has no cause for complaint. He ought to know that interest is a function of two variables.

(3) In reply to Mr. Lock's request for other slight inaccuracies I might ask, without leaving the subject of interest, what under the sun "inverse interest" is: but though inaccuracies of language are not desirable in a school-book, I prefer to draw his attention to more important matters. Every arithmetician knows that the practical questions which come under such headings as Simple and Compound Interest, Exchange, Discount, Stocks, &c., are not questions of a different kind *arithmetically*, being all so-called "proportion" questions, and that no more important fact can be taught to the student of arithmetic regarding them. Now here is Mr. Lock's treatment. Simple and compound proportion questions are put under the headings "Problems" and "Complex Problems"—names, by the way, quite illogically chosen and not consistently adhered to. Exchange is tacked on to Complex Problems by the words, "examples in Exchange can be worked by the above method"—indeed, these words and a worked example constitute the sum total of information given in the book on this subject. No one could object to the union here indicated, but surely the same is equally true of several of the other subjects. After Exchange comes a section headed "On Problems concerning Time: I., Time and Distance." These are not problems in the sense previously specified, but belong to the genus of examination questions which concern bodies moving in the same path with different speeds. A like remark applies to the section which follows, headed "II., Time and Work." Late in the day, after Interest, Discount, &c., there appears a chapter "On the Use of the Term *Per Cent.*" So far as it is on anything (for it consists of seven or eight lines of introduction, three worked and forty-five unworked examples), it is on the calculation of rates of gain and loss. Now all this, one is bound to affirm, is strangely illogical, and tends to give a most erroneous conception of arithmetic as applied to practical affairs. I used the expression "slight inaccuracies of thought" in referring to such a mode of treatment, because it was impossible to be more severe without going into detail, and because it seemed imperative to say something against a practice, which our examination system fosters, of forming text-books by collecting all the kinds of exercises met with in examination papers and separating them into carelessly ticketed groups prefaced by a definition or two. The purely arithmetical, and larger, part of Mr. Lock's book is not of this character, and is, especially as regards the definitions, very carefully prepared; he would considerably enhance the value of the whole by wisely modifying the rest in the second edition. THE REVIEWER

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I THANK you for your courtesy in permitting me to see the reply of your reviewer to my letter which appeared in NATURE of June 3 (p. 100). That my letter was written under very exceptional circumstances will be clear to any one who will take the trouble of comparing your reviewer's defence of his criticism with the book itself. I will, however, with your permission, make one or two comments on his reply.

(1) That a wrong inference was suggested by the words of the reviewer is, no doubt, of little consequence, except that it afforded me a ground for an appeal to you for further information.

(2) Your reviewer did not quote in his review, as he now does, my definition of rate of interest; he asked whether rate of interest is totally independent of the time, implying that I stated that it was so, and ignoring the fact that the manner in which time is involved in Interest (not in *rate* of interest, on which point your reviewer seems a little confused) is gradually explained in the next few pages. Might I ask your reviewer whether in Compound Interest the Interest varies simply as the Time?

(3) In his third paragraph your reviewer gives his answer to my request that he should quote *verbatim* the other instances on which he based his unfavourable criticism. There is little or nothing here for me to answer, except that I am compelled, in justice to myself, to point out the reviewer's own mistakes. (i.) He suggests that Exchange ought logically to be placed between Compound Interest and Discount. It would seem necessary to remind him that in questions on Exchange there is no reference to *time*, and that it is the peculiar manner in which time is involved, which distinguishes Interest and Discount from other Problems involving money. (ii.) He states that questions which I have called "Problems concerning Time" are improperly so-called. It will be clear to any one who reads the chapter on "Problems" that a Problem is a question on Variation; so that problems concerning time are exactly what their name indicates. But (even supposing your reviewer were right on these two points) in charging me with being strangely illogical as regards the order of my chapters, he must have overlooked the fact that in the preface I expressly state that "novelty in arrangement has been avoided as much as possible," but that "the order in which his chapters are taken may be varied at the discretion of the teacher." For my part I think that the established order of subjects is not to be lightly upset, certainly not without more sound and weighty reasons than those adduced by your reviewer.

