

from St. Gervais itself. Count J. de Nicolay particularly recommends Louis Mollard (at La Villette), Estivin and A. Magnin (at St. Gervais), and François Perroud (at Bionassay).

THE MONTANVERT HOTEL.—We understand that A. Simond is keeping on this hotel, having taken it for another term of years.

PRESENTS TO THE CLUB.—A mounted chamois-head, which has been very finely set up by Zollikofer, of St. Gallen, has been presented to the Club by Mr. G. Stallard. Mr. A. B. Thorold has presented to the Club two very interesting coloured prints of 'De Saussure's Ascent of Mont Blanc.' One is entitled 'Montée de M. de Saussure sur la Cime du Montblanc au Mois d'août 1785,' publié par J. P. Lamy à Berne, Basle et Genève; the other, 'Descente de M. de Saussure de la Cime du Montblanc au Mois d'août 1785,' publié par J. P. Lamy à Berne, Bâle, Lausanne, Genève—Grundmann del. The size of each plate is 12 in. by 9 in. Their interest lies in the fact that, though they closely resemble the coloured prints of the same subject already in the possession of the Club, which were published by Ch' de Mechel in 1790, yet they are not exact copies, for the figures, though in much the same attitudes and positions, differ in number, and the arrangement of the crevasses and seracs differs slightly. Mechel's well-known plates also measure 18 in. by 14 in. Curiously enough, none of the De Saussure prints are mentioned in Ebel's 'Switzerland,' 1809, which contains a very complete list of contemporary prints.

## CORRESPONDENCE.

### HIGH ALTITUDES.

*To the Editor of THE ALPINE JOURNAL.*

SIR.—In the February number of the 'Alpine Journal,' at the Geographical Society's meeting on March 28, and at the Alpine Club meeting on March 29, Mr. Freshfield has taken opportunities to put forward his views about certain matters connected with some high mountain ascents in opposition to published opinions of my own. In the preface to my 'Climbing in the Himalayas' the following passage occurs:—

'The expedition made by the brothers Robert and Adolph Schlagintweit in 1854-56 into Nepal and other portions of the Himalayas was not properly a mountaineering expedition, though some mountains were climbed, and a height of 22,239 ft. was reached. But Mr. W. W. Graham's expedition in 1883 to the mountains of Kumaon and Sikkim was a mountaineering expedition, because Mr. Graham was a trained climber; he was accompanied by two Swiss guides of repute, Emil Boss and Ulrich Kauffmann; and the making of ascents was his object. Unfortunately, he was not acquainted with the use of instruments, did not take photographs, and was thus without means for fixing his positions

with certainty, or for measuring the approximate altitudes of points reached by him. He believed that he ascended Kabru, a peak of about 24,000 ft., but his experiences differ so widely from those of Dr. Güssfeldt, Mr. Whympfer, Captain Bower, and all the members of my party at altitudes of 19,000 ft. and upwards, that it is more than likely he was mistaken as to the point he climbed. Though hereafter he may be proved to have accomplished what he thought he accomplished, his ascent cannot for the present be accepted as authentic.

This passage was not attacked by Mr. Freshfield, nor, as far as I know, by any opponent. Its conclusion has been frequently re-stated, and continually implied. The other day, in an article on 'Mountaineering,' in the 'Encyclopædia of Sport' (January, 1898), I had occasion to approach this question again, and wrote as follows:—

'Mr. Graham was accompanied by Swiss guides, but he used no scientific instruments of precision, made no survey, and relied for information as to his position on his interpretation of the map. It must be remembered that the map makes no pretence to being a mountain map, or, save as to the position of the summits of peaks, of rendering, with even approximate correctness, the form of mountains above the level of cultivation; it would, therefore, be extremely easy for a traveller, especially if he were not a topographical specialist, to mistake his position, and believe (as men have often done\*) that he was on one peak when he was actually on another. Mr. Graham thought that he climbed a peak 24,000 ft. high, named Kabru, but it is the matured conviction of English officials, who were in the country at the time, and who discussed the matter with Mr. Graham, when his memory as to the things he had seen was fresh, that he was mistaken, and that he reached no such great altitude. At the height he did attain neither he nor his companions experienced any of the effects of diminished atmospheric pressure, such as have invariably been observed by all who have reached 20,000 ft. and upwards. Mr. Graham's ascents are not, from a scientific-geographical point of view, thoroughly identified and authenticated. He did not measure his altitudes, and he did not fix his positions; the omission to do so deprived his expedition of some of the importance it deserved.'

