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SIGN LANGUAGE IN PRINT.

BY FREDERICK STARR, UNIVERSITY OF CHICAGO, CHICAGO, ILL.

MY attention has lately been called to a matter which seems to me of some interest. It is well known to all readers of *Science* that gesture language is a common means of communication between our different Indian tribes.

Mr. Lewis Hadley of Chicago is at present engaged upon a plan for reducing the sign language to print. The purpose of the work is benevolent and religious, the idea being to bring religious instruction to the old Indians. It is well known that old Indians will never learn to read our language. It is believed by Mr. Hadley and his friends that they will quickly learn a printed sign language. Of course, all these old men make constant use of gestures and signs; and, if they take kindly to the printed gestures, there is no question that considerable progress might be made.

Mr. Hadley has had difficulties to contend with in carrying out his work. He has been hampered by the lack of funds and by the novelty of the undertaking. In his first experiment he cut the dies for printing himself, and the resulting impressions were black designs with the figures in white lines, and the result was exceedingly ugly. He has since then simplified the designs and made them in the form of ordinary type, and has now an extensive font of several thousand types, which will be used in printing cards and tracts for the instruction of the Indians.

There are two points to be considered in reference to this plan: First, its feasibility; second, its methodology.

There are three questions that arise in reference to feasibility:—

1. Is there a universal sign language among the Indians?
2. Can the signs be represented by type?
3. Will the Indians care to learn it?

1. As regards the first of these, Mallery says that there is no absolutely fixed sign language in general use among the Indians. While this is true, it is also true that all Indians gesture, and the gestures are so natural and so self-expressive that there is no question that natural signs, although new, would be generally understood.

2. There is, of course, a difficulty in representing the gestures by type so that they can be readily recognized. This difficulty all who have attempted to work in the subject of gesture language realize. Mr. Hadley has changed the forms of his type repeatedly. He has produced finally what appears to be simple, plain, and easily understood characters. Many of these may have to be still further changed, but in large measure they meet the requirements.

3. There is a very serious question as to the favorable reception of this printed gesture language by the Indians themselves. It is, however, in a certain sense picture-writing, and picture-writing is natural to the North American. Mr. Hadley is doing, on a large scale and at one stroke, what the Indians have begun to do in many cases. Colonel Mallery has shown in his papers the

close relationship between gesture language and pictography. The picture character is often only an attempt to represent a gesture. This being so, it may be possible that a kind reception will be given by the Indians to the printed sign language.

As to the method of introducing the printed sign language into use, Mr. Hadley has devised a game of cards, which, he believes, will help greatly in the work of teaching. Each card has upon its face, in unusually large type, a gesture. Upon its back is printed the English equivalent for the gesture. The game to be played with these cards is based upon certain gambling games, already quite familiar to the Indian, and success in the game depends upon the Indian giving the English word for the sign represented. All games of an instructive kind are more or less of a nuisance, but it is not impossible that these cards may be successful in the way they are intended. Besides the game of cards, the purpose of which is really to teach the speaking and reading of English through the printed sign language, a considerable number of texts, mainly of a religious character, are to be issued. It is expected that an Indian who has a story or a passage printed in the sign character will himself make the signs represented, and by making the signs he will gain the idea to be conveyed.

Every text of the sign type has under it the English equivalent words. In order to convey an idea what this test is like, I present herewith a line of the text as it appears in print.



It will be seen that, quite apart from its religious and educational purpose, this matter is one of scientific importance, and we shall watch with interest how far it may succeed.

CURRENT NOTES ON ANTHROPOLOGY.—XXVIII.

[Edited by D. G. Brinton, M.D., LL.D., D.Sc.]

The Present Position of the Hittite Question.

THE ethnic position of the Hittites has been a perplexing question for many years. It seems to have been answered in a certain degree by the recent excavations of Von Luschan at Sindjirli, which is in ancient Hittite territory. Halévy has shown that the two stelae brought from there to Berlin are in a Phoenician dialect. The Hittites of the Bible were, therefore, probably Semites.

