British Association, 1833, the attention of the mathematical and physical section was largely given to the subject, and Herschel, Airy, and others spoke warmly in praise of the discovery. In the introductory discourse with which the proceedings of that meeting was opened, Prof. Whewell made it a topic, and expressed himself in the following words: 'In the way of such prophecies few things have been more remarkable than the prediction that under particular circumstances a ray of light must be refracted into a conical pencil, deduced from the theory by Prof. Hamilton and afterwards verified experimentally by Prof. Lloyd.' Previously, in the same year, Prof. Airy had publicly recorded his impression upon the subject as follows: 'Perhaps the most remarkable prediction that has ever been made is that lately made by Prof. Hamilton.'"

The view Hamilton himself took of the discovery of conical refraction was characteristic. "It was," he writes to Coleridge on February 3, 1833, "a subordinate and secondary result when compared with the object I had in view to introduce harmony and unity into the contemplations and reasonings of optics regarded as a branch of pure science."

At the close of this volume we still leave Hamilton quite a young man. The great labour of his life has not yet commenced; its nature has not indeed even dawned upon him. We shall therefore look forward with pleasure to the continuation of the present most interesting work. The development of Hamilton's more mature genius, his correspondence with De Morgan, in itself no inconsiderable mass, and above all the gradual evolution of quaternions, will form most attractive materials for his biographer.

It is by the liberality of the Board of Trinity College, Dublin, that the present instalment of the work has been brought out, and we sincerely trust that the same liberality will be extended to enable the biographer to continue to do real justice to his subject. But besides the present work another debt is due to his memory. Hamilton's earlier papers are very inaccessible: many of them are scattered about in various periodicals, and his two noble treatises on quaternions are out of print. A complete edition of Hamilton's works would be an appropriate sequel to this biography, and they would be not unfitting companions for the works of Lagrange and of Gauss. It is not often that a University has so gifted a son as Hamilton. Let us hope that the University which is proud to claim him will see fit to raise this further monument to his genius.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Sheet-Lightning

IN NATURE, vol. xxvii. p. 576, a statement is made that the "opinion so long and generally entertained" that "sheet-lightning and the so-called summer or heat-lightning are nothing else than the reflection of, or the illumination produced by distant electrical discharges, is not supported by observation." This statement surprises me, for I should have said that the opinion once commonly entertained that sheet-lightning is a distinct form of lightning unaccompanied by sound, is now for the most part rejected, the results of observation being distinctly against it. The question is an old one; but as the writer of the above statement only refers to the observations made at Oxford during the twenty-four years ending 1876, I will confine myself in the main to an examination of these. I must premise that I do not assert that lightning never occurs at such an altitude that the thunder accompanying it is not audible. In rare instances in Europe lightning is observed in the zenith, followed after an interval of twenty seconds or more by faint rolling thunder immediately overhead. It is therefore antecedently probable that lightning may occur at too great an elevation for the thunder to be heard at the earth's surface at all; and this is especially likely to happen in some of those thunderstorms within the tropics, the altitude of which is extremely great.

The distance at which the illumination produced by lightning in a dark night can be observed depends upon the altitude and the intensity of the discharge, and further upon the altitude, character, and amount of the clouds. It is possible that the diffused particles of ice (at a much greater altitude than the cirri), which produce the phenomenon called "rayons du crépuscule," are capable in some cases of reflecting the illumination. However this may be, it is certain that the illuminations of an ordinary thunderstorm at midnight, when there is no moonlight, have an average radius of more than forty miles. The distance at which thunder is heard depends on a variety of conditions; but we may safely state that in the open country in calm weather at midnight the sound is rarely heard at a greater radius than fifteen miles. At the Radcliffe Observatory, which is scarcely out of the reach of rumbling sounds produced by the traffic of a town, the average distance at which thunder is distinguished may probably be safely reduced to seven miles. Assuming then that at Oxford the area of illumination has a radius of forty-two miles, and that of thunder one of seven miles (and in this assumption we are probably not very far from the truth), we conclude that in the darkest hours "lightning without thunder" should occur at Oxford with a frequency which is expressed by the figures 35 : 1 as compared with "thunder with or without lightning." A deduction ought, of course, to be made for the effects of moonlight. But when this has been made, the figures quoted by your reviewer are not only satisfied by the hypothesis for the refutation of which he employs them, but further, if his mode of reasoning were legitimate, they would lead us to the conclusion that in nearly seven cases out of eight the thunder heard at Oxford is not the result of electrical discharge at all! Such thunder does not occur elsewhere, and was not in vogue at Oxford "in my time."

Practically, however, two considerations must not be omitted: (1) some localities enjoy a special immunity from thunderstorms, while others are responsible for the production of an exceptionally large number; in the former the frequency of illumination will be greater in comparison with the frequency of thunder, in the latter it will be less: (2) (and this is a consideration of much more importance, though frequently neglected when a conclusion is deduced from records of phenomena) the relative frequency of two sets of *occurrences* often differs widely from the relative frequency of the *records* of the occurrences. The relative frequency of records of thunder and of lightning is to a large extent dependent on the position of the observer's residence, his habits, the keenness of his eyes and ears respectively, and his attentiveness to the impressions which those organs respectively experience.

No one who has on a summer night carefully watched the gradual approach of a great thunderstorm, counting the flashes, and registering the time-interval and number of claps from the minute when the first flickers begin above the southern horizon to that at which the storm is in its full roar and rattle overhead; no one who in a long night journey by train has run into a thunderstorm whose distant coruscations he has noticed two or three hours beforehand; no one, at least, who after watching sheet-lightning in one particular direction has made careful inquiries as to the occurrence or otherwise of thunder over the district from which the light proceeded, will hesitate in pronouncing the verdict that ordinary sheet or summer lightning is simply the illumination produced by a distant thunderstorm.

W. CLEMENT LEY