their models of turtles among the best of their clay sculptures. Nor are we disappointed in this, as may be judged from the two drawings (figs. 3 and 4) from a specimen of this kind in my possession.

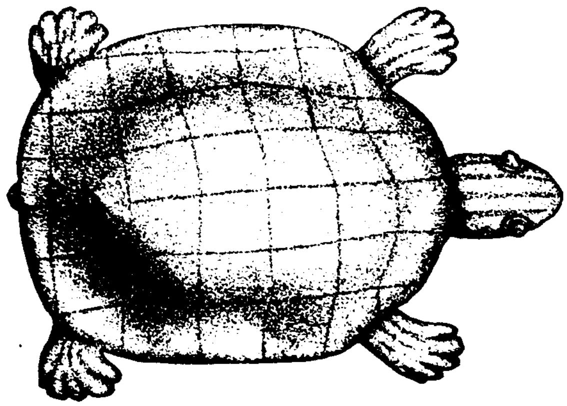

FIG. 3. — Dorsal view of turtle modelled in white clay by Zuñi Indian.

The carapace of this figure is painted a deep brown; while the epidermal plates are simply indicated by six transverse lines, crossed by the same number of longitudinal ones, both in a flesh-red color. This latter tint has also been used to paint the plastron and longitudinal lines on the deep-brown head and feet. This coloration gives it a not distant resemblance to some form of Chrysemys. Two such specimens are in my collection; and in both the designer has represented the toes by simply slitting the clay a little ways, in one instance correctly, as seen in the figure; and in the other by three slits, giving each foot only four toes.

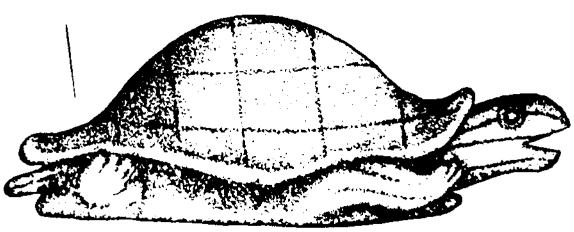

FIG. 4. — The same, lateral aspect. Both less than half the size of original.

I have never seen the turtle depicted upon any of their pottery, and I believe it must be one of their rarer forms to model in clay. So far as I can remember, Mr. Barber does not mention it, or figure the turtle in his article in the *American naturalist*, published some four years ago; nor does Mr. Stevenson allude to it, by word or figure, in the catalogue of his enormous collection of 1879 already quoted.

Mr. Stevenson's figures support another curious fact which I have observed, and will allude to before concluding. It is this: they seem to reserve their amblystomas, their axolotls, their tadpoles, and their bugaboos of human form, to illuminate the quaint clay baskets they manufacture, which usually have handles, and are ornamented with fancy serrated edges, and are of odd shapes. Almost invariably they represent the tadpoles upon side view, and take especial pains to draw the suctorial lips and the eye. The tail, however, is drawn simply by a wriggling line, and is not the broad tail of the tadpole, seen upon lateral aspect of this creature. R. W. SHUFELDT.

TYPES AND THEIR INHERITANCE.

THE object of the anthropologist is plain. He seeks to learn what mankind really are in body and mind, how they came to be what they are, and whither their races are tending; but the methods by which this definite inquiry has to be pursued are extremely diverse. Those of the geologist, the antiquarian, the jurist, the historian, the philologist, the traveller, the artist, and the statistician, are all employed; and the science of man progresses through the help of specialists. Under these circumstances, I think it best to follow an example occasionally set by presidents of sections, by giving a lecture rather than an address, selecting for my subject one that has long been my favorite pursuit, on which I have been working with fresh data during many recent months, and about which I have something new to say.

My data were the family records intrusted to me by persons living in all parts of the country; and I am now glad to think that the publication of some first-fruits of their analysis will show to many careful and intelligent correspondents that their painstaking has not been thrown away. I shall refer to only a part of the work already completed, which in due time will be published; and must be satisfied if, when I have finished this address, some few ideas that lie at the root of heredity shall have been clearly apprehended, and their wide bearings more or less distinctly perceived. I am the more desirous of speaking on heredity, because, judging from private conversations and inquiries that are often put to me, the popular views of what may be expected from inheritance seem neither clear nor just.

