOSEPH BARCROFT, F.R.S.

SCIENTIFIC men, according to no less an authority than Longstaffe, were the pioneers of mountaineering. Its vogue as a sport followed. "During the latter part of the eighteenth century, that great period of awakening interest and research into physical science, mountain ascents were encouraged and performed only by scientific men. Such men, practical observers, and expecting to be severely affected by what we consider to be only moderate diminutions in atmospheric pressure, noted even the smallest abnormal symptoms in themselves. . . . On the other hand, during the last fifty years mountaineering has become a sport and is practised by a much larger and very different class, although it is true that many men of scientific attainments are to be found among the ranks of the modern mountaineer."

The question naturally arises: why did the ascent of mountains awaken such keen interest in the scientific men of bygone days? And it is followed by another, namely: what has been the fruit of their researches? Those were the days in which almost nothing was known about the causation of disease—in fact it is difficult to realise how little was known on this subject even so recently as falls within the memory of the still middle-aged. I cannot say what may be the limits of middle age, but I know many men born in the early 'seventies who would account themselves young rather than old, and yet if we look back to, say 1872, and ask ourselves what was then known about the causation of disease, the answer is instructive enough. Since that date the whole science of bacteriology has arisen and has enlightened us as to the cause of innumerable complaints, the micro-organisms of which are known, and by analogy as to a number of others attributable to a flora as yet undiscovered. Then there are all the diseases which are caused by trypanosomes and other parasites, the life histories of which have been worked out; the hosts which transport them have been recognised, and therefore the mechanism of their causation is understood. Or, again, consider such troubles as swollen joints, which scarcely rank among the epidemics, and yet in how many cases are they caused by micro-organisms which can be recognised. In 1872 all these things were unknown, and therefore a pathological condition which could be pinned down to an evident cause was a *rara avis* among diseases.

Such was mountain sickness: by ascending you could acquire it, and, what was perhaps more remarkable, by descending you could, within limits, throw it off. Such was the interest which, as I make out, it held for scientific men.

Mountain sickness is called in the Peruvian Andes "seroche," and the fact that it is given a definite name, which appears in medical cyclopædias, coupled with the natural scepticism of the scientific inquirer, leads to the question, "Is mountain sickness a definite medical entity?" The question is the more pertinent because its manifestations differ greatly in different individuals—differ in degree and in kind. Dr. Alfred C. Redfield, in a paper read to an American society, the manuscript of which he kindly sent me, has stated the claim of "seroche" to a place among human maladies so clearly that I can do no better than quote him. "So definite," he says, "is its symptomatology, so general is its occurrence in these not unpopulous regions, that it deserves some attention as a clinical entity. Its severity is sufficient to give it, in connexion with the mining industry, a certain economic importance. While a few men were met who had never felt it, and many who had suffered but mildly, a very large number are so greatly affected as to be completely incapacitated for several days. In at least one authentic instance the 'seroche' of a normal healthy individual has been terminated by death. Each case is an individual story and up to the present no one has been able to predict who will and who will not be affected. A description of cases of two degrees of severity will serve to picture the chief features of the disorder. Making the ascent by train,[1] one lightly touched by 'seroche' experiences his first symptoms at an altitude of 10,000 feet or more. Subjectively lassitude, then headache, usually frontal, growing in severity, and perhaps nausea, are felt. One feels cold, particularly in the extremities, the pulse quickens, respiration becomes deeper and more frequent, the face is pallid, lips and nails are cyanotic. On descending from the summit to Oroya (12,000 ft.), though a marked improvement is felt one finds himself reduced to a helpless condition of weakness which renders the least muscular effort irksome and productive of shortness of breath, dizziness and palpitation. The night's sleep is restless, and on waking one feels much as he does on venturing to his feet after recovering from an acute infection. In two or three days one's strength returns, the colour improves somewhat, and all but the more severe forms of exertion may be taken without distress. The majority are less fortunate than this. During the ascent, the symptoms are qualitatively the same, but frequently more severe, and nausea gives way to vomiting. The night's sleep fails to bring relief. Severe headache, gastro-intestinal instability, and weakness continue for several days; the body temperature may be supra-normal (102° F. in the rectum) and at times one is aware of palpitation. Cyanosis is marked. After three or four days in bed relief comes, and in a week normal activity may be resumed."

