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, Anschütz, 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 $\sum_{n=1}^{\infty} a_n$ and $\sum_{n=1}^{\infty} b_n$

is equal to $\sum_{n=1}^{\infty} (a_n - b_n)$, 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 $\prod (1 - a_n)$; 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 $\sum a_n$ is greater than unity, all that has been proved is that $\prod (1 - a_n)$ 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 $\prod_{n=1}^n (1 - a_n)$, 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 $\sum a_n$ 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 arithmetic, 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.

Philadelphia, March 14.

J. HARKNESS,
F. MORLEY.

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

instance, the view taken in Herr Dedekind's book, "Was sind und was sollen die Zahlen." As regards the opening sentences of Chapter xv., the authors have apparently misunderstood the point of my objection. With the usually received definition of convergence of an infinite product, $\prod(1 - a_n)$, if convergent, is different from zero. So far as the passage quoted goes, $\prod(1 - a_n)$ might be zero; and it is therefore not shown to be convergent, if the usual definition of convergence be assumed. As to the passage quoted from p. 232, I must express to the authors my regret for having overlooked the fact that the particular rearrangement, there made use of, has been fully justified in Chapter viii. Whether $\log x$ is or is not, at the beginning of Chapter iv., defined by means of a string and a cone, will be obvious to any one who will read the whole passage (p. 46, line 16, to p. 47, line 9) leading up to the definition.

W. BURNSIDE.

The Tetravalency of Oxygen.

THE discovery of the new and easy method of preparing the pure oxygen phosphide OP_4 (or Le Verrier's phosphorus suboxide) by the acidification of the solution of phosphorus in "alcoholic potash," should draw attention to the remarkable fact that the oxygen atom can be therein symmetrically represented *only* as tetravalent and not divalent, and that it is probably an inorganic, non-carbon, closed-ring molecule:—

Without discussing the valency or "validity," as I have termed it, of phosphorus, my point is that oxygen is here tetravalent. The silver analogue (?) OAg_4 , or

is known. And doubtless others. There ought, therefore, to be no further hesitation in definitely accepting the oxygen atom in the CO molecule as tetravalent, and in proceeding with investigations which shall elucidate, in terms of physical chemistry, the remarkable fact of the "alternating valency" of the non-metallic atoms. Oxygen has as clearly two valence values as phosphorus and nitrogen.

The change in the valency of oxygen seems to be connected with "polymerisation." In 1888 I wrote in the *Philosophical Magazine*, vol. xxv. p. 232:—

"The stimulus—if I may so term it—to polymerisation in these cases seems to be due to the development, in ways that current research are seeking to explain, of the higher valency, or, as it may be preferably termed, the validity of oxygen, or the tervalidity, for instance, of chlorine in "hydrochloric acid solution" used as "a polymeriser," $H-Cl = \dots$. It is significant that there are few cases of polymerisation where oxygen is not concerned. . . . Probably more ring formulæ in inorganic bodies will come to light, and they will affect the maypole hypothesis—as it may be called—of the domination of the central polad atom."

This prediction has, I believe, since been verified, and the benzene-ring style of Kekulé is no longer viewed as exclusively characteristic of carbon. The remarkable volatile osmium compound is probably

where oxygen is tetravalent as it is in Friedel's $(H_3C)_2OCH$.

I would, therefore, venture to suggest that possibly my two papers in the *Phil. Mag.* may still be worth half an hour's attention from gentlemen who would find some curious compounds therein discussed—compounds whose physical chemistry may perhaps now be found worth examination in the light of the newer methods, and also in the light of the developing investigations concerning the ether and the intra-molecular equipoise of the atoms constituting the chemical molecule.

Banbury, March 25.

J. F. HEYES.

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THE PROGRESS OF WIRELESS TELEGRAPHY.

WE recently referred to the first messages transmitted over the English Channel between the South Foreland and Boulogne by Marconi's system of wireless telegraphy. During last week the English station was visited by Prof. Fleming, who has written his experiences to the *Times*.

An idea of the present state of the problem can be conveniently gathered from this communication. First as to the certainty of the results obtained, Prof. Fleming states:—

"Throughout the period of my visit messages, signals, congratulations, and jokes were freely exchanged between the operators sitting on either side of the Channel and automatically printed down in telegraphic code signals on the ordinary paper slip at the rate of twelve to eighteen words a minute. Not once was there the slightest difficulty or delay in obtaining an instant reply to a signal sent. No familiarity with the subject removes the feeling of vague wonder with which one sees a telegraphic instrument merely connected with a length of 150 feet of copper wire run up the side of a flagstaff begin to draw its message out of space, and print down in dot and dash on the paper tape the intelligence ferried across thirty miles of water by the mysterious ether."

Signor Marconi by much work has arrived at great simplicity.

"With the exception of the flagstaff and 150 feet of vertical wire at each end, he can place on a small kitchen table the appliances, costing not more than 100*l.* in all, for communicating across thirty or even one hundred miles of channel. . . . The distance to which effective signalling extends varies as the square of the height of the rod. A wire 20 feet high carries the effective signal one mile, 40 feet high four miles, 80 feet sixteen miles, and so on."

We are very glad to print the following extract from Prof. Fleming's letter:—

"The general public are not much concerned with questions of priority or with the claims or suggestions of rival experimentalists, but they are interested in ascertaining the serious possibilities of that which has been actually achieved. Signor Marconi has never hesitated to acknowledge that he has built upon the foundations laid by others, but a vast gulf separates laboratory experiments, however ingenious, from practical large scale demonstrations conducted with all that regularity and freedom from failure which is the absolute condition of their public utility."

"I cannot help thinking that the time has arrived for a little more generous appreciation by his scientific contemporaries of the fact that Signor Marconi has by minute attention to detail, and by the important addition of the long vertical air wire, translated one method of space telegraphy out of the region of uncertain delicate laboratory experiments and placed it on the same footing as regards certainty of action and ease of manipulation, so far as present results show, as any of the other methods of electric communication employing a continuous wire between the two places. This is no small achievement."

There can be no doubt that what this system will do in the future for those who live on coast-lines or go down to the sea in ships is destined to be of great importance. Already the usefulness of light-ships is increased tenfold. The fleet manœuvres of the future may be flagless. Sea routes by means of an international concert pitch may be turned into exchanges—but there is no end of the possibilities thus opened out by this new development of the results of the study of the "useless."

Prof. Fleming, in the course of his long and interesting letter, points out the importance of some new Board of Trade regulations "for the use of the ether"—the term is distinctly good—lest vagrant electric waves should interfere with the official ones. Even the ether, then, may yet be dismissed with costs.