routine of everyday town life. And, by the law of contrast, such a short and concentrated spell of the primitive, of life stripped to its mere essentials, gave incomparably more than a longer holiday arranged in a more orthodox way. In fact, such a holiday had to be short, for you could not stay too long in the hills living in this way, as you cannot stay too long to bathe in an icy river. The refreshing qualities of both are similar.

A good companion is essential in this sort of mountaineering even more than in any other. I had the great luck of having such a companion. Adam Karpinski had considerable experience of the Alps, had visited the Andes, and also had to his credit a previous attempt of ridging in winter the whole of the High Tatra. This attempt was not fulfilled, for in some ten days he managed to carry out only half of this formidable programme. His mountaineering plans for the future were focussed on the Himalaya. This was a subject of common interest to both of us, and the background of our common winter adventures in the Tatra. We gained there the experience necessary to carry out our plans. Later we found this experience most useful, particularly for the many details of camping technique and equipment.

Years have passed since I last saw the Tatra. But I hope to go there again some time. Though I do not expect to ridge the whole of the Tatra from end to end, and, as time goes on, I may find that there is something to be said in favour of huts and hotels, I still look forward to some good climbing there, both in winter and in summer, as well as to some skiing in the Carpathians. For, although there are many greater, higher and, objectively speaking, finer mountains, there is nothing like your home hills. ' On revient toujours à ses premiers amours.'

JAMES DAVID FORBES

By E. H. STEVENS

JAMES DAVID FORBES was the first Honorary Member elected to the Alpine Club,[1] and so holds the premier position among our Pioneers. That high place is his due, partly as the leading Alpine traveller, explorer and writer of his time, and partly as the chief author of the revolutionary view of the nature and movements of glaciers embodied in his famous ' Viscous Theory.'

He was born in Edinburgh on April 20, 1809, the youngest of the six children of Sir William and Lady Forbes. Both his parents were of ancient Highland stock. His father was a lifelong friend of Sir Walter Scott, and his mother had been Scott's first love. She died of consumption when her youngest child was a year and a half old, and the boy grew up in the anxious care of his father (till his death in 1828)

[1] On July 19, 1859.

and of his elder sisters. He never went to school, but had various lessons at home. Substantially, however, he was self-educated, though, as his father wished him to study for the Scottish bar, he attended lectures on law as well as other subjects at Edinburgh University. He was twice head of Sir John Leslie's class in Natural Philosophy, and Leslie told him that he was only deterred from getting him to officiate for him in his absence by the fear that he might be thought too young. He ' passed advocate ' in 1830, but for several years he had been devoting himself with almost passionate ardour to the study of astronomy and physics, mastering by private study the branches of higher mathematics[2] necessary for these subjects, and keeping elaborate scientific journals.

In 1826–7 the Forbes family enjoyed an extensive continental tour, visiting Innsbruck, Vienna, Rome, Naples, Chamonix. Two ascents of Vesuvius and an excursion to Montenvers with Saussure's old guide Cachat le Géant, were among the experiences that greatly impressed young Forbes. During this tour he continued to send contributions on scientific subjects—largely on the physical features of the Bay of Naples—to Brewster's *Philosophical Journal*, and in 1830 Brewster procured his election to the Royal Society of Edinburgh. In 1831, Forbes was able to make a journey to the south which enabled him to make the acquaintance of the leading English men of science, by whom he was everywhere warmly welcomed. ; in London by Lyell, John Herschel, Babbage ; in Oxford, by Buckland and Phillips ; and especially in Cambridge, by Whewell, Airy, Peacock, Sedgwick and others. For many years to come Whewell and Airy (that ' inimitable man ') remained his chief scientific friends and advisers, and Cambridge men and methods his guiding lights. He took an active part in aiding Brewster to get the British Association founded at York in 1831, and an elaborate report on Meteorology which he prepared for the Oxford meeting of 1832 was followed by his election as F.R.S. in that year, when he was only 23.

Soon afterwards Sir John Leslie died, and Forbes at once entered as a candidate for the vacant chair. His most formidable rival was Brewster, nearly thirty years his senior, and by far the most distinguished man of science in Scotland. It must have cost Forbes a pang to find himself opposing his own friend and patron. It is pleasant to know that the old friendship was presently re-established, and remained unbroken to the end. The election lay with the Town Council of Edinburgh—the last time they had to exercise such a right—and Forbes was elected by 27 to 9 (January 1833). They may well have been glad to choose a young candidate, sprung from an ancient Tory family, who was so highly gifted and so strongly recommended. In June, Forbes was elected to The Athenaeum, and in the autumn he

[2] It is amusing to find that he ' began Euclid ' at 16, but for one who professed ' little esteem for mathematics, except in their practical applications ' it says much that by 20 he was deep in the great French treatises on the Differential and Integral Calculus, which, he said, ' astonished and delighted me.'

began his work as Professor. He held the post till his health broke down in the spring of 1859, when, by the irony of fate, he succeeded Sir David Brewster (then appointed Principal of Edinburgh University) as Principal of the United College in the University of St. Andrews. He died on the last day of 1868.

In his youth Forbes was a handsome man, tall and slightly though strongly made, with finely marked features, and especially expressive eyes. His physical powers were great, and he was both active and enduring, as his mountain expeditions sufficiently show.

