showing the relations of the observed azimuths of drift to the sidereal time is very remarkable. These tables indicate a motion of the solar system in a direction and with a velocity in good accordance with the independent results obtained by Dr. Strömberg and others.

LUDWIK SILBERSTEIN.

Washington, D.C., April 30.

Phylogeny as an Independent Science.

UNDER the title "Phylogeny as an Independent Science," E. W. M. gives in *NATURE* of December 20, 1924, a critical review of my "Geschichte der Organismen." Some points in this review cannot be passed without comment.

Almost at the beginning, the reviewer sums up the tenor of the book in the words: the author "treats of the evolutionary history of every phylum both in the animal and in the vegetable kingdom!" He has thus neither learned from the title, preface, and contents of the book, nor communicated to the reader of the review, the fact that "Geschichte"—that is, history—is not identical with "evolutionary" history. In my mind, the history of organisms tends to follow three courses, namely: (1) a simple account according to the chronology of the fossils and to the recent changes of fauna and flora; (2) showing the superiority of differentiation and centralisation (integration, Herbert Spencer), in contrast with the inferiority of differentiation alone; (3) phylogenetical connexion, the latter being the hypothetical part of the history. Thus the reviewer apparently did not understand that this mode of treatment leads, among other things, to securing, for his part, in good cases phylogeny more dependent than hitherto on empirical chronology, and to gain perhaps here or there an adaptation to each other of isolated phylogenetical hypotheses.

Furthermore, the reviewer enumerates two themes, of which he laments the absence of any attempt at explanation in the book. Well, the law of recapitulation is by no means explained in detail, but "the value and the limitations of the evidence from fossils" is discussed in a distinct manner in what to me is one essential respect, namely, that, on the average, the number of fossils diminishes at an accelerated rate when we go down to the older geological formations, and that, where it completely vanishes in pre-Cambrian times, we are still in a period approximately recent relative to the periods of origin of life, as well as of forming the chief branches of the genealogical tree.

Concerning the phylogeny of the Turbellaria, the reviewer says: "Not a word is mentioned of Lang's brilliant theory of their derivation from Ctenophora . . ." etc. This theory, however, is mentioned in the book, pages 305-6, in the chapter on the Ctenophora, in some twenty lines, where I say that it seems to me "somewhat too distinct." Moreover, the more recent view of Wilhelmi, perhaps not yet known to the reviewer, is recorded and adopted. The same theory is briefly mentioned again on page 659, in order to compare the hypothetical place of the creeping ctenophores with that of the dipnoid fishes and that of the Bennettit plants.

The reviewer remarks, "how ill-founded is Franz's comparison of the cystid and the echinoid because both have a spherical shape." The book, however, says that probably the echinoids were derived "from cystids with five short arms." Therefore, according to my meaning, these cystids, besides the ancestors of those known as fossils, were not spherical, as also

in the greatest number of fossils the arms are only broken off.

With this, to be sure, I do not wish to deny that the reviewer's objection, that the sucker of attachment of asterid larvæ is identical with the attachment of the stalk of the crinoids, can be right; and here again, if I may add in short a phylogenetical consideration, I should not believe Bury's and the reviewer's phylogeny of the Echinodermata to be persuading, nor should I mean that F. Mueller's and the reviewer's interpretation of the Nauplius (ancestral) is more suggestive than mine (modified metatrochophora). As to the Echinodermata, I do not understand how it can be overlooked that by far the greatest probability is in favour of the origin of the radiometry of this great phylum in fixation, since fixation has effected all other radiometry, or, at any rate, nearly all others, in the animal and vegetable kingdom, and since Balanoglossus, the creeping allied form to the echinoderms, is not radial. There is a striking parallelism: annelids→brachiopods, enteropneusts→echinoderms, if we assume that the non-radiometry of the brachiopods has caused lower vitality or victoriousness and the less modulation of these animals also fixed, compared with the echinoderms. To present such ideas, connected with each other, though in many cases partially hypothetical, and surely to be in future again adapted to the considerations presented from other points of view, is to be one of the tasks of the "history of organisms."

As to the Nauplius, the identification, though only approximate, of a living marine larva with a Cambrian fossil would be strongly against the tendencies of the book, and to the intentions of the author. Moreover, I see no essential resemblance between the four-segmented Nauplius and the multisegmented Cambrian Marrella, without regard to the question whether this interesting form, perhaps nearly the missing-link between brachiopods and trilobites, could have been mentioned in the book.

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I AM sorry if I misunderstood the objects aimed at by Prof. Franz's book, "Die Geschichte der Organismen." I admit that the German word "Geschichte" is capable of being understood in two senses, namely, (1) a general descriptive account, and (2) evolutionary history. Since Prof. Franz holds the chair of phylogeny in his university, I understood "Geschichte" in the latter sense. All I can say is that if he intended it to be understood in the former sense, a task of such gigantic dimensions could not be attempted in a work of the size of his book. I gather that he intended to bring phylogenetical hypotheses into relation with fossil discoveries; in this aim I am in entire sympathy with him, however little success may have attended his efforts.

The object of my review was thus to point out that phylogenetical theory must remain a matter of personal taste, until the foundations on which it should rest are discussed and defined. These foundations are, as it seems to me, three, namely:

(1) When a number of closely allied species or genera are compared together, the more specialised amongst them have been evolved from the more generalised.

(2) When a close succession of allied fossil forms has been discovered in the same locality, becoming gradually changed as we pass from older to younger beds, this indicates a true evolutionary series.

(3) When the same larval form is found in the life