The subject of my remarks will be 'Types and their inheritance.' I shall discuss the conditions of the stability and instability of types, and hope, in doing so, to place beyond doubt the existence of a simple and far-reaching law that governs hereditary transmission, and to which I once before ventured to draw

Opening address before the section of anthropology of the British association for the advancement of science, by FRANCIS GALTON, F. R. S., etc., president of the section. From advance sheets of *Nature*.

attention on far more slender evidence than I now possess.

It is some years since I made an extensive series of experiments on the produce of seeds of different size, but of the same species. They yielded results that seemed very noteworthy; and I used them as the basis of a lecture before the Royal institution on Feb. 9, 1877. It appeared from these experiments that the offspring did *not* tend to resemble their parent seeds in size, but to be always more mediocre than they, — to be smaller than the parents, if the parents were large; to be larger than the parents, if the parents were very small. The point of convergence was considerably below the average size of the seeds contained in the large bagful I bought at a nursery-garden, out of which I selected those that were sown.

The experiments showed, further, that the mean filial regression towards mediocrity was directly proportional to the parental deviation from it. This curious result was based on so many plantings, conducted for me by friends living in various parts of the country, — from Nairn in the north, to Cornwall in the south, during one, two, or even three generations of the plants, — that I could entertain no doubt of the truth of my conclusions. The exact ratio of regression remained a little doubtful, owing to variable influences; therefore I did not attempt to define it. After the lecture had been published, it occurred to me that the grounds of my misgivings might be urged as objections to the general conclusions. I did not think them of moment; but as the inquiry had been surrounded with many small difficulties and matters of detail, it would be scarcely possible to give a brief, and yet a full and adequate, answer to such objections. Also, I was then blind to what I now perceive to be the simple explanation of the phenomenon; so I thought it better to say no more upon the subject until I should obtain independent evidence. It was anthropological evidence that I desired, caring only for the seeds as means of throwing light on heredity in man. I tried in vain for a long and weary time to obtain it in sufficient abundance: and my failure was a cogent motive, together with others, in inducing me to make an offer of prizes for family records, which was largely responded to, and furnished me last year with what I wanted. I especially guarded myself against making any allusion to this particular inquiry in my prospectus, lest a bias should be given to the returns. I now can securely contemplate the possibility of the records of height having been frequently drawn up in a careless fashion, because no amount of unbiassed inaccuracy can account for the results, contrasted in their values, but concurrent in their significance, that are derived from comparisons between different groups of the returns.

An analysis of the records fully confirms, and goes far beyond, the conclusions I obtained from the seeds. It gives the numerical value of the regression towards mediocrity as from 1 to $\frac{2}{3}$, with unexpected coherence and precision; and it supplies me with the class of facts I wanted to investigate, — the degrees of family likeness in different degrees of kinship, and the steps through which special family peculiarities become merged into the typical characteristics of the race at large.

The subject of the inquiry on which I am about to speak was hereditary stature. My data consisted of the heights of 930 adult children, and of their respective parentages, 205 in number. In every case I transmuted the female statures to their corresponding male equivalents, and used them in their transmuted form; so that no objection, grounded on the sexual difference of stature, need be raised when I speak of averages. The factor I used was 1.08, which is equivalent to adding a little less than one-twelfth to each female height. It differs a very little from the factors employed by other anthropologists, who, moreover, differ a trifle between themselves: anyhow it suits my data better than 1.07 or 1.09. The final result is not of a kind to be affected by these minute details; for it happened, that, owing to a mistaken direction, the computer to whom I first intrusted the figures used a somewhat different factor, yet the result came out closely the same.