His work as Professor was marked by many distinctions. In 1836 he was awarded the Keith Medal of the R.S.E., and in 1838 the Rumford Medal of the Royal Society, for his researches on the Polarisation of Heat. In 1842 he was elected a Corresponding Member of the Institute of France, and in 1843 received the Royal Medal of the Royal Society for researches (described in the 'Bakerian Lecture') on the law of extinction of the solar rays in passing through the atmosphere. In 1845 he was granted a pension of £200 a year by the Prime Minister, Sir Robert Peel. In 1865, the R.S.E. gave him the Keith Prize for researches on the Conduction of Heat. The first and last of these awards were for two of the most important researches Forbes carried out. The early research on the polarisation of radiant heat required, with the apparatus then available, great experimental skill; it provided the final and conclusive proof of the physical identity of radiant heat and light, established Forbes's reputation amongst European men of science, and formed 'an epoch in the history of Natural Philosophy' (P. G. Tait). The research on conductivity—restricted by Forbes's failing health to the case of iron—introduced an original method, in which all the necessary quantities were measured experimentally, without the theoretical assumptions previously thought necessary; it led to the first reliable measurements of absolute conductivity at various temperatures, and has remained a standard method in physics. The very elaborate 'Bakerian Lecture' described observations made with the actinometer in 1832 and 1841, from which it was deduced that 'about one-third, or rather more, of solar heat is lost by vertical transmission through the atmosphere,' so that 'the radiant heat of a summer day is far more intense about the level of the snow line than in the valleys.' These are only a few of Forbes's researches, which he described in more than 100 papers contributed to the chief English, French and German scientific journals.

The grant of the pension of £200 a year 'for services rendered to science' made in 1845, 'through the joint recommendation of many friends,' is noteworthy for various reasons. Forbes was only 36, and though by reason of a serious illness in 1843 he had had a year's leave, he was back in the full activity of his Professorship in 1844, so that the grant could hardly have been made on compassionate grounds. It must therefore be a proof both of an almost startlingly unusual benevolence of the Government towards science, and of the lofty position which Forbes had attained in the scientific world. Perhaps the

grant was facilitated by the wide popular interest aroused by his glacier theories, and by the success of his splendid book *Travels through the Alps of Savoy*. How it compares with the knighthoods conferred about this time on Brewster, Murchison and Lyell it would perhaps be invidious to inquire.

Turning now to Forbes's more particular claims on the interest of the Alpine Club and mountaineers generally, we note that these claims rest in the first place on his extensive travels and explorations in the chief mountain districts of Europe from the Pyrenees (1835) to Norway (1851). The practice of the Scottish Universities of concentrating their teaching work into six months, leaving the other six free, enabled Forbes to make lengthy journeys abroad, and he used these opportunities to the full. Always, like Saussure, with some scientific object in view, he displayed remarkable enterprise in exploring, usually alone, little known districts, and crossing high passes, often as the first traveller to do so. In the winter of 1842–3 he wrote: 'I had the advantage of receiving my first impressions of Switzerland in early youth, and I have carefully refreshed and strengthened them by successive visits to almost every district of the Alps between Provence and Austria. I have crossed the principal chain of the Alps 27 times, generally on foot, by 23 different passes, and have intersected the lateral chains in very many directions.' It may be of interest to consider some of the chief features of these expeditions.

His first lengthy foreign tour was in 1835, when he explored the Pyrenees. After visiting the Cirque de Gavarnie and the Brèche de Roland (9200 ft.), he worked eastward, and from the Port de Vénasque (nearly 8000 ft.) he admired the splendid view of the Maladetta group 'with its prodigious glaciers, which seemed to me to rival those of Mont Blanc.'[3] Descending into Spain, he examined the very remarkable 'Trou de Toro,' where the stream from these glaciers plunges underground, to reappear in the next valley as the infant Garonne at a point two and a half miles away and nearly 2000 feet lower. Returning through Auvergne he made long excursions, mainly of geological interest, among the extinct volcanoes round Clermont-Ferrand. He ascended the Puy de Dôme 'as much in reverence for Pascal's memory as on any other account'.

In 1837 Forbes spent nearly three months in Germany, mastering the language and making the acquaintance of the chief professors at Bonn, Göttingen and Berlin. An extensive tour in the Eastern Alps made him familiar with the Glockner, Ortler, Marmolata, Langkofel ('with its towering pinnacles and amazing crags') and their surrounding valleys and passes. In many of these districts of Tyrol and the Dolomites he was probably the first British visitor.

It is with 1839 that Forbes's career as a mountain explorer and climber actually begins. Passing again through Paris—where, as usual, he had friendly intercourse with the leading French scientists,

[3] *Cf.* Spender and Smith, *Through the High Pyrenees*, p. 99. 'It is a view worthy of the High Alps.'

such as Arago and Cauchy—he went on once more through Auvergne to Dauphiné. By way of the Durance and Ubaye valleys and the Col de Cula (*ca.* 9700 ft.) he reached St. Véran, then supposed to be the highest village in Europe, and Abriès, and from a bivouac at about 6600 feet in the Guil valley effected, in a 16 hours' march, the first complete circuit of Monte Viso. He crossed in succession the Col de la Traversette (9679 ft., on the north of the mountain), the Col dei Viso (8704 ft., east of it), the San Chiaffredo Pass (9069 ft., south-east of the peak) and the Col de Vallante (9269 ft., north-west of it)—the last two involving a descent, reascent and further descent (to the Guil valley) of nearly 3000 feet in each case. This was a particularly enterprising expedition as, once over the Col de la Traversette, Forbes and his guide had to discover the rest of the route for themselves. After visiting the Waldensian valleys, Forbes crossed the Mont Genèvre and walked in one day from Monêtier to Bourg d'Oisans (33 miles), whence he made an excursion up the Vénéon valley to La Bérarde, doubtless as the first British visitor. 'The scenery,' he said, 'is stupendous,' and the impression it made on him is well borne out by the fine drawing of the Ecrins which he made from above Les Etages.[4] Returning to Chamonix—his third visit—he made the Tour of Mont Blanc to Courmayeur and Aosta, and reached Turin via Cogne (where he inspected the iron mines) and the Col della Nouva (9623 ft.). Finally he returned home by way of the Mont Cenis, the 'savage and wild' Col d'Iseran (9085 ft.), the 'abominable' chalets of Tignes, and Bourg St. Maurice. The circuit of the Viso, and the pioneer visits to La Bérarde and Cogne—places now so beloved of mountaineers, but then almost entirely unknown—make this journey memorable.

