

boys reads aloud, some of them often give, instead of a word, its synonyme, though the latter be quite different in sound from the former. "The boys who were most apt to do this were the boys whose power of hearing was already under suspicion; and I inferred that they associated the printed letters, not with their sound, but with the concrete thing which they represented, much as if they had been a picture."

Another interesting observation is that of a boy of eleven years of age, who is a bad speller, and, when writing from dictation, makes mistakes in words which have an *r* or an *l* in them. He cannot pronounce those letters; but his failure is believed to be the result of a defect of ear, though he is by no means deaf, quite as much as the result of a defect of tongue or palate. Some of his misspellings are 'sunderelents' for 'sundry rents,' 'compreated' for 'complicated,' 'laserlacions' for 'lacerations.'

The writer points out that a want of power to distinguish vowel-sounds is quite as likely to be the cause of bad spelling in common words as carelessness amounting to *malice prepense*, or a weakness in the machinery which connects the movements of the hand with the orders of the ear. He continues, "We might have expected, that, on the analogy of color-blindness, vowel-sounds would be more likely to be confused than consonant-sounds. So far as my present experiments have gone, I infer that the inability to distinguish consonants is as common as a want of discrimination between vowels."

"The confusion caused by explosive consonants is, however, more remarkable than that from vowels; the inexperienced ear which is dull at catching consonants is capable of any distortion of sounds. To illustrate this, an experiment was tried with a class of eleven boys, averaging ten and a half years of age, and all able to read fluently, one or two of them being somewhat extensive readers. Some short ordinary words were selected, which nearly all got right, and then words specially to test the power of hearing, some of which, it was hoped, the subjects of the experiment had never heard before. Here are the variations of five words (the italicized vowels show interchange in the hard-vowel scale):—

	different capable	ultramarine	spectroscope	Epaminondas
1.	dirfreant capbul	ultramean	spaccrow	apnonas
2.	different	ultramarine	specourouscope	aparmondas
3.	diferent capeperbul	altrmerine	speckshow	aponedondas
4.	diferrent capperble	altrmererein	speckros-cop	achappynomeen
5.	diferant cameble	oltremere	spkerrope	appanandex
6.	drifrent capable	untummerrein	specteroskop	eupameondeous
7.	diffrent capabybely	ultriean	spesptroscope	emeandass
8.	drifent capebibel	ultrernn	spectuscope	epermondes
9.	different capebale	ultrmeriem	specktrocope	apporymondas
10.	differant ackable	ultomarien	spreting	apanenondes
11.	differint caperble	ultrumeree	spatroscope	appongamanges

"The room was a small one, and the words slowly pronounced twice, each word being written immediately after it had been read out. The majority of these boys are unusually intelligent. The worst speller but one recited, soon after his eighth birthday, 'The Battle of the Lake Regillus.'

"Twenty words in all were read out. Among them were 'yellow,' which all got right; 'instance,' five right, one of the best readers giving 'insentsess'; 'aniline,' of which there appeared these variations, 'haniyne,' 'anileling,' 'anelile,' 'animiene,' 'aleline,' the rest being at any rate phonetically correct.

"In the majority of these misspellings we at once detect want of experience in the use of the arbitrary connection between signs and sounds, and feel confident of improvement in course of time; but when we find a particular phonetic mistake frequently recurring, such as the substitution of *l* for *n* in 'aniline,' we suspect some defect either in the writer or dictator; and if the possibility of mispronunciation in the reader is eliminated, then we have to look for defect of ear or hand, or both, in the writer. Supposing that in correcting the misspellings we find that one or two subjects cannot recognize a word after the correct spelling has been shown them, while others have no difficulty, we must conclude that the ear is at fault, in the one or two; and if we find that the same individuals can recognize some sounds and not others, then the phenomenon of sound-blindness is established, and we have a satisfactory reason for the fact that some persons seem to spell natu-

rally, while others never learn; as, indeed, how should a man learn to spell even phonetically to whom not only the printed sign, but also the distinction of sounds, is arbitrary and conventional? and how should he not learn whose ear is a torturing conscience? Sound-blindness will account for dialectic variations. The ear being, as physiologists tell us, an even more delicate and complex structure than the eye, we can understand that the physical conditions of certain localities may produce insensibility to particular variations of sound. Perhaps the interminable rattle of London may account for the awful vowel-system of commercial men in the metropolis."

ETHNOLOGY.

American Languages.

IN the Proceedings of the Canadian Institute, Toronto, October, 1887, Mr. A. F. Chamberlain discusses the relation of American and Asiatic languages in connection with the question of the origin of the Indians. The concluding remarks of his article are so judicious that we wish to repeat them here: "The case for the eastern Asiatic origin of the American peoples rests too much upon apparent phonetic resemblances. Before any (phonetic) law like that of Grimm can be discovered and demonstrated between the American and related linguistic families, a thorough understanding of the relations which exist between the individual members of each branch of the American stock is requisite and of paramount importance." But we believe that an application of such principles to Chamberlain's own remarks will show that they are not well founded.

