tubes, which on closer examination proved to occur always in pairs and to project from the openings of the burrows formed by the shipworms. Plainly the tubes had been formed around the siphons of the Teredo. They were of varying length, depending presumably on the thickness of the deposits, the longest being some two-fifths of an inch. The general appearance of the wood is shown in Fig. 1.

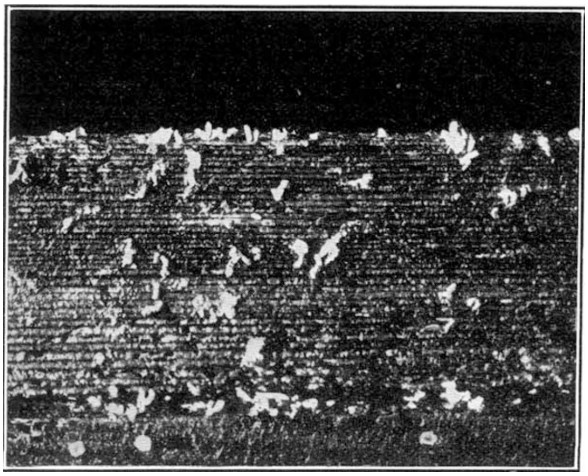

Photo.] [*A. J. Smith.*

FIG. 1.—Portion of wood badly infected with *Teredo norvegica*. The white objects are the protruding calcarerous siphonal tubes which appeared after the fæcal deposits had been washed off. In several cases the paired tubes can plainly be distinguished.

Normally the external openings of the tubes of Teredo are very difficult to distinguish, consisting of a pair of minute openings ringed with calcareous matter out of which project the siphons and within which these are immediately withdrawn on stimulation. The presence of fæcal deposits, which had accumulated to an abnormal degree owing to the lack of water currents to remove them, would tend to obstruct the passage of the siphons and so endanger the life of the animals within. The response of the animals to this abnormal and dangerous state of affairs was to lay down calcareous tubes around the siphons, which by this means were able to maintain free contact with the water.

Dr. W. T. Calman has directed my attention to the fact that the giant shipworm, *Kuphus arenarius*, which lives vertically embedded in the mud of mangrove swamps in the Pacific, normally has the siphons encased in this manner, a fact which was known to Rumphius so far back as 1741, and was figured by him (as *Solen arenarius*) in his "D'Amboinsche Rariteitkamer." This animal lives normally under conditions in which the Teredo in the Plymouth tank lived for some four months, namely, in constant danger of being suffocated by accumulating deposits—in one case of mud, in the other of fæcal matter.

This accidental production of calcareous siphonal tubes in Teredo is therefore of some considerable interest, since it provides a very striking case of an immediate and highly successful response by an animal to changed environmental conditions; a response, moreover, which has taken the form of a permanent adaptation in related animals living under conditions very similar to those accidentally produced.

C. M. YONGE.

Marine Biological Laboratory,
Citadel Hill, Plymouth,
November 15.

Fluctuations in the Abundance of a Species considered Mathematically.

WITH regard to Prof. Volterra's interesting article, "Fluctuations in the Abundance of a Species considered Mathematically," in NATURE of October 16, page 558, I may be permitted to point to certain prior publications on the subject, of which Prof. Volterra seems to be unaware. The general theory as well as a number of special cases have been set forth in "Elements of Physical Biology" (published by Williams and Wilkins, Baltimore, 1925), in which work a considerable number of references to the journal literature are given. Among other things Prof. Volterra's diagram "Fig. 2" will be found on page 90 of the book cited; the expression for the period of isochronous small oscillations in the case of two species is also found on the same page. Prof. Volterra refers to certain applications of his analysis to problems of sea fisheries, to a passage in Darwin's "Origin of Species," to extinction of species, to pathogenic germs, and to parasitology. An application to sea fisheries is found in the book cited on page 95; to a passage in Herbert Spencer on page 61; to the extinction of species on pages 94, 95; to pathogenic germs on pages 77, 79, 147 *et seq.*; to parasitology on page 83.

The effect of introducing a third species into a system of two species is discussed on page 94; the effect on equilibrium of changing various factors is treated in Chap. xxii., "Displacement of Equilibrium," and, in particular, the effect on equilibrium between food and feeding species is analysed on page 289. The distinction between oscillatory and aperiodic systems, and its relation to certain quadratic forms, is referred to on pages 146, 148, and 159.

It would be gratifying if Prof. Volterra's publication should direct attention to a field and method of inquiry which apparently has hitherto passed almost unnoticed. ALFRED J. LOTKA.

Metropolitan Life Insurance Company,
New York City, October 29.

In the above letter from Dr. Lotka, which is in accordance with our preceding correspondence, following upon the publication of my article in NATURE, he justly observes that he had obtained the differential equations in the case of two species, one of which feeds upon the other, that he had given, as well as myself, the same diagram of the integral, and also the period in the case of small fluctuations. In this I recognize his priority, and am sorry not to have known his work, and therefore not to have been able to mention it.

I did not even know other publications of the same kind by other authors, for example, the work of Sir Ronald Ross on malaria, which precedes the writings of Dr. Lotka, who has, however, found so many new and important results.

The other observations mentioned in the above letter refer to points which I have not treated; but as to the sea-fisheries, while I refer to the laws (which I believe to have been the first to formulate) and principally to the third law, which gives an easy way of calculating the maximum output of fisheries; he, on his part, considers the case of the addition of a third species, which seems to me a different problem.

I also think that the quotation of Darwin's acute intuition referring to my third law, and the quotation from Spencer, which touches only the principle of the existence of fluctuations, are essentially different.

