

CORRESPONDENCE.

THE FIRST ASCENT OF THE GRIVOLA.

To the Editor of the ALPINE JOURNAL.

SIR,—On p. 306 of the new edition of Ball's 'Western Alps,' it is stated that in 1859 'Messrs. J. Ormsby and R. Bruce made the first ascent of the peak, starting from the Val Savaranche,' whereas Mr. Ormsby, after having stated * that the party did *not* reach the top, appends a foot note (p. 336), from which the following is an extract:—

'I now believe it was much more childish not to do the thing completely when we were about it. As we were descending, the *garde-chasse* [F. A. Dayné], above mentioned, said it would never do to come down without leaving a flag (in sober language, a bit of calico on a stick) in testimony of our ascent. The proposition was too innocent to be objected to, but *when we found he was making for a part of the ridge where we had never been* [the italics are mine.—F. T. W.] it struck us both as not being an altogether honest proceeding, and we proposed returning for the purpose of giving due effect to the solemnity by the presence of the whole party. Cachat, however, pooh-poohed the idea.'

It is thus quite clear from the above that not only did none of the party ascend to the top except F. A. Dayné, but that Messrs. Ormsby and Bruce themselves were both entirely ignorant as to *where* the true summit exactly was!

I do not forget that it has been set down as 'pedantic' to claim the first ascent of a peak merely because one happens to have been the first on the actual summit, whereas the 'main difficulty' of the mountain has been previously overcome by another,† and that Mr. D. W. Freshfield subscribes to the same view in the 'Academy' for February 23, 1884, and also that in 'Climbers' Guides' ('Mountains of Cogne') Messrs. Yeld and Coolidge tell us that 'it is now recognised that . . . the honour of making the first ascent [of the Grivola] belongs to the whole party [Messrs. Ormsby and Bruce's].'‡ On the other hand, however, I would point out that, for instance, although the Ost Spitze of Monte Rosa was ascended by the three brothers Smyth in 1854, and by Mr. E. S. Kennedy in the same year,§ yet the first ascent of Monte Rosa has universally been reckoned as unquestionably belonging to Messrs. Hudson, Smyth, Birkbeck, and Stevenson when they ascended the Dufour Spitze, hard by the Ost Spitze, and not more than some 20 ft. higher, in 1855.

F. A. Dayné was the first to reach the summit of the Grivola, in 1859, making the ascent from Val Savaranche; whilst the first time that the mountain was climbed by a traveller from the

* *Peaks, Passes, and Glaciers*, vol. ii. p. 335.

† *Alpine Journal*, vol. xii. p. 341.

‡ *Bell*, 1891, p. 22.

§ *Alpine Journal*, vol. xvi. p. 47.

|| *Vaccaren*, pp. liv. lv; *Peaks, Passes, and Glaciers*, vol. ii. pp. 318-338.

Val Savaranche side was when I myself ascended it from Dégioz in 1876, accompanied by Laurent Proment, of Courmayeur, and J. J. Blanc, of Dégioz.*

I am, Sir, yours truly,

F. T. WETHERED.

September 19, 1898.

[As Mr. Douglas Freshfield has been appealed to on the subject, we have thought it best to submit Mr. Wethered's note to him. He sends us the following letter, to which we do not think it necessary to add.—EDITOR.]

SIR,—I am obliged to your courtesy in placing before me Mr. Wethered's note, in which he cites opinions I have at different times been called on to express, either as Editor of the 'Alpine Journal' or elsewhere, as to what constitutes a First Ascent. I gladly take the opportunity you afford me of stating the principle on which I have based these opinions.

What substantial importance (if any) ought to be attached to claims to first ascents must, I think, depend in each disputed case on local circumstances, and still more on the common sense of the climbing community. The climber who is too sedulously minute, who looks into the details of his climbs with a 'microscopic eye,' runs, it seems to me, some risk of making himself ridiculous in place of famous.

Let me illustrate this proposition by a personal reminiscence. Many years ago François Dévouassoud and I had preceded another party by a short half-hour in a new ascent. I was sitting on one of the boulders that formed the summit of the peak, and using the highest crag as a table. I pointed it out to our followers on their arrival as the Allerhöchste Spitze. They solemnly stepped on to it in turn, and on the strength of this admission on my part they subsequently claimed in print the first ascent.