Unfortunately, this solution leaves the real problem untouched; for it is now clearly established by Belck, Lehmann, and others, that the mysterious syllabic inscriptions and bas-reliefs at Pteris, Nymphi, and about Lake Van, were not by the Semitic Hittites, but by a wholly different people, who called themselves Chaldi, after their chief divinity, Chaldis. To their land they gave the name Biaina (Urartu, —Ararat, in Assyrian), and their chief city Van, their own name of which was Tuspa-na, was founded about 833, B.C., by their early king, Menuas.

The confusion partly arose from the fact that the Semitic Hittites, previously tributary to the Assyrian monarchs, were subjugated by the Chaldi king, Argistis I., about 800, B.C.; and, further, that at the fall of the Chaldic kingdom, about the close of the seventh century, B.C., many of the Chaldic people were driven southward into Cilicia and its neighborhood.

The question therefore remains, Who were the Chaldi? The prevailing theory has been that their language had Mongolian or Turkish affinities; but Professor Sayce has pretty clearly shown that it had regular declensions, a nominative ending in *s*, an accusative in *n*, oblique cases in terminal vowels, and an adjective which followed the noun and agreed with it in these respects. This is not at all analogous to any Mongolian or Turanian language, and, if correct, disproves the theory.

A bolder one is advanced, not entirely for the first time, by M. Salomon Reinach in the *Révue Archéologique* for January. He maintains that the migration of the Chaldi, or supposed Hittites,

was from west to east; that they were emigrants from European lands; and, in fact, were none other than a part of our old friends, the Pelasgi of Greece; and these in turn were of the same stock as the Tyrseni and Etrusci of Italy. Sufficient can be said for the theory to make it worth further and serious investigation.

An older theory, to wit, that the true Hittite is an Indo-European language most akin to the modern Armenian and probably its ancestor, has been revived with considerable force by Professor Peter Jensen of the University of Marburg. His article is in the *Sunday-School Times*, March 25 and April 1. His criticism on Peiser's theory, that it is allied to the Turkish, is severe and merited. With reference to the puzzling and complex questions suggested by the inscriptions and ethnic types presented by the ancient monuments of Cilicia, he proposes the theory that the rulers of this district were at one time Semites or strongly semitized, while the mass of the population was of Indo-European blood. His opinion of his predecessors' studies is briefly summed up in these words, with reference to those of Sayce, Conder, Peiser, Ball, and Wright: "All are without foundation, and their results are destitute of value"!

A Linguistic Map of Guatemala.

Dr. Karl Sapper of Coban, Guatemala, has published in the first number of *Petermann's Mittheilungen* for the current year a map showing the present distribution of the native languages in Guatemala, accompanying it with a carefully prepared article on the dialects and culture conditions of the descendants of the aborigenes of that country. Much of it is from his own studies, much of it from the excellent works of Dr. Stoll. He does not seem to be aware of the publication by me of the material collected by Dr. Berendt on the Xinca, the Pipil of Acasaguastlan, and other dialects. He falls into the rather serious error, which I pointed out in a paper published by the Congress of Americanists (session of 1890), of locating a language of the Mixe group in Guatemala, though he adds that no signs of it now exist. It never was there. He fails to solve the only real obscurity which remains in the linguistics of Guatemala, that is, the identification of the Popolucan located by the historian Juarros at Conguaco, in the partido of Guazacapan, which was *not* Xinca.

The language of Yupiltepec he considers a dialect of the Zinca, and brings into closer relationship the Chorti and the Chol. His expressions about the dialect of the Cajaboneros are not clear; in one sentence he speaks of their tongue as containing elements fundamentally diverse, "ursprünglich fremd," to the Kekchi; and in another refers to this element as perhaps Chol, which is merely another Maya dialect.

While Dr. Sapper's work is open to these slight criticisms, it is in the main worthy of the highest praise.

The Earliest Extension of the Iron Age.