Forbes's next visit to the Alps was in 1841, and proved a turning point in his life. In the previous autumn he met at the British Association in Glasgow M. Agassiz, a Swiss naturalist who had devoted much time and attention to the phenomena of glaciers, and accepted an invitation to join him on the Unteraar Glacier in the following August. Before doing so he carried out a most spirited and epoch making foray into Dauphiné. With Joseph Rodier of La Bérarde as guide he crossed the glacier pass of the Col du Says (10,289 ft.) to the remote and almost unknown Val Gaudemar, returning two days later by another glacier pass, the Col du Sellar (10,063 ft.), to Vallouise. Both these lofty passes are named on Bourcet's map (1754), which was used by Forbes, but he was almost certainly the first traveller to cross each of them, and these expeditions—by no means easy ones for the time—form the starting point of the later explorations of the district by W. Mathews, Bonney, Whymper, Tuckett and Moore in the 1860's, and by Coolidge and many others in subsequent years.

A week's strenuous walking via the Little St. Bernard and the Col Ferret brought Forbes to the Grimsel Hospice on the evening of August 8 to join Agassiz's party as arranged. Next day, the 9th, they

[4] Plate ix in *Norway and its Glaciers*.

J. D. FORBES
in early manhood.

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proceeded to the Unteraar Glacier. Forbes at once remarked what is called the veined or ribboned structure of the ice,[5] and was surprised to find that Agassiz had either not previously noticed it, or thought little of it.[6] Forbes showed that the structure was to be found throughout all the glaciers he was able to examine—notably in those of the Rhône and the Brenva—and considered it a feature of cardinal importance in the economy of glaciers. Agassiz, when later convinced of the generality and importance of the structure, maintained in his *Système Glacière* (1847) that it originated in the well known stratification of the névé, the nearly horizontal layers of the wide névé basins being squeezed and upturned as the glaciers were forced through the narrow valleys by which most of the Alpine glaciers escape from the heights. Both Forbes, ánd later his unsparing critic Tyndall, rejected this idea, and both made great but unconvincing efforts to find other explanations. Nowadays, largely through the work of H. Crammer, and especially of Prof. H. F. Reid—who has traced the structure right up to the névé basins of the Forno and Unteraar Glaciers—the stratification theory is generally accepted, as e.g. at the International Glacier Congress of 1905. So *one* of the most hotly debated glacier problems may perhaps be regarded as solved, though it must be admitted that the arguments urged by Forbes, by Tyndall and by Heim against the theory are difficult to meet.

Forbes remained some three weeks with Agassiz's party, sometimes at the ' Hôtel des Neuchâtelois '—a shelter under a great rock on the moraine of the Unteraar Glacier, five hours from the Grimsel Hospice—sometimes at the Hospice itself. During this time he made some considerable expeditions. He crossed the Gauli Pass (10,519 ft.) to the Urbachtal, making the second ascent of the Ewigschneehorn (10,929 ft.). On August 27, the party crossed the Oberaarjoch (10,607 ft.), and by way of the great Viescher Glacier reached in twelve hours' walking the Märjelen chalets. Next day the Jungfrau was climbed (fourth ascent) by Agassiz, two of his friends and Forbes, with four guides. They had started late (6 A.M.), and Forbes was troubled with a sprain from a fall on the Gauli Glacier a week before, so they did pretty well to reach the summit at 4 P.M. This was Forbes's greatest expedition, being, as he said, a more formidable undertaking than the ascent of Mont Blanc. It is curious that in spite of his frequent visits to

[5] The structure consists of thin parall l layers of compact blue ice alternating with layers of more porous, whiteish ice. In the central parts of many glaciers these layers are conspicuous, and are more or less vertical and parallel to the sides of the glacier. Lower down the layers assume a spoon shape, and at the snout these are often visible as nearly horizontal bands.

[6] Later that autumn Agassiz unfortunately claimed the discovery as his own, and this inevitably led to the rupture of the friendly relations which had subsisted between himself and Forbes, who had cordially and publicly acknowledged the hospitality shown him. This action of Agassiz—who never attempted to answer Forbes's devastating account of the affair—together with the fact that he clung so long to his belief in the quicker motion of the sides of glaciers, largely accounts for the comparative neglect in this country of his many valuable observations on glacier phenomena.

Chamonix it never seems to have occurred to him to follow in the steps of his master Saussure, by climbing the loftiest summit of the Alps.

As a result of the party's observations and discussions on glacier problems, Forbes became convinced that it was necessary to put the whole question on a firmer foundation by making—as had not yet been done—exact measurements of the motion of the ice. He foresaw that the best method for doing this would be by the use of the theodolite. He recommended this method to Agassiz (who made preparations for the purpose by planting a row of six stakes across the Unteraar Glacier), and explained it in detail in an article in the *Edinburgh Review* of April 1842, a translation of which was published also in the *Annales de Chimie*.

