consequence that this standard cannot be a unique one. When, for example, Lord Rayleigh conceived and carried out in 1902 an experiment in which he sought to find evidence of double refraction in a plate of glass owing to its motion through the æther, Sir Joseph Larmor gave it as his opinion that the negative result was to be expected on theoretical grounds.

It may be taken indeed as proved that in so far as matter is electrically constituted, the form of the equations which embody the theory is such that effects due to the motion of bodies as a whole through the æther must always be concealed.

But is matter of purely electromagnetic constitution? Are existing theories able to give a complete account of those phenomena which have actually been experimentally investigated?

The classical experiment of Michelson and Morley may be taken as an example on which to test these questions. It is generally admitted that this experiment shows that we cannot detect a difference in the velocity of light relative to the earth in two directions at right angles one of which may be thought of as parallel and the other perpendicular to the motion of the earth through the æther. Such a difference must exist if light is thought of as being propagated with the same velocity in all directions relative to the æther.

The only suggestion that could be made to reconcile the failure of the search for this difference with the theory of a stationary æther was that of FitzGerald, that the motion of the apparatus through the æther so modifies its internal constitution that it automatically contracts to an extent which exactly neutralises the effect which would otherwise be observed. It was in the effort to give a reason for this contraction that the theory that has been referred to was developed. But whether we take the presentation given by Larmor or Lorentz we find that the general equations of the electromagnetic field have to be supplemented at some point by a hypothesis as to the nature of the electrons which are the elementary constituents of matter, in order to make the scheme sufficient to determine the way in which they will move. Now the length of a body, thought of as constituted by electrons, depends upon the motions of those electrons. If we are to think of any piece of matter whatever as contracting according to FitzGerald's hypothesis, we are bound to think of the paths of the electrons within the body as being modified in some corresponding way. Thus the hypotheses that may be adopted as to the nature of the electron are not arbitrary, but must be such as will lead to the contraction hypothesis as a consequence.

Similarly, if we consider the experiment of Rayleigh referred to above, the refracting properties of glass are conceived to be due to the light waves falling upon electrons which have inertia and which have to be moved by the electrical forces produced by the light. If we were to assume that the electrons have a definite mass in the Newtonian sense, then Rayleigh's expectation

of a double refraction when the glass is moving would be justified. Lorentz is able, however, by assuming *among other things* that the electron is a spherical nucleus which itself is subject to the FitzGerald contraction, to extend his argument to cover the null result of this experiment. But the special assumptions which he makes were all made with an eye towards the result, namely, the failure of experiment to give a positive evidence of motion through the æther. They were hypotheses *ad hoc*, and to that extent they were really, though the name had not been invented, applications of the principle of relativity. It cannot be shown from the form of the general equations of the electromagnetic field alone that null effects are to be expected, for the experimental results most certainly extend into regions where these equations are insufficient; they do not cover, for instance, the whole theory of refraction, of conduction of electricity, or of the exterior configuration of a given body.

It is for this reason that the hypothesis that the *fact of motion relative to the æther must be for ever concealed*, becomes of importance as a general and independent principle. It becomes a criterion and a guide, for example, as to the form that is to be chosen for the constitutive relations which connect the electric force and displacement, the magnetic force and induction, and the current in moving bodies. It leads us to the conclusion that the Newtonian conception of a constant mass needs some revision if the hypothesis is true, and at this point comes into touch with the experiments on the variation of the apparent inertia of a negative electron with its velocity, and in fact is here confirmed.

But although experiment suggested and has so far confirmed the validity of the hypothesis, yet two serious objections are raised against it. The first is that it conflicts with our simplest ideas as to the measurement of space and time, and the second is that it abolishes the æther as a unique and objective medium, the seat of all electrical activity. In a succeeding article an attempt will be made to indicate what position in regard to these two very important points the adoption of the hypothesis requires us to take.

E. CUNNINGHAM.

DR. J. REYNOLDS GREEN, F.R.S.

THE announcement of the death of Dr. Reynolds Green, on June 3, will have been received with unfeigned regret by all his scientific fellow-workers, whether botanists or physiologists. For those who, like myself, have known him throughout his career with a considerable degree of intimacy, regret amounts to a deep sense of personal loss. It is some consolation to me to have this opportunity of writing a few words in appreciation of him who was so closely associated with me first as pupil, then as collaborator, always as friend.

