

once more to the case of pigment in relation to light. For thousands of generations no pigment has been developed, say, on the lower side of a flat fish, no light having fallen on it. The skin is experimentally exposed to light, and pigment appears: therefore the acquired character of absence of pigment, after thousands of generations, has produced no hereditary change, has not altered the potentialities of the tissue. The argument is fallacious, because the question of how much pigment is entirely ignored, and also the question how long the development of pigment experimentally takes. The force of the argument is entirely on the other side. Assume in this case, as Prof. Lankester does in his general argument, that the old character, the absence of pigment, is an acquired character. Then experiment has shown that this character is inherited: that is to say, the action of light obviously overcomes a resistance in producing pigment, and after years does not produce as much as on the upper side is present from the beginning. This resistance can be nothing else than heredity, the inheritance of a tendency to pigmentlessness. Therefore the acquired character is inherited. It is undeniable and indisputable that the argument propounded by Prof. Lankester proves the inheritance of acquired characters, if it is properly applied in accordance with the facts. This is on the assumption that the "old characters" are acquired. If they are not acquired, the argument has no force at all. The facts allow us to say that the tendency to pigmentlessness, or the resistance to the development of pigment on the lower side of a flounder, is certainly inherited, but whether or not it is due to the absence of light during many successive generations we do not know. As Sir Edward Fry says, if we by definition confine the term "acquired character" within the limits of an individual history, then of course an acquired character can never be inherited. The question is whether the conditions which produce a change in the individual can affect the offspring? The experimental investigation must take the following course. Suppose a given amount of stimulation X to act upon individuals in successive generations, producing in the first generation a result x . Then the question is if X remains the same, does x remain constant or not? If there is no inherited effect, then x must remain constant in all succeeding generations. If x increases by some amount, however small, and becomes $x+a$, then a is not acquired by the individual, but inherited, and it is clear that the result will go on increasing to $x+2a$, $x+3a$, and so on to $x+na$, where n represents the number of generations. In my own opinion, there is evidence that something of this kind does occur, though definite investigations are much to be desired.

Plymouth, January 11.

J. T. CUNNINGHAM.

As one who has been reading the discussion in your pages on the meaning of the term "acquired characters," I may perhaps be permitted to direct attention to the history of the term. It was first used with reference not to species but to individuals. Every character of an individual is either derived from the fecundated ovum or acquired during life. This was obvious; and the question arose: Could acquired characters be transmitted? As long as the term is applied to an individual, it has that kind of precision which is desirable in all scientific terminology, namely, that it perfectly explains itself.

Glasgow, January 12.

JOHN CLELAND.

Chinese Theories of the Origin of Amber.

IN my letter on "Some Oriental Beliefs about Bees and Wasps" (NATURE, vol. I. p. 30, May 10, 1894), I have traced the origin of the Chinese belief in the production of amber from bees into the presence in amber of hymenopterous remains. Apparently developed from this belief, there is another misconception recorded by Cháng Hwá (killed 300 A.D.), whose passage on the subject reads as follows: "In 'Shinsien-chuen,' it is said, the resins of the pine and arbor-vitæ, after remaining underground for one thousand years, are turned into *Pachyma cocos* (Fuh-ling),¹ which is turned into amber." Notwithstanding this statement, the Mount Tai produces *Pachyma*, but no amber; whereas Yung-chang . . . produces amber, but no *Pachyma*. Another theory is that amber is made by burning the honey-combs. Which is true of these two theories is not yet decided.²

Of all Chinese theories propounded to account for the origin

¹ Identified thus in Dr. K. Itô's "Nihon Sambutsu-shi," part vii "Poh-wuh-chi," tom. iv., sub. "Yon-wuh."

of amber, the most veracious one is given in Li Shi-Chin's work,³ thus: "Amber originates in the resin of pines; when the pines, with their branches and knots luxuriantly growing, were heated by the sun, the resin came out of the wood; it coagulated after days and sunk underground, and after undergoing subterranean changes, left behind the lustrous substance [which is amber]. In this condition still it has in it the tenacity of resin, so that when it is rubbed and warmed between the palms, it can pick up particles of dust. Those insects in its enclosure had cohered with it before its sinking underground."

