

described forms, but since the older authors had little or no conception of the true specific differences in this group, many names have necessarily been set aside as practically meaningless. The treatment throughout is full and sufficient; the genera and species are separated by keys, and the descriptions of the species are quite detailed. Types are carefully designated, and localities and collectors are cited. Biological details are given when available. In all respects the book worthily initiates a series which may be expected to take first rank among those devoted to zoological subjects.

From a postscript at the end it appears that two of the species described by Dr. Aldrich were published a little earlier, under quite other names, by Dr. R. R. Parker. It seems strange that when there are only two persons in the Western Hemisphere working on a subject, they can not consult together sufficiently to avoid such conflicts. Figure 110, as I learn from Dr. Aldrich, though labelled *Sarcophaga bison* is in fact *S. bullata* Parker. The former name was a manuscript one of the author's, and was altered in the text at the last moment, because Dr. Parker published the species as *bullata*.

We hear much these days about the encouragement of research, but it is often overlooked that adequate facilities for publication are essential. Authors are not justified in spending months and years in the preparation of monographs which may never appear in print or serve any useful purpose. There are at this moment many excellent contributions the publication of which is indefinitely postponed, or which must be split up into short papers in order to see the light. To those who are familiar with actual conditions the situation is rather discouraging, and it is not mended by the appearance of a certain number of large books in sumptuous and extravagant form. The Thomas Say Foundation, from necessity no less than choice, publishes as cheaply as is consistent with excellence, and in this respect earns the gratitude of students.

T. D. A. COCKERELL

UNIVERSITY OF COLORADO

THE ORIGIN OF THE PRE-COLUMBIAN CIVILIZATION OF AMERICA

MY attention has just been called to the letters (SCIENCE, October 13, 1916) in which Dr. A. A. Goldenweiser and Mr. Philip Ainsworth Means have put a series of questions for me to answer. As the problems to be solved involve the validity of the foundations upon which has been built up (as the result of more than half a century's intensive studies on the part of leading scholars of every civilized country) a vast superstructure of ethnological doctrine and complex rationalization, perhaps you will afford me the opportunity of replying in some detail to these criticisms, and of adding to the article of mine which appeared in SCIENCE on August 11, 1916, some further reasons for thinking that this elaborate edifice of ethnological speculation will have to be demolished.

While admitting that in the end my contention may be justified, Mr. Means makes the significant comment that "it will be a long time before American anthropologists will be forced to accept these views as final." All that I have attempted to do is to "force" them seriously to examine the foundations of their beliefs, being firmly persuaded that such of them whose minds are still sufficiently alert to be no longer blinded by the outworn dogmas of Bastian and Tylor¹ will be led to accept the views which I have sketched as the only possible interpretation of the facts.

One of the three difficulties suggested by Mr. Means I have already discussed at some length.²

¹ By the same mail that brought me the proofs of this letter also came the tidings of the death of the veteran ethnologist whose teaching is so frankly criticized in it. But though his theories of "animism" and "independent evolution of culture" have been a serious factor in clouding the vision of ethnologists, the great merit belongs to Sir Edward Tylor of stimulating a widespread interest in the subject and thereby contributing materially to the advancement of learning, which has earned him the grateful tribute of all scholars.

² "Ships as Evidence of the Migrations of Early Culture," *Journal of the Manchester Egyptian and Oriental Society*, 1916.

It is significant that, when citing six memoirs relating to shipping, some of them quite irrelevant, Mr. Means should have omitted all reference to the writings of Pâris, Pitt-Rivers, Assmann and Friederici, where he will find the evidence he imagines to be non-existent. But does the argument from ships really help his case? Where is the "similarity of the working of the human mind" if the highly civilized people of Peru and Mexico hadn't sufficient of what Dr. Goldenweiser calls "happy thoughts" to accomplish more in the way of ship-building? Is not this paucity of shipping merely a token of the remoteness of America from the home of its invention?

The fact that the culture-bearers who first crossed the Pacific by the Polynesian route were searching for pearls and precious metals³ is surely a sufficient explanation of their desertion of the sea once they reached the American eldorado.