Joseph Reynolds Green came up to Cambridge in 1880 as a scholar of Trinity College, in which

year he also took the B.Sc. degree in the University of London. In 1883 he duly gained a first-class in the Natural Sciences Tripos, part i., a success which was followed in 1884 by a first class in part ii., his subjects being botany and animal physiology. After taking his B.A. degree, there was some uncertainty as to which science he would pursue, but his inclination was to botany, his first scientific contribution being a paper on the glands of the Hypericaceæ, which appeared in the *Journal of the Linnean Society*, 1884. Circumstances, however, led him to devote himself for a time to animal physiology; in 1885 he was appointed senior demonstrator in that subject by the late Sir Michael Foster, a position which he held for two years. Nevertheless, he was engaged, during that time, in botanical research, the results of which were published in two papers read before the Royal Society: the one on the proteid substances in latex (*Proc. Roy. Soc.*, 1886); the other, larger and more important, on the changes in the proteids in the seed which accompany germination (*Phil. Trans.*, 1887), in which he confirmed for the Lupin the discovery by von Gorup-Besanez (1874) of a proteolytic enzyme in the seeds of the Vetch. These papers indicated the direction in which his future work was to lie.

His appointment, in 1887, as professor of botany to the Pharmaceutical Society of Great Britain enabled Green to devote himself entirely to botany, and this he did whole-heartedly. During the twenty years that he held this office, his literary output was voluminous. The first twelve volumes of the *Annals of Botany* (1888-98) contain a number of papers by him on various points in the biochemistry of plants; and he contributed several articles to the first series (1894-8) of *Science Progress*. Perhaps the most important of his investigations during this period were, that on the germination of the seed of the castor-oil plant (*Proc. Roy. Soc.*, 1890), in which he detected the fat-splitting enzyme (lipase), a subject to which he returned years afterwards (*Proc. Roy. Soc.*, 1905); that on the germination of the pollen-grain (*Phil. Trans.*, 1894), proving the presence and activity of amylolytic enzymes both in the grains and in the tissue of the style; and that on the action of light on diastase (*Phil. Trans.*, 1897), where the effect of light on diastase is investigated and it is shown that whereas the red and the blue rays favour the formation of the enzyme, the green, the indigo, the violet, and especially the ultra-violet rays destroy it; and the striking suggestion is made that "vegetable structures have a power of absorbing radiant energy, which is not connected with the presence and activity of chlorophyll."

In addition to these papers and articles, Green found time to write three considerable books: "A Manual of Botany based upon that of the late R. Bentley, 1895-6; "An Introduction to Vegetable Physiology," 1900; and "The Soluble Ferments and Fermentation," 1899. All three went on to a second edition, but the last was the most successful and important of them; a German transla-

tion of it, by Windisch, was published. They are characterised by the lucidity of exposition that he possessed in a high degree.

Owing to failing health, Green resigned his professorship in 1907, and undertook the less onerous duties of the Hartley lectureship on vegetable physiology in the University of Liverpool, still, however, residing at Cambridge. He was commissioned by the delegates of the Clarendon Press, Oxford, to write a continuation, published in 1909, of Sachs's "History of Botany" (1530-1860), to bring the record up to the end of the nineteenth century; a difficult task which he performed with as much success as the circumstances permitted. He became so interested in work of this kind that he planned, and I believe completed, a history of botany in England, which, unfortunately, has not yet been published.

A few personal details in conclusion. Green proceeded M.A. at Cambridge in 1888, D.Sc. in 1894; he became a Fellow of the Linnean Society in 1889, and was elected to the Royal Society in 1895. He was president of Section K (botany) at the Belfast meeting of the British Association in 1902; and in the same year he was elected Fellow of Downing College, Cambridge.

S. H. VINES.

NOTES.

THE June conversazione of the Royal Society will be held on Tuesday next, June 16.

SIR WILLIAM OSLER, F.R.S., Regius Professor of Medicine in the University of Oxford, has been elected a foreign Associate of the French Academy of Medicine.

PROF. A. LACROIX, professor of mineralogy at the Paris Natural History Museum, has been elected permanent secretary of the Paris Academy of Sciences in succession to Prof. Van Tieghem.

DR. R. S. LULL, professor of vertebrate palæontology at Yale, will this summer conduct another western expedition from the Peabody Museum for the purpose of securing skeletons of prehistoric horses.

THE council of the Royal Society of Arts, with the approval of the president, H.R.H. the Duke of Connaught, has awarded the Albert medal for the current year to Chevalier Guglielmo Marconi, "for his services in the development and practical application of wireless telegraphy."

THE Khedive has conferred the third class of the Order of the Medjidieh upon Mr. W. Lawrence Balls on the occasion of his retirement from the service of the Egyptian Government. This is, we believe, the first decoration given for agricultural work since the foundation of the Department of Agriculture in 1910.

PROF. H. HERGESELL, of Strassburg, has been appointed to the direction of the Royal Prussian Aeronautical Observatory at Lindenberg, near Berlin, and desires that communications intended for him or the International Commission for Scientific Aeronautics, of which he is president, should be addressed to the