So in June 1842, he proceeded to put his method into execution on the Mer de Glace. He was accompanied by Auguste Balmat, who soon proved himself an ideal assistant in the operations. In a few days' work he was able to prove (*a*) that the motion of the ice is continuous (i.e. without jerks) even from hour to hour ; (*b*) that it goes on day and night, though more slowly by night ; (*c*) that *the centre of the glacier moves considerably faster than the sides*, ' quite contrary to the opinion generally entertained,' e.g. by Agassiz. These all-important results he announced in a letter to Prof. Jameson dated July 4, which was published in October. Paying a flying visit to Turin to observe the total eclipse of the sun, he returned via Courmayeur—where he ascended the Crammont (8980 ft.) with Chanoine Carrel, and examined the great Brenva and Miage Glaciers—and the Col du Géant (11,060 ft.), where, as he said, ' I had the satisfaction of still finding the remains of Saussure's cabane of 1788.' The next day, July 24, having walked up in the evening to ' the hill of Charmoz ' above Montenvers (where a large cairn now stands), he saw for the first time the ' dirt bands,' sweeping in hyperbolic curves across the glacier *with their convexities downwards*, and the loops of the curves coinciding with what he had already shown to be the line of swiftest motion. There flashed upon his mind, already prepared by the measurements he had made, the conception that the glacier moved as a viscous fluid moves. He was able to prove that *in every respect* the motion of the glacier resembles that of a river, allowing of course for the immensely greater viscosity of the ice. The most conclusive piece of evidence in addition to the results of 1842 was his skilful measurement (in 1846) on the snout of the Mer de Glace of the velocities of the ice at the surface, 143 feet above the base (10·8 in. per day), at a point 54 feet above the base (9·7 in. per day), and at 8 feet above the base (6·7 in. per day, or 62 per cent. of the velocity at the surface.)[7] The retardation towards the bed thus shown, combined with that towards the sides, *proves* that the glacier moves as a viscous body, as even W. Hopkins, the ablest of Forbes's early critics, admitted. This then is the famous Viscous Theory. It seemed so opposed to the apparent rigidity and brittleness

[7] These results were almost exactly confirmed by Tyndall, eleven years later, on an ice wall near the Tacul.

of glacier ice that it excited violent and long continued opposition, but Forbes's untiring efforts to explain, defend and illustrate it gradually prevailed, and the Viscous Theory became the orthodox doctrine.

It may be noted that Agassiz's measurements on the Unteraar Glacier (half of which were announced a month after Forbes) had added to the proof of the greater central velocity, but Agassiz had drawn no conclusions from his measurements, and indeed kept half of them unpublished till 1847. Again it is true that ideas similar to those of Forbes had been briefly suggested by Bordier (1773),[8] and more elaborately in a remarkable essay by Bishop Rendu, published in 1840 in the *Mémoires de la Société Royale Académique de Savoie*.[9] Forbes obtained a copy of this from the author early in 1843 (months after his Viscous Theory had been announced), and in his *Travels* made some striking quotations from it and flattering references to it, which were the means of making it generally known. But Rendu fully admitted that he could not explain *how* the motion occurred. After making various suggestions, including Saussure's sliding theory, and what is rather like the first half of Tyndall's theory of fracture and regelation, he said ' Je l'ignore,' but seemed inclined to favour the Dilatation theory of Charpentier and Agassiz, which Forbes had shown to be inconsistent with elementary physical principles. These foreshadowings no more derogate from Forbes's credit as ' the Copernicus of glacier theory ' (P. G. Tait), than the partial anticipations of Huyghens and Hooke diminish the glory of Newton as the discoverer of Universal Gravitation, or the significant researches of Balfour Stewart and Stokes that of Kirchhoff as the pioneer of Spectrum Analysis.

How great was the change effected by Forbes in the general opinion is best described by Ruskin.[10]

> ' We all knew that glaciers moved . . . that there were cracks all through them and moraines all down them ; that some of their ice was clear, and other ice opaque . . . We were all puzzled to account for glacier motion, but never thought of ascertaining what the motion really was . . . None of us ever had the slightest idea of the ice's being anything but an entirely solid substance, which . . . was always rigid and brittle like so much glass or stone. This was the state of affairs in 1841. But in 1842 Forbes solved the problem of glacier motion for ever—announcing to everyone's astonishment . . . that glaciers were not solid bodies at all, but semi-liquid ones, and ran down in their beds like so much treacle.'

But in 1857 Huxley and Tyndall—no unwilling opponents of orthodox doctrines—reopened the question, and Tyndall's *Glaciers of the Alps* (1860) contained what were in substance, even if not in form, damaging criticisms of Forbes and his work. Tyndall not only exalted

[8] See *A.J.* 9. 325, and especially Freshfield, *Life of de Saussure*, p. 194.

[9] The French text, with an English translation by Mr. (afterwards Sir Alfred) Wills, and articles by Prof. George Forbes, Prof. P. G. Tait and Mr. Ruskin, was published in this country in 1874.

[10] *Fors Clavigera*, letter 34.

Rendu at Forbes's expense, but clearly implied that ' Forbes had treated him with calculated unfairness.'[11] Forbes made a dignified and masterly reply, and the Alpine Club will no doubt accept the emphatic verdict of Sir Alfred Wills[12]—friend of both parties, Judge of the High Court and President of the Alpine Club—that ' the position of Forbes is impregnable upon the merits and the evidence.' It is pleasant to know that Rendu himself had fully recognised Forbes's merits. In 1844 he wrote ' votre théorie de la marche des glaciers finira par être la seule admise parce qu'elle est, selon moi, la seule vraie,' and gave him a cordial invitation to stay with him at Annecy, an invitation which Forbes was able to accept in 1846. It is curious that it should have been reserved for Tyndall to find out, some 15 years later, that justice had not been rendered to the Bishop by Forbes !

