stant, and opens the terminal end of the corresponding tube near the subject's nose.

A pneumograph records the respiration. The pulse, vaso-motor and respiratory curves, the signal and time records (in seconds) are all traced in ink on a horizontal kymograph.

Explanation of Natural Immunity. GEORGE M. STERNBERG.

Dr. Sternberg, after a review of the experimental evidence relating to the cause of the natural immunity which exists among animals against parasitic invasion by various pathogenic bacteria and by putrefactive microörganisms, said that the experimental evidence submitted, considered in connection with the extensive literature relating to 'phagocytosis,' leads us to the conclusion that natural immunity is due to a germicidal substance present in the blood serum, which has its origin (chiefly at least) in the leucocytes, and is soluble only in an alkaline medium. And that local infection is usually resisted by an afflux of leucocytes to the point of invasion, but that phagocytosis is a factor of secondary importance in resisting parasitic invasion.

WARREN P. LOMBARD,
UNIVERSITY OF MICHIGAN. *Secretary for 1894.*

*AN INHERENT ERROR IN THE VIEWS OF GALTON AND WEISMANN ON VARIATION.**

WEISMANN'S name has become so intimately associated with the doctrine of germinal continuity that he is often regarded as its first advocate, although it is an old conception which has found expression in many writings.

Among others I myself stated it in the following words in a book printed in 1883, before the publication of Weismann's first essay on inheritance.

"The ovum, like other cells, is able to reproduce its like, and it not only gives rise, during its development, to the divergent cells of the organism, but also to other cells like itself. The ovarian ova of the offspring are these latter cells or their direct unmodified descendants."

After the appearance of Weismann's essays, and the revival of discussion on the views of Lamarck, I was much surprised to find my book referred to as a Lamarckian treatise, and my reason for quoting this passage now is not to claim priority, but to show that, in 1883, I, like Weismann, attributed inheritance to germinal continuity.

I may take this occasion to say that I still regard inheritance as a corollary or outward expression of the continuity of living matter, although I am less confident than I was in 1883 of the importance of the distinction between somatic and germinal cells. So much for the doctrine of germinal continuity.

Passing now to another topic, we find that the two most prominent writers on inheritance, Wiesmann and Galton, base their views of variation on the assumption that, at each remote generation, the ancestors of a modern organism were innumerable, although a little reflection will show that this assumption is untenable.

Weismann, at least in his earlier and simpler writings, finds the cause of variation in the recombination, by sexual reproduction, of the effects of the diversified influences which acted upon the innumerable protozoic ancestors of each modern metazoon.

If it can be proved that these protozoic ancestors were not innumerable, but very, very few, and that these few were the common ancestors of all the modern metazoa, his position is clearly untenable.

Galton's view of the cause of individual diversity is very similar to Weismann's. He says: "It is not possible that more than one-half of the *varieties* and number of the parental elements, latent or personal, can on the average subsist in the offspring.

* A paper read, by invitation, at the meeting of the Society of Naturalists, in Baltimore, Dec. 27, 1894.

For if every variety contributed its representatives each child would on the average contain, actually or potentially, twice the variety and twice the number of the elements, whatever they may be, that were possessed at the same stage of its life by either of its parents, four times that of any of its grandparents, 1024 times as many as any of its ancestors in the tenth degree and so on."

As he holds that each offspring must therefore get rid, in some way, of one-half the variety transmitted from its ancestors, he finds an explanation of the diversity between individuals in the diversity of the retained halves of their variety.

Each person has two parents and four grandparents; but even in a country like ours, which draws its people from all quarters of the earth, each of the eight grandparents is not always a distinct person; for when the parents are cousins, this number is six, or five, or even four, instead of eight.

Among more primitive people who stay at home generation after generation, and marry within the narrow circle of their neighbors, a person whose ancestors have transgressed none of our social laws may have a minimum ancestry of only four in each generation.

The maximum ancestry and the minimum fixed by our customs are given for ten generations in the two lines below.

2–4–8–16–32–64–128–256–512–1024=2046.
2–4–4–4–4–4–4–4–4–4=38.

Few persons who can trace their ancestry back for ten generations are descended from 1024 distinct persons in that generation, and in all old stable communities of simple folks the number is very much smaller. In the long run the number of ancestors in each generation is determined by the average sexual environment, and it is a small and pretty constant number.