It is this second statement that appears to have provoked Mr. Freshfield to a protest, which the first failed to elicit either from him or from anyone else. If he had protested sooner, evidence would have been forthcoming, which is forthcoming no more. It was Colonel Tanner, R.E., who had the conversations with Mr. Graham, above referred to. Colonel Tanner was himself no mean climber. He had probably ascended oftener to a height of 18,000 ft. and thereabouts than any member of the Alpine Club. He knew Sikkim, he was an excellent topographer, and he was one of the best mountain-

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\* Instances might be quoted from both Alps and Caucasus.

draughtsmen that ever lived. He was a passionate lover of the mountains, and was the last man to be compared with Russian officials in the Caucasus and other incredulous lowlanders as Mr. Freshfield has compared him. In most parts of the world there is a *primâ facie* willingness in the minds of residents and officials to doubt the reality of mountain ascents. Amongst Anglo-Indians there exists no such prejudice. They tend rather to be over credulous about human achievement. The existence in such a society of the incredulity I have mentioned is a fact that cannot be lightly passed over. Unfortunately, Colonel Tanner, who might have given his detailed reasons for the conclusion he came to, which he did not hesitate to assert, has recently died.

In 'Good Words' for January, February, and March, 1885, Mr. Graham published articles on his Himalayan journey. On page 98 he described an ascent to an altitude which he *estimated* at 22,700 ft., where, as he says, 'we had set at rest, as regards ourselves at least (*i.e.* Graham and Boss), the vexed question about the rarity of the air.' 'Boss and I,' he continues, 'ascended certainly to 22,700 ft., and neither there nor at any point of the ascent did we feel any inconvenience other than the natural loss of breath consequent on every ascent. Nor was the ascent a mere uphill walk; on the contrary, it presented quite as many difficulties as any ordinary Alpine peak, and on the rock occasionally demanded very great exertions. It is my deliberate opinion that any man in sound health and fair training may work and be capable of great muscular exertion *with no more inconvenience when the barometer stands at 13 in. than when it marks 30 in.* At any rate, we could not detect any difference.' The italics are mine. On page 177 Mr. Graham describes his climb on Kabru. 'Kauffmann led all the way, and at 12.15 we reached the lower summit of Kabru, at least 23,700 ft. above sea. . . . The last 300 ft. were the hardest of any, yet no more difficulty in breathing was noticed than if they had been 10,000 ft. below.'

We have thus as precise a statement as could be required to the effect that at 22,700 ft., and again at 23,700 ft., neither Mr. Graham nor either of his companions experienced any effect due to the rarification of the air. Against this observation we have the more or less carefully recorded observations of the Schlagintweits, Mr. Johnson, Mr. Whympfer, Dr. Güssfeldt, Captain Bower, my party of 1892, Mr. Mummery's party, and the FitzGerald party—in all over a score of persons who have been at 20,000 ft. and upwards. These people, though they differed in the degree in which they suffered and in the symptoms of their suffering, agree that at such altitudes certain disagreeable effects are felt. These effects, in their mildest as in their severer forms, in no wise resemble the effects of fatigue. They differ from fatigue in kind. It is easy for those who have not been to 20,000 ft. to deny their existence. At 28,000 ft. I feel convinced that no man could fail to experience them.

I am therefore compelled to the conviction either that Mr.

Graham was mistaken as to the heights he reached, or that he was an inaccurate or forgetful observer, and has incorrectly reported the experiences of his party. Mr. Freshfield devotes attention to the topographical details of Mr. Graham's story. I have other work on hand which leaves me no leisure to work up the topography of the Sikkim Himalaya. Moreover, arguments which I might draw from book and map learned topography would convince nobody. Mr. Graham and Emil Boss may have been men of exceptional physique, but Ulrich Kauffmann, with whom I have climbed, though a strong man, is not in any sense exceptional. Mr. Graham's claim involves the assumption that the chance that brought him and his two companions together (for the tale of his journey shows that it was chance, not careful selection), brought together three men so exceptionally constituted that their experiences are utterly at variance with those of over a score of men, several of whom were amongst the most powerful and enduring that have ever been out on a hillside.

