rule are much the same as those enumerated in the above paragraphs. Of the three pronunciations of this termination *-īde*, *ĭde*, and *ĭde*, in varying degrees of usage amongst us, the second appeared undoubtedly to be the most preferable; *-īde* is an uncommon, almost unnatural, pronunciation of the vowel in English, although it would bring our usage into unison with that of European countries, and simplify phonetic values for the ears of foreigners; *-īde* leads frequently to confusion with *-īte*, and is the value of *i* farthest removed from European usage; *-ĭd* approximates closely to the Continental *ĭ*, into which it is easily lengthened, is readily recognized by the foreign ear, is not confused with the termination *-īte*, is in line with present phonetic progress, and has the backing of authority and usage. The short sound of *i* naturally dictates the dropping of the final *e*. "According to Smart and Cull, chemical terms ending in *-ide* — as bromide, chloride, etc. — should be pronounced with the *i* long; but all other orthoëpists are unanimous in making the vowel short; and the propriety of the latter mode of pronunciation is established by the fact that this whole class of words is not unfrequently spelt without the final *e*, thus *bromid*, *chlorid*" (Webster's Dictionary, "Principles of Pronunciation," p. xliv.).

In conclusion, it may be said that the chemical section of the American Association recognizes the fact that there is still room for advancement in the path of phonetic reform, and that questions may still arise with regard to divergent usage or defects in existing rules. The task of collecting and collating such questions and of presenting them at a later date to the Association for action has been assigned to Professor Jas. Lewis Howe of Louisville, who will gladly receive all information, suggestions, or propositions pertinent to the subject from those interested in the perfecting of our chemical nomenclature.

BOSTON SCHOOL-BOYS.

BY FRANCIS GALTON, F.R.S., LONDON, ENGLAND.

NUMEROUS results may be shown to flow from the excellently arranged data in the valuable memoir of Professor H. P. Bowditch on the Growth of Children (Twenty-Second Annual Report of the State Board of Massachusetts, Boston, 1891). Permit me to draw attention to two of them.

It is necessary to premise that the method was adopted by him of describing classes by means of eleven percentiles, but, for the present purpose, three are enough, namely, the 10th, 50th, and 90th. In other words, it is sufficient now to deal with the statures of the persons who occupy those posts in any class along whose length 100 posts have been marked at equal intervals. It follows that 10 per cent of the whole class are shorter than the 10th percentile and 90 per cent are taller. These conditions are reversed in respect to the 90th percentile; as for the 50th, it is the median value, which one half of the class falls short of and the other half exceeds. The median in most series differs little from the arithmetical mean, and may be used instead of it, as a serviceable standard of comparison.

The variability of a series may be measured by the difference between any two named percentiles. The wider these are apart the more is the scale magnified; on the other hand, the less trustworthy does the measure become. In the present series we can with propriety use the difference between the 10th and the 90th percentiles, but we cannot in all cases, owing to the paucity of data, use that between the 5th and the 95th; the former will therefore be here adopted as the measure of variability.

In order to compare on equal terms the variability in stature of growing boys at different ages we must so reduce their measures that the median shall in all cases be the same. It is customary for this purpose to take the median as 100, but there is more significance in the results when it is taken at a value that represents the average stature, or thereabouts, of male adults. Here it will be taken at 67 inches. In the following table the 10th and 90th percentiles for the several ages are those given by Bowditch, after multiplying them by 67, and then dividing the result by the median stature at that age.

Calculated from Tables by Bowditch of Heights of Boston School-boys.

Age last Birthday.	Of American Parentage.					Of Irish Parentage.				
	Number of Cases.	Median.	Percentiles × (67 ÷ Median).		Differences.	Number of Cases.	Median.	Percentiles × (67 ÷ Median).		Differences.
			10°	90°				10°	90°	
5	201	41.84	62.9	71.0	8.1	366	41.70	63.3	71.2	7.9
6	342	44.00	63.5	71.1	7.6	503	43.90	63.3	70.7	7.4
7	369	46.36	63.5	70.9	7.4	562	45.68	63.5	70.9	7.4
8	407	48.34	63.2	70.9	7.7	588	47.80	63.6	70.6	7.0
9	381	50.07	63.5	71.2	7.7	556	49.61	63.7	70.9	7.2
10	360	52.24	62.7	70.5	7.8	571	51.62	63.6	71.1	7.5
11	350	54.14	62.5	70.5	8.0	548	53.17	63.4	70.6	7.2
12	373	55.68	63.4	72.2	8.8	497	54.89	63.2	70.9	7.7
13	391	58.14	62.3	71.8	9.5	463	56.58	63.7	71.9	8.2
14	386	60.77	62 6	72.8	10.2	334	58.81	63.3	71.5	8.2
15	342	63.17	62.3	71.4	9.1	155	60.98	63.0	71.9	8.9
16	232	66.03	62.5	69.9	7.4	61	64.42	(61.8)	(70.4)	(8.6)
17	128	69.39	63.2	71.0	7.8	26	Too few.			(?)

