known for a newspaper", Ludlow resigned[9]. Ludlow henceforward confined himself to contributions for *Macmillan's*, the *Spectator*, the *Contemporary* and the *Fortnightly*, and to the development of his Working Men's Colleges.

Reprinted from Nature, December 9, 1869

THE PROJECTED CHANNEL RAILWAYS

THE first question to be asked about a railway between England and France would be properly one upon its importance, and on the value of such a railway to the social and commercial interests of the two countries.

Let us consider the present situation and the circumstances which would affect, favourably or otherwise, a Channel Railway.

We have no *definite* plan of the proposed bridge, but we have a Channel Bridge Company; we have not even a definite outline of the main engineering features, but we have the assurance of the success of a model which, in the opinion of the projector, might be enlarged to *any extent*. The span of the bridge is evidently not agreed upon, nor is the construction of the piers determined; we are assured that we may have any span we like, and that there is no difficulty about the piers; in fact, the only thing wanted to complete this great national work in *three* years, appears to be a subscription of eight millions sterling to the credit account of the Channel Bridge Company.

The vagueness of the scheme is the safeguard of its promoters. We cannot even describe the propositions without running the risk of being contradicted on every point; it is even intimated that it is premature to discuss the scientific questions of the Channel Bridge scheme.

We have a few facts, however, on which we may safely enlarge. It is admitted, that from Dover to Blanc Nez, a distance of twenty-one miles, a number of piers are indispensable. In 1868, the distance was to be crossed by ten spans, each over 9,000 feet in the clear, and we have a diagram of that monster bridge. In 1869 rumour will have it that the number of spans is to be increased from ten to thirty, making the reduced span still over 3,000 feet in the clear. With the first proposition we should have had nine piers, with the latter, twenty-nine, washed by the waters of the Channel.

So much, then, about the piers. It may give the ordinary reader an idea of the character of this scheme. Shall we say anything about the 9,000 and odd feet clear span? At first sight it appears to be a typographical error; surely 900 and odd feet were meant; but then we meet with the fact of the Channel being divided into *ten* spans, so there is no getting out of it.

The whole proposition is the offspring of a highly imaginative mind. Of all the schemes or suggestions to cross the Channel by rail, this is the most incoherent. *There is nothing in it*—not one point of merit. It is not bold, because it lacks the spirit of boldness, viz. Sense. Not a trace of an engineer's mind is to be found in it. Our asylums produce innumerable schemes of this kind, but they are not permitted to disturb the public mind. It is a relief to have done with it. We are glad to say there are several projects which do not lack either sense or ability on the part of the originators. Some of them appear practicable, and one or two highly promising of success, and these will form the subject of our next communication.

On Ludlow's resignation, David Masson, new professor at University College, assumed control. Since 1859, Masson had been a good friend of Huxley's circle[10] (it was indeed through the intervention of his scientific friends, Thomas Hirst and William Sharpey, that he was elected to the *Athenaeum*) and it was natural that, following his successful work with *Macmillan's Magazine*, Huxley should turn to him. It is curious that twice in five years Masson should assume control over something Ludlow wished to start. But even Masson's presence did not save the journal, and its financial position failed to improve. In the meantime, its fate was shared by another review called the *Scientific Record* which had appeared briefly in April 1864. The *Record*, published by Frederick Mathieson on Telegraph Street, promised[11] to be a "Weekly Journal of Scientific Progress" and a record of the "progress of man in conquest of nature". As usual, it included book reviews, notes of the proceedings of learned societies and miscellaneous news and intelligence. It also boasted a weekly editorial which could well have been written by the Young Guard. "The editors hope", it proclaimed, "to render the *Brotherhood of Science* a real service, and to make their journal ever welcome, not only amongst scientific men, but also in those happy English homes where the light of Nature prevails"[12]. But its proud hopes had no chance. Once again expenses sapped the journal's strength.

In its second issue, the editors admitted defeat and sadly observed that "science is not sufficiently advanced in England, notwithstanding the labours of our scientific men, to ensure for such a paper as the *Record* at present popular appreciation and patronage". The editors had faith "that a weekly scientific journal is much wanted and when subscribers are prompt, they will be equally prompt in reissuing the *Record*". Reviews like the *Reader* would not suffice. As one correspondent exclaimed: "Science is too important and its objects are too multifarious to be adequately disposed of in what I may call the Postscripts of Literary Journals". This letter hailed the *Record's* articles in astronomy. It was signed "J. R.", but it embodied the sentiments of Norman Lockyer.