Another of Mr. Means's difficulties I fail to understand. Why was eight centuries too brief a time for a ship to have made its way from the Red Sea to America? Before the introduction of steam-ships what was to prevent a vessel doing the journey as quickly in the eighth century B.C. as in the eighth, or perhaps even the eighteenth, A.D.? There are reasons, given in detail by Aymonier and others, for believing that western culture had already made its influence felt in Cambodia before the close of the seventh century B.C.: Indonesia and even Japan received the leaven at the same time: and it can hardly be in doubt that the ancient mariners did not limit their easterly wanderings to Indonesia, but pushed out into the Pacific, and soon afterwards crossed it to America.

The remaining difficulty which is holding Mr. Means back is that the Pre-Columbian Americans did not use wheeled vehicles. See-

³ W. J. Perry, "The Relationship between the Geographical Distribution of Megalithic Monuments and Ancient Mines," *Manchester Lit. and Phil. Soc. Memoirs*, November, 1915; and J. Wilfrid Jackson, "The Geographical Distribution of the use of Pearls and Pearl-shell," *ibid.*, September, 1916.

ing that the whole of the migration, which I have described as extending from the Red Sea to America, consisted of a series of maritime expeditions, it is not altogether clear what Mr. Means is referring to when he asks:

Is it not inevitable that they would have made use of such vehicles during their long journey?

At the time the great cultural movement took place it is quite likely that none of the wanderers had ever seen, or even perhaps heard of, a wheeled vehicle. Even if, on some rare occasion of state, in Egypt or one of the Asiatic monarchies, they had seen the king drive in a chariot, was that an adequate reason why these sailors, when, after many years of adventure, they at last reached the American coast, teeming with the spoils they coveted, should have remembered the chariot, and at once set to work to build carts and train llamas to draw them? Surely the utter improbability of this whittles down Mr. Means's difficulty to the vanishing point. Or alternatively, if there is any substance in the "psychic unity" hypothesis, why didn't the Americans get a "happy thought" and invent "so simple and obvious a device" as a wheeled vehicle?

Dr. Goldenweiser's objections are much vaguer and less well-defined. From the latter part of his letter I gather that he is not acquainted with what I have written elsewhere on this subject.⁴

At the outset I must repudiate Dr. Goldenweiser's unwarranted charge that I have "apparently embraced the articles of the Graebnerian faith." My attitude towards the problems of ethnology is that which prevailed

⁴ "On the Significance of the Geographical Distribution of the Practise of Mummification," *Mem. and Proc. Manchester Lit. and Phil. Soc.*, July 7, 1915; republished by the Manchester Univ. Press under the title "The Migrations of Early Culture," August, 1915; "The Influence of Ancient Egyptian Civilization in the East and in America," *Bull. John Rylands Library*, March, 1916; "Ships as Evidence of the Migrations of Early Culture," *Jour. Manc. Egy. and Oriental Society*; and *Nature*, November 25, 1915, p. 340; December 16, 1915, and January 27, 1916, *inter alia*.

amongst most intelligent men until Waitz, Bastian, Tylor, and their innumerable recent disciples, obscured the clear meaning of the facts by a cloud of empty sophistry and misapplied Herbartian philosophy. In many other branches of learning, such as archeology, philology and the history of many of the arts, numerous scholars, who have escaped the vicious influences of this reactionary school, have continued to rely upon facts and interpret their meaning straightforwardly. The writings of Graebner, Frobenius, Ankermann, Foy, Schmidt and Montandon were quite unknown to me when my conclusions were first formulated; their views and mine have nothing in common except that both repudiate the speculations and the antiquated psychology which for far too long have been permitted to hide the truth.

As a guest at the meeting of the British Association in 1911, when Dr. Rivers devoted his presidential address to the discussion of this matter, Dr. Goldenweiser had every opportunity for appreciating the magnitude of the gap that separated his (Rivers's) views from Graebner's. It is straining the truth to brand Rivers as a recruit of the latter's.

The Graebnerian attitude is largely the outcome of the revulsion of modern German opinion against the whole conception of evolution. It included within the scope of its hostility the method in ethnology which has been misnamed "evolutionary."

