space in your columns to air another subject which I also brought before Convocation, with, I am sorry to say, equal want of success, and that is the desirability of modifying the examinations for the degree of Bachelor of Science by omitting the biological subjects, so as to induce engineering students to take it. At present the biological subjects required for the degree, viz., Zoology, Botany, Physiology, and Organic Chemistry, are so entirely foreign to the studies and requirements of such students that in most cases it is scarcely practicable, even if it were desirable, for them to travel so far out of their regular line of work, for the purpose of getting them up for the Bachelor of Science examination. Such a course would be precisely analogous to that which is now prescribed for medical students proceeding to their M.B. degree, who are required to take up those subjects of the B.Sc. examination which are cognate to their routine of study, and who then branch off to those of a purely professional character.

Only two objections were urged in Convocation against this scheme which are worth consideration. The first was that it would tend to lower the standard of the degree by diminishing the comprehensiveness of the examination. In order to meet this objection I suggested that candidates not wishing to take up the biological subjects should be required to substitute for them others of a mechanical nature, such as Applied Mechanics, Engineering and Architectural Construction, and Geometric Drawing, which, as all who have had any experience in teaching them know, are quite as capable of being made efficient educational tests as those which they would replace. The second objection was, that to make such a change would be equivalent to instituting a degree in engineering. That this would be the practical result of the suggested alteration I am prepared to admit, and it is the object which I had distinctly in view in proposing it. What there was in the suggestion to provoke the unconcealed opposition of so many of the members of Convocation, I am a loss to imagine, unless it was the illusion that the profession of engineering is a less scientific one, and the education of its members less worthy of being encouraged, than that of the professions of law and medicine, to which so large a proportion of the London graduates belong.

For my own part, it seems to me a scandal of no mean gravity that, whilst the practice of that profession requires intellectual qualifications of the highest order, and a scientific training of the widest kind, no means should exist in this country whereby either the public should be provided with any guarantee that those who practise it possess either of these qualifications, or its practitioners themselves should be enabled to give evidence of the fact of their own accord. I do not know of any department of education in which the University of London could, at the present time, do more service than in this, and, I trust, there are men in its Senate, who, with more breadth of appreciation than the majority of Convocation, will give the matter their earnest attention.

FRANCIS T. BOND

Hartley Institution, Southampton

Mechanical Equivalent of Heat

I AM afraid your publication, without adding the date, of my letter last week (which I only saw this morning) puts me in a false position in regard to Dr. Joule, inasmuch as it appears to ignore a correspondence of mine with him, which took place between the time that letter was written (now a long time since) and the time of your publishing it.

In that correspondence I allowed that Dr. Joule's theory remained the same in its main features, though I thought he virtually retracted one statement which I had particularly argued against. Dr. Joule, however, did not allow he had made any alteration.

He also informed me that a paper of mine had been read at the meeting of the Manchester Literary and Philosophical Society, in which I showed (as I believe) in a detailed examination that his theory was inconsistent with the results, both of his own and of M. Favre's experiments. Dr. Joule also kindly communicated to me the substance of the reply which he had made, but I have not seen either in print. Of course the question is one of facts; are facts consistent with the new laws of thermodynamics as supposed to have been established during the last twenty years? Tait, in his preface to his Thermodynamics, says: "The subject s one of vast importance, but very few indeed are yet acquainted with even its most elementary facts; and by many of these it is not yet accepted as true." These laws, therefore, can scarcely yet be put on a level with Newton's laws, even if they should be shown to be consistent with facts, which, at least in their present form, I believe to be impossible.

H. HIGHTON

May 18

MR. HIGHTON'S letter in NATURE is almost identical with his communication to the *Chemical News*. My answer is similar to that which I sent to the latter publication, viz., that the object of my paper in the Proceedings of the Literary and Philosophical Society was simply to place the theory of the electro-magnetic engine in a form which might prove useful to those who had not worked on the subject, and not in any respect to withdraw the reasonings in what Mr. Highton is good enough to term my "famous paper." Mr. Highton handsomely acknowledged the justice of my note to the *Chemical News* in a letter addressed to me on the 28th ult.

JAMES P. JOULE

Optical Phenomenon

IN reading over Prof. Clerk Maxwell's paper on Colour in NATURE (Vol. iv. p. 13), I was reminded of the following, to me, curious phenomenon which was seen by me on several occasions in the summer and autumn of 1859.

Whilst standing before a black board, making geometrical figures in white chalk, I was struck by one side of each chalk line appearing blue, the remaining half retaining its proper white. The cause was at once evident to me, for I found that the sun shone fully upon one eye, but not upon the other. By closing the eye upon which the sun shone, the chalk marks appeared wholly white. Opening the eye again, the half blue, half white marks appeared; then closing the eye upon which the sun did *not* shine, the whole of the marks appeared a pale blue, scarcely so deep in colour as when in contrast with the white. By squinting, or forcing the eyes to see double, two sets of marks appeared, the one set all blue, the other wholly white.

Subsequently, with the sun upon both eyes, the whole of the marks were blue; whilst upon another occasion, when the sun shone very fully upon both eyes, only the white marks were evident; but shading the eyes by the hand, and allowing a ray to fall upon one eye, the usual half blue half white lines appeared.

On every occasion that I tried the experiment I met with the same results, and when I looked away a beautiful orange-coloured spot—the complementary colour of the blue, I suppose—appeared for some time wherever I looked. What is the cause why only the blue rays were visible? and why blue rather than red or yellow?

THOS. WARD

Yellow Rain

THE following notice will perhaps be of some interest to the readers of NATURE. In December 1870, after a heavy rain at Rosario de Cucuta (New Granada), a great many small round specks of a yellow clayish substance were found on the leaves of plants that had been exposed to the rain. A sample of this substance was sent to Dr. A. Rójas, of this town, who forwarded it to me in order to examine it under the microscope. It proved to be composed almost entirely of a species of *Triceratium*, and another of *Cosmarium*, which must have been carried away by a violent storm from their lacustrian abodes.

Caracas (Venezuela), April 1871

A. ERNST

The Irish Fern in Cornwall

MY first impulse, on reading the note on this subject in NATURE for the 4th of May, was to apologise to Mr. Dymond for having caused him so much regret by making known a Cornish station for this fern. This first impulse was however checked by the reflection that something is due to the advancement of the study of distributive botany; and I could scarcely have expected even Mr. Dymond to place any very great degree of confidence in my bare assertion, unaided by any reference to localities.

Now that I have done the mischief and made known that the Trichomanes is a Cornish plant, and have been corroborated by Mr. Dymond, it would be interesting to know whether the writer of the note on this fern in the Cheltenham Natural History Society's report found his specimen at the same place, *i.e.* at St. Knighton's.

EVERARD F. IM THURN