Besides the resin of pines, the exudation from the "Fung" (*Liquidambar Maximowiczii*) is asserted by Kán Páu-Shing (lived in the tenth century A.D.) to be a nascent form of amber,⁴ the opinion well coinciding with the Western idea that has given to styrax the name "Liquidambar."⁵

In "Shi shwoh" (written in the fifth century A.D.) amber is said to be formed from the subterranean metamorphosis of the gum of peach trees,⁶ which reminds us of the simile, "Like gum from the cherry," used by Pliny in his exposition of the resinous origin of amber.⁷

Some other theories are full of absurdity. One of these holds that the dragon's blood buried underground turns to amber, and the demon's to agate.⁸ Also, the etymological origin of "Hú-pèh," the Chinese name for amber, is involved in myth. In ancient times this word was written in two letters, together signifying "Tiger's Soul," which is explained in this way: "At night the tiger applies its one eye for illumination, and another for vision. When it is shot with arrow the light of the eye, which is the tiger's soul, sinks underground, and turns into a white stone. . . . Amber resembles this stone; hence the name."⁹

According to "Hwái-nán-tze" (written in the second century B.C.), "the dodder is the outgrowth of amber."¹⁰ Almost inexplicable as this story may appear, I have found certain clues to its elucidation. Káu Yü (lived in the second century A.D.) gives "Nü-ló" (i.e. *Usnea longissima*)¹¹ as a synonym of "Tú-sze" (i.e. the dodder).¹² From this it is evident that the early Chinese have confounded *Usnea* with dodder—the confusion caused by the superficial resemblance and similar habitats of the two plants.¹³ Now, there is a Chinese belief recorded about 240 B.C., that *Pachyma cocos* is the root of dodder,¹⁴ which has doubtless grown out of the common occurrence upon and under the pines of the *Usnea* and *Pachyma*. And as this *Pachyma* had been held as an intermediary phase through which resins were to pass into amber (see above), it would seem that the story which affirms the dodder to be the outgrowth of amber, was not inconsistent with the understanding of the early Chinese theorists.

KUMAGUSU MINAKATA.

January 11.

Rhynchodemus Terrestris in Germany.

IN NATURE of October 25, 1894 (p. 617), Mr. Scharff mentioned *Rhynchodemus terrestris*, as stated, in Germany, near Würzburg, by Semper. It would seem that the worm was exceedingly rare. But I found it repeatedly at several points of Saxony and Thuringia, in the mountains and in the plain, in leaved and fir wood, under moss or dead leaves. Sufficient attention would detect it without doubt in many regions. Recently Mr. Ehrmann found several specimens feeding on a dead *Arion empiricorum*.

H. SIMROTH.

Leipzig.

The "Proceedings of the Chemical Society."

THE title-page and index of this periodical have just come to hand, and on the title-page occur the words, "Edited by the Secretaries." It appears to me right that authors should know

³ "Pan-tsau Káng-muh," 1578, art. "Hú-pèh."

⁴ *Ibid.*

⁵ Loudon, "Encyclopædia of Plants," 1830, p. 798.

⁶ T'wan Ching-Shih, "Yü-yang Ts'ah-tsü," tom. xi.

⁷ "Natural History," English translation, Bohn's edition, vol. vi., p. 401.

⁸ T'wan Ching-Shih, *loc. cit.*

⁹ "Pan-tsau Káng-muh," *loc. cit.* and art. "Hú."

¹⁰ T'wan Ching-Shih, *ubi supra*.

¹¹ Identified thus in Dr. M. Miyoshi's article in the *Shokubutsugaku*

Zasshi, No. 34, p. 435, Tokyo, Dec. 10, 1889.