In these notes (*Science*, March 10) I referred to some recent studies on the early Iron Age in central Europe. The question still remains, When and how did the art of working iron reach those localities? Two valuable papers of late publication have interesting suggestions touching this point. One is on "Le Premier Age du Fer au Caucase," by M. Ernest Chantre, who for twenty years has travelled, studied and excavated in the Caucasus; the other by M. Louis Siret, scarcely less distinguished for his archæological campaigns in Spain. Some remarkable coincidences are pointed out by both.

M. Chantre finds that the most ancient sepulchres in Lower Chaldea which contain iron are shown by their funerary contents to be contemporaneous with the third and fourth dynasties of Egypt, at which period occur the first signs of this industry on the Nile. At the lowest, this would place them 2500 years, B.C. The knowledge of the metal reached the southern and central vales of the Caucasus about 1500 B.C., through the extension of a "Semitic-Kushite" people, who were the ancestors of the modern Ossetes. They were distinctly non-Aryans, and the art of working iron was not introduced by them into Europe. Later on, about the seventh century, B.C., their culture was deeply

modified by irruptions of Mongolic hordes from the East. (All this in spite of the fact that the modern Ossetes speak an Aryan tongue!)

The proof of this early Semitic influence is found in the identity of art-motives, decorations and methods, and especially in the numerous traces of the worship of the goddess Ishtar, the Astarte of the Phœnicians. In the Caucasus, as elsewhere, her favorite symbol, the dove, is constantly met with in ancient tombs; as is also that of the hand, employed in her rites as the symbol of adoration and peace.

It is true, as M. Chantre remarks, that in every station of the earliest iron age in Europe, from Greece to Scandinavia, we find figurines of birds, evidently sacred, and all to be traced to the dove of Astarte. They are proofs of what impressed M. Siret so much in his study of the earliest civilization of the Iberian Peninsula,—"the worship of a female deity represented under various symbols." He also, in his article in *L'Anthropologie*, 1892, No. 4, is forced by the results of his own excavations to assign this civilization to the daring early navigators of Semitic blood, to the Phœnicians, sailing from the far east of the Mediterranean, rounding the rocky shores of Spain in search of tin from the Cassiterides, or amber from the far-off shores of the Baltic. The first signs of iron there follow without a break on a highly developed bronze period; and its earliest discovered use was as rivets to fasten together plates of bronze. This indicates peaceable introduction and artistic growth, not the result of violence and conquest. The merchant, not the warrior, was the civilizer.

LETTERS TO THE EDITOR.

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On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Sham Biology.

THE article "On the Emergence of a Sham Biology in America" undoubtedly brought much joy to many botanists. Some of us know from experience that many American botanists are never so much tickled as when some one has gotten them to believe a zoölogist is hopping about a botanist and is worrying over the adhesive soil the botanist is trying to shake from his own trousers and boots onto those of the zoölogist.

Feeling himself above any and all of the charges made in the interesting tirade, the present writer has concluded he ought to at least make an attempt to show how strong a position his colleague had taken. No chuckling botanist can have any rational ground for gleefully pointing to me as a zoölogist badly hurt. Notwithstanding this, I am fully aware of the fact that nothing others may do or say is too minute to impel some people to strike their breasts, pour out eloquent prayers of thanks, and then go their way rejoicing over the capital they imagine can be made out of the sins of others. I also must say I am not at all sure of what my fellow-zoölogists will think of me for daring to answer for others. The unqualified and sweeping statements in several places technically include me, and this fact I offer as an excuse for attempting to indicate to botanists that the "sham biology" article is not so impregnable a piece of scientific work as I know many think it is.

It may be well to forestall possible taunting thrusts by stating that I have never desired to give a course in "general biology;" that I never attempted to plan, nor even thought of planning, a course in general biology to be given under my supervision, though the opportunity to do so was before me when I came to the University of Minnesota as an instructor. I have always insisted, and now insist, upon the independence, the autonomy of the two sub-departments of biology. My whole record stands as a proof of this, and therefore it cannot be said with justice that I belong to a class often called "sore-heads."

Let us at the outset agree to take the figures of speech for what they evidently were intended to illustrate, and not try to divert the real issues by seizing an opportunity to nag our immaculate brethren in botany.