How came a man of such undoubted generosity and love of justice as Tyndall to treat Forbes with such unfairness ? Temperamentally the men were opposites : Forbes the reserved, dignified scion of ancient Highland stock, of intensely conservative convictions, profoundly religious, in close touch with Cambridge, but not with London ; Tyndall, the hot tempered Irishman, by nature a rebel against authority and a foe of orthodoxy, a leading figure in the free-thinking scientific circles then so influential in the metropolis and the Royal Society. Tyndall seems to have felt that Forbes and his friends were too insistent on his exclusive claims in respect of the theory of glaciers, and having found in Rendu's lengthy essay two striking passages which Forbes had not quoted, jumped to the conclusion that he had purposely concealed his indebtedness to his predecessor. This wounding suggestion was never withdrawn, and indeed was repeated in *Forms of Water* (1872) after Forbes's death. It is necessary to point out that in at least two other cases—one of them of special interest to the Alpine Club—Tyndall had similarly allowed his generous zeal for a protégé to lead him into injustice. In his advocacy of J. R. Mayer's claims in regard to the doctrine of the Conservation of Energy he was unfair to Joule, and came into sharp collision with Sir W. Thomson and P. G. Tait, and in seeking to explain Bennen's failure to complete the ascent of the Matterhorn he was equally unfair to Carrel, and incidentally to Whymper. Whymper's temperate verdict exactly meets the case. ' I cannot but think that Dr. Tyndall, in his great zeal for his favourite guide, does not do justice to the merits of the other, and that it is possible he may, by the very ardour of his advocacy, defeat the ends he has in view.'[13]

Apart from these unhappy personal issues, the scientific discussion largely turned upon the question ' is glacier ice really viscous ? ' Tyndall denied this, though he agreed that ' it is undoubted that the

[11] So A. L. Mumm, article on Tyndall in *Alpine Club Register*, vol. 1.

[12] The quotation is from the unsigned review of the *Life of Forbes* in *A.J.* 6. 244-253, but I entirely agree with Mumm that ' there can be no question that the review is from Wills's pen.'

[13] *A.J.* 5. 334.

glacier moves like a viscous body.'[14] It is curious that neither Forbes nor Tyndall ever defined exactly what they meant by viscosity, and each of them often used *plastic* as an alternative term to *viscous*. The terms were scientifically discriminated once for all by Clerk Maxwell,[15] who showed for instance that a stick of sealing wax must be regarded as a very viscous liquid, as Forbes had virtually remarked long before.[16] The essentials of the matter were seen, and stated in his own characteristically picturesque way, by Ruskin, who, as Mazzini said, had ' the most analytical brain in Europe,' and was deeply interested in crystallography.

> ' *Plastic* is the term proper to be used of the state of butter, on which you can stamp whatever you choose, and the stamp will stay ; *viscous* expresses that of honey, on which you can indeed stamp what you choose, but the stamp melts away forthwith. And of viscosity itself there are two distinct varieties—one glutinous, like that of treacle, and the other simply adhesive, like that of mercury or melted lead. Glacier ice is practically plastic, but actually and inherently viscous and not plastic. Here then at last I have got Forbes's discovery and assertion put into accurately intelligible terms.'[17]

Tyndall, the Professor of Natural Philosophy at the Royal Institution, might surely have been expected to treat the question of viscosity scientifically, but he strangely contented himself with quoting ' men of high culture ' (unnamed) who regarded viscosity as equivalent to ' gluey tenacity.'[18] Of course he had no difficulty in showing that the formation of crevasses disproves the possession of any such property by glacier ice, and he regarded this as fatal to Forbes's theory. Later he revealed his fundamental error on the subject by speaking of ' that power of stretching which is characteristic of a viscous substance.'[19] Viscosity is *not* necessarily or always associated with a ' power of stretching,' as Ruskin pointed out in the quotation above. The conclusive scientific verdict on the question is that of Thomson and Tait.[19a] ' Forbes's theory is the proof by observation that glaciers have the property that mud and mortar (heterogeneous), pitch and water (homogeneous) all have, of changing shape indefinitely and continuously under the action of continued stress.' *But this is the strict definition of viscosity*, and Forbes was therefore absolutely right in asserting that glacier ice, for all its apparent solidity, is actually viscous. Tyndall, however, was justified in pointing out that Forbes's theory did not answer the question ' what is *the physical quality of the ice* in virtue of which a glacier moves like a river ? ' Tyndall's own answer was based

[14] *Forms of Water*, § 411.
[15] *Theory of Heat*, chap. xxi.
[16] *Occasional Papers*, p. 35.
[17] *Deucalion*, chap. vi. Ruskin's insight into such problems is illustrated by the fact that he put his finger on the weakest spot of Forbes's discussions—his attempt to explain the veined structure, which Ruskin showed would not account for the facts. (Library Edition, vol. xxvi, p. xli.)
[18] *Glaciers of the Alps*, p. 312 *et seq.*
[19] *Forms of Water*, § 410.
[19a] *Natural Philosophy*, § 741 (note).

on Faraday's discovery (1850) of the now familiar property of regelation, by which pieces of thawing ice, when brought into contact, freeze together. He repeated, in ingeniously varied forms, the experiments, first made by the Schlagintweits, on the compression of ice in moulds, obtaining thus compact and transparent ice of new shapes, and he conceived of a glacier as suffering continual *fracture*, continually repaired by *regelation*, and thus enabled to adjust itself to the irregularities of its channel. These interesting experiments had a great popular appeal, and Tyndall's theory, so easy to understand, and depending on such a familiar property of ice, was widely adopted.[20] In December 1864, Forbes wrote to Sir W. Thomson, ' I surrender to public clamour any hope of present justice on the glacier theory.'