Such a strong *prima facie* case against any given ascent might, of course, be upset, but could only be upset by the direct evidence of scientific observations systematically made. We are now arrived at a time when, in consequence of the approaching exhaustion of unexplored regions of the earth, the great mountain ranges are destined to attract the attention of explorers. Mountain exploration is sure to be much carried on in the coming century. It should be the business of the Alpine Club to watch over that exploration and be identified with it. But mountain exploration will not obtain the respect of men of science if it is carried on in a slipshod fashion. It is the easiest thing in the world for a man to mistake his peak in a badly-mapped region, and to estimate his altitudes wrongly by several thousand feet. Unless he makes a series of observations by which his route can be controlled, or unless he is a man of such tried veracity and skilled and proved topographic sense that his word carries quite exceptional weight, his mere assertion that he climbed a certain remote peak in the midst of a vast mountain region, where mistakes are probable, does not suffice to establish his ascent as a scientific fact from which to deduce arguments as to the effect of high altitudes upon the human frame.

MARTIN CONWAY.

London, April 14th, 1898.

DEAR SIR,—In Mr. Douglas Freshfield's article in the 'Alpine Journal' for February last, he has opened up an entirely new view in the study of mountain sickness, when he says, 'Looking to the experience of the past hundred years, it is at least open to argument that in the next century persons of good digestion and stolid temperaments, starting in fine training from tolerably comfortable huts or bivouacs, may hope to disregard the "rarity of the air" on any mountain on the face of the globe.'

May I be permitted to point out at least two difficulties in reconciling the descriptions of various observers?

First, the confusion of the symptoms of mountain sickness with those produced by any other form of diminution of atmospheric pressure. There are three ways, other than by mountaineering, in which reduction of atmospheric pressure may be made to produce its effects on the body. These are aëronaut experiments; experiments conducted in a laboratory under metal cylinders from which the air is exhausted; and in cases where the atmosphere of the caissons is exchanged for that of the ordinary atmosphere, often producing caisson disease. The conditions under which these cases occur can be shown to be totally different from those which exist in mountaineering; and the symptoms complained of in each case are precisely the same, differ in every particular from those of mountain sickness, and suggest in a very obvious way an entirely different pathology.

M. Paul Bert did not experiment, as the authority quoted by Mr. Freshfield from the 'Edinburgh Review' states, to prove that the symptoms of mountain sickness were due to diminution in the supply of oxygen, but that the symptoms produced by aëronaut and laboratory experiments were due to this cause; a point which he failed to prove, as in all his experiments inhalation of oxygen did not remove the symptoms, though they were relieved.

The symptoms occurring in these three classes of cases are due mainly to the reduction of atmospheric pressure being produced very *rapidly*, a fact which can be clearly made out in all the recorded experiments.

Reduction of pressure in mountaineering is very *gradual*; and I fail to see how it is possible for gradual reduction of atmospheric pressure *per se* to produce any symptoms. It is only the diminution of oxygen, which runs concurrently with the diminution of pressure, that can by physiological action produce any effect.

Secondly, the similarity of the symptoms produced by fatigue and those of mountain sickness.

Now, fatigue may be produced in two ways—either by the circulation of fatigue products (whatever they may be) through the muscles, the blood supply originally being good and containing proper nutriment, or by supplying the healthy muscle with deficient nutriment, so that it is unable to perform its work properly. It is easy to see how fatigue may thus be produced by unhealthy organs, or by insufficient or unsuitable food.

The symptoms of fatigue (at ordinary sea level) are exactly the same as those of mountain sickness, with the exception that in fatigue vomiting occupies a prominent place, which it does not in the case of true mountain sickness; and this fact accounts for the earlier observers describing symptoms at altitudes lower than 17,000 ft.

The one point which has yet to be settled in connection with the relief of mountain sickness is whether inhalation of oxygen removes the symptoms.

I will conclude by saying that as long as it is an acknowledged physiological fact that when blood is exposed to gradually decreasing oxygen pressures it at first gives off only a small quantity of oxygen, but that when the pressure reaches 60 mm. of Hg a sudden dissociation takes place and a large quantity of oxygen is suddenly given off from the blood, it will be hard for us to accept with confidence the delightful prophecy mentioned in Mr. Freshfield's paper.