I shall explain with fulness why I chose stature for the subject of inquiry, because the peculiarities and points to be attended to in the investigation will manifest themselves best by doing so. Many of its advantages are obvious enough, such as the ease and frequency with which its measurement is made, its practical constancy during thirty-five years of middle life, its small dependence on differences of bringing up, and its inconsiderable influence on the rate of mortality. Other advantages which are n[illegible] equally obvious are no less great. One of these li[illegible]n the fact that stature is not a simple element, but [illegible]um of the accumulated lengths or thicknesses of more than a hundred bodily parts, each so distinct from the rest as to have earned a name by which it can be specified. The list of them includes about fifty separate bones, situated in the skull, the spine, the pelvis, the two legs, and the two ankles and feet. The bones in both the lower limbs are counted, because it is the average length of these two limbs that contributes to the general stature. The cartilages interposed between the bones, two at each joint, are rather more numerous than the bones themselves. The fleshy parts of the scalp of the head and of the soles of the feet conclude the list. Account should also be taken of the shape and set of many of the bones which conduce to a more or less arched instep, straight back, or high head. I noticed in the skeleton of O'Brien, the Irish giant, at the College of surgeons, which is, I believe, the tallest skeleton in any museum, that his extraordinary stature of about seven feet seven inches would have been a trifle increased if the faces of his dorsal vertebrae had been more parallel, and his back consequently straighter.

The beautiful regularity in the statures of a population, whenever they are statistically marshalled in the order of their heights, is due to the number of variable elements of which the stature is the sum. The best illustrations I have seen of this regularity were the curves of male and female statures that I obtained from the careful measurements made at my Anthropometric laboratory in the International health exhibition last year. They were almost perfect.

The multiplicity of elements, some derived from one progenitor, some from another, must be the cause of a fact that has proved very convenient in the course of my inquiry. It is, that the stature of the children depends closely on the average stature of the two parents, and may be considered in practice as having nothing to do with their individual heights. The fact was proved as follows: After transmuting the female measurements in the way already explained, I sorted the children of parents who severally differed 1, 2, 3, 4, and 5 or more inches into separate groups. Each group was then divided into similar classes, showing the number of cases in which the children differed 1, 2, 3, etc., inches from the common average of the children in their respective families. I confined my inquiry to large families of six children and upwards, that the common average of each might be a trustworthy point of reference. The entries in each of the different groups were then seen to run in the same way, except that in the last of them the children showed a faint tendency to fall into two sets, one taking after the tall parent, the other after the short one. Therefore, when dealing with the transmission of stature from parents to children, the average height of the two parents, or, as I prefer to call it, the 'mid-parental' height, is all we need care to know about them.

It must be noted that I used the word parent without specifying t sex. The methods of statistics permit us to emp this abstract term, because the cases of a tall fath being married to a short mother are balanced by those of a short father being married to a tall mother. I use the word parent to save a complication due to a fact brought out by these inquiries, that the height of the children of both sexes, but especially that of the daughters, takes after the height of the father more than it does after that of the mother. My parent data are insufficient to determine the ratio satisfactorily.

Another great merit of stature as a subject for inquiries into heredity is, that marriage selection takes little or no account of shortness or tallness. There are undoubtedly sexual preferences for moderate contrast in height; but the marriage choice appears to be guided by so many and more important considerations, that questions of stature exert no perceptible influence upon it. This is by no means my only inquiry into this subject; but, as regards the present data, my test lay in dividing the 205 male parents, and the 205 female parents, each into three groups, — tall, medium, and short (medium being taken as 67 inches and upwards to 70 inches), — and in counting the number of marriages in each possible combination between them. The result was that men and women of contrasted heights, short and tall, or tall and short, married just about as frequently as men and women of similar heights, both tall or both short: there were thirty-two cases of the one to twenty-seven of the other. In applying the law of probabilities to investigations into heredity of stature, we may regard the married folk as couples picked out of the general population at haphazard.

The advantages of stature as a subject in which the simple laws of heredity may be studied will now be understood. It is a nearly constant value that is frequently measured and recorded; and its discussion is little entangled with considerations of nurture, of the survival of the fittest, or of marriage selection. We have only to consider the mid-parentage, and not to trouble ourselves about the parents separately. The statistical variations of stature are extremely regular; so much so, that their general conformity with the results of calculations, based on the abstract law of frequency of error, is an accepted fact by anthropologists. I have made much use of the properties of that law in cross-testing my various conclusions, and always with success.

The only drawback to the use of stature is its small variability. One-half of the population with whom I dealt varied less than 1.7 inches from the average of all of them; and one-half of the offspring of similar mid-parentages varied less than 1.5 inches from the average of their own heights. On the other hand, the precision of my data is so small, partly due to the uncertainty in many cases whether the height was measured with the shoes on or off, that I find by means of an independent inquiry, that each observation, taking one with another, is liable to an error that as often as not exceeds two-thirds of an inch.

