

some of its adjectives, and supply their position by blank spaces!

In the *Anthropologist* I asserted that in his so-called 'Ethnology' Mr. Keane 'pursues the same plan, treating the same subjects in nearly the same order' as I did in my 'Races and Peoples,' published six years ago. Mr. Keane now professes to have 'but the haziest recollection' of the contents of that book (though in his note in the *Anthropologist* he acknowledges to have read it). Its very title he had quite forgotten! His 'treacherous memory' led him to mention it under quite a different name from the one it bears! How, then, 'can he truthfully say' (to quote his words) that the scheme of his book has *not* the singular similarity I noted to that of my own? He is convicted out of his own mouth of denying the charge I made, without pretending to ascertain whether it is true! I challenge comparison of the books by readers not disabled by a morbid self-esteem from deciding correctly. I challenge the production of any other work on this science, published in any language, since 1889, so obviously akin in plan and treatment to my 'Races and Peoples,' as is Keane's 'Ethnology.' I am quite willing to allow Mr. Keane the plea of 'unconscious memory;' but the facts speak for themselves.

Mr. Keane makes the assertion that I brought a 'false charge' against him in reference to Virchow's opinion about the Neanderthal skull. He quoted Virchow as stating that the skull was 'possibly pathological.' I quoted Virchow's own words, giving them in the original German, that he had offered 'the positive proof' that it was pathological. The 'false' statement is unquestionably Mr. Keane's; but then he suffers from such a 'treacherous memory!'

Mr. Keane seems much disturbed at my statement that he had not consulted the best and most recent studies on American aboriginal ethnography. In reply, he makes no pretence that he did so, but follows the legal precept, 'When you have no defence, abuse the opposite counsel.' I turn to his index and look in vain for the names of Adam, Bandelier, Ehrenreich, Leon, Middendorf, Quevedo, Seler, Steinen and many others, without a knowledge of whose

excellent labors it is presumptuous in a writer to pretend to any but a second-hand and superficial knowledge of American ethnography.

It is needless to occupy more space with such a discussion. I reiterate the justice of my criticisms on Mr. Keane's book; and as a set off to his report of the 'acclamation' with which, he informs us, it has been accepted in England, I add that I have received letters from several prominent anthropologists in the United States telling me that I had dealt with its errors and crudities much too leniently.

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TO PREVENT THE GROWTH OF BEARD.

IN March last, Dr. B. F. Egeling, of Monterey, Mexico, sent to the Department of Agriculture several specimens of the cocoons of a large Bombycid moth, with the statement that these cocoons are worn by the natives around the neck and are believed to prevent the growth of beard on the chin. Dr. Egeling wished to know the name of the species. Specific determination was impossible from the cocoons alone, but on May 18th a fine female specimen of one of the handsomest of the Central American Attacine moths issued and proved to be *Attacus jorella*, of Westwood, described in the Proceedings of the Zoölogical Society of London, 1853, pp. 150-160, and figured at Plate XXXII., Fig. 1. The locality given by Westwood is Cuantla, Mexico, and the statement is made that the type specimens were reared in August from cocoons spun the previous October. The use to which the cocoons are said to be put by the natives is new to the writer. Perhaps it has been recorded by some collector of facts of this nature.

L. O. HOWARD.

THE CHILD AND CHILDHOOD IN FOLK-THOUGHT.

TO THE EDITOR OF SCIENCE: The author of 'The Child and Childhood in Folk-thought' has no desire to enter the lists on behalf of his book, being willing to have its fate decided by those to whom it has appealed and for whom it was written. But against the general dogmatic tone of the reviewer (SCIENCE, N. S. Vol. III., No. 72) he ventures a mild protest. Hardly does the present state of the science justify the

cocksureness there displayed, nor is the reviewer vindicated in his certainty that the author intended to force a 'psychological connection' here, or ought to have made out one there. If Dr. Boas, remembering that all writers have not reached that eminence of synthesis and systematization on which he so conspicuously dwells, will once more peruse the volume he will discover that neither in its claims nor in its execution does it traverse those sound principles of the comparative method of which a peculiar interpretation belongs to him. In these the writer believes as thoroughly as does the reviewer. But, as to the exact manner and method of determining where a 'psychological connection' exists, or what phenomena are 'derived psychologically or historically from common causes,' a great deal of reasonable difference in opinion exists, and this the author has not ignored. The reviewer has throughout attributed to the writer a much more ambitious thesis than he really attempted, and has apparently seen efforts at connection and comparison where none such existed or were thought of. That the author has completed the task he set himself, other reviewers have perceived and acknowledged; to have accomplished the task the reviewer sets him, he had needs be the reviewer himself.

ALEX. F. CHAMBERLAIN.

WORCESTER, MASS., May 15, 1896.

'THAT GREAT LAW OF LOGIC.'

IN a recent number of this JOURNAL (p. 668 above) I ventured to criticise Professor Brooks for using ambiguously the phrase 'test of truth,' and for not appreciating the force of a letter by M. M., calling attention to this. I then pointed out what seemed to me an analogous confusion in regard to the material and the efficient causes of evolution, saying that I did this at the risk of being accused of irrelevancy by Professor Brooks. I did not at all intend to include Professor Brooks with those who have confused material and efficient causes, and his reply (p. 779 above) should have been directed to Professor Cunningham who in the May number of *Natural Science* makes, I think incorrectly, this charge.

Professor Brooks is mistaken in saying that I

did not specify anyone who seems to me to use the word 'cause' ambiguously. It is, indeed, easy to adduce other eminent naturalists in addition to the one to whom I referred. Thus Professor Weismann writes in his most recent paper (*On Germinal Selection*, authorized translation: Chicago, 1896): "The protective coloring * * * * arose not because it was a constitutional necessity of the animal's organism that here a red and there a white, black, or yellow spot should be produced, but because it was advantageous, because it was necessary for the animal." Weismann's state of mind seems to be similar to that of the little boy who was watching at a hole for a woodchuck to come out, and when asked how he knew there was a woodchuck in the hole said "because we have company for dinner and there is no meat in the house."

While Professor Brooks replies to a question in which we agree he neither defends nor retracts the statement which I think is guilty of an analogous blunder, and it seems as though he does not appreciate the point raised by M. M. It is, perhaps, merely a matter of words, but when words are used ambiguously arguments become fallacious. When Professor Brooks writes advocating "that great law of logic, 'the test of truth is evidence and not conceivability,'" does he mean to deny that conceivability is a sufficient proof of truth or to deny that conceivability is a necessary condition of truth, and what does he mean by conceivability?

In the curious history of thought we have had inconceivability urged as a proof of truth, but not, so far as I am aware, conceivability; no one holds that the situations in the modern realistic novel have occurred because they are conceivable. It has, however, been claimed that conceivability is a necessary condition of truth, and by one who holds this position (as Mr. Herbert Spencer) Professor Brooks' statement could neither be affirmed nor denied any more than he could answer yes or no to the question "Did you hold the lantern when your father robbed the stagecoach?"

Then Professor Brooks' 'great law of logic' is doubly illogical because he also uses the word 'conceivability' ambiguously. When he writes