I am, Sir,

Your obedient servant,

MALCOLM L. HEPBURN, M.D.

28 Marine Parade, South Lowestoft,  
April 11, 1898.

SIR,—It would, I think, save both themselves and your readers some confusion and trouble if those who feel moved to take part in this discussion could find time previously to study what has been written in past volumes of the Journal. Since we have the aid of Mr. Wallroth's excellent index, the recommendation can hardly be thought an unreasonable one. Had Sir Martin Conway's various avocations allowed him to act in accordance with it, the present note would probably not have been called for.

As it is, I must state plainly that in my recent article I made no comparison affecting the late Colonel Tanner. The writers I expressly referred to and compared to my Caucasian critics were the anonymous and other correspondents who in 1884 wrote to the Indian newspapers asserting, amongst other things, 'that the Indian school of mountaineers maintain their belief that no Himalayan summit of over 20,000 feet has yet been reached.' These were the persons—writers who contradicted their own Survey Reports—whom I lately accused of prejudice. I made this clear by a specific reference, which I here repeat, to the paper on 'Himalayan and Alpine Mountaineering,' in volume xii. of this Journal, in which I commented in detail on their criticisms. As regards Colonel Tanner, I have no knowledge that he has expressed anywhere in writing any 'conclusion' as to Mr. Graham's ascents. I knew him well, for he served with me for two years on the Council of the Royal Geographical Society, and at my instigation wrote a paper on his Himalayan travels. Neither to the Council nor to myself, officially or privately, did he communicate, in writing or conversation, any opinion on the matter in question. But had he done so, I am bound to add that I should not have attached any great weight to it. For though Colonel Tanner was, as Sir Martin Conway says, a true mountain lover and an excellent draughtsman and engineer, he was not, in the European sense of the word, a mountaineer. In the Himalayas, where the snow-level is 17,700 feet, ascents of 18,000 feet do not count for more than climbs of 9-10,000 feet in the Alps, and they can be made without mountain craft. Moreover, Colonel Tanner in his paper ('Geographical Proceedings,' 1891, p. 410) has expressly stated that he never visited the glacier region of the Kinchinjunga group. Indeed, in an official

report, reprinted in vol. xii. of this Journal, he declared that Kinchinjanga had 'no glaciers worthy of the name,' which, of course, is very much the contrary of the fact. I must refer to this paper and my comments on it for confirmation of the view I take of Colonel Tanner's competency as an observer and a critic with regard to the physical features of heights above the snow-level and *à fortiori* of mountaineering in that region. I am confirmed in it by a friend of Colonel Tanner's who knew him more intimately than I did, and who is also an ex-President of our Club. In these circumstances it was impossible for me to imagine that Sir Martin Conway, when he quoted 'English officials' who were 'on the spot,' meant only Colonel Tanner.

I do not wish to be needlessly controversial, but I can hardly allow to pass altogether without protest the groundless suggestion that—because Colonel Tanner happened to die about the time when I thought it expedient to notice the matter at issue—I have done Sir Martin Conway any injury or deprived him of evidence which might have supported his views. I ought, I think, to state that Colonel Tanner died on March 16 last, after only three days' illness—a month, that is, after the publication of my note, and nearly two months after proofs of it had been in Sir M. Conway's hands and a reference to him had been possible. As I said in the last number, it was the appearance as a judicial utterance in a work of reference of what had previously been only an expression of a personal opinion in a book of travel which decided me to reopen the discussion.

With regard to Dr. Hepburn's letter, I am quite content to leave him to settle any physiological questions with the 'Edinburgh Reviewer.' I desire only to disclaim the originality he credits me with. In the sentence Dr. Hepburn quotes, I have said no more than has been said often before by a physiologist, Mr. Clinton Dent, F.R.C.S. Some readers may remember his dictum, cited in these pages (vol. xii., p. 133), 'I believe most firmly that it is humanly possible to climb Mount Everest.' I can only suppose that Dr. Hepburn has read into the time-honoured phrase *rarity of the air* (which I purposely used, in inverted commas, because, so much being still in dispute, it seemed to me preferable to any more particular and controversial form of words) a much narrower sense than I intended it to bear.

