La Photographie Animée. By Eug. Trutat, Director of the Natural History Museum, Toulouse. Pp. xii + 185. (Paris: Gauthier-Villars, 1899.)

THIS volume, introduced by a preface by M. Marey, the well-known chronophotographer of animals and human beings in motion, for purposes of study, will be found useful to all interested in the subject of animated photography.

The author devotes the opening chapter to a short review of the history of the subject, explaining the application of the phenomenon of persistence of vision in such early instruments as the phénakisticope and zootrope of Plateau and Clerk Maxwell.

He then traces the evolution of the apparatus from the multiple cameras of Muybridge, Anschutz, Londe and his own to the first employment of a fixed plate by M. Marey, and then to the continuous band machines of Marey, Edison, Demeny and others. In this chapter will be found well-illustrated descriptions of most of the French machines which have proved successful.

The third and concluding chapter deals with the various manipulations necessary for obtaining the photographs, and afterwards exhibiting them. The operations of exposure, development, and printing of the positive film are lucidly explained, and then details are given for the management of the film in the lantern.

There is no doubt of the usefulness of the treatise, but its value is somewhat lessened by the descriptions being almost entirely confined to French apparatus, the author giving no signs of being familiar with the successful machines which have been produced outside his own country.

LETTERS TO THE EDITOR.

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The Interferometer.

THE questions raised by Mr. Preston (NATURE, March 23) can only be fully answered by Prof. Michelson himself; but as one of the few who have used the interferometer in observations involving high interference, I should like to make a remark or two. My opportunity was due to the kindness of Prof. Michelson, who some years ago left in my hands a small instrument of his model.

I do not understand in what way the working is supposed to be prejudiced by "diffraction." My experience certainly suggested nothing of the sort, and I do not see why it is to be expected upon theoretical grounds.

The estimation of the "visibility" of the bands, and the deduction of the structure of the spectrum line from the visibility curve, are no doubt rather delicate matters. I have remarked upon a former occasion (*Phil. Mag.*, November 1892) that, strictly speaking, the structure cannot be deduced from the visibility curve without an auxiliary assumption. But in the application to radiation in a magnetic field the assumption of symmetry would appear to be justified.

My observations were made with a modification of the original apparatus, which it may be worth while briefly to describe. In order to increase the retardation it is necessary to move backwards, parallel to itself, one of the perpendicularly reflecting mirrors. Unless the ways upon which the sliding piece travels are extremely true, this involves a troublesome readjustment of the mirror after each change of distance. The difficulty is avoided by the use of a fluid surface as reflector, which after each movement automatically sets itself rigorously horizontal. If mercury be contained in a glass dish, the depth must be considerable, and then the surface is inconveniently mobile. A better plan is to use a thin layer standing on a piece of copper plate carefully amalgamated. A screw movement for raising and lowering the mercury reflector is still desirable, though not absolutely necessary.

RAYLEIGH.

Theory of Functions.

IN his review of our book on "Analytic Functions" (NATURE, February 23), Prof. Burnside makes three specific charges of inaccuracy; we shall show that the inaccuracy is his, not ours.

(1) One charge relates to the difference of two convergent series. There is an elementary and well-known theorem which states that the difference of two convergent series $\overset{\infty}{\underset{1}{\Sigma}} a_n$ and $\overset{\infty}{\underset{1}{\Sigma}} b_n$ is equal to $\overset{\infty}{\underset{1}{\Sigma}}(a_n - b_n)$, no matter whether the convergence of the series be unconditional or conditional. Prof. Burnside has, then, fallen into a very serious error when he says of this very operation of subtraction that "the rearrangement involved is one which cannot be used with conditionally convergent series, as indeed the authors have shown most clearly in an earlier chapter." We must add that there is no "rearrangement," and that we have tried in § 68 to put the reader on his guard against this very error of Prof. Burnside.

(2) A second charge relates to infinite products. In § 109 we consider a certain infinite product $\Pi(1 - \alpha_n)$; in regard to this product, Prof. Burnside complains that we have not explained "what is implied in calling such a product convergent." As a matter of fact we treat an infinite product as an instance of an infinite sequence, and convergence for infinite sequences has been already explained in § 47. He falls into another inaccuracy when he says that "if $\Sigma\alpha_n$ is greater than unity, all that has been proved is that $\Pi(1 - \alpha_n)$ is less than unity and greater than some definitive negative quantity." We have proved much more than this, namely that there is a limit for the numbers $\overset{n}{\underset{1}{\Pi}}(1 - \alpha_n)$, when n tends to infinity (see § 45).

We did not intend to go into the case where the sequence associated with an infinite product converges to zero, because there is as yet no final agreement as to whether the product is or is not to be called convergent in this case. The product in § 109 does not converge to zero. Prof. Burnside does not allude to this point; but we should like, nevertheless, to take this opportunity of saying that we ought to have added a proof that the convergence of $\Sigma\alpha_n$ excludes this special case, instead of assuming that the reader knows the proof, as given, for instance, in Hobson's "Trigonometry."

(3) The third charge relates to our use of the word "infinity" on p. 3. This word "infinity," in the earlier parts of the higher arithemetic, has but one accepted meaning; to quote the words of M. Tannery, "la notion de l'infini dont il ne faut pas faire mystère en mathématiques se réduit à ceci; après chaque nombre entier il y en a un autre." We have used the word "infinity" in this, its legitimate sense. Failure to perceive the "variable" character of infinity has led to many misconceptions in the past. We cannot understand Prof. Burnside's objection except on the supposition that he has, for the moment, confused this "variable" infinity with the discredited "constant" infinity.

On the score of accuracy we wish to point out that we gave two chapters to elliptic functions, not three, as the reviewer states; and that Log x is not *defined* (the italics are the reviewer's) by means of a piece of string and a cone. We *define* the logarithm by means of an equiangular spiral, in a way somewhat similar to that used in Clifford's "Common Sense of the Exact Sciences," and we indicate, incidentally, a mechanical construction of the curve.

It is always an ungracious task to reply to a review, especially when it is in general appreciative, and written by a mathematician of acknowledged standing; but in the circumstances we felt that we had no alternative. We believe that Prof. Burnside will be the first to recognise that his specific criticisms are based on misconceptions.

J. HARKNESS.
F. MORLEY.

Philadelphia, March 14.

THE criticism on the passage quoted from p. 3 of the book by Profs. Harkness and Morley (NATURE, February 23, p. 347) turns on the fact that, in dealing with number divorced from measurement, the authors have used the phrase "an infinity of objects" without an explicit statement of its meaning. I am not sure that I understand the passage in their letter which refers to this point; but it seems to me to imply that the distinction between "finite" and "infinite" is one which does not require definition. This is not the only accepted view. It is not, for