It seems clear, however, that Tyndall misinterpreted his own experiments. The effects he produced must have resulted from the fact, deduced theoretically by Prof. James Thomson, and verified experimentally by his brother, Sir W. Thomson, that ice under pressure melts at a lower temperature than the ordinary melting point. This is because the melting of ice is accompanied by a considerable diminution of volume (about 10 per cent.), and pressure therefore assists the change from ice to water, and enables it to take place at a lower temperature than would otherwise be necessary. Now in Tyndall's experiments he says that his pieces of ice were crushed or broken by the sudden pressure, and the fragments ' regeled.' It seems unlikely, to say the least, that this supposed regelation of fragments could produce clear transparent ice. Moreover Tyndall admits that *the ice must be in a thawing condition*, and adds that ' by applying the pressure carefully, rude fracture may be avoided and the ice compelled slowly to change its form.'[21] It is practically certain therefore that his ice blocks, or the broken fragments of them, *melted* under pressure, and refroze as clear ice when the pressure was relaxed. This was seen and pointed out by Forbes,[22] in his final pronouncement on glacier controversies (1865), and was *proved* by Mousson. Having strongly compressed a cylinder of ice with a metal index on top, on releasing the pressure and opening the press he found the index at the bottom of the cylinder of ice, which must therefore have melted and let the index fall through the resulting water.[23] The application of this to glaciers is clear. The whole mass of ice, it is now agreed, is at or near the melting point corresponding to the pressure at every depth; at points of maximum pressure the ice melts, the resulting water escapes to places of less pressure, where it refreezes because its temperature was lowered

[20] For example, H. B. George, in his charming book *The Oberland and its Glaciers* (1866) said, ' Professor Tyndall's theory has, in the eyes of the great majority of scientific men, distinctly superseded all others.' George himself was a devoted adherent of the theory, which secured also the weighty approval of von Helmholtz.

[21] *Forms of Water*, §§ 419, 421. [22] *Life and Letters*, p. 513.

[23] Quoted from Mousson's original paper in *Poggendorf's Annalen* (1858) by Chappuis and Berget in their great *Physique Générale*, tome I, p. 461, where also it is held that the Thomson effect explains Tyndall's moulding experiments.

by absorption of (' latent ') heat on melting. Thus while the continuity of the ice is preserved, it is enabled to move downwards and make the necessary changes of shape to conform to the accidents of the channel in which it flows.[24] It must be added that, for what seem insufficient reasons, neither Forbes nor Tyndall accepted the Thomson effect as explaining the motion of glaciers. Yet Tyndall admitted that ' the liquefaction of the ice at places of violent pressure . . . must play an important part in the consolidation of the glacier.'[25] But if in the consolidation, why not in the motion ?

It has since become clear that the *granular* structure of glacier ice—little noticed by Forbes and Tyndall—is of capital importance in explaining its motion. The glacier grains, which are developed in the névé[26] and increase in size throughout the mass of the glacier, consist of nearly pure ice, and are usually cemented together by less pure ice, originating from a film of thaw water surrounding them and containing air bubbles and dissolved salts from the surrounding rocks. Such impure ice melts at a lower temperature than pure ice,[27] and as pressure increases at various places the cement melts first and facilitates the motion of the grains among or over one another.

This is not the place for any further discussion of the many problems presented to the inquirer by glaciers, or of the theories put forward to explain them. I have sought chiefly to clarify those aspects of the main problem which concern Forbes's theory and the controversies to which it gave rise.

Early in August, 1842, Forbes, with Victor Tairraz of Chamonix, met his friend Prof. B. Studer of Berne at the Great St. Bernard for a joint expedition. Forbes was grateful to Studer for taking his part, in spite of his older friendship for Agassiz, in the annoying dispute about the discovery of the veined structure. He found him a most congenial companion and dedicated his *Travels* to him. Descending to Orsières, they went up the Val de Bagnes, crossed the Col de Fenêtre (9141 ft.) to Ollomont, and ascended the Valpelline to Prarayé. Thence they traversed the little known Col de Collon (10,270 ft.) to Arolla and Evolena, where again Forbes was probably the first British visitor. The accommodation at Evolena was so bad that Studer fled to the Val d'Anniviers, leaving to Forbes the honour of making—under the leadership of Pralong—the first certain passage of the 11,418 feet Col d'Hérens, ' as I propose to call this pass, which has

[24] It is of course not suggested that regelation plays no part in the natural history of glaciers. Obviously ' it enables two tributary glaciers to weld themselves to form a continuous trunk ; thus also crevasses are mended, and the dislocations of the glacier consequent on descending cascades are repaired.' These are the only specific cases adduced by Tyndall (*Glaciers of the Alps*, p. 423). But for the further assertion that ' this healing of ruptures extends to the smallest particles of the mass ' there is no evidence, and the Thomson effect supplies a far more probable and more general explanation.

[25] *Forms of Water*, § 452.

[26] See Seligman's *Snow Structure and Ski Fields*, pp. 118, 125, 320.

[27] Sir Charles Blagden in 1788 had shown that salt solutions freeze—and therefore melt—at lower than 32° F.

not yet received a name.' With it he combined the first ascent of the nearby Stockhorn (11,795 ft.) commanding a magnificent view of Monte Rosa, the Dent Blanche, Dent d'Hérens and especially the Matterhorn.

At Zermatt—where Forbes lodged with the village doctor, Lauber—he ascended the Riffelhorn, and from some point on the Riffelberg made his famous drawing of the Matterhorn (plate vii in *Travels*), ' beyond comparison the most striking object in the Alps.' According to Ruskin, who gave an outline of this drawing in *Modern Painters*, vol. iv, ' it was evidently made under the influence of considerable excitement ' but ' with little exaggeration.' Sir George Airy, the Astronomer Royal, writing to Mrs. Forbes in 1872, said, ' I suppose it may be asserted that the present popularity of Zermatt, a place which before was scarcely known, is almost entirely due to Professor Forbes's picture of the Matterhorn.'