The conclusion of the whole matter, to my mind, is that the sooner some competent climbers go to Sikkim the better. No one probably can prove or disprove with absolute certainty past ascents. But when they have been outdone, they and the personal questions involved in 'a record' will sink to their proper importance. Let me further express my hope that young mountaineers will not be deterred by any asseverations of their elders that they must make a business of a pleasure. 'A series of observations' is a most admirable thing, but to get to the top of your mountain is still better, and the two are not always compatible. First ascents may, without being scientific, open the way to science. Saussure would hardly

have got up Mont Blanc had not Balmat been there before him. I have so often preached the scientific mountaineering I have myself very indifferently practised that I may be forgiven if for once I confess that there are times when a mercurial barometer may be a burden, and even a small plane-table a bore, and that mountaineering pure and simple has joys and leaves memories which no other form of recreation, much less any serious pursuit, can equal, and from which even the incredulity of a scientist can take nothing.

I am, sir, yours obediently,  
DOUGLAS W. FRESHFIELD.

DEAR SIR,—Dr. Hepburn charges me, in the fourth paragraph of his letter (*supra*), with an inaccuracy of statement. The only passage quoted by Mr. Freshfield ('Alpine Journal,' No. 139, p. 51, from the 'Edinburgh Review' (July 1897), in which reference is made to M. Bert, is as follows:—

'He [Sir Martin Conway] asserts roundly that the cause of mountain sickness is diminution of the supply of oxygen, adopting the view that M. Paul Bert insisted on so strongly.'

I am unable to see how this sentence justifies Dr. Hepburn's remarks. If he had some other passage in mind, I should be glad if he would cite the words. I did not say that M. Bert 'experimented to prove that the symptoms of mountain sickness were due to diminution of oxygen.' I merely stated that M. Bert adopted this view strongly. Dr. Hepburn must have forgotten the passage in M. Bert's book ('La Pression Barométrique,' Paris, 1878), in which he deals with the subject of 'Voyageurs en Montagne' (p. 1096, *et seq.*), especially the concluding lines of the chapter (pp. 1104, 1105). If Dr. Hepburn refers to this expression of M. Bert's opinions, I am sure he will admit that the passage quoted from the 'Edinburgh Review' is a correct statement of M. Bert's views.

I am, sir, yours obediently,

THE WRITER OF THE ARTICLE IN THE  
'EDINBURGH REVIEW.'

[MR. HINCHLIFF ON ALTITUDES.—Owing perhaps to its title, 'Over the Sea and Far Away,' Mr. Hinchliff's last work, published in 1876, while he was President of the Alpine Club, is much less known than it deserves to be, and probably many of our readers are unaware that it contains a picture and an eloquent description of Tupungato, one of the great peaks recently climbed by members of Mr. Fitzgerald's expedition.

Our immediate object, however, in referring to a volume which should take its place in every Alpine library, is to call attention to the example it affords of the risk run, even by an eminent mountaineer, when he takes a man's highest in the decade in which he chances to write as a limit likely to be permanent. The following passage needs little comment (p. 90). Mr. Hinchliff wrote:— 'Lover of mountains as I am, and familiar with such summits as those of Mont Blanc, Monte Rosa, and other Alpine heights, I

could not repress a strange feeling as I looked at Tupungato and Aconcagua, and reflected that endless successions of men must in all probability be for ever debarred from their lofty crests. When we used to look at the highest peaks and passes of the Alps, the only question which suggested itself was "which is the best way to get there?" In the presence of the huge peaks of the Andes I could but think of the great probability that no one would ever get to them at all. There they reposed in divine dignity, too great for mortal approach, and suggesting the abode where the "gods of Epicurus" sit careless of mankind and careless of the tremendous calamities dealt out to men by the fires concealed beneath the feet of these glorified monsters. The Alps have been conquered, and Mont Blanc has been obliged to bow down to the monarch of the Caucasus; but nature proclaims the existence of an impassable limit somewhere, and the latest conquerors of even Elbruz and Kasbek have been compelled to admit the effects of the rarefaction of the air. Those who, like Major Godwin Austen, have had all the advantages of experience and acclimatisation to aid them in attacks upon the higher Himalayas, agree that 21,500 ft. is near the limit at which man ceases to be capable of the slightest further exertion. [Since this was written I find that Mr. Johnson, of the Indian Survey, has crossed a pass at 22,000 ft. above the sea, but the slope was probably long and gradual.] Even this has only been attained by halting after a very few steps and lying down exhausted in the snow. Mr. Simpson, whose pictures of Himalayan scenery are so well known, tells me that he and his party suffered severely in crossing the famous Purung Pass, which is 19,000 ft. above the sea; and that some of the natives from the plains declared that they were not only dying but dead! None could advance without more and more frequent halts. There is reason to believe that, from some climatic reason, this difficulty of breathing, called *puna* in South America, is experienced with greater severity in the Andes than in other great ranges. When Mr. Darwin crossed the Portillo Pass to Mendoza, at the height of 13,000 or 14,000 ft., he found that "the exertion of walking was extremely great, and the respiration became deep and laborious." With their 9,000 or 10,000 ft. above this, Aconcagua and Tupungato may probably defy intrusion, unless through the medium of a balloon.'