But besides this your reviewer draws an unfair inference, due I suppose, to mere carelessness. The words "Inverse Interest" appear only as the heading of pp. 187, 188, and are obviously an abbreviation for convenience of printing of the words on p. 186, "Inverse questions on Interest." As far, however, as I can understand the general effect of your reviewer's explanation, his objection to my book seems to be this—that it fails to bring into sufficient prominence the fact that the Practical Applications of Arithmetic (which, in accordance with established custom, I have collected under the heads of Exchange, Problems concerning Time, Interest, Proportional Part, &c.), really present the same idea under different circumstances, expressed in different language. I entirely agree with him as to the importance of this fact, and endeavoured, as far as the scope and object of my book would allow, to give it due prominence. For example, for this reason, it seemed unnecessary in Exchange and in the chapter on Profit and Loss to give more than a few words of explanation in addition to the examples worked out. JOHN B. LOCK

Gonville and Caius College, June 14

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## *PASTEUR'S RESEARCHES*

IN the current number of the Royal Agricultural Society's *Journal* (vol. xxi. part 1) is a full and able account of the work of the great French experimenter from an agricultural and veterinary point of view, by Dr. George Fleming. The development of Pasteur's genius is traced from his early chemical researches on dextro- and lævo-tartrates to fermentations in milk and in malt. The combination of microscopic with chemical modes of investigation led him to the definite determination of the part played by living organisms in acetic, butyric, and alcoholic fermentations. In these inquiries his own labours were almost entirely original, but it must not be forgotten that a few microscopists in England and many in Germany were working on the same lines, and contributed to the establishment of the modern doctrine that fermentation and putrefaction are both processes dependent on the presence and growth of minute parasitic plants. Pasteur's experimental investigations led him in two directions—in one to the establishment of the now accepted theory of biogenesis: that every living thing is the product of a living parent; in the other to the practical application of the facts ascertained to the manufacture of vinegar and the process of brewing.

Ingenuity in devising experiments and patience in carrying them to a successful issue belong more or less to every successful investigator, but the union in addition of clear theoretical conceptions with skill in the useful application of results is characteristic of Pasteur as it was of Faraday and a few other of the highest intellects.

His investigation into the cause of *pébrine*, or silkworm disease, was undertaken against his will, in deference to the urgency of the eminent chemist Dumas. Pasteur wished to return to his original department of chemistry, and it is remarkable that having once left it he has been drawn further and further into biological researches, while Dumas, who began with valuable work on the development of the ovum, was diverted to chemistry and there made his enduring reputation. Perhaps no instance more remarkable than Pasteur's work on the *pébrine* can

be cited of the value of science in a commercial and national point of view. A great industry was all but extinguished, and the impending catastrophe became a question for parliaments and statesmen. A scientific investigator was appealed to; he set to work in 1865, and after four years' continued application he had solved the problem, and delivered his country from the incubus on her industry. It has been well said that Jenner by his discovery of vaccination saved more lives than Napoleon ever destroyed; so Pasteur saved France in 1869 from a far greater tribute than the Prussian conqueror imposed on her in the following year.

This brilliant success, which could be neither concealed nor depreciated, led to the successful experimenter (barely recovered from an attack of paralysis, which ended his last laborious research) being called on to devise a means of checking the ravages of splenic fever (*anthrax*) among horses and cattle. Dr. Fleming gives an interest-account of this terrible scourge, and explains the methods adopted by Pasteur to investigate it. He discovered a method by which its virus may be "attenuated," and thus used for protective inoculation in the same way as vaccination protects against small-pox. This method, though often successful, has not proved uniformly so, and more must be done before its general efficiency is established. Dr. Fleming refers to the results in Algeria, in Prussia, and in Hungary, and to these he might have added those obtained by Dr. Roy in Buenos Ayres.

On the other hand, the treatment by inoculation of a contagious disease among poultry (ill-named *choléra des poules*), a method which was also discovered by Pasteur, appears to be uniformly successful.

The last investigation of the great French experimenter is that upon hydrophobia, which the world is still anxiously watching. This also is described by Dr. Fleming. We have kept our readers informed of the progress of this vast practical trial of a scientific mode of treatment on the victims of a hopeless malady. Every month brings fresh accumulation of evidence on the subject, and we hope soon to have the report of the Commission sent from this country to ascertain M. Pasteur's precise methods and their results. If he should be honoured to be an instrument in the hands of Providence for averting one of the most shocking and terrible diseases to which mankind is subject, the name of Pasteur will live as one of the greatest benefactors of our race. But in any case his work already achieved and its results established form an ample title to the admiration and the reverence of all who can estimate genius or value its conscientious devotion to the service of mankind.