All genealogy bears indirect evidence of this familiar fact which has not been adequately recognized by students of inheritance.

I have made a computation from the history of the people of a small island on our Atlantic coast. They lead a simple life, or have done so in the past, but most of the men have been sailors, and have ranged much farther in search of mates than agricultural people. I have selected three persons whose ancestry is recorded in detail for some seven or eight generations. These three persons have no parents or grandparents of the same name, and they would not be popularly regarded as near relations, although two of their twelve grandparents were cousins. The generations are not quite parallel, and the period covered by eight in one line is covered in the two others by about seven, and it may be put at about $7\frac{1}{2}$ for the three. In $7\frac{1}{2}$ generations the maximum ancestry for one person is 382 or 1146 for three persons.

The names of 452 of them, or nearly half, are recorded, and these 452 named ancestors are not 452 distinct persons, but only 149; many of these in the remoter generations being common ancestors of all three persons in many lines. If the unrecorded ancestors were interrelated in the same way as they would surely be in an old community, the total ancestry of the three persons for $7\frac{1}{2}$ generations would be 378 persons instead of 1146.

Few persons know even the names of all the living descendants of each of their sixty-four ancestors of the sixth generation, and marriage with one of them is a pure chance, depending on the size of the circle of acquaintance and the distance to which ancestors wandered.

If a city like Baltimore, where the strangers to each one of us outnumber our acquaintances a thousand fold, could be quarantined against people from outside for a thousand years, each generation would be much like the present one so far as known relations are concerned, although at the end of the period the inhabitants would certainly not be descended from the Baltimorians of our day, but from only a very few of them. Most of our lines would be extinct, and the few which survived would include most of the Baltimorians of the year 2900 among their descendants, who, while unconscious of their common origin, would be allied with each other by common descent from their virile and prolific ancestors of the year 1894.

This is proved indirectly but conclusively by genealogical statistics, and while a thousand years are but as yesterday in the history of species, zoölogical considerations furnish evidence that allied animals at two successive geological periods must be related like these successive generations of Baltimorians. Of all the individuals of a species which lived at a given period, very few would have descendants at a later period, and these few would be the common ancestors of all the individuals which represent the stock at the later period.

The extinction of species is a familiar conception. The extinction of the lines of descent from individuals is no less real, and infinitely more significant in the study of inheritance.

As we trace back the ancestral tree it divides into two branches for the parents, and again into four and eight for the grandparents and great-grandparents, and so on for a few generations, but a change soon takes place. The student of family records may be permitted to picture genealogy as a tree whose branches become more and more numerous as we get farther and farther from the starting point; but this cannot be permitted to the zoölogist.

On the contrary, we must admit that, on the average, the number of ancestors in each generation can never be greater than the number of individuals in the average sexual environment. It may be very much less, however, since most of the individuals in each generation must fail to perpetuate their lines to remote descendants.

Now no animal in a state of nature ranges so far as man in search of a mate, and the sexual environment of many plants and animals, such as the fishes in a brook or a pond, or the parasites in the intestine of a mammal, is very narrow. While new blood, no doubt, finds its way in from time to time, this is more than balanced by the extinction of genetic lines. The series of ancestors of each modern organism is long beyond measure, but the number of ancestors in each remote generation can never be very great, though it may be extremely small.

The data of systematic zoölogy also force us to believe that the ancestry of all the individuals of a species has been identical, except for the slight divergence in the most recent part of their history.

The zoölogist must picture the genealogy of a species not as a tree, but as a slender thread, of very few strands, a little frayed out at the near end, but of immeasurable length and so fine that the thickness is as nothing in comparison. The number of strands is fixed by, but is much smaller than, the average sexual environment. If we choose we may picture a fringe of loose ends all along the thread to represent the ancient animals which, having no descendants, are to us as if they had never been. Each of the strands at the near end is important, as a possible line of union between the thread of the past and that of the distant future.

The gist of the whole matter is this, that we must picture this slender thread as common to all the individuals of the species, whose divergence from each other is infinitesimal compared with the ancestry they share in common.