I cannot consider that where one rock or one snow crest is but a trifle higher than another on the same ridge, and the two are separated neither by difficulties real or imaginary, nor by distance worthy to be reckoned in the day's work, any claim to the honours of a first ascent, made on the ground that the preceding party did not complete the climb, ought to be recognised.

Whether the case of the Grivola falls into this category is a question of fact. Having before me both recent photographs, Mr. Ormsby's note, cited by Mr. Wethered, and the printed descriptions of the summit written without any personal bias by other travellers, lately collected by Mr. Coolidge, the proper verdict seems to me beyond doubt. Ambroise Dayné's claim was of the character of my rivals', and probably would never have been heard of but for local patriotism, which has been even more active than personal feeling in providing material for discussions of this character.

On the other hand, in the case of Monte Rosa, the summits reached by the different parties are obviously distinct. The first ascents of the Ost Spitze and the Dufour Spitze were made by

* *Alpine Journal*, vol. viii. p. 83.

wholly different routes, and the crest between them, which the first party fancied to be impracticable, was not, I believe, traversed until many years later.

I am, yours obediently,

DOUGLAS W. FRESHFIELD.

October 10, 1898.

HIGH ALTITUDES.

To the Editor of the ALPINE JOURNAL.

DEAR SIR,—I am, of course, ready to acknowledge Mr. Freshfield's superior literary ability, but I hardly think, on this account, he is justified in taking advantage of a very questionable grammatical error in a paragraph the sense of which I am surprised he fails to understand. If the expression 'rarity of the air' is used to describe the various conditions present in the atmosphere at high altitudes, it is obviously the duty of those who wish to study and avoid the symptoms produced by this 'rarity of the air' to ascertain, if possible, whether each of these conditions gives rise to its own set of symptoms which can be more or less differentiated; and this is clearly my meaning.

I do not propose, however, to answer separately all Mr. Freshfield's misrepresentations of the position I take up, as my desire to be brief in my remarks may have laid me open to them; but I would remind him that, in my first letter, I did nothing more audacious than accuse him of making a suggestion in the words 'opened up an entirely new view.' As a suggestion of this character had never before appeared in such a form from the pen of so high an authority as Mr. Freshfield, I presumed he had some good reasons for making it; and it was because I considered that suggestion very misleading to those who had not studied the subject that I ventured to comment on it in the hope of finding out those reasons. Had the treatment been simple and unimportant, it would not have been so misleading to suggest that the 'rarity of the air' might, under the conditions he mentions, be disregarded; but when the treatment means a very considerable addition to the climber's burdens by carrying up large supplies of oxygen, it almost becomes a question whether the treatment can be undertaken at all. Therefore, in any case, the symptoms of mountain sickness will always present serious difficulties to the mountaineer, and can never be lightly spoken of without very good reasons.

I had not originally intended, Sir, to trespass on your valuable space by giving in full my reasons for holding the opinion I do; nor can I do so now; but, in self-defence, I feel bound to mention, as briefly as possible, a few points to indicate the actual position I have taken up after working at the subject a long time.

1. Although there are two main conditions present in rarefied air—viz. diminished atmospheric pressure and diminution of oxygen per unit volume—yet the symptoms produced by the one *can* be differentiated from those produced by the other.

2. Symptoms produced by *gradual* reduction of atmospheric pressure are totally different from those produced by *rapid* reduction of pressure; and, therefore, aeronaut and laboratory experiments, as at present performed, are useless in studying the cause of mountain sickness.

3. Symptoms of true mountain sickness (*i.e.* gradual reduction of pressure) are closely allied to those of fatigue, but one can be distinguished from the other by the fact that in the latter vomiting is generally present.

4. After eliminating fatigue and indigestion, true mountain sickness still remains to be overcome by the climber above 17,000 ft.