The author first discusses the Eskimo dialects, and gives a brief comparative vocabulary of different tribes in order to show their similarity. The words contained in this table are not taken from the best originals, and, besides, words of different meaning are compared. What the author considers as differences of dialect must in many cases be ascribed to a difference of the grammatical forms, or to the nationality of the collector. As his material is of so little value, the comparative Eskimo-Turanian vocabulary cannot be considered a good proof for his opinion that the American and Turanian languages have a common origin. We cannot consider the similarities of sound between the two groups other than fortuitous. There is another point of view in the paper which we cannot accept. Chamberlain uses the migration legends as a proof of earliest migrations. To a certain extent this may be right, but it is well known, that, if a tribe changes its seat, its legends are attached to new localities, and for this reason no conclusion on the migrations in a very remote period can be made from such facts. The migration of legends among aboriginal tribes is a problem of great difficulty, and one in which rash conclusions ought not to be made. The study of European folk-lore has shown that the origin of legends and their migrations are often wonderful, and that the most painstaking care must be taken in dealing with this subject. These considerations prevent us from accepting Chamberlain's theories as superior to those propounded by other authors. Our knowledge of American and North Asiatic ethnology and philology has not yet arrived at that stage in which we can deal satisfactorily with the question discussed by Chamberlain.

ETHNOGRAPHICAL MUSEUMS.—Dr. Kristian Bahnson publishes an interesting account of his thorough study of the principal European ethnographical museums ('Ethnografiske Museer i Udlandet,' in *Aarbøger for nord. Oldk. og Historie*). His concluding remarks are of particular interest, as they refer to the much-discussed question of museum arrangement. He shows that the arrangement according to objects has gradually been abandoned by all museums, and that the ethnological method, i.e., the arrangement according to tribes, has been adopted in its stead. He says about the former, the sociological plan, "The plan as a whole is absolutely wrong, in the first place, because those groups which ought to be the principal divisions in an ethnological museum, are made subdivisions. The ethnic individuality, which is a whole, is decomposed into a great number of elements. By an arrangement according to the character and purpose, the objects are taken out of their natural place, and they want the environment, which alone can explain their real meaning. In each stage of civilization there is a deep connection between the several ethnological peculiarities

of a people. Although this connection is not that 'harmony' from which conclusions have been made from the development in one region upon a similar development in another, it exists, influenced by numerous conditions, — climate, nature of the land, mental development, intercourse with adjacent countries, etc., — and it is necessary to convey the idea of such a connection in a museum, the object of which is to throw light upon the origin of a certain stage of civilization. Groups of objects, severed from their ethnic environment, may be valuable for the study of industry and technology, but an ethnographical museum cannot be arranged in this way, as objects belonging together from an ethnographical point of view are separated, while others of entirely different origin are united in one group. The other system of arranging museums is the ethnographical one. The system according to which all objects belonging to one tribe are placed together, and the single tribes are joined to ethnic groups according to their affinity, is so simple and natural in itself, that it has been generally accepted. It is a long time since ethnologists have ceased to invent more or less valueless systems of mankind which played an important part in the early history of ethnography. The attempt is being made to construct, by the use of strict inductive methods, the science of ethnology on the foundation of careful researches on single tribes and nations; and not until this has been done will problems of the mutual relation of tribes be taken up. In this way the foundation is laid for researches on the history of civilization. There is no lack of theories on the material and intellectual development of man, on the progress and decline of primitive nations; but here, also, the only way to reach satisfactory results is the inductive method; here also, a vast number of researches is necessary, before it is possible to gain that general point of view which is the ultimate aim of ethnology. In consideration of these facts, the ethnological arrangement is the only one fit for a museum. It does not give a solution of great problems, it does not give results of doubtful value, but it gives, what must be asked by the descriptive treatment of ethnography, the material arranged in such a way that it may be as easily accessible to students as possible. It is only in this way that the museum is able to illustrate the different states of civilization which occur in a single tribe, and, as the principal groups of nations are kept together, it shows the peculiar development in each group. All museums of any importance have adopted the ethnographic principle in laying out their plans, but not all have been equally successful in carrying their plans out. There are collections in which incidental arrangements disturb the general plan, and they show that the utmost consistency in adhering to the plan is necessary, else the museum will become a mere store-room." Dr. Bahnson further says, that, in carrying out such a plan in a large museum, the ethnological collections of each tribe must be subdivided according to sociological principles. These statements and views of the author are of great weight, as they are based on the study of a great number of collections and of a vast material. He shows plainly that there is no foundation to the alleged impossibility of arranging ethnological collections on an ethnological plan, and his idea that such a museum is the only one that can serve for the study of the history of civilization is undoubtedly correct.