> Politics and social questions very properly claim priority of place and importance in the daily newspapers. A weekly scientific journal, therefore, in which "all important discourse in science will be faithfully recorded" containing also "Scientific Notes and Queries" and "admitting correspondence on general scientific topics of interest" was certainly a desideratum which your journal will opportunately supply.

In the summer of 1863, the future of the *Reader* was uncertain. Masson left the editorship. Among the sub-editors, Norman Lockyer was especially anxious that it should not stop, and he asked Herbert Spencer's advice. Spencer canvassed support for "a new weekly paper of literature and science" which would "eclipse the existing one". "If a few selected men were to combine", Spencer said, "each to write one article weekly on a subject within his own province, a periodical might be produced that would have great weight and authority." The paper could be published for £2,250, so forty shares of £100 were issued[13]. Tyndall said, however, that there were only thirty-five shares issued altogether.

All the scientists concerned canvassed their friends. "We sadly need a good weekly scientific organ," Tyndall[14] wrote to Herschel, "and the *Reader* . . . promises to supply

this want. It is exceedingly well supported. Would you object to adding your name to the list of shareholders? . . . There never was a better opportunity for establishing a healthy weekly scientific organ." But Herschel declined to pledge himself. With his best wishes he added: "I wish it could be accomplished to write the best matter which crops up, not being in the form of memoirs presented to the great scientific bodies of the country but into one really good monthly or bi-monthly journal of science, but that, I fear, is hopeless"[15]. Past experience augured ill for the fate of any such enterprise, but by November things looked brighter; in the end, about thirty-five shares were sold and the Young Guard was strong enough to bid £2,000 for the paper and to have working capital besides. Mr Huth agreed to invest £500 on condition that it be maintained an "organ of free opinion", and Hughes held a meeting to settle questions of policy. Octavius Smith, a philosophical correspondent of Spencer and proprietor of one of the largest distilleries in England, bought several. Henry Huth took five; James Campbell, another friend, took two; Huxley, Francis Galton, John Cairns, Sir Frederick Pollock and Spencer each took one. Bence Jones of the Royal Institution bought another and William Spottiswoode was proposed as printer. "The paper is not yet quite paying its expenses," Spencer wrote to Mill, "but it can scarcely be doubted that with the concentration of faculty now about to be engaged upon it, it will soon do so, and may not improbably become a good investment." By December, Mill had promised his support and Darwin and Lubbock soon followed[16].

On November 3, 1864, the reconstituted journal was discussed at the first meeting of the X-Club. The nine members—Huxley, Tyndall, Spottiswoode, Hooker, Lubbock, Busk, Spencer, Hirst and Frankland—were among the most influential scientists in the country. There was still some question about a general editor. On November 22, Thomas Hughes expressed to Huxley his strongest belief that "Lockyer can do the general editing and will be the best man for us. He knows the machinery, having been there from the first, has been in constant relations with such men as Ludlow, etc. . . . has the science already in the right grooves and is not above taking advice, is a real good worker and above all has his heart in the business. . . . He will do the work too gladly at a lower figure than any other competent man, a consideration to be regarded at the present until we get more capital and know where we are"[17]. John Dennis, a noted literary scholar and critic, was apparently appointed. But in the end Sir Frederick Pollock (whose brother, Walter Herrie, edited the *Saturday Review* was literary editor-in-chief. Pollock's father, George Frederick, was a Trinity mathematician and a close friend of Faraday. The Pollock family, in turn, was close to Tyndall and his neighbours in Hindhead. Tyndall and Huxley agreed to edit science, Huxley specializing in physiology and Tyndall in physics. John Llewellyn-Davis did theology and Galton travel, ethnology and natural history. Lockyer assisted in astronomy, Spottiswoode in physics, and John Cairns in political economy. G. H. Lewes did fiction and poetry, and Spencer did philosophy and psychology.

A programme advertising the new *Reader* was drawn up, listing 75 men of science, and, on February 4, 1865, a new prospectus was issued. One passage in the new prospectus[18] was especially significant:

> The very inadequate manner in which THE PROGRESS OF SCIENCE AND THE LABOURS AND OPINIONS OF OUR SCIENTIFIC MEN are

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The Veined Structure of Glaciers

I THINK there is no one point in connection with glaciers more interesting than their veined structure, or one upon which so much has been written that remains equally unsettled. The difference of opinion about it between the authors who have published most upon the subject are not less remarkable than the phenomenon itself: no two are agreed, except in considering it as a constitutional feature.