[The writer was an expert, the evidence he adduces is considerable and fairly related; yet his conclusion has been proved erroneous. One point raised, the exceptionally deleterious quality of the atmosphere of the Andes, deserves investigation. It is a branch of a general question—how far local conditions modify the effects of altitude?—which has not hitherto attracted the attention it deserves.—ED.]

#### THE ALPS IN 1864.

To the Editor, 'ALPINE JOURNAL.'

DEAR SIR,—By the courtesy of Major Moore I have obtained permission to reprint and publish the Journal of his brother, the

late Mr. A. W. Moore, so well known under the title of 'The Alps in 1864.' The publication will be undertaken by Mr. Douglas, of Edinburgh, the editing by myself. I propose that the book should be illustrated by the best photogravure reproductions of original photographs, illustrating Moore's route, which I can obtain. I write this letter in the hope that through the 'Alpine Journal' I may come into communication with some members of the Club, or perhaps other climbers, who have, or might be willing this summer to take, photographs such as are required, and who would afterwards be willing to give me the opportunity of seeing them with a view to their reproduction, if suitable.

The route taken by Moore in 1864 was in outline as follows:—

St. Michel—Col des Aiguilles d'Arves—Bec du Grenier—Brèche de la Meije—Col des Ecrins—Les Ecrins—Col de la Pilatte—Mont Blanc, from the Belle Vue Pavilion to Chamonix—Col du Chardonnet—Fenêtre de Saleinaz—Grand Cornier (unsuccessful)—Col d'Hérens—Rympfischhorn—Dom—Bies Joch—Morning Pass—Aletsch Horn—Beich Grat—Wetter-Lücke—Eiger (from the Wengern Alp)—Wetterhorn.

I may remind any gentlemen who are willing to help me in this matter, that the views which are wanted are not those *from* the places I have named, so much as views *of* those places, which therefore may sometimes require to be taken from quite different points to those actually traversed by Moore himself. The most convenient size would be half-plate, but smaller negatives can be enlarged if necessary.

I remain, yours faithfully,

ALEX. B. W. KENNEDY.

#### PROCEEDINGS OF THE ALPINE CLUB.

A GENERAL MEETING of the Club was held in the Hall of the Club on Tuesday evening, February 1, at 8.30 P.M., Mr. Charles Pilkington, *President*, in the chair.

The following candidates were balloted for and elected members of the Club:—Messrs. C. S. Ascherson, F. Ball, P. M. Barnard, W. G. Edwards, F. de Filippi, E. J. Mazzuchi, H. J. Mothersill, J. S. Phillimore, A. G. S. Raynor, H. J. Synnott.

The *PRESIDENT* announced that while the Exhibition held last summer had unfortunately been but poorly attended, the recent Exhibition had been very successful, as 500 to 600 people had been present on the opening day, and about 900 afterwards, which was very gratifying. The credit was greatly due to the exhibitors, who had sent a very beautiful and interesting set of photographs; and also to those who had managed the Exhibition, especially to Mr. Carr and the Hon. Secretary. Mr. Carr had managed the Exhibitions for several years, and had done much hard work, and the Club owed him hearty thanks for his trouble.

With reference to the circular in regard to Ball's 'Alpine Guide,' the *President* called special attention to the last paragraph, in which it