THE BASQUES. — Chamberlain's view in regard to the connection between the Basques and Americans is in part founded on the alleged similar character of their languages. Many grammatical forms of the Basque consist of a combination of mutilated elements, and it was believed that this process was similar to the synthesis of American languages. Recent researches do not confirm this view. W. J. van Eys, one of the most learned Basque scholars, considers such combinations or contractions as similar to those occurring frequently in the vulgar dialects of Romanic and Teutonic languages. He mentions the Dutch *hy't'm*, which stands for *hebt gy het hem*. Our 'ain't ye' for 'are you not,' and others, belong to the same class. He ascribes the occurrence of such contractions in literary Basque to the late date at which the literature of this people developed. Gerland, who gives a very clear sketch of the Basque language and its relation to the ancient Iberian in 'Gröber's Grundriss der romanischen Philologie,' expresses the same opinion, and thus far-reaching conclusions on the connection between Americans and Basques must fall to the ground.

HEALTH MATTERS.

Cholera and Cold Weather.

IN a recent editorial in the *New York Medical Record* the statement was made that "cholera is always stopped by cold weather, and an epidemic here now would be impossible." In a letter to the editor of that journal, Dr. Reginald H. Sayre of New York takes exception to the statement, and quotes a number of instances to show that cholera is one of those scourges whose march is not stopped by heat or cold, high or low altitude, dryness or dampness, or any other condition of the weather. He says, —

"In 1830 the cholera appeared in Moscow in the month of October, and continued its ravages until the end of December, in spite of the severities of a Russian winter, and caused the death of 8,130 persons out of a population of 250,000, or about 1 in 30. From Moscow it went north to Yarasy, thence to Rybinsk, sixty leagues north of Moscow, where it appeared on March 19, 1831, in spite of the ice and snow which covered the ground.

"In October, 1831, the cholera appeared in Great Britain, and continued there until March, 1832, doing most of its destruction in December. About one-third of the people affected died.

"On the 27th of March, 1832, the disease appeared in Paris, and the mortality was so frightful that 861 people died in ten days.

"In 1848 the emigrant ship 'New York' left Havre on the 9th of November, having no sickness on board, and no cholera being then in Havre. During the voyage the weather became bitterly cold. There were some German emigrants on board, from a town where cholera had prevailed, who had a trunk which had belonged to a man who had died of cholera. They opened the trunk, took out the clothing, and wore it. On Nov. 22 a child died of cholera, and seven persons in all succumbed to it before reaching New York harbor. They were strictly quarantined, and the disease limited to those who died on Staten Island in the quarantine.

"About this same time another vessel from Havre, bound for New Orleans, developed the cholera on the twenty-seventh day out, and, owing to imperfect quarantine regulations, the disease spread rapidly through the town soon after the arrival of the vessel, there being then no other cases in the United States except those in the quarantine on Staten Island. From New Orleans the disease travelled to Memphis, appearing there toward the end of December, and at St. Louis in the first week of January, 1849. Toward March several places in the Upper Mississippi valley were affected, and then gradually the disease moved east through Chicago, which it reached in May, to New York, which became infected then, *and not till then*, although the disease had been imported to the city six months previously, but had not been allowed to land; and the city in this way kept free from infection until the cholera effected a flank movement, by way of New Orleans, and attacked her in the rear, having made its progress in spite of the winter, and having attacked the cities through which it passed in the cold weather.

"These facts in regard to the prevalence of cholera in spite of cold, and the well-known futility of a quarantine on land, make any attempt to lull the medical profession into a false sense of security fraught with great danger to the country, and I have therefore wished to call attention to the fact that cholera is *not* stopped by cold, and that to be quarantined effectively it must be arrested in our ports, which can only be done by having a general quarantine under the direction of the Federal Government."

In answer to this, the editor of the *Record* says that cholera has never prevailed in New York City in the winter-time, and rarely in any northern latitude save under very peculiar and exceptional circumstances. In support of this view, he quotes from Ernst's 'Reference Handbook of the Medical Sciences,' which states that "the progress of an epidemic is invariably arrested by cold, the winter season having always stopped those of which we have any record." He further says that cholera has frequently been imported into warm New Orleans in the winter-time, notably in 1873, when it commenced in February. But it did not winter over that year, or notably any other year. But constant importations into New Orleans almost every month of the year during the California gold-fever times sent much cholera to St. Louis and Chicago, and other Western places, almost every month in the year; so that it seemed to winter over, but, in fact, was kept alive by almost incessant new importations. The effect of cold on the further spread of cholera,