There is probably more unanimity now than at any other time as to the cause of "seroche," for there can be few, if any, men of science who do not attribute it to deficiency of oxygen. At an altitude of about 14,200 feet, the barometer stands at 457 mm., and the partial pressure of oxygen in the air cells of the human lung is just about half the normal—about 50-55 mm. instead of 100–110.

During the century and a half which have elapsed since the time when, according to Longstaffe, scientific men commenced to make observations upon mountain sickness, opinion as to the cause of the complaint has vacillated a good deal. Two theories have been serious competitors to that stated above, and they have come from very different sources: one was the invention of the distinguished physiologist, Mosso;

[1] Of the Central Railway of Peru. The train leaves Callao at the sea level about 6 A.M. and reaches the summit, 15,885 feet in altitude, near the station of Ticlio, about 3 P.M. the same afternoon.

the other was due to the entrance of the sportsman into mountaineering. Let me first discuss the latter of these two theories; it is that mountain sickness is due to a combination of fatigue, cold, bad feeding, and other discomforts incident to attempts by unfit or unathletic men to climb mountains which are beyond their physical powers.

I yield to none in my esteem of the "modern mountaineer," but, with all due respect, I would submit that he is the last person whose opinion on this subject is of value, and I have no difficulty in "stating my reasons in writing." It is of the nature of all sound scientific investigation that, in seeking out the cause of any phenomenon, only one variable should be altered at a time. Of the four variables, temperature, physical exertion, diet, and oxygen pressure, the Alpine rock climber alters all simultaneously and then pronounces to which the "seroche," if felt, is to be attributed. If you wish to see mountain sickness, do not study it among trained athletes in the Alps, whose very training makes them abnormal beings, who can only attain the rather moderate altitudes on European mountains by the performance of notable feats of physical prowess combined with exposure to extreme cold and long periods spent without a normal meal. If you wish to find out whether or no "seroche" is due to oxygen want, go to some place at which the alteration in oxygen pressure can be attained by all and sundry for a sufficient time to bring on the symptoms, but without the complications to which reference has been made. Of these localities, Pike's Peak in Colorado is good, but the best is undoubtedly that to which reference has already been made, namely, the Ticlio summit on the Central Railway of Peru.

There (year in, year out), on any of the six working days of the week, you may see a train full of passengers arrive; and if you see what I saw at Ticlio station, it will be two long coaches of the Pullman type with a head extended from each window, for, as Dr. Redfield truly says, "The majority are less fortunate . . . nausea gives way to vomiting." Here is a train load of persons, Indian, half-breed, Peruvian, European, who have been transported, if going east, from the sea level in eight or nine hours with no effort to themselves. They have had hot meals on the way, for the principal occupation of the inhabitants of the town of Matucana, at an altitude of 7700 feet, appears to be that of feeding the passengers on the train. They have not been subjected to cold; Ticlio itself is not above the snow line, though about the height of the summit of Mt. Blanc, and even if it were, the train (or at least the compartment in which I was) was heated by a stove. There, and not in the Alps, you see the clean experiment; but the truth is that you may see it without going so far afield. Many laboratories contain respiration chambers of one sort or another in which the partial pressure of oxygen may be reduced either by exhaustion, as at the Consumptive Hospital at Brompton, or in Prof. Dreyer's laboratory at Oxford, or by partial replacement of the oxygen by an indifferent gas, such as may be done in the chambers used by Dr. Haldane and by his workers in the Cambridge Physiological Laboratory. In this last-named structure it has been my unhappy fate to suffer the classical symptoms of "seroche." The pressure of oxygen was decreased from day to day till it fell to that which corresponded to an altitude of 18,000 feet. After sleeping (I use the word rather euphemistically) the night in that atmosphere, I arose to make the usual analysis of the air which was my first duty each morning. I was scarcely out of bed before vomiting set in, I suffered from an intense headache, and gas analysis was a matter of great difficulty; by an effort of concentration I could read the graduations on the gas burette, but all outside the very centre of the field of vision was a blur.