¹² "Lü-shi Chün-tsü," Japanese edition, N.D., tom. ix. p. 9, Káu Yü's

note.

¹³ Cháng Hwá appears to have well distinguished the two plants. He says, "Usnea lives upon the dodder, and the dodder upon trees." "Poh-wuh-chi," *loc. cit.*

¹⁴ "Lü-shi Chün-tsü," *loc. cit.* text.

the precise significance of these words, as lately determined by the Council of the Society.

Two courses appeared to be open: either to submit proofs to the authors of the abstracts of their papers sent to the Society, if any substantial (*i.e.* more than typographical) alteration had been made; in which case, the authors themselves would naturally bear the responsibility of their statements; or to throw the whole responsibility on the Editors, leaving them to make any excisions or alterations they may choose in the abstracts sent to them; or indeed, if they so think fit, entirely to rewrite them. The Council, in order to secure rapid publication, have chosen the latter alternative; and it should be understood that the abstracts are now "official"—*i.e.* the responsibility for all statements put forth rests solely on the Editors of the *Proceedings*.

WILLIAM RAMSAY.

University College, London, W.C., January 14.

Philosophy and Natural Science.

WHILST feeling obliged to your reviewer's appreciation of my essay (p. 220), I am bound to rectify some very glaring discrepancies.

(1) As plainly stated in my preface, my essay has *not* obtained the Philosophical Society's prize, but only an "honourable recognition," and two fifths of the prize sum.

(2) Eighth line from bottom (p. 220), for "physical," read "psychical," as said in my paper (p. 30).

(3) Your reviewer makes me say: "Physical development is not the cause, but the effect of psychical development"; whereas, I have expressly *combated* this view of Wundt's (p. 32).

(4) Neither did I say: "The modifications in the brain and nervous system throughout the animal kingdom are intelligible as resulting from psychical causes . . ." but only (p. 32) that *in many cases* the beginnings of modifications are intelligible from the psychical side—*e.g.* the modifications of many organs—resulting from sexual selection.

(5) Lastly, far from saying that the high mental position of man, on the one hand, and of ants on the other, "is independent of the structure of the nervous system," my sentence (p. 34) is: "Here, where the organic substratum (*i.e.* the brain) in both types differs even in its principal morphological features, it is most evident how occult are the processes which constitute the proper material side of psychical phenomena."

Freiburg, Badenia, January 5. DAVID WETTERHAN.

(1) THE facts are that the Philosophical Society of Berlin offered a prize of 1000 marks for an essay on "The relation of philosophy to the empirical knowledge of nature."

The essay reviewed, only obtained 400 marks of this prize, and an honourable mention. In a hasty glance at the preface I overlooked the words "ein Antheil von vierhundert Mark," which occur in the next line to "der als Preis ausgesetzten Summe," which caught my eye.

(2) This is evidently a slip of the pen, which I regret was overlooked in proof.

(3) In my notes, jotted down as I read the pamphlet, I put Wundt's words in quotation marks, intending to point out Mr. Wetterhan's opposition thereto; but in writing the review, I unfortunately omitted the commas, and, I regret, entirely misrepresented the author's views. Perhaps I may quote from p. 32 of the pamphlet: "Man durfte Wundt's Satz, 'dass die physische Entwicklung nicht die Ursache, sondern vielmehr die Wirkung der psychischen Entwicklung ist,' zu weitgehend, und auch in seinen Konsequenzen bedenklich finden." We are then referred to page 46, where we read: "Der Ausführung dagegen, welche Wundt (s.o. p. 32) jenem Prinzip gegeben hat, vermag ich kaum eher beizustimmen, als der verwandten Ideen Schopenhauer's."

(4) It appears to me that the passage will bear the construction which I put upon it; though perhaps "throughout" the animal series is too inclusive as a rendering of "der Tierreihen."