But the very essence of the conception of evolution is the derivation of all organisms from a common source. It is the teaching of Bastian and Tylor which is a repudiation of evolution; for it is a much closer approximation to the biological idea to look upon similar complex organizations of a series of artificial civilizations as having been derived from the same common source, just as all vertebrate animals were the offspring of one stock, which, after spreading abroad, became more or less specialized in a distinctive way in each locality. To adopt the attitude, which Dr. Goldenweiser is championing, of regarding as the common parent of all these similar customs and beliefs some mystical "psychic unity" is

to place ourselves upon the same mental plane as the aboriginal Australian who believes that children are spirits which have entered their mothers in some mysterious fashion.

But, as he devotes the greater part of his criticism to this matter, I must deal with the specific questions he puts to me.

Dr. Goldenweiser asks me to "name *one* ethnologist who can be shown to have attributed similarities in cultures to the working of highly specialized human instincts." Although every ethnologist who subscribes to the modern Tylorian doctrines necessarily adopts a theory of the working of the human mind which, on analysis, can hardly be differentiated from what the modern psychologist regards as instinct—and an instinct which leads men on the two sides of the Pacific independently the one of the other to look upon a serpent equipped with wings and deer's antlers as a power controlling water can hardly be otherwise defined than as "highly specialized"—very few of them, since the time of Daniel Wilson, have had the frankness to admit a fact which would have branded their speculation as a *reductio ad absurdum*. At the meeting of the British Association in 1912 (see Report, p. 607) I discussed this question, and no one attempted to refute the argument that the adoption by two peoples of highly complex and arbitrary practises along with scores of identical and unessential details can be explained only by the assumption of their (*i. e.*, the customs) derivation from a common source, or by postulating human instincts of so complex a kind that no modern psychologist will admit their reality. Several ethnologists accepted the definition of such phenomena as instinctive. Professor Flinders Petrie made the fantastic claim that there was an instinct to build chambered tumuli which could be explained on biological principles. In a written communication Mr. Cecil Firth argued that if the beaver instinctively built his dam, why shouldn't men for analogous reasons build dolmens? But most of my critics stopped short of admitting that such actions were instinctive, though no one attempted to rebut my argument that the modern ethnological

hypothesis when closely analyzed was tantamount to claiming the existence of highly specialized human instincts. I am, of course, not unaware of the way in which this essential question is usually evaded, by the attempt to explain how similar needs, circumstances and environment, can call forth men's activities and shape them so as to lead to identical cultural developments, quite independently one of the other. But such theorizing inevitably ignores the fact that in the majority of cases such identities of culture actually occur under circumstances and to meet needs as dissimilar as they possibly can be. Whereas of two kindred peoples living under precisely similar circumstances in neighboring islands, say in Indonesia or Melanesia, one of them may possess the whole of the complex culture of the stone-using peoples (Perry), and the other not one of the numerous constituent elements of this exotic civilization. It is when we leave vague speculation and consider specific cases that the so-called "evolutionary" doctrine in ethnology collapses.

The common line of argument is that which is displayed in its frankest form by the late Daniel G. Brinton, and his disciples, such as Spinden and Joyce. In his "Myths of the New World," Brinton writes (pp. 126-127):

No citizen of the United States will be apt to assert that their instinct led the indigenes of our territory astray when they chose with nigh unanimous consent the great American eagle as that fowl beyond all others proper to typify the supreme control and the most admirable qualities, and he explains what he means by this in the previous paragraph:

For the winds, the clouds, producing the thunder, and the changes that take place in the ever-shifting panorama of the sky, the rain-bringers, lords of the seasons, and not this only, but the primary type of the soul, the life, the breath of man and the world, these in their rôle in mythology are second to nothing. Therefore as the symbol of these august powers, as messenger of the gods, and as the embodiment of departed spirits, no one will be surprised if they find the bird figure most prominently in the myths of the red race.

This is rationalization pure and simple, which can be proved to be false in every item.

For we are now sufficiently acquainted with the earliest literatures of Egypt, Babylonia and India, to know that the association of the eagle or hawk with all these varied phenomena was not due to the reasons Brinton gives. Every one of these manifold attributes became added to the eagle's repertoire as the result of fortuitous circumstances utterly alien to those assumed by Brinton. The mingling of eagle-people with sun-people, and the association of the latter with serpent-people and with the worshippers of Osiris (the controller of water) was the beginning of the complex blending of the symbolism of the sun, the serpent, the eagle and water. In the Babylonian thunder-bird further attributes were added, and others again in India, the Far East and America.