On examining the columns of differences, we find a remarkable increase in the differences between the 10th and 90th percentiles during the interval between the ages of 11½ and 15½ years; that is, of boys who at their last birthday were 11 or 15 years old. The period in question is that during some portion of which the growth is apt to be temporarily accelerated, but the precise epoch of acceleration differs; some boys being more precocious than others. Consequently the variability among boys of the same age, between the ages of 11½ and 16½ years, is greater than at other times. The point to which I wish now to direct attention, is the much greater variability during this period of the children of Americans than of those of Irish, for which it seems difficult to account. It can hardly be owing to variations of nurture, because its influences would probably be greatest on those classes who were least assured in their habits of life; now it is difficult to suppose that the Irish in Boston are, as a class, better established and more well-off than the Americans. As regards the effects of race, it is true that the Americans are more mixed in origin than the Irish, but we should have expected purity of race to manifest itself by a reduced variability at all ages, and not only at the particular period we are considering. However, it seems to be otherwise, and that the great variability of American children at the time in question may really be due to their mixed ancestry. In confirmation of this variability being a racial effect, we note how much earlier the epoch of its increase sets in among the children of Americans than among those of Irish, the difference amounting to at least one year. Anyhow, these statistics suggest the possible existence of an hitherto unobserved physiological difference between the children of the Americans and of the Irish, which might repay investigation.

A considerable agreement will be found in the figures contained in each of the four columns of percentiles in the table; their variations ranging through 1.2, 1.9, 0.7, and 1.3 inches, respectively. In other words, they range between limits that are hardly more than one inch on the average apart, while of course the range in other percentiles that are nearest the median is progressively smaller, till at the median itself the range is *nil*. There is, therefore, a fair approximation towards constancy in the ratio between any given percentile and the corresponding median that holds good for all these ages. It follows that if we are given all the eleven percentiles of stature that are found in Bowditch's memoir, together with the median heights for the several successive ages, we should have sufficient data to reproduce, in a roughly approximate way, the entire table of distribution of growth. The variability and the median are not such independent

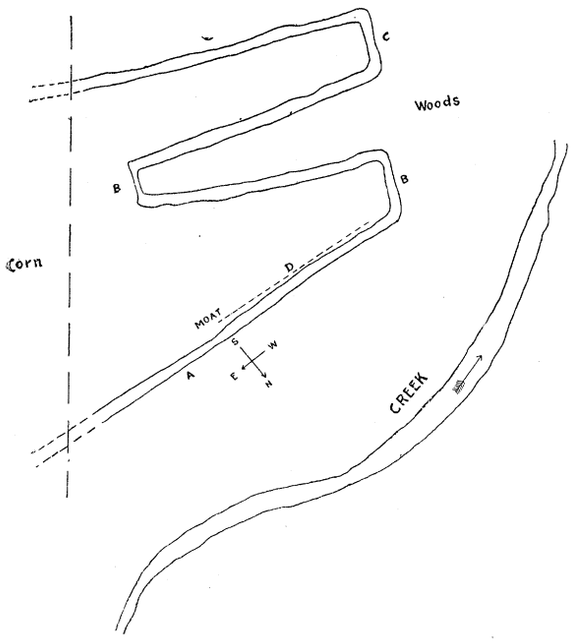
values as they are commonly considered to be. As, for example, the amount of the changes in the length of a chain under the influences of changing temperatures is related to the length of the chain, so we should expect the variability in the growth of large organisms to be on a larger scale than in small ones. There are more cells affected simultaneously by the same environing conditions. The rationale of a connection between the variability and the median may in some cases admit of being clearly made out, and in all cases it deserves more thought than it has hitherto received.

AN ARCHÆOLOGICAL DELUSION.

BY THOMAS GORDON KING.

THE daily papers have lately published accounts of a new "serpent" effigy. It existed in southern Ohio, in Warren County, and, according to two doctors of the neighborhood, measured some nineteen hundred feet in length. It was said to surpass the famous Adams County serpent.