(5) The author had been discussing the similarity of habits and instincts in ants and termites, and then remarks that there is a distinct agreement in the mental functions ("von geistigen, ja gemüthlichen Funktionen") of bees with those of the higher

animals. He refers to Darwin's opinion that the small brain of a bee is a more wonderful thing than the brain of a Man; and I think I was entitled to make the obviously true remark that this "mental development is independent of the structure of the nervous system." I was not quoting Mr. Wetterhan's words, but giving the general sense of the passage.

In conclusion, I must express my regret that the condensation of some of the author's remarks should have resulted in a confused expression of his views.

THE REVIEWER.

SOME EARLY TERRESTRIAL MAGNETIC DISCOVERIES PERTAINING TO ENGLAND.

IT should be a source of considerable pride to British men of science that so many of the discoveries in terrestrial magnetism have been made in England. And yet, owing to the absence of a complete and carefully written history of the development of this science, probably few could enumerate all the achievements in this subject by Englishmen.

In February 1893 the writer had the good fortune to light upon a book,¹ by Will Whiston, containing matter pertaining to the terrestrial magnetism of England, which appears to have been entirely overlooked by prominent terrestrial magneticians. Owing to pressure of work, this interesting book, of which a copy was found in the Royal Library of Berlin, could not be subjected to a critical examination until the early part of 1894, when the writer called the attention of prominent Berlin investigators, such as Prof. Hellmann and Dr. Eschenhagen, to it.² In the meantime, Dr. W. Felgentraeger, Assistant at the Göttingen Magnetic Observatory, made an independent discovery of Whiston's book, and carefully worked up part of the material contained therein.³ The writer has since found time to complete his examination of Whiston's contribution, and has embodied his results in a paper⁴ presented by Prof. Cleveland Abbe before the Philosophical Society of Washington on November 10, 1894. In the following these results will be briefly sketched.

As will appear from the title of Whiston's work, the chief object was the exposition of a method for determining the longitude and latitude by means of the magnetic dip-needle, *i.e.* by means of the angle which a magnetic needle mounted on a horizontal axis, when placed in the vertical plane of passing through the magnetic meridian, makes with the plane of the horizon. It will be recalled that at that time great prizes had been offered by the English Parliament for an easy and trustworthy method of determining longitude at sea. From the very birth of terrestrial magnetism we find methods proposed for determining longitude by means of magnetic observations, and, like the problem of perpetual motion, these magnetic methods were revived every once in a while until the beginning of the nineteenth century. Owing to the irregular distribution of magnetism within the earth's surface, and on account of the many fluctuations terrestrial magnetism is subject to, these magnetic attempts to determine geographical position have been doomed to failure. They, nevertheless, have done much to promote the science of terrestrial magnetism. A striking instance of this is the book of Whiston's. The prime object of the book has failed of

¹ "The Longitude and Latitude found by the Inclinator or Dipping Needle; wherein the Laws of Magnetism are also discover'd. To which is prefix'd an Historical Preface; and to which is subjoin'd Mr. Robert Norman's New Attractive, or Account of the first Invention of the Dipping Needle." By Will Whiston, M.A., sometime Professor of Mathematicks in the University of Cambridge. (London, 1721. 8vo, xxviii. 115, iv. and 43 pp. 2 charts and 3 cuts.)

² See remarks in *Physical Review*, vol. ii. No. 1, p. 72.

³ "Die Isoklinen-karte von Whiston und die säkulare Aenderung der magnetischen Inklination im östlichen England." Von W. Felgentraeger. Reprint from *Nachrichten der k. Gesell. der Wiss. zu Göttingen Math. Phys. Klasse*, 1894. No. 2. 8vo, 12 pp.

⁴ Entitled "The Earliest Isoclinics and Observations of Magnetic Force." (*Bull. Phil. Soc., Wash.*, vol. xii. pp. 397-410.)