If the followers of Brinton deny that the American thunder-bird came from the Old World they will be faced with this dilemma:—as the origin of the confusion is known (from the earliest Egyptian writings) to be the result of wholly fortuitous circumstances, if the American symbolism (which arrived at essentially the same arbitrary result—on this see Brinton) was developed in a totally different manner, what becomes of the sacred principle of "psychic unity," the "similarity of the working of the human mind"? I wonder which of the two explanations Dr. Goldenweiser would call the "dogmatic or uncritical method"? To indulge in pure speculation, dogmatic assertion and unsupported rationalization, or to go straight to the facts and recognize that the American thunder-bird and the winged snake with deer's antlers certainly came from the Old World?

We can trace the association of the deer with control of the waters from Babylonia along the whole Asiatic littoral, watching the symbolism gradually increase in richness and complexity as, in its passage from west to east, it blends with a variety of other elements, until eventually it emerges in the Chinese dragon, which it supplies with antlers.⁵

⁵ I have discussed the whole subject in the forthcoming report of my lecture on "Dragons and Rain-Gods."

In the light of the complex history and the scores of wholly chance circumstances that contributed to the making of this Asiatic wonder-beast, is it at all credible that the Algonkin and Iroquois serpent with wings and deer's horns is an independent invention?

I have so frequently discussed the question of man's inventiveness⁶ that it would be unjustifiable to take up more space for this matter here.

When Dr. Goldenweiser claims that Spencer, Tylor, Lubbock, Frazer and Lang "may have neglected to make sufficient use of the concept of the diffusion of culture through historic contact," I agree with him; but I think the words "may have" are superfluous. Yet American scholars, such as Brinton, Hopkins, Spinden and many others, as well as many writers, such as Keane, on this side of the world, have repeatedly attacked Tylor for *over-using* the concept of diffusion.

It is a quaintly piquant situation to find Tylor, who more than any one is responsible for the modern attitude of denial of these cultural migrations, being reproved by his more reckless followers for not pushing his views to the limits of absurdity, and Dr. Goldenweiser, in a letter that is frankly ultra-Tylorian, pretending to hold the scales impartially between the conflicting views.

It is very surprising that so eminent a scholar as Professor Hopkins joins in this attack on Tylor, especially as he can give no reason in justification of his attitude except the flimsy pretext that "we require more proof than Aztec pictures of hell to believe any such theory" ("Religions of India," p. 557, footnote 4). For the very chapter of Hopkins's book where this statement occurs is de-

voted mainly to the use of precisely the same kind of argument as he condemns when Tylor uses it. He is urging the claim that Indian culture exerted a great influence upon Greece from the sixth century B.C. onwards. The evidence he makes use of is of precisely the same kind as, but infinitely less voluminous and precise than, that which goes to prove an analogous influence of India in America. He rightly claims that "such coincidences are far too numerous to be the result of chance." But if that is so, why is it forbidden to use the same argument in the case of "the pictures of hell"? Are they the sort of thing two peoples would have independently invented?

But Professor Hopkins goes much further than this. In developing the argument (pp. 161 et seq.) that certain elements of culture in India can not be regarded as tokens of Aryan influence, he cites a very remarkable series of exact coincidences between complex Hindu and Iroquois beliefs and ideas. So intent is he upon the demolition of the Aryan argument that he does not seem to realize the more important outcome of his demonstration. For, if it is permissible to use the method of reasoning which he himself employs in the case of Greek borrowing from India, Hopkins has also proved up to the hilt, though without realizing it himself, the Asiatic derivation of many of the religious ideas of the American Iroquois. To quote his own words again, "such coincidences are far too numerous to be the result of chance."

In the light of our present knowledge it is now possible⁷ to refer to its original source the germ of a very large number of the elements in the Pre-Columbian civilization of America.

