in one way or another, the cavities of the water-conducting elements, and then observing whether the current is interrupted. Sachs and Dufour endeavored to attain the result by sharply bending the stems of actively transpiring plants, but this method is obviously unsatisfactory, owing to the difficulty of proving that the cavities are really closed. Elvring attacked the problem in a different way. He injected portions of the stem of woody plants with cocon-butter, melted at a temperature of 32°C., and satisfied himself that the cavities were really filled up when the injected material had solidified. Under these conditions he found that a pressure of 60 cm. of mercury failed to force any water through the wood, though before the injection 1 cm. of water had sufficed to cause filtration.

To Elvring's experiment two objections have been made. On the one hand, Dufour argued that the absence of the action of transpiration, rather than the closure of the cavities, might well explain the result of the experiment. On the other hand, it was objected by Sclat that the action of the fatty cocon-butter on the membranes would probably render them impermeable to water, and thus account for a negative result. Prof. Errera has succeeded in modifying Elvring's method in such a way as to meet both these objections.

In the first place, actively transpiring branches were employed for the investigation, *Vitis vinifera* being selected for experiment on account of the large diameter of its vessels. Secondly, instead of cocon-butter, a solution of gelatin melting at 35°C. was used as the injected material. This was emulsified with Indian ink, so that its presence in the vessels might be easily demonstrated. The action of the transpiration was in all cases assisted by the pressure of a column of water 50 cm. in height. The experiments were carried out with all possible precautions, and the result in every case was that the injected branches took up no water, and failed in a few hours, while, under precisely similar conditions, un.injected branches remained perfectly fresh for three days at least, and during that time transpired many cubic centimeters of water. For details and numerical results we must refer to the original.

Prof. Errera's experiments certainly add greatly to the already existing probability that the cavities of the tracheal elements of the wood constitute the channels through which the sap ascends.

D. H. S.

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**LETTERS TO THE EDITOR**

(The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts.

The Editor urges authors to communicate their communications as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communication interesting and of novel facts.

The Lost Found—Boole Justified and Monge Restated

In his Rights by Prof. Bernam of the University of Michigan, U.S.

In the report of my public lecture on Reciprocity, published in the *Nature* of January 7 (p. 222), mention is made of a formula, given by Boole in his book on "Differential Equations," which he attributes to Monge.

Ravenna, Boole's edition, and Paris were the scenes of the passage in Monge in which it occurred, and very diligent search was made, as well in the printed works as in the manuscripts of Monge in the library of the Institute, to accomplish this object.

But all these researches were fruitless, and the opinion was come to by the Patriarch of Monge that Boole had made a misinterpretation, and that the formula ascribed by him to Monge was not to be found in his works. The formula is one of very great interest, as being the first instance on record of a multi-nomial projective reciprocal.

Knowing how scrupulous and painstaking Boole was, and the

least likely of all men to make a quotation at random, I never sequenced in this belief, but entertained little or no doubt that any one would ever succeed in unmasking a reference, which had defied all the endeavors of Monge's own countrymen to verify.

But fate has designed otherwise, as we shall see from the following letter. In addition to the satisfaction of a controversial point being settled and Boole's character freed from a misapprehension of impropriety, it is to my mind, and will probably be so to many of the readers of *Nature,* a peculiar source of pleasure to contemplate the occurrence as an illustration or note of the unity not merely of occupation, but of feeling also, which binds together mathematical workers in all parts of the world.

To think that such a task found impossible in London and Paris should have been accomplished in the most satisfactory manner at Yale and Michigan !

Without further comment I submit the letter in its entirety as written, for the interest which it is to me and the world-wide-diffused crotina de *Nature,* and think that all its readers will join with me in acceding to the cordial vote of thanks to Prof. Bernam for his valuable contribution to mathematical history.