## *LYCOPODS*[1]

THE attention of the readers of NATURE has already been directed towards recent work on the Lycopodiaceæ by the publication of a *résumé* of the researches of Dr. M. Treub, Director of the Botanic Gardens of Buitenzorg, Java. He is the first botanist who has succeeded in giving a connected account of the prothallus, sexual organs, and development of the embryo of any species of Lycopodium, and now his first paper, which dealt with *L. cernuum*, has been rapidly followed by a still more complete and successful study of *L. Phlegmaria*, L. It might be expected that the second paper would be in great measure a repetition of the first, but this is not so; and it may be regarded as one of the most interesting results of this very suggestive and luminous investigation that it brings into prominence the greatness of the possible differences in development of two plants which have hitherto passed, and will continue to pass, under the same generic name. The observations detailed in this second paper are so important in their bearings on our views regarding other allied forms that it is desirable that at least the more striking points should be recorded here.

Attempts to germinate the spores of *Lycopodium Phlegmaria* were at first unsuccessful, but after more than a year young plants were found on one of the tree trunks on which spores had been sown, and subsequently similar young seedlings were found in large numbers in the forest. The germination of the spores appears to be slow, and Dr. Treub is of opinion that the culture of prothalli from spores will never be easy, a view which is supported by the fact that the oophore is capable of various modes of asexual multiplication; indeed it appears that the majority of the prothalli found owed their origin to this source, and not directly to the germination of spores. An autonomous existence of the prothallus, independent of the formation of sexual organs, has been demonstrated by Goebel in the case of *Gymnogramme leptophylla*, and a similar, but still more pronounced condition is found in this Lycopod. The prothallus grows in the dead external layers of the bark of trees; it is as a rule devoid of chlorophyll, and consists of cylindrical branches, covered with absorbing hairs. These cylindrical organs branch monopodially, the branches being usually formed in acropetal order; they have a terminal growth with two initial cells, each of which gives rise to half of the cylindrical organ. It is worthy of note that there is a great similarity between the structure of this apical meristem and that of the stem of the sporophore. In the fully-differentiated parts of the prothallus a peripheral tissue one layer of cells in thickness may be distinguished; this gives rise to the rhizoids. The mass of tissue inclosed by this superficial layer, though it shows some slight varieties according to the mode of development of the branch, never attains any high state of differentiation.

The lateral branches, which are not very numerous, take their origin from the peripheral layer, several cells taking part in the formation of each. The growth of these branches may be long-continued, and it is not arrested on the formation of an embryo on another branch. By progressive rotting of the older parts branches may be separated from one another, and this constitutes the simplest mode of increase in number of individuals But, besides this, two other modes of vegetative propagation are known—(*a*) by ordinary propagating organs: these are small ovoid multicellular bodies, which originate from single superficial cells, and are set free by rupture of their pedicels; (*b*) by thick-walled organs, smaller than the above, which only appear on weakly prothalli: these may undergo a period of rest. Among the vascular Cryptogams the only organs hitherto known of a similar nature to these are those described by Cramer; Dr. Treub is, however, of opinion that a truer comparison may be be made to the gemmæ of the Hepaticæ, and especially of *Blasia*, while in many of their general characters, which may be recognised on inspection of the twenty beautiful plates, the prothalli of *L. Phlegmaria* show points in common with the oophore of certain of the Muscineæ.

The sexual organs of *L. Phlegmaria* are produced on the upper surface of the prothallus, and are always accompanied by paraphyses, structures which are absent in other Vascular Cryptogams, but frequently present in the Muscineæ. The position of the antheridia is variable; sometimes they are scattered singly on the vegetative branches, sometimes they are associated in groups, and are then often borne on the considerably thickened extremities of branches. Their development is similar to those of *L. cernuum*, while the antherozoids have two cilia, and resemble those of *Selaginella*. The archegonia have a more definite position, and they appear subsequently to the antheridia, on those thickened extremities of branches which have already borne antheridia: they project from the surface of the prothallus, and have three to five canal cells, while the highest number hitherto recognised

[1] "Études sur les Lycopodiacées," par M. Treub. Part II. *Annales du Jardin Botanique de Buitenzorg*, vol. v. 2ième partie.