But I should not like Dr. Goldenweiser to mislead the readers of SCIENCE into the belief that I am ignoring considerations of the working of the human mind and of the importance

⁶ See for example "Ships as Evidence of the Migrations of Ancient Culture," *Journal of the Manchester Egyptian and Oriental Society*, 1916; also *Man*, February, 1916, p. 27: and if independent witness is desired, see Pitt-Rivers, "Evolution of Culture," p. 91 et seq.; the whole question has been discussed by Professor Frederick J. Teggart, of the University of California, in his admirable "Prolegomena to History," 1916, pp. 111 et seq.

⁷ I have in manuscript an analysis of many scores of American practises, beliefs and myths, each of them traced back to its home in the Old World. Some of these are now being published in the reports of two lectures, "Incense and Libations" and "Dragons and Rain-Gods," in the *Bulletin* of the John Rylands Library.

of local developments in shaping customs and beliefs and giving them their distinctive characteristics. What I object to on the part of ethnologists is not the use of psychological arguments, which are necessarily at the root of the whole matter, but the resort to an effete system of psychology which is utterly repudiated by practically all real psychologists, except Wundt and his disciples.

When a small band of immigrants, intent upon exploiting the mineral wealth, forces its way into a barbarous country, and, in virtue of its superiority of weapons or of skill and knowledge, is able to dominate the local people, and compel it to work for them, the stamp of the alien civilization, its practises, its customs and beliefs, can be imprinted upon a large servile population. Nor must it be assumed that the new learning is adopted wholly and without change. For every people has its own cherished beliefs and customs which no power can wholly eradicate. What happens in such cases is that the new practises are blended with the old; and in course of time, as the mixture becomes more and more intimately rationalized, a new and distinctive cultural compound is developed, which can not strictly be regarded either as the indigenous or the introduced culture, but a new structure which has been built up by the spirit of the local population out of the new and the old materials. Thus even when the same elements of a new culture are introduced into a series of localities the resultant civilizations are not identical; but each takes on its distinctive characters, which are determined partly by the circumstances under which the new leaven has been impressed, and partly by the nature of the pre-existing culture, and possibly to some extent by the character and abilities of the people of the country. For a people's aptitude and inclination to adopt alien practises clearly counts for a good deal in this process. Essentially the same external influences were brought to bear, in varying ways and in different degrees, upon India, Indonesia, Australia, eastern Asia, Oceania and America; but how strikingly different were the results in each of these domains!

The subject, however, is much too vast and intricate profitably to be discussed in a letter. I have already collected enough material for several large volumes on the part played by the "working of the human mind" in the history of civilization. All that I aim at achieving at present is to persuade ethnologists to do what is constantly being done in every *true* science, namely, impartially to examine the foundations upon which its theory rests. If they will consent to do this I have no doubt as to the outcome.⁸

G. ELLIOT SMITH

THE UNIVERSITY OF MANCHESTER, ENG.

THE AMERICAN PHYSIOLOGICAL SOCIETY

THE American Physiological Society held its 29th annual meeting in association with the Federation of American Societies for Experimental Biology and the American Association for the Advancement of Science in New York City December 27, 28, 29 and 30. The meetings were all held at the Cornell Medical College buildings where convenient arrangements had been provided also for the other societies of the federation. One of the most pleasant features of the meeting was the arrangement for luncheon, which brought together the members of the different societies.

On December 28 the annual federation banquet was held at the Hotel McAlpin with a large attendance. A similar dinner was held at the Chemist Club December 29.

The new members elected to the society were:

William T. Bovie, Harvard Medical School, Boston.
William John Crozier, Bermuda Biological Station for Research, Agassiz Island, Bermuda.
Admont H. Clark, Johns Hopkins Medical School.
Frank A. Hartman, University of Toronto.
S. H. Hurwitz, Hooper Foundation, San Francisco.
R. W. Keeton, Northwestern University.
Edward C. Kendall, Mayo Clinic, Rochester, Minn.
Charles E. King, University of North Dakota.
Dean de Witt Lewis, Rush Medical College.
David I. Macht, Johns Hopkins Medical School.
Frank C. Mann, Mayo Clinic, Rochester, Minn.
Victor C. Myers, New York Post-graduate Hospital Medical School.
Oscar H. Plant, University of Pennsylvania.

⁸ I think it is only right that your readers should be informed that my article in *SCIENCE*, August 11, 1916, was written in May, 1915, and that by a careless mistake, the uncorrected stenographer's copy was sent to you.