Professor Agassiz maintains (*Atlantic Monthly*, Dec. 1863) that the horizontal layers of pure ice which are formed between the beds of snow from which a glacier is born, constitute many of the identical veins or plates of pure ice which pervade the glacier when it is in full life and activity; and attributes the inclination which they make, in the latter case, to their former horizontal position, to the contortion, bending, or folding, to which they have been subjected on their downward course; but, at the same time, he distinguishes between these veins—the result of stratification, and others which he terms bands of infiltration, and which he believes to have been formed by the infiltration and freezing of water.

The late Principal J. D. Forbes maintained ("Occasional Papers on the Theory of Glaciers," 13th letter) that the veins of stratification were annihilated at a certain point, and that at precisely the same time other veins, approximately at right angles to the former ones, were formed. These effects he referred to intense pressure.

Professor Tyndall ("Glaciers of the Alps," pp. 380, 425-6), agrees with Professor Forbes "in ascribing to the structure a different origin from stratification," and, if I understand him rightly, does not believe that *any* portion of the (approximately) vertical veins have such an origin. He divides the veins into marginal, transverse, and longitudinal structure, and asserts that all are produced by pressure, which causes partial liquefaction of the ice, and that the water is refrozen when the pressure is relieved.

If any one cause produced the whole of the veins of pure ice that are found in the imperfect ice of glaciers (which all are agreed are a constitutional feature of those bodies), it is obvious that that cause would have to be equally generally distributed. It is indisputable that all the veins are not veins of stratification, because examples have been frequently observed crossing (cutting) the strata lines at a larger or smaller angle. But although such observations prove conclusively that all the veins must not be attributed to stratification, they do not prove any more. I believe, with Professor Agassiz, for reasons advanced elsewhere,* it can be demonstrated, equally conclusively, that many of the veins which are seen in the lower courses of glaciers in the Alps are veins originally produced by stratification, and dissent entirely from the "annihilation" of Principal Forbes. But as it is proved that some have a different origin, we must look to other causes for an explanation. It is probable that the theories quoted above offer a practical solution of the difficulty, although they are unfortified by direct proofs. But I have seen examples which it was difficult to explain by either one or the other.

There is one means by which the veins might be produced, which, if not overlooked, is at least not generally advanced. All glaciers have crevasses; a glacier is known by its crevasses. The sides of all crevasses become more or less weathered and coated with a glaze of pure ice. When they close up again, when the sides join by virtue of regelation, does this leave no trace? Can it be annihilated? Or, do the two coalesced films leave their mark as a vein of pure ice throughout the generally whitish mass of the glacier? I consider a large number of the veins of pure ice which constitute the "veined structure" of glaciers as nothing more than the *scars of healed crevasses.*

It is not easy to say whether this was the meaning of the following passage, taken from p. 201 of Forbes's "Occasional Papers:" "Most evidently, also, the icy structure is first induced near the sides of the glacier where the pressure and working of the interior of the ice, accompanied with intense friction, comes into play, and the multitudinous incipient fissures occasioned by the intense strain are reunited by the simple effects of time and cohesion." Judged by his preceding pages, it is not, and I am unaware that it has been, advanced in any other place. Some of your readers may perhaps be able to throw some light upon the subject.

Dec. 13, 1869 EDWARD WHYMPER

recorded in the weekly press and the want of a weekly organ which would afford scientific men a means of communication between themselves and with the public, have long been felt. They have been the subject of special consideration lately by some of the leaders of Science in London.

To remedy this, the *Reader* expanded its science space to eight pages out of twenty-five. The new prospectus was issued and a new series launched. Spencer told Mill[19]: "If this opportunity of establishing on a safe footing an organ of scientific thought and of conscientious literary criticism is lost, it may be long before this very desirable object can be achieved".

In November 1864, Francis Penrose, archaeologist and astronomer, enthusiastically told Lockyer[20] that he had seen Ludlow "from whom I learnt that the *Reader* was casting off its old slimy skin and coming out renewed in better style. I hope it is for your sake—you were very despondent about it when I saw you". For about ten months the journal attracted scientific work; Lockyer published Croll's paper on the physical causes of climatological changes during geological time[21], and a paper by William Huggins on nebulae.