Professor Putnam's assistant, Mr. H. I. Smith, spent some time surveying the structure, this summer. He trenched the embankment in several places and searched the neighboring fields for traces of a village site. The accompanying rough outline gives an idea of the "serpent" so far as it can be traced. In the cornfield it will be seen that the embankment cannot be distinguished; in the woods it is plain. The part in the woods, which at present is some two feet in height, does not appear to be serpentine in character. It is almost unnecessary to add that if the remaining part of the structure does not represent a serpent, the obliterated portion never did. There is not the slightest grounds for the assumption that this figure in any way resembles the Adams County effigy. The latter is laid out in graceful curves, which *suggest* the character of the effigy. The embankments of the Warren County structure resemble those of Fort Ancient. The long straight line *A*, and the sharp, squared bends *B* and *C* are the exact counterpart (although much smaller) of certain parts of south Fort Ancient.



A live snake could not take the form of this "new serpent" without breaking his back in three places. (I write under the impression that aborigines imitate living and not dead animals). There is a slight moat at the base of the embankment, which, although nearly filled, can still be traced. To one who has seen all the shell, bone, stone, and clay representatives of serpents and serpent-symbols displayed in the museums of this country, the "new serpent" does pot appear serpentine. I cannot see how the angular corners *B* and *C* and the moat *D*, and the embankment *A*, mark other than parts of a peculiar defensive earthwork.

The primitive Americans in drawing, moulding, building, or sculpturing snakes evinced a certain similarity of idea in design, and employed a common mode of execution. Yet this "new serpent" has nothing in common with other serpents! (Read Holmes on "Art in Shell.") As this new serpent is such a poor representative that Professor Putnam and other competent judges dare not place themselves on record in naming it, I have no hesitancy in calling it a rude fortification. The native Americans were sufficiently competent to execute a figure with such distinctness and closeness of resemblance as would allow of no dispute. Those who are interested in following the discussion further will please compare the diagram submitted with Squier and Davis's plan of the Adams County effigy. There are many similar combination works in the Ohio Valley, and it is probable that the thorough exploration of several might furnish evidence as to the purpose for which they were erected.

LETTERS TO THE EDITOR.

Man and the Glacial Period.

I ACKNOWLEDGE with pleasure the courtesy with which Dr. Brinton, in his review of "Man and the Glacial Period," has dealt with the question of the genuineness of the reported discoveries of implements in the glacial gravels of the United States. This, of course, was the first question to be settled, Were implements of human manufacture really found in undisturbed strata of gravel which was deposited during the glacial period? If this question is settled in the affirmative, then all glacial geology has direct bearing upon the question of archæology. If it is decided in the negative, glacial geology remains the same, but it ceases to have interest in connection with archæology. I am glad to have the issue so clearly made by Dr. Brinton, and thereby to have occasion to present more specifically my reasons for belief in the genuineness of these discoveries.

The evidence naturally begins with that at Trenton, N. J., where Dr. C. C. Abbott has been so long at work. Dr. Abbott, it is true, is not a professional geologist, but his familiarity with the gravel at Trenton where he resides, the exceptional opportunities afforded to him for investigation, and the frequent visits of geologists have made him an expert whose opinion is of the highest value upon the question of the undisturbed character of the gravel deposit. The gravel banks which he has examined so long and so carefully have been exposed in two ways: 1st, by the undermining of floods on the river side, but principally by the excavations which have been made by the railroad and by private parties in search of gravel. For years the railroads have been at work digging away the side of the banks until they had removed a great many acres of the gravel to a depth of twenty or twenty-five feet. Anyone can see that in such conditions there has been no chance for "creep" or landslides to have disturbed the stratification; for the whole area was full of gravel, and there was no chance of disturbance by natural causes. Now Dr. Abbott's testimony is that up to the year 1888 sixty of the four hundred palæolithic implements which he had found at Trenton had been found at recorded depths in the gravel. Coming down to specifications, he describes in his reports the discovery of one (see "Primitive Industry," 492) found while watching the progress of an extensive excavation in Centre Street, which was nearly seven feet below the surface, surrounded by a mass of large cobble stones and boulders, one of the latter overlying it. Another was found at the bluff at Trenton, in a narrow gorge where the material forming the sides of the chasm had not been displaced, under a large boulder nine feet below the surface (ib. 496). Another was found in a perpendicular exposure of the bluff immediately after the detachment of a large mass of material, and in a surface that had but the day before been exposed, and had not yet begun to crumble. The specimen was twenty-one feet from the surface of the ground.

In all these and numerous other cases Dr. Abbott's attention was specially directed to the question of the undisturbed char-