**University of Michigan, Ann Arbor, Michigan, April 3, 1886**

**Prof. J. J. Sylvester,**

*To Sir,—You will find Monge's form of the differential equation of the cone in his memoir, "Sur les Equations différentielles des Courbes du Second Degré" (Comptes Rendus, 1859, 100, pp. 51, 54), and in *Bulletin de la Société de Philosophie, Paris, 1883, pp. 87, 88, the first as having been contributed directly by Monge, and the second as having been copied from the first.

I have not seen the journal myself, but the references have been verified for me at the Yale University Library. The actual form is \[ \rho^2 - 4\rho^2 + 4 = 0. \]

The term "Mongeian" can now be used without brusquetry by you.

I remember noticing this form when I began reading Boole's "Differential Equations," and also noticed Halphen's method in Jordan's "Cours d'Analyse." It never occurred to me that Halphen considered this form original with himself, I thought that his method, probably, of deducing it was different from Monge's.

With kind recollections of having met you at Johns Hopkins once upon a brief visit when Prof. Cayley was there,

I am yours very sincerely,

W. W. Story,*

*Assistant Professor.

Since writing the above, in fact this very afternoon, I have received a letter from the Universal Knowledge and Information Office containing the same reference as those given by Prof. Bernam, which will speak for itself, and cannot fail to draw the attention of the reader of *Nature* to the important service which this Society is capable of rendering to all engaged in research of whatever nature in enabling them to discover the origins and hunt up the authorities of any subject on which they may desire to obtain information.

It is certainly a singular coincidence that after the lapse of four months the desired information in this case should have reached me from such widely distant sources at an interval of less than forty-eight hours. The letter, which I cannot, in good conscience, be allowed to suppress, is as follows:

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**Letters to the Editor**

"... It takes the form

\[
\frac{1}{\sqrt{x}} + \frac{1}{\sqrt{y}} = \frac{1}{\sqrt{z}}\]

..."
Mathématiques. — Sur les équations différentielles des Cours du Second degré, par M. Monge. L'équation générale des coniques du second degré étant
\[ \alpha^2 + \beta x + \gamma = 0, \]
dans laquelle \( \alpha, \beta, \gamma \) sont les constantes, M. Monge a donné l'équation différentielle déduite de toutes ces constantes, et il a trouvé l'équation submue de l'ellipse, de l'hyperbole, etc.

\[ \frac{dy}{dx} = \alpha \frac{dx}{dy}, \quad \frac{dy}{dx} = \beta \frac{dy}{dx}, \quad \frac{dy}{dx} = \gamma \frac{dy}{dx}. \]

Les quantités \( y, x \) sont définies par les équations suivantes:
\[ \frac{dy}{dx} = \alpha, \quad \frac{dy}{dx} = \beta, \quad \frac{dy}{dx} = \gamma. \]

Il est important de noter que l'intégrale d'une équation d'un ordre inférieur qui satisfait à cette équation \( \alpha = \frac{dy}{dx} \) a une dérivée l'équation différentielle \( \frac{dy}{dx} = \alpha \). Il est important d'éviter toute solution de l'équation différentielle qui satisfait à cette équation.

Le même mérite pourrait s'appliquer aux équations des nœuds d'un arbre supérieur au second.

A note is added to the effect that "Cet article est édité de la Commission des écoles de l'Académie Impériale de l'École Technique, réorganisée par M. Lachaise; sur le 2 de 2e volume, 2e série." This proves the work of this article at the British Museum in PP. 1542.

Regarding that this is the reference you are in search of, and that the long delay in the discovery of it may be excused when it is realized that the reliability of identifying a particular passage (known perhaps only in its full extent to those whose chief work is concerned with such matters) is considered.

