

as a high or a low pressure area, such a place as the North American polar region and a Pacific Ocean.

The reader will find sets of curves* showing such coincidences as Prof. Bigelow thinks to have discovered between certain periodic phenomena of terrestrial magnetism and certain periodic meteorological phenomena. The writer is unable to give any definite help towards a clear understanding of these curves, indeed, "A complete exposition of the data is impossible in this connection, and therefore no values are assigned to the ordinates of the several curves."†

In conclusion, let it be said that the writer has had occasion to examine irrational writing before, but he has never encountered such froth till now. The more excusable nonsense, and often the more evident, is that which is built, it may be with care, upon false conceptions; but these papers of Prof. Bigelow's are devoid of all conceptions, and at best they are mere pretension.

The writer begs the reader's indulgence in what may seem to be undue severity in this, to the writer, questionable business; but having been vexed with it for more than a year, between the difficulty of bringing it to an end, on the one hand, and the impossibility of putting it aside, on the other, he is now chiefly anxious to be done with it, and is inclined to give, with a minimum of argument and example, the plainest and sternest statement of fact.

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DR. BRINTON ON KEANE'S 'ETHNOLOGY.'

TO THE EDITOR OF SCIENCE: IN SCIENCE, March 20th, Dr. Brinton has a notice of my *Ethnology*, which is so manifestly unfair that I will ask you to allow me a little space for a brief reply. The 'title is an error,' because I take ethnology to be 'nearly synonymous with anthropology as employed in modern science.' On the contrary, I carefully distinguish between

general anthropology, which, of course, covers 'all branches of knowledge whose subject is man,' and *special* anthropology, to which ethnology is 'complimentary' (pp. 1-2). Dr. Brinton does not call attention to these distinctions, thus leaving himself convenient scope to quibble and misrepresent.

My theory of races 'is a modern recast of that of Blumenbach.' Not so; on this point I reject Blumenbach and state in the clearest language that 'Linné's original fourfold division must be upheld' (p. 222). Blumenbach's Malayan race is 'explained away as partly Ethiopic, partly Caucasian.' Rejecting Blumenbach's five divisions, I had no occasion to 'explain away' his 'Malayan race.' Nor do I represent this race as 'partly Ethiopic, partly Caucasian,' but 'distinctly Mongoloid, one might almost say Mongolic without reservation' (330).

I refer to opponents as 'eccentric or reckless or extravagant.' These epithets are used sparingly and never personally, but only in reference to strange or impossible theories, such as: 'evolution with a jump' (p. 235), and the like.

I 'do not hesitate to strain a point to defend his [my] opinion,' and Virchow on the Neanderthal skull is given as a proof. Here the point is strained, not by me, but by Dr. Brinton, who omits Virchow's last word on the subject, which is that he never maintained 'the *absolutely* pathological character of the skull' (p. 424). This, no doubt, leaves Dr. Brinton *im Stiche*, but that is no reason why he should bring false charges against me.

I claim 'as original' to myself, amongst other theories, 'the relationship of Basques and Berbers.' No! what I claim as original is my 'general treatment of * * * the Ibero-Berber question' (xv.), which Dr. Brinton knows is quite a different thing.

"The relationship of the members of the various races is shown by 'family trees,' an ancient and misleading device." These trees are not 'ancient;' they are mine; or will Dr. Brinton tell us where else he has seen them? But they are 'necessarily misleading;' yes, if the accompanying text be overlooked, and the branches wilfully entangled, and then notes of exclamation added as thus: "The Teutons and Slavs are on a different branch!" The Teutons and

* Report for 1891-2, of the Chief of the Weather Bureau, plate IV., Am. Jour. Sci., 3, 48, p. 448.

† Report for 1891-92, of Chief of the Weather Bureau, p. 525.

Slavs are on *two* different branches!! Again, "The Kolosch and Selish are depicted as proceeding from the Eskimo!" I write Kolushan Salishan, plainly showing, as explained in the text (p. 360) that I mean these to be taken as *stocks* (notsecondary groups), in accordance with Mr. Powell's 'convenient plan.' But Dr. Brinton suppresses the final *an* and is thus able to hold me up to ridicule by the long discredited *suppressio-veri-et-suggestio-falsi* argument.

"The chapter on the American race is replete with positive assertions, nearly always unsupported; for instance, 'the alleged impassiveness of the native character.' " Well, I devote five pages (353-357) to that subject, and support my contention by the authority of Pastor Egede, Reclus, Catlin, J. P. Dunn, Jr., Humboldt, E. F. Knight, E. im Thurn and Darwin!! So it is Dr. Brinton's charge that is 'unsupported.'

I refer to 'a highly respected American writer, as Mr. Thomas Cyrus (p. 370).' Yes, but Dr. Brinton forgot to tell his readers that this was the merest slip, as clearly shown by the correct references to that excellent authority at p. 107, p. 343 and in the index.

But "it is obvious that the author has not consulted the best and most recent studies in American aboriginal ethnography." How can this be when Dr. Brinton tells another circle of readers (Dr. Brinton spreads himself considerably) that my work is "scarcely more than an expansion of the one referred to, pursuing the same plan, treating the same subjects in nearly the same order, and in various portions advancing as his own the opinions set forth by that referred to, to wit: 'Races and Peoples, Lectures on the Science of Ethnography, by D. G. Brinton, New York, 1890' (*American Anthropologist*, March, 1896, p. 100). If, I say, my ethnology is scarcely more than an expansion of a book by Dr. Brinton, how can he now truthfully say that I have 'not consulted the best, etc.,' on the subject? Or has the sage of Philadelphia such a poor opinion of his own compilations as to regard them as 'the worst, etc.?' I may incidentally add that this disgraceful charge of wholesale plagiarism is as baseless as all of Dr. Brinton's other charges. His *Races and Peoples* was never once consulted

by me on any single point, and at the present moment I have but the haziest recollection of its contents, even giving it an incorrect title in the reference made to it from a treacherous memory in the preface, p. vii.

Dr. Brinton again refers to my 'theory of the Malayan race,' which should be my theory of the 'Interoceanic Races,' of which the Malayan is but one. This theory, he writes, "we may allow is at present, and is likely to be his [my] own peculiar property." This is hitting me below the belt with a vengeance, for Dr. Brinton, who knows everything, knows quite well that the theory in question, first brought by me before the British Association in 1879, has since been accepted in its essential features both by Dr. Hamy and de Quatrefages, two of the most distinguished French anthropologists of our times.

Dr. Brinton, however, is gracious enough to make one concession. He is willing to allow that one particular chapter 'might have been much more uninteresting.' To be sure, this may be 'meant sarcastic,' or may even be regarded by some as a choice specimen of concentrated malevolence. In any case, it is not much for a book which I am able to inform Dr. Brinton has been received with acclamation in England, which has been spoken well of in the far West (*American Journal of Sociology*, Chicago, March, 1896), and which has been accepted on the continent as *le meilleur traité d'ethnologie que nous possédions jus qu'à présent* (*Rev. Bibliographie*, Feb., 1896, p. 100).

With this I may confidently leave 'this fellow here with envious carping tongue' (Shakespeare) to the judgment of your American readers.

A. H. KEANE.

ARÂM GÁH, 79 BROADHURST GARDENS,
LONDON, N. W., April 22, 1896.

I CLOSED my notice in the *American Anthropologist* of Mr. Keane's work with an expression of regret at the discourteous language he uses toward those with whom he disagrees. If other evidence were lacking to prove the justice of my remark, it would be supplied by the above letter. So abusive was that sent by Mr. Keane to the *Anthropologist*, in reference to my notice, that the editor felt constrained to omit

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