Studer rejoined Forbes at Zermatt, and the friends proceeded to carry out the ' Tour of Monte Rosa,' crossing six passes from the Théodule to Monte Moro, and visiting all the southern valleys between them. Forbes had the pleasure of meeting two of the early explorers of the Monte Rosa peaks—Zumstein, who had made the first ascent of the Zumsteinspitze (15,004 ft.) in 1820, and who now guided Forbes on an excursion to the Lys Glacier, and Gnifetti, the curé of Alagna, who had made the first ascent of the Signalkuppe (14,965 ft.) a month before Forbes met him. At Macugnaga Forbes carried out a very complete examination of the great glacier, and then returned to Chamonix by way of Monte Moro, Saas and the Rhône valley. In the course of this journey—described in full but most interesting detail in *Travels*—he had visited every major valley of the Pennine Alps except the Val d'Anniviers, crossed some ten passes, and carefully examined as many first class glaciers. During the same summer he had carried out, single-handed save for Auguste Balmat, the difficult survey of the Mer de Glace, resulting in the splendid large scale map of that glacier issued with his *Travels*—the first accurate map of any Alpine glacier. Little wonder that this summer was ' the happiest that he had ever spent.'

The winter of 1842–3 was occupied in the composition of his great book *Travels through the Alps of Savoy*, and in the preparation of the map and the illustrations for it. This completed, he married Miss Wauchope on July 4, 1843, and at once started for a honeymoon on the Continent. Unhappily he was struck down at Bonn on July 20 by an attack of gastric fever, complicated by inflammation of the lungs, which endangered his life and from the effects of which he never completely recovered. As the doctor who attended him strongly advised him against travelling north, he obtained a year's leave of absence from his Professorship, and spent the winter with his wife in Naples and Rome. Visits to Sicily and the Italian lakes followed, and during a short stay at the Simplon he made his last mountain climb, the first ascent of the Wasenhorn (10,680 ft.). Here too he met Ruskin, then

aged 25, who was travelling as usual with his parents. Volume I of *Modern Painters* had appeared the previous year. ' My father and mother and I were sitting at one end of the long table in the evening, and at the other end of it, a quiet, somewhat severe-looking and pale English (as we supposed) traveller with his wife ; she, and my mother working ; her husband carefully completing some mountain outlines in his sketch-book.' Shyness at length overcome, some of Ruskin's drawings were shown. ' Forbes's eye grew keen and his face attentive, as he examined the drawings, and he turned instantly to me as to a recognised fellow-workman—though yet young, no less faithful than himself. He heard kindly what I had to ask about the chain I had been drawing ; only saying, with a slightly proud smile, of my peak supposed to be the Matterhorn, " No—and when once you have seen the Matterhorn, you will never take anything else for it ! " '[28] Ruskin became a devoted admirer of Forbes and his work, and later defended him with indignant effect against Tyndall's attack.

That autumn, Forbes was able to resume his Professorial duties, and in 1845 was granted the pension already referred to. In 1846 he visited Bishop Rendu at Annecy. ' He was so cordial, so unselfish as to his own claims on the plasticity theory, so much interested in my present undertaking, that I was quite delighted with him.' Then he spent a long time at Montenvers, ' where as usual everything is better than at Chamonix : bread (most decidedly), butter (most incomparably), mutton, honey, tea, cream, wine.' During his stay he made a plucky attempt, with David Couttet and Auguste Balmat, to climb the Aiguille du Moine, apparently by the south-east ridge. They got up about 2000 feet, but were then stopped by ' an abrupt turret of granite occupying the whole of the ridge, which is perfectly precipitous towards the Glacier de Talèfre.' The chief new observation he made that year was the very important determination already referred to of the retardation of the motion of the ice near the bed of the glacier as compared with that at the surface.

Forbes's next and practically his last visit to the Alps was in 1850. A long day's excursion (14 hours) took him high up on the Glacier du Géant, and he was able to say, ' I feel today quite my old self again.' So he had little difficulty in climbing the Aiguille de la Glière (*ca.* 8800 ft.) in the Aiguilles Rouges, from which he sketched the Glacier du Tour—the only glacier descending into the Chamonix valley which he had not examined. A few days later, leaving the Col de Balme with Balmat and M. Charlet, he explored this glacier, crossed the rather difficult Col Blanc (11,162 ft.) to the Plateau de Trient, and thence passed through " the magnificent gateway ' of the Fenêtre de Saleinaz (so named by Wills, 10,709 ft.)—' a very striking and peculiar pass '—on to the glacier of the same name, by which, and by the steep rocks of its right bank, they finally reached Orsières in the remarkably good time of 12½ hours.[29] This was Forbes's last great Alpine tour; according

[28] *Deucalion*, chap. x.

[29] Seven years later Sir Alfred Wills and two friends, starting also from the Col de Balme with Auguste Balmat to repeat this expedition, were benighted on the rocks, and took altogether 26½ hours to reach Orsières. *P.P.G.*i, ch. 1.

to Adams Reilly it was ' perhaps the most interesting and certainly the most difficult expedition ever made by him in the Alps.' That autumn he was Vice-President of the British Association meeting at Edinburgh.