The branches of a human genealogical tree diverge for a few generations by geometrical progression, but we soon find traces of a change, and if the record were long enough to have any evolutionary significance we should surely find all the members of a species descended from a few remote ancestors, and these few the common ancestors of all. If one metazoon is descended from pre-Cambrian unicellular ancestors, the same unicellular individuals were the common ancestors of all the metazoa, and we may be confident that there were not very many of them in each generation. It is quite possible that they were even so few as a single pair or even one.

There is nothing very novel in all this. Galton has himself devoted an appendix to the mathematical study of the extinction of family names, although he and other writers on inheritance seem to forget it when they assume that the remote ancestors of two persons, A and B, were, like the parents, distinct individuals, and that the offspring must have twice as much ancestry as either parent, and, therefore, twice as much variety, unless there is some way to cancel out half of it at each step.

I called attention to the bearing of this convergence of ancestry on the problem of inheritance in 1883, in words which still seem to be a clear statement, although the views on variation of both Galton and Weismann are based on the unfounded assumption that each sexual act brings together two totally dissimilar sets of factors, instead of factors which are identical in innumerable features for each one in which they differ.

My statement is as follows: "In order to breed together, animals must be closely related; they must belong to the same species or to two closely allied species. Since the individuals which belong to two closely related species are the descendents of a common and not very remote ancestral species, it is clear that almost the whole course of their evolution has been shared by them in common; all their generic characters being inherited from this ancestor. Only the slight differences in minor points which distinguish one species of a genus from another have been acquired since the two diverged, and not even all of these slight differences. * * We know that the duration of even the most persistent species is only an infinitesimal part of the whole history of their evolution, and it is clear that the common characteristics of two allied species must outnumber, thousands of times, the differences between them. It follows that the parents of any possible hybrid must be alike in thousands of features for one in which they differ. * * Crossing simply results in the formation of a germ by the union of a male and a female element derived from two essentially similar parents, with at most only a few secondary and comparatively slight differences, all of which have been recently acquired."

I trust that you will not think me unwarranted in the assertion that due consideration of the substance of this extract might have saved us much unprofitable discussion of the causes of variation, for I hope I have made it clear that these must be sought in the modern world and not in the remote past; that, as I expressed it in 1883, "the *occurrence* of a variation is due to the direct action of external conditions, but its precise character is not."

I sought by these words to express the familiar fact that the stimulus under which a vital action takes place is one thing, while the character of the action itself is quite another thing.

This fact seems, from its very simplicity, to slip out of the minds of naturalists, and I should like to improve this opportunity to approach it from another standpoint.

We have been familiar for many years with two views of the nature of the process of development from the egg.

One school of embryologists holds that the organism arises from the egg by virtue of its inherent potency; that the constitution which the germinal matter has inherited is in some way an embodiment of all that is to be unfolded out of it; while the other school finds, in the stimulus which one part of the segmenting egg or of the growing organism exerts on other parts, the explanation of each successive step in the process of development.

Advocates of these two views generally regard themselves as opponents, but is there any real antagonism?

We now have positive evidence enough for each view to convince me that both are true; that every change which takes place in the organism from the beginning of segmentation to the end of life is called forth by some external stimulus either within the body or without; and yet that the outcome of the whole process of development is what it is because it was all potential in the germ.

The gun does not go off until the cap explodes; but it hits the mark because it is aimed.

While the distinction between the stimulus to a vital change and the nature of the change itself is obvious enough in simple cases, we may easily become confused and lose sight of it in handling complicated problems.

A hen's egg does not develop without the stimulus of heat, but the view that heat causes the chick is too grotesque for a sane mind.

What interests us is not that it becomes a chick while a duck's egg in the same nest becomes a duckling, but that the one grows into exquisite adjustment to the life of fowls, while the other becomes as admirably adapted for the life of ducks.

Here the stimulus comes from the external world, but the case is just the same when it is internal.

The well-known results of castration prove that the normal development of male animals is dependent on some stimulus which comes to the parts of the growing body from the reproductive organs, but who can believe that this is an adequate explanation of the short, sharp horns, the thick neck and the ferocity of the bull, or the bright colors and high courage of the cock?

The only explanation of the origin of these useful structures worth considering is that which attributes them to the retention by the germ of the effects of past ages of selection.