I think that the study of the general case of the convivence of n different species, subject to the hypothesis which permits me to distinguish the case of conservative association from that of dissipative association, and to obtain integrals and laws of fluctuation which form the essential and the greatest part of my memory, is absolutely new. To conclude, I recognize the existence of some common points in Dr. Lotka's work and my own, in which he has priority, but my work and his diverge in all the rest.

Working independently the one from the other, we have found some common results, and this confirms the exactitude and the interest in the position of the problem. I agree with him in his conclusions that these studies and these methods of research deserve to receive greater attention from scholars, and should give rise to important applications.

VITO VOLTERRA.

Via in Lucina, 17,
Rome, November 27.

The Polishing of Surfaces.

DR. HAMPTON of West Bromwich has directed my attention to Mr. J. M. Macaulay's letter on "The Polishing of Surfaces" in NATURE of September 4, p. 339.

In conversation with Sir Herbert Jackson, Mr. Twyman, and others, I have once or twice had occasion to point out that the energy available in practice for liquefying the surface layer of glass is many hundreds of times what is theoretically necessary. It is known that in polishing glass, the amount of glass removed corresponds to a solid layer of the order of ten wavelengths in thickness. The total quantity of heat necessary to liquefy or even vaporise a layer of this thickness is not great in comparison with the energy expended in the actual process of polishing. The figure given by Sir George Beilby of four pounds per square inch as a pressure sufficient to start flow has no significance. In the process of polishing glass on a commercial scale, pressures very much less are the rule. In the polishing of plate glass, for example, they are of the order of half a pound per square inch; in the spectacle industry they are commonly of the same order; in the optical industry the specific pressures used become greater and greater as the surface becomes smaller.

There is every reason to believe that glass will polish with the most insignificant pressures that can be attained in practice; but of course the lower the pressure the longer the time required. The coefficient of friction which Mr. Macaulay takes as 0·3 is a long way out. In polishing with felt and similar materials the coefficient ranges from about 0·85 to 1·1, and is usually taken by designers as from 0·95 to 1·0. In polishing with pitch the apparent coefficient of friction fluctuates very widely, because the film of moisture between the pitch and the glass renders the interfacial pressure itself either very great or very small according as the quantity of moisture becomes less or greater. However, whatever assumptions may be made about the pressures and coefficients of friction, it may be taken that in the polishing of large surfaces of glass about ½ kilowatt hour is expended over a square foot of surface polished. In the optical industry, where surfaces are smaller and preliminary grinding is better, an expenditure of energy of about half this amount suffices. From this it may be calculated, I think, that the efficiency of the glass polishing operation is (on the assumption that the energy is required for liquefying a thin surface layer) not more than about one-half of one per cent.

I am not a great believer in the surface flow theory. In various papers to the Optical Society of England and elsewhere I have given reasons for believing that whatever part surface tension effects may play, the process of polishing is at bottom primarily one of abrasion.

F. W. PRESTON.

222 E. Clay St.,
Butler, Pa., October 26.

THE practical information which Mr. Preston gives is of considerable interest and value. His observations appear, on the whole, to support the view expressed in my previous letter, that glass surfaces are actually fused in the process of polishing.

One wonders whether the conception may not approximate in some degree to Mr. Preston's belief that "the process of polishing is at bottom primarily one of abrasion." One can imagine the surface molecules in the liquid state being, so to speak, picked off by the rouge particles, thus giving, so far as the resulting debris would indicate, an abrasion effect.

JAMES M. MACAULAY.

Natural Philosophy Department,
The Royal Technical College,
Glasgow, C.1,
November 24.

Origin of Yolk in the Eggs of *Luciola gorhami*.

THE eggs of the coleopteran fire-fly, *Luciola gorhami*, found in the plains of the Punjab, have proved to be objects of rare value for the study of the problem of the origin of yolk. There are two kinds of yolk in these eggs: albuminous and fatty. The former arises directly from nucleolar extrusions of a remarkable type. At a very early stage in the growth period the nucleolus shows signs of intense activity and buds off numerous round bodies of different sizes, which are thrown out in the cytoplasm. The nucleolus continues to throw out these extrusions until the very last stage in oogenesis. At the beginning of this process the extrusions migrate towards the periphery of the egg-cytoplasm, where they grow in size, perhaps at the expense of food materials derived from the follicle cells.

The whole process is reminiscent of what has been described in the cockroach and certain Hymenoptera by Hogben (*Proc. Roy. Soc.*, 1920, A, and 1920, B) and in Saccocirrus by Gatenby (*Quart. Jour. Micr. Sci.*, 1922). Nucleolar extrusions preceding the appearance of albuminous yolk have, of course, been described in some other eggs, *e.g.* Lithobius (King, *Scient. Proc. Roy. Dub. Soc.*, 1924, and Nath, *Proc. Camb. Phil. Soc. Biol. Sci.*, 1924) and Buthus and Euscorpius (Nath, *Proc. Roy. Soc.*, 1925), etc., but in Luciola it is noteworthy that the process of nucleolar budding lasts practically throughout oogenesis, and the process of the growth of nucleolar extrusions into the albuminous yolk spheres can be studied with diagrammatic clearness.

The origin of the fatty yolk from the Golgi elements is no less clear. The latter exist in the form of rings and crescents. The rings might also be appropriately described as vacuoles (cf. 'vacuome' theory of Parat), with a sharp chromophilic rim and a central chromophobic substance (idiosome). When the solid osmicated fat spheres are decolorised in turpentine they also show a chromophilic rim and a central chromophobic substance, exactly like the Golgi rings. On further decolorisation they appear like clear vacuoles.

We emphasise this morphological similarity between the Golgi rings and the fatty yolk spheres. It seems clear that the fat spheres arise directly from the Golgi rings, in the interior of which free fat, not