

that much to which the author originally pinned his faith is no longer tenable. Spiral nebulae are proving to be something bigger than the author at first imagined them to be, and both mathematicians and observers feel doubts as to whether their particular branches of astronomical science will altogether confirm the author's predictions as to the course of events. At the same time the author has always regarded his theory as one to be continually modified in the light of new facts, so that the question of present interest is whether the theory can be fitted to new knowledge without entirely losing its original character.

The book will be welcomed as providing a complete and authoritative account of a hypothesis which must be considered along with others in our efforts to unravel the history of our system.

J. H. J.

The Earliest Voyages Round the World, 1519-1617. Edited by P. F. Alexander. Pp. xxiii + 216. (Cambridge: At the University Press, 1916.) Price 3s. net.

IN the century, 1519-1617, covered by this travel-book there were six voyages round the world—one Spanish, led by a Portuguese, Magellan; two English, led by Drake and Cavendish; and three Dutch, led by Van Noort, Speilbergen, and Le Maire and Schouten. Mr. Alexander includes in this volume Pigafetta's account of the Magellan expedition; Francis Pretty's narratives of Drake's piratical voyage, and of Cavendish's first voyage; and an account of Le Maire and Schouten's discovery of the route round Cape Horn. There are numerous illustrations, including a sixteenth-century map of Drake's voyage corrected by the great navigator; a dozen pages of useful notes; a brief introduction to the narratives; and a table of important dates in the history of discovery. As a contemporary source book, which maintains the atmosphere of the great days of the early voyages, this compilation will prove extremely useful and stimulating.

Large-Scale Map of the French Battle-Front. (London: G. W. Bacon and Co., Ltd.) Paper, 1s. net; cloth, 1s. 6d. net.

THIS map, on a scale of four miles to an inch, shows the battle-front from Peronne to Verdun. There is a gap of about twenty miles to the south of Peronne, but the advance of the Allies will no doubt soon bring this part of the battle-line within the area of the sheet. The map shows woods in green, and the present front, approximately as it was on November 10, by a red line. There is, unfortunately, no attempt to show elevation either by contours or spot-heights. Rivers, railways, and canals are clearly and accurately shown, and there is an abundance of names. The map should make it easy to follow the course of any advance on this front, though the absence of indications of relief will not help the reader to grasp the significance of the line of front. There is a companion map at the same price of the British front to the north.

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LETTERS TO THE EDITOR.

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The Permanence of Finger-Print Patterns.

I RECEIVED a few days ago Sir Wm. J. Herschel's brochure, "The Origin of Finger-Printing." His object—in addition to examining other claims to this method—is stated to be the desire to place on record the discovery of this method of identification "in Bengal in 1858," and the author seems to be piously grateful for the "gift granted" to him of that great and most useful discovery. The evidence for this early date is contained in the imprint of a single hand of one Kōnāi, made at that time. This was issued on a single sheet some years ago, but when, as an eager student of the subject, I applied to the publishers for a copy, I was told it was issued only for private circulation, and could not be supplied to me. I now hasten at the first opportunity to give my own opinion of this impression, long guarded so carefully from the inspection of the expert critic.

The fateful lines so dear to palmistry are quite nicely shown up, and many of the skin furrows, or *rugae*, on the palm are printed with considerable clearness. That is, the part of the hand not at all used in the official system of identification is well done, but what of those parts on which the system entirely relies? The significant pads at the last joint of each finger, which are full of intricate patterns in every human, or monkey, finger, are not shown at all. They are mere uniform blotches of ink. There is absolutely no trace of a pattern of the simplest kind in any one of the five fingers shown. I wish to be understood as not exaggerating for any controversial effect, and appeal to any trained detective if this is not as I represent. No identification could be effected on such a basis, and the system was therefore clearly *not* discovered in 1858 by the baronet. I cannot perceive that even now the author has any adequate conception of what the system is, now in general and satisfactory operation throughout the civilised world. A most curious confusion has arisen from an original police blunder that no two single finger patterns are ever alike, for which, I think, Sir William himself is mainly responsible. I am quite sure that there is no scientific basis for such an assertion. My syllabic system of classification, applied to a large collection, would enable such an assertion to be severely tested, but I know of no other method in existence which could do so. To compare finger by finger in a large collection is utterly impossible. But by giving a short syllabic name to the pattern of each finger those names can be assorted apart from the hand collection, and those which are similar can be compared individually. I have no doubt in my own mind that such a search would reveal closely similar fingers in different individuals, so closely similar indeed that the slightest blur in printing would lead to the fallacious conclusion of identity. It was on getting a clear perception of this very dangerous fallacy, still manfully held and expounded by one or two police experts, that from 1879-80, when I first made public the method, I insisted on the use of the whole set of ten fingers, serially and consecutively printed, for criminal identification. It affords an example of mutations, but for trivial purposes fewer fingers might do very well. The English method, now practically used everywhere, cannot be greatly improved upon in this respect for identifying old convicts on reconversion.

The question of what degree of evidence a single bloody smudge may give of the identity of some supposed miscreant with a convict having a previous official record is a matter for dispute. Still more is that of a case where, say, the right forefinger of Richard Roe may be practically identical with the left ring-finger of John Doe. Such similarity would be no evidence whatever for personal identification. In labouring to be brief I trust I have not been quite obscure. Sir William, in his review of the history of this discovery, has not made any reference to my little contributions on the subject. He, however, did acknowledge my priority of publication in your columns of November 22, 1894, and for that "gift granted" I must feel grateful.