We have no reason to take a different view when the result varies with the stimulus. Under one internal stimulus a bud becomes a jelly-fish, while under others it may become a hydranth, or a machopolyp or a blastostyle, but the problem we have to solve in this case as in others is the origin of a beautifully coördinated organism, with the distinctive characters of its species, and with exquisite fitness for a life like that of its ancestors.

I showed some years ago that a small crustacean, Alpheus heterochelis, develops from the egg according to one plan at Beaufort in North Carolina, according to a second at Key West in Florida, while it has still a third life history at Nassau in the Bahama Islands, but no one can believe that the influences which cause this diversity have anything to do with the final outcome of the process.

The case is exactly the same when a cell which normally gives rise to a half or a quarter of the body produces the whole under a different stimulus.

All the machinery in a great industrial exposition may be started by a single electric contact, but however much the discovery of the button may interest us, it helps us little to understand the result.

So it is with living organism. External conditions press the button, but it takes all the inherited potency of living matter to do the rest.

It is an error to believe that great knowledge is needful for a clear grasp of first principles. Too often a great store of information is like riches, "it cannot be spared nor left behind, but it hindereth the march;

yea, and the care of it sometimes loseth or disturbeth the victory."

Students who are drifting on the sea of facts with which the modern laboratory has flooded us declare that the doctrine of adaptation is antiquated and unscientific and pernicious.

They tell us organisms have many properties which are not adaptive, and that in many other cases we cannot tell whether a property is adaptive or not. Of course this is true. No one supposes that susceptibility to poisons, for example, is adaptive, and our knowledge of nature is incomplete beyond measure.

They tell us, too, that many attempts to explain the uses of parts are fanciful and worthless. Unfortunately, this is true also, but the logic which makes it a basis for denying the reality of adaptation is enough to call Paley from his grave.

While protoplasm is the physical basis of life, the intellectual basis of biology is adjustment.

I should like to see hung on the walls of every laboratory Herbert Spencer's defininition to the effect that life is not protoplasm but adjustment, or the older teaching of the Father of Zoölogy that the essence of a living thing is not what it is made of nor what it does, but why it does it.

Spencer has given us diagrams to prove that the vertebral column has become segmented by the strain of flexion, but Aristotle tells us that Empedocles *and the ancients* are in error in their attempts to account for the jointing of the backbone by the strain of flexion, for the thing to explain, he says, is not how it becomes jointed, but how the jointed backbone has become so beautifully adjusted to the conditions of life.

"Is there anything of which it may be said: See, this is new. It hath been already in the old times which were before us."

W. K. Brooks.

Johns Hopkins University.

CURRENT NOTES ON ANTHROPOLOGY (III.).

THE EARLIEST ENGLISHMEN.

Some interesting studies as to the earliest signs of human industry in England deserve a notice.

The description by Professor Prestwich of some flint implements found by Mr. Harrison in pre-glacial strata on the chalk plateau of Kent seems to have added an impetus to such researches. Mr. O. A. Shrubsole describes a series of those relics from pre-glacial hill gravels in Berkshire, in the Journal of the Anthropological Institute for August, 1894; and in the May number of the same journal, Mr. A. M. Bell replies with considerable force to the objections which had been urged againstProfessor Prestwich's reasonings; vindicating for the Kent implements an antiquity beyond that of the formation of the present river valleys.

A pleasantly written volume on the subject is one by Mr. Worthington G. Smith entitled, *Man the Primeval Savage.* He discovered a true palaeolithic workshop, or rather several of them, in undisturbed relations, near Dunstable, about thirty miles north of London. The heaps of chips and broken flints lay just as the primeval artist had left them, covered to many feet in depth by the washings from the boulder clay. Mr Smith was able to collect the chips in a number of instances, and by fitting them together, reconstruct the original flint block from which the instrument had been formed; and then to make a cast of the size and shape of the tool represented by the cavity. This beautiful demonstration leaves nothing to be desired. He does not believe, however, that either his finds or those of the others mentioned are pre-glacial. His book is agreeably written and well illustrated. (Published by E. Stanford, London.)

THE TRIBES OF THE 'GRAN CHACO.'

The 'Gran Chaco,' or 'Great Hunting-ground,' merits its name, for it extends 850