5. It can be shown, from well-known physiological facts, that *gradual* reduction of atmospheric pressure *per se* can produce no symptoms whatever, though *rapid* reduction of pressure produces a very definite set of symptoms; so also does gradual as well as rapid *increase* of pressure. There remains, therefore, only diminution of oxygen per unit volume to account for the symptoms of mountain sickness.

This last point I have not been able to prove experimentally, since it necessitates either laboratory experiments of some hours' duration, or else mountaineering above 17,000 ft. with large supplies of oxygen.

I trust Mr. Freshfield will now see that, instead of 'dismissing at once all the Alpine experiences and experiments of travellers and men of science, and excluding' his own experiences, I have used them very largely in forming my opinion, and respect them accordingly.

I am, Sir,

Your obedient servant,

MALCOLM L. HEPBURN.

South Lowestoft: October 8, 1898.

To the Editor of the ALPINE JOURNAL.

1 Airlie Gardens, W.: October 1898.

SIR,—Dr. Hepburn takes me too seriously. Since he has twice insisted on my use of 'loose' language, I think I ought to have been allowed to convict him of a 'questionable' expression without being consequently accused of any want of understanding! Again, the sentence of mine he first took exception to was not, as he supposes, a new departure. It was an epitome of a view I had more fully expressed elsewhere.* On that occasion I anticipated Dr. Hepburn in mentioning the possible utility of oxygen bags, originally suggested, I believe, by M. Paul Bert.

Dr. Hepburn complains of my misrepresentations of the position he has taken up. I quote his own statement of it from a paper published in the 'St. Bartholomew's Hospital Report' for 1896, vol. xxxi., of which he has kindly sent me a copy.

* *Exploration of the Caucasus*, vol. ii. p. 168, 1896.

'As far as treatment of mountain sickness is concerned the only methods by which it can be combated are—

'(1) By climbing at as slow a rate as possible with frequent halts, and taking a good many days to complete the ascent. This, of course, will necessitate camping out on the mountain for some time.

'(2) By taking up a supply of oxygen. This would be, of course, a most inconvenient addition to one's burdens, and it cannot be undertaken without much more careful thought and consideration than this bare statement admits of.

'I believe that if these two rules be followed and are found practicable, mountain sickness will not prevent the highest point on the earth's surface being reached by man.'

My statement in my last letter was 'that Gaurisankar will be climbed . . . is the view, as far as I can make out, of Dr. Hepburn, provided his treatment or system of training, which will include oxygen refreshers, is adopted.'

This is the central point of Dr. Hepburn's argument, and where, I must ask, is the misrepresentation?

If in any matter of detail Dr. Hepburn thinks that I have not done him justice, I can only repeat his expression of regret that brevity and exactitude are so difficult to combine. Of course, when I complain that he dismisses all Alpine experiences, I mean that by denying their connection with variations in the atmosphere he dismisses them from the present discussion.

Dr. Hepburn's novel hypothesis that any discomforts felt by mountaineers below 16,500 or 17,000 ft. are solely the result of indigestion and fatigue, while those felt above that level are complicated by 'rarefied air' (an expression which it appears is scientific, though 'rarity of the air' is loose) remains as yet, in my opinion, unsupported by any adequate evidence in facts. Again, *gradual* reduction of pressure can be, and has been, tested in the laboratory, and such experiments are surely not useless. But in holding that the reduction of pressure is not one of the causes of mountain-sickness, Dr. Hepburn is, I think, right. Further researches will, I believe, probably show us that, even if we are on the true track, the causes of this discomfort are more complex and intricate than Dr. Hepburn assumes, and that it is premature therefore to dogmatise as to its treatment. Those who are interested in the subject will do well to consult Dr. Mosso's book, 'Life of Man on the High Alps,' recently issued in an English form by Mr. Fisher Unwin. There, coupled with much uncritical repetition of antiquated and sometimes contradictory experiences, and some serious citations of statements hardly meant by their authors to be taken as scientific dicta—for instance, Sir Martin Conway's rash assertion that 'the climber hates civilisation, and is usually a dolichocephalous black man'—they may find a record of careful observations, which, if not conclusive, is at any rate very suggestive.

I am, Sir, yours faithfully,

DOUGLAS W. FRESHFIELD.