Unhappily, these good omens were short lived. Unexpected difficulties of communication and disagreements about agreed commitments arose among the team. Moreover, according to Spencer, John Bohm, the paid subeditor and the only man with journalistic experience, found himself out of sympathy with his scientists. "In the end their aims as well as the expectations of our subscribers were balked." In April 1865, Spencer significantly tried to arouse wider interest by getting[22], "so far as possible, occasional brief letters from the leading men of science announcing such interesting novelties as admit of being understood by the general public and one of fit nature to be quoted for our columns". In August 1865, Thomas Bendyshe*, a professional author, editor and anthropologist, purchased the paper and the management improved a little, but personal relations between the editor and the staff remained poor and the journal ran aground in stormy literary disputes.

There were other disappointments. Huxley, struggling with the *Natural History Review*, found the strain of one collapsing journal too much and he wrote nothing for the *Reader*. In January 1867, the last issue appeared and the *Reader* died soon afterwards. The paper had suffered, according to Galton[23], from continuing inefficiency in operation and dullness in style "notwithstanding some really good articles".

> The management was naturally too amateurish; promised articles were delayed and the time of the Committee was too much wasted in frequent discussions about first principles upon which Spencer loved to dilate.

But there were other reasons for its failure. Karl Pearson[24] noted first that, while its scientific list "was a tremendous force to bring together", because "there was no one man who would devote his whole life to the projected task, the *Reader* came to nought". Second, the powerful scientific guns who had given tacit support remained silent when asked for reviews. The great men of the day—Lyell, Darwin and Herschel, for example - were solicited but did not reply. Lacking this appeal, the journal's circulation could only suffer. Third, the journal was dissipated by divided responsibility among the editors[24]. "The ship had too many first rank commanders aboard and no one whose livelihood depended on successful voyages. It is small wonder that it never reached port." As Pearson adds, *Pereat lector, Natura resurgat*. Indeed, at its death the *Reader* was the closest approximation yet to the wishes of the London scientific circle.

* Thomas Bendyshe, educated Eton and King's College, Cambridge, M.A. 1852 and Fellow of King's. Called to the Bar, Inner Temple, 1857. Translated and edited treatises from Latin, German and French for the Anthropological Society. Venn. *Alumni Cantabrigiensis*.

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The Corona

In connexion with Mr. Lockyer's paper "On the Recent Total Eclipse of the Sun," the following observations may be useful.

I observed the total eclipse of July 1860, in company with my friends Professor Chevallier and Mr. B. E. Hammond, at the village of Pancorbo, in Spain. We were on the summit of a mountain of considerable height, about 5,000 feet above the sea, and were therefore under somewhat peculiar atmospheric conditions. I observed specially four things:—

(1) Venus; which was then extremely near the sun, the thickness of the crescent being only 1 or 2 seconds, and therefore very favourably placed for observing whether it has an atmosphere.

(2) The extent of the corona, and its form. This I am sure was very irregular; very nearly, if not quite, permanent during the three minutes of totality; was nowhere less than 25′ in breadth; in one part, the top in an inverting telescope, 40′ in breadth; and in another, the right, was more than 60′ in breadth, running out in a long wavy line like floss silk. I have before me the drawing I made at the time, during the totality.

(3) The amount of light given by the corona. This was estimated by a photometer, consisting of a wedge of dark glass, with a moveable slit, contrived by Mr. Chevallier, and now, I believe, in the possession of the Astronomical Society, with the place marked through which I saw the corona. It was as bright as a small cloud, distant 8° from the sun, 10 minutes after reappearance; or as the moon when 2½ days old, as the sun was setting.

(4) The colours shown by a variety of coloured ribbons during totality. Of these, the only observation that bears on Mr. Lockyer's paper, was that on the extent of the corona. I estimated it twice; once as reaching, to the right, 2¾ diameters of the sun, and once, later on, at nearly 2½ diameters. I had no micrometer, but could not possibly have been wrong by so much as 10′. I wrote down at the time, that it underwent no perceptible change during the eclipse. It remained visible for six seconds after the reappearance of the sun.

I had, and have, little doubt that the corona is in the solar, and not terrestrial atmosphere.

Rugby School, Nov. 11 JAMES M. WILSON