HENRY FAULDS.

Stoke-on-Trent.

I HAVE to thank you for your courtesy in forwarding me a copy of Mr. Faulds's letter to you, and, in compliance with your request, I submit the following remarks.

The only point I feel bound to notice is his complaint that I have not mentioned his name in my story of "The Origin of Finger-Printing." Mr. Faulds's own account of his claim has been so fully placed before the public in his letters to you from Japan, of October, 1880, and later, that I think I was right in keeping to that period of history, twenty years further back than his, which lay within my own knowledge.

But his present letter breaks through all bounds of social courtesy, and it is only his position as a professed man of science that justifies me in correcting him. Mr. Faulds has the temerity to scout my statement that I was moved to study finger-printing by the fascination of Kōnāi's hand-mark (taken as it was for the same purpose as finger-prints now are). The finger-tips were badly smudged, but the small furrows on the palm were exquisite, and moved me to take better impressions than his from my own fingers, as I tell the reader on the same page, only Mr. Faulds ignores it. This is not the spirit of science.

I will now, with your permission, show reason why I could not honestly have introduced Mr. Faulds's name. His letter of 1880 announced that in the previous year his attention was directed to the peculiarities of finger impressions on pottery, and that he had come to the conclusion, by original and patient experiment, that finger-prints were sufficiently personal in pattern to supply a long-wanted method of scientific identification, which should enable us to fix his crime upon any offender who left finger-marks behind him, and equally well to disprove the suspected identity of an innocent person. (For all which I gave him, and I still do so, the credit due for a conception so different from mine.) But he went on to say:—"There can be no doubt of the advantage of having a copy of the for-ever-unchangeable finger-furrows of important criminals."

This expression made me protest at once, in my reply, that I could not understand how, in less than two years, he could have come to the knowledge that the furrows were unchangeable. It had taken me nearly twenty years of sustained experiment to demonstrate this persistence of the patterns for at least fifteen years of a man's life, and it is plainly impossible for any man with a scientific turn of mind to put this doctrine forward after only twenty months or so of experiment. My reply, therefore, of 1880 expressly challenged his authority for the statement, and he has never justified it. My challenge did oblige him to meet it as best he could, but the nearest approach I have seen to an answer is the following extract from an article of his in *Knowledge*, April, 1911:—

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"The mode I took to test whether the ridges ever shifted their situation or changed their form was by shaving away their elevations . . . having first taken careful imprints of the patterns. After the skin grew up again, fresh imprints were taken and compared with the old ones, . . . but in many hundreds of cases, tested thus three or four times, not one solitary example of a variation in pattern was detected." His return to England broke the further investigation. He goes on to say:—"The firm conviction, however, was established in my mind, which nothing has occurred to change, that skin furrows for the purposes of identification are invariable throughout life."

This quotation is his latest statement of his authority, but it needs to be read with an extract from a previous letter of his, dated June 5, 1909, in which he says:—

"One of my earliest experiments was to shave off the ridges of the finger-tips with razors; the pattern on the skin was reproduced with quite unvarying fidelity, unless part of the true (deep) skin was removed."

I take it that this is the only foundation he has for his claim to have known the law of persistency in 1880. I leave it to men of science to judge whether his experiments sufficed to prove persistency of a finger pattern for life.

W. J. HERSHEL.

Warfield.

The Date of the Introduction of the Term "Metabolic."

THE concept and the term "metabolism" have played such a prominent part in the development of physiological science that it should be interesting to know by whom, and when, the term was first used. Prof. Bayliss, in his "Principles of General Physiology" (1915, p. 263), says that, so far as he can discover, "metabolism" was first used by Sir Michael Foster in his "Text-book of Physiology," the first edition of which was published in 1883. It seems, however, that there is a still earlier use of the term in the writings of no less well known an investigator than Theodore Schwann, enunciator of the cell-theory. The passage I allude to occurs in the chapter called "Theory of Cells," the last in Section III. of Schwann's classic, "Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants, by Dr. Th. Schwann, Professor in the University of Louvain," published in Berlin in 1839. My translation of it is that made in 1847 by Dr. Henry Smith, of London, for the Sydenham Society; it runs thus (p. 193):—"The question, then, as to the fundamental power of organised bodies resolves itself into that of the fundamental powers of the individual cells. . . . These phenomena may be arranged in two natural groups: first, those which relate to the combination of molecules to form a cell; secondly, those which result from chemical changes either in the component particles of the cell itself or in the surrounding cytoblastema, and may be called *metabolic* phenomena (*τὸ μεταβολικόν*, implying that which is liable to occasion or suffer change)." The italics are in the original. Here, then, so far as I know, is the first use of the term "metabolic," though undoubtedly not the first occurrence of the conception of chemical changes in living matter. Schwann uses the term "metabolic" exactly in its present-day sense, the phenomena of change, interchanges, of material in and by living matter.

The year 1839 may be taken as the date of the introduction into biological terminology of the expression "metabolic," and the person Theodore Schwann, at one time professor in the ancient University of Louvain.