The theory of Mosso that "seroche" is due to acapnia, or insufficient carbonic acid dissolved in the tissues of the body, is not seriously now held.

Taking it, therefore, as settled that mountain sickness is due to oxygen want, the question arises, "Oxygen want of what?" And the answer is, "Of the brain."

Such evidence as is at our disposal goes to show that the brain wants but little oxygen; that little, however, it wants very badly indeed. Complete deprivation of oxygen would abolish consciousness in a matter of a few seconds. An appreciable inadequacy in the supply produces the symptoms which Dr. Redfield has enumerated. These symptoms are manifested by many organs in the body, but a careful scrutiny of them shows that they are essentially not the effects of want of oxygen on the organs themselves, but they are evidence of deficient oxygen supply to the centres in the medulla oblongata which govern the activities of these organs. Let me take a single example, that of palpitation of the heart. The effects of oxygen want on the vertebrate heart when isolated and kept beating outside the body are slowing and ultimate cessation of the beat, and a lengthening of the time which elapses between the auricular and the ventricular contractions. Yet the effect of insufficient oxygen on the heart beat in the human body is just the opposite. Dr. Somerville stated in an account of the doings of the 1922 Everest Expedition, at the Royal Society of Medicine, that on the day on which he approached 27,000 altitude his pulse was about 200 "all day" (this last phrase colloquially).

Such a phenomenon may be due to lack of vagus control over the heart, or it may be due to asphyxial nervous stimulation of the suprarenals through the sympathetic system or to direct sympathetic stimulation, or to all together, but it is not the direct effect of oxygen on the heart tissue. Yet this nervous quickening of the heart may have results which are far-reaching enough. The only observations of which I know on the circulation in a case in which the heart beat at 200 were made on a student in the Cambridge Laboratory who suffered from occasional attacks of paroxysmal tachycardia. The heart in these attacks became very inefficient, only driving about half the normal quantity of blood round the body, and almost none through the skin, which therefore became very cold. Such a condition on Everest would court frost-bite in circumstances when already it is difficult enough to cope with the cold. In this last connexion it is very interesting to note, in the experience of the Everest party, the breathing of oxygen at once brought a glow of warmth to the skin.

So it is with the other symptoms; the vomiting is no doubt the effect of oxygen want, not in the alimentary

canal but in the brain; the breathlessness also is a medullary effect, but it at least has a beneficent aspect, for the greater the quantity of air passed through the lungs (other things being equal) the higher the oxygen pressure in the air-cells, and therefore the less the tendency to mountain sickness.

We may conclude with a few sentences on the subject of acclimatisation. In an article in NATURE on the return of the Cerro de Pasco Expedition nearly two years ago (vol. 110, p. 152, July 29, 1922), I have already discussed acclimatisation in some detail. So far as the observers on that expedition could judge, the factors in acclimatisation were, as had been found by previous workers, of two categories, those which tended to increase the pressure of oxygen in the arterial blood, and those which tended to increase the quantity of oxygen which each cubic centimetre of blood carried at any specified pressure. In the latter category are (1) the increase in the number of corpuscles in each cubic centimetre of blood coupled with a corresponding increase in the hæmoglobin; (2) an increase in the affinity of the hæmoglobin for oxygen, which is probably caused by a greater alkalinity of the interior of the red corpuscle.