I remain, Sir, faithfully yours,

J. J. Sylvester

New College, Oxford, April 19

On the Velocity of Light as Determined by Foucault's Rotating Mirror

If there has been shown by Lord Rayleigh and others that the velocity \( U \) which has been observed by a group of waves is propagated in any medium may be calculated by the formula—
\[ U = 1 + \frac{1}{2} \log v \]
where \( V \) is the wave-velocity, and \( \lambda \) the wave-length. It has also been observed by Lord Rayleigh that the fronts of the waves, reflected by the revolving mirror, become more extended on one another, and in consequence must rotate with an angular velocity—
\[ \frac{d\theta}{dt}, \]
where \( \theta \) is the angle between two wave-planes of similar phase. When \( \theta / \pi \) is positive (the usual case), the direction of rotation is such that the following wave-plane rotates towards the position of the preceding wave whose angle may be calculated by the formula—
\[ U = 1 + \frac{1}{2} \log v \]
where \( V \) is the wave-velocity, and \( \lambda \) the wave-length. It has also been observed by Professor Rayleigh that the fronts of the waves, reflected by the revolving mirror, become more extended on one another, and in consequence must rotate with an angular velocity—
\[ \frac{d\theta}{dt}, \]
which reduces by the first formula to \( \theta / \pi \).

The discussion of the experiment by following a single wave, and taking account of its rotation, is a complication that easy to leave out of account some of the elements of the problem. The principal objection to this, however, is its unsoundness. If the dispersion is considerable, a wave which leaves the revolving mirror at an angle of \( 45^\circ \) from the axis would have a wave which he used the same exact value \( \theta / \pi \).

By the kindness of Prof. Michelson, I am informed with respect to his recent experiments on the velocity of light in a medium of carbon that he would be inclined to place the maximum brilliancy of the light between the spectral lines D and E, but nearer to D. If we take the mean between D and E, we have:
\[ U = 1.745 \]
\[ \lambda = 1.737 \]
\[ n = 2.5 \]

\[ A = \text{denoting the velocity in vacuo} \]
\[ v \]

The number observed was \( 1.74 \) with an uncertainty of two units. In the second place of decimals of this agrees best with the first formula. The same would be true if we used values nearer to the D line.

J. W. Gibbs

New Haven, Connecticut, April 1

The Effect of Change of Temperature on the Velocity of Sound in Iron

I wonder if the attention to an error relating to the above subject, which originated with Wahrnehm, still holds a place in some of the modern books on science. According to Wahrnehm, the velocity of sound in iron and steel is increased by rise of temperature not extending beyond 100° C. Now in the case of iron, the statement is not made that this is true of the longitudinal elasticity of iron, as determined by the elastic constants, will be found greater at 100° C than at 0° C. Provided we begin with the lower temperature first and the note has not, after the original measuring, been previously heated to 100° C, but the argument is supposed increase of elasticity is really a process at once (Phil. Trans., part 1., 1883., p. 305.), and if the wire be repeatedly heated to 100° C, and afterwards cooled, subsequent tests will always agree with the lowest temperature than that at the lower, if sufficient rest after cooling be allowed. When, however, we come to such small molecular displacements as are involved in the passage of a wave through a wire, even the apparent increase in elasticity mentioned above vanishes. I have been able to prove that, when an iron or steel wire is thrown into longitudinal vibration, so as to produce a music note, the pitch of this note becomes lower as we raise the temperature, even when the wire is heated for the first time after it has left the maker's hands.

It seems from this that the effect of a wave to so long been affected to remain uncorrected, for it has been known for many years that the pitch of a tuning-fork made of steel is lowered by small rises of temperature, and the main part of this lowering must be due to the decrease of elasticity of the steel.

H. T. Tomlinson

King's College, Strand, April 10

Sound-producing Apparatus of the Cichlids

With regard to the above subject, treated of in an article by Mr. R. Lyon Morgan in February last (NATURE, February 18), I may mention that some time ago I examined the drum of the common cichlid found plentifully in the Nilbrasses near Simla, and which in the evening, and end forth a desparing morn from the thud-dendron trees like the whirr of large machinery, Generally the arrangement of the drum and the powerful muscles was as figured by Mr. Morgan, but I also noticed the following particulars not mentioned by him:

The puffer mouth in the membrane of the drum were not parallel, but converged slightly towards one point of the mem-