In 1851, Forbes made a journey to Norway to examine the glaciers and compare them with those of the Alps. He found that ' the conditions and structure of the Norwegian glaciers are almost identical with those of Switzerland, with the exception merely of the table-like forms of the snows with which they are connected.' Veined structure, crevasses, dirt bands were all in evidence ; assuming the latter to be of the nature of annual rings, a velocity of 168 feet per year was deduced for the Krondal Glacier. His conclusions were based on a series of very laborious and trying excursions, ranging from Christiania to the Lofoten islands and Hammerfest. These strained his strength to the uttermost, and in December his health suddenly broke down. As soon as he could travel he removed with his family to Clifton, where, under the skilful care of Dr. Symonds—who became one of his closest friends—he slowly recovered, and was able after nearly three years to resume his work in Edinburgh. During this time he completed what he called his ' Opus Magnum,' a very remarkable ' Dissertation on the Progress of Mathematical and Physical Science ' for the *Encyclopædia Britannica* (8th edition), for which he received the handsome honorarium of 400 guineas, and also composed his book on *Norway and its Glaciers*. Of this Sir Archibald Geikie has said ' it may be taken as the model of a journal of a scientific tour. I can bear witness to the accuracy of his sketches, alike of pencil and of pen. His two chapters on the Physical Geography of Norway have always appeared to me to be a masterpiece of careful yet rapid observation, broad generalisation and clear description.' Appended to the main narrative are the very important chapters on his earlier expeditions in Dauphiné and the Oberland.

In November 1854, Forbes was able, with the assistance of his former pupil, Dr. Balfour Stewart, to resume his duties as Professor, and carried them on till April 1860, when he exchanged his post for the dignified and less exacting office of Principal of the United College in the University of St. Andrews—an office which he held till the final breakdown of his health in the autumn of 1868. He died at Clifton on the last day of that year. During these last years he must have been greatly cheered by the invitation, transmitted through his old friend Sir Charles Lyell, to accept nomination as President of the British Association meeting at Dundee (1867), though the state of his health obliged him regretfully to decline the honour, as he also had to decline the Presidency of the Royal Society of Edinburgh, of which he had been the secretary for many years.

Though his impaired health made it impossible for him to revisit the Alps, he never lost his love of them. ' My heart remains where my body can never be. My yearnings towards the Colinton banks ' (the home of his youth) ' and towards the Alps are much on a par—both *home-sickness*.' But though he could not go there himself, he took

the liveliest interest in the climbs and explorations of Wills, of Tuckett, whom he urged (in 1859) ' to give a little more attention to scientific questions,' and of Adams Reilly. The latter owed much to Forbes's encouragement and instruction in his single-handed task of surveying the Mont Blanc group and constructing a detailed map of it—' a British work, the first true delineation of the most interesting ground in Europe.' Forbes used all his influence to get this remarkable map published by the Alpine Club. Other efforts that he made to get its members to undertake scientific observations met, as he good-humouredly complained with obstacles in their passion ' for unbounded muscular exertion and unfettered freedom of range.' How closely he kept in touch with Alpine matters is well illustrated by the following incident. At the meeting of the British Association at Birmingham in the autumn of 1865 there was ' a large Alpine party at the house of C. E. Mathews,' and there Forbes met Reilly and Whymper, and heard from the latter the details of the Matterhorn catastrophe. ' Mr. Whymper's letter to *The Times*, so perfect in taste and tone as well as in narration, raised him immensely in my estimation, and the impression was confirmed by his bearing and behaviour at Birmingham.' Doubtless as a consequence of this meeting Forbes urged Whymper in the spring of 1866 ' to endeavour to find out more about the veined structure of glaciers, which he considered was very much in want of elucidation.' (*Cf.* note 17.) Whymper's efforts to do this by digging a great pit in the névé near the Col de Valpelline are described in an appendix to *Scrambles*.

Wills and Adams Reilly became very valued and intimate friends of Forbes ; their affectionate devotion, based on the closest knowledge, must far outweigh the ill will of a few hostile critics, who so completely misconceived the lofty and chivalrous character of the man whom they vainly sought ' to humiliate as well as depreciate.'[30] Wills testifies that ' the preponderating quality of his mind was justice ' and Reilly that he had ' a heart so gentle, guileless and tender as has seldom been given to the sons of men.'

The chief characteristics which distinguish Forbes's Alpine work (apart from his scientific labours on glacier problems) have been well summed up by Coolidge.[31] He was one of the earliest British explorers of the High Alps ; his few predecessors made only isolated expeditions, not prolonged tours among unfrequented valleys and the world above the snow line. He wrote the first detailed book in English (and the first of any real importance since Saussure) on such explorations. His remarkable thoroughness of observation, and the lively interest so clearly perceptible beneath the sober clearness of his descriptions make it, even today, a pleasure to read and study his chapters. Finally he was a link between Saussure—whom he avowedly took for his master and his model—and the younger English moun-

[30] This is the verdict of so able and impartial a critic as A. L. Mumm, *loc cit*.

[31] In his introduction to the collected edition (1900) of Forbes's Alpine writings, to which, and to his notes on the text, I am much indebted.

taineers who founded the Alpine Club and carried on the work of exploration and climbing with such energy and success. Remarkable evidence of his influence in this respect is to be found in the frequent references—there are about 40—to him and to his *Travels* in the three volumes of *Peaks, Passes and Glaciers*. To quote one instance—W. Mathews, the prime mover in the consultations which led to the formation of the Alpine Club, and its fifth President, wrote : ' . . . Professor Forbes, who, in this (the Viso) as in so many other districts, was one of the first of English explorers, and whose name will ever be held in veneration by the many mountaineers who, like myself, owe their first love of the high Alps to the perusal of the fascinating pages of the *Travels through the Alps of Savoy*.'[32]

A few words may be added about Forbes as a Professor. His students bore frequent testimony to the lucidity and thoroughness of his lectures, and to the unfailing success of his illustrative experiments. He did much to raise the standard of work in the University, and it is significant that among his pupils were Clerk Maxwell, P. G. Tait (who was to succeed to his chair), and Balfour Stewart. While he was regarded as a somewhat stern disciplinarian, those students who were keen on their work found him a genial guide and in many cases a sincere friend. It is safe to say that the better he was known, the more he was respected and loved.

APPENDIX.

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[32] *P.P.G.* ii. p. 170.