Among the factors which tend to increase the pressure of oxygen in the alveoli of the lung, and so of the arterial blood, is the increase in the volume of air passed through the lungs. Probably the peculiar size and shape of the chest of the native inhabitants of the Peruvian Andes tends in the same direction. A native whose height is sixty-one inches has a chest of about the same size as a European of seventy inches in stature. No account of the factors concerned in acclimatisation can be complete without reference to a possible one, namely, a more copious blood supply to the medulla. Stress has already been laid upon the fact that symptoms of mountain sickness are medullary, and more accurate knowledge is desirable of the factors which govern the blood supply of this part of the brain. Since our return from Peru, however, the work of Roberts has shown that the medullary blood supply is not simply the toy of circumstances, but is under the control of the brain itself.

Insects and Flowers.

By Dr. E. J. SALISBURY.

THE more definite relationships which subsist between organism and organism, whether it be between the algal and fungal partners of the lichen complex; between the forest tree, the orchid, or the gentian and their respective mycorrhiza; or between the Planarian worm *Convoluta Roscoffensis* and the alga Carteria, all alike bring home to one the delicacy of biotic relationships and the efficiency of whatever be the *modus operandi* of the evolutionary process. The relation between entomophilous flowers and the insects through the agency of which their pollination is effected constitutes no less remarkable an example of mutual specialisation than those already cited. It is scarcely surprising that the subject has attracted the attention of a considerable number of investigators from the observational period rendered notable by the publications of Koelreuter (1761), Delpino (1867), Mueller (1883), Darwin (1876), Kerner (1876), and Knuth (1898), to the experimental period of modern times initiated by the extensive researches of Plateau (1877–1910), and so ably followed by Lubbock (1882), Frisch (1913–19), Knoll (1919–22), and the work of Clements and Long, which has prompted and is the basis of the present article.[1]

The problems involved, though they admit of approach from either the botanical or the entomological point of view, can only be appreciated adequately if the interaction and interdependence of plant and animal be constantly before the investigator's mind. On the other hand, the earlier writers often obscured important issues by too teleological an attitude, as in the assumption that nectaries had been developed as organs of attraction for insects, whereas it is probable that, as the writer suggested fifteen years ago, all nectaries, both floral and extrafloral, originated as osmotic hydathodes and have secondarily acquired biological significance in relation to pollination. Occasionally extrafloral nectaries may themselves be important sources of honey supply, as has recently been noted for the partridge pea, *Cassia chamaecrista* (*Dixie Beekeeper*, 1922).

Every one recognises that insects, and especially bees, visit flowers for either honey or pollen, and that the process of pollination is incidental thereto. That this result ensues is the outcome of two factors, namely, the efficiency of the flower and the efficiency of the insect. With respect to floral structure, it is significant that, when closely related actinomorphic and zygomorphic types are compared, the latter are usually found to exhibit a reduction in the number of stamens and not infrequently in the ovary also; a reduction that could scarcely have come about without detriment to the race, but for the increased precision of the pollination mechanism which the zygomorphic form ensures.

It is, however, too often forgotten that pollen wastage would be enormous and insect agency almost as precarious as anemophily, were it not for the habit which bees in particular exhibit, of restricting their attentions, on a given flight, to a particular species of flower. Aristotle commented on this disposition, but the evidence respecting the degree of constancy seems to be somewhat conflicting, largely perhaps owing to lack of discrimination between the behaviour of the individual and the behaviour of the species. The observations of Clements and Long, for example, showed that the bumble bee, *Bombus juxtus*, visits at least twenty-seven species of flowers belonging to as many as twenty-two genera, and including such varied and specialised types of floral mechanism as Aconitum, Monarda, Thermopsis, and Gentiana. But when attention is confined to single individuals of this same species, the apparent fickleness of behaviour is seen to be illusory. This is sufficiently shown by the examinations of the pollen loads of twenty-five individuals of *Bombus juxtus* carried out by these investigators. Of these loads, nineteen consisted of one type of pollen only, whilst the remaining six were mixed pollen. Similar examinations of thirty-one honey-bees yielded

[1] "Experimental Pollination: an Outline of the Ecology of Flowers and Insects." By F. E. Clements and F. L. Long. Pp. vii.+274, with 17 Plates. Carnegie Institution, Washington. 4.00 dollars.