It must be clearly understood, that my inquiry is primarily into the inheritance of different degrees of tallness and shortness; that is to say, of measurements made from the crown of the head to the level of mediocrity, upwards or downwards as the case may be, and not from the crown of the head to the ground. In the population with which I deal, the level of mediocrity is 68¼ inches (without shoes). The same law applying with sufficient closeness both to tallness and shortness, we may include both under the single head of deviations; and I shall call any particular deviation a 'deviate.' By the use of this word, and that of 'mid-parentage,' we can define the law of regression very briefly. It is, that the height-deviate of the offspring is, on the average, two-thirds of the height-deviate of its mid-parentage.

If this remarkable law had been based only on experiments on the diameters of the seeds, it might well be distrusted until confirmed by other inquiries. If it were corroborated merely by the observations on human stature, of which I am about to speak, some hesitation might be expected before its truth could be recognized in opposition to the current belief that the child tends to resemble its parents. But more can be urged than this. It is easily to be shown that we ought to expect filial regression, and that it should amount to some constant fractional part of the value of the mid-parental deviation. It is because this explanation confirms the previous observations made both on seeds and on men, that I feel justified on the present occasion in drawing attention to this elementary law.

The explanation of it is as follows: The child inherits partly from his parents, partly from his ancestry. Speaking generally, the farther his genealogy goes back, the more numerous and varied will his ancestry become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race: in other words, it will be mediocre. Or, to put the same fact into another form, the most probable value of the mid-ancestral deviates in any remote generation is zero.

For the moment let us confine our attention to the remote ancestry, and to the mid-parentages, and ignore the intermediate generations. The combination of the zero of the ancestry with the deviate of the mid-parentage, is that of nothing with something; and the result resembles that of pouring a uniform proportion of pure water into a vessel of wine. It dilutes the wine to a constant fraction of its original alcoholic strength, whatever that strength may have been.

The intermediate generations will, each in its degree, do the same. The mid-deviate of any one of them will have a value intermediate between that of the mid-parentage and the zero value of the ancestry. Its combination with the mid-parental deviate will be as if not pure water, but a mixture of wine and water in some definite proportion, had been poured into the wine. The process throughout is one of proportionate dilutions, and therefore the joint effect of all of them is to weaken the original wine in a constant ratio.

We have no word to express the form of that ideal and composite progenitor, whom the offspring of similar mid-parentages most nearly resemble, and from whose stature their own respective heights diverge evenly, above and below. He, she, or it, may be styled the 'generant' of the group. I shall shortly explain what my notion of a generant is, but for the moment it is sufficient to show that the parents not identical with the generant of their own offspring.

The average regression of the offspring to a constant fraction of their respective mid-parental deviations, which was first observed in the diameters of seeds, and then confirmed by observations on human stature, is now shown to be a perfectly reasonable law which might have been deductively foreseen. It is of so simple a character, that I have made an arrangement with one movable pulley, and two fixed ones, by which the probable average height of the children of known parents can be mechanically reckoned. This law tells heavily against the full hereditary transmission of any rare and valuable gift, as only a few of many children would resemble their mid-parentage. The more exceptional the gift, the more exceptional will be the good fortune of a parent who has a son who equals him, and still more if he has a son who overpasses him. The law is even-handed: it levies the same heavy succession-tax on the transmission of badness as well as of goodness. If it discourages the extravagant expectations of gifted parents that their children will inherit all their powers, it no less discountenances extravagant fears that they will inherit all their weaknesses and diseases.

The converse of this law is very far from being its numerical opposite. Because the most probable deviate of the son is only two-thirds that of his mid-parentage, it does not in the least follow that the most probable deviate of the mid-parentage is $\frac{3}{2}$, or $1\frac{1}{2}$ that of the son. The number of individuals in a population who differ little from mediocrity is so preponderant, that it is more frequently the case that an exceptional man is the somewhat exceptional son of rather mediocre parents, than the average son of very exceptional parents. It appears from the very same table of observations by which the value of the filial regression was determined, when it is read in a different way, namely, in vertical columns instead of in horizontal lines, that the most probable mid-parentage of a man is one that deviates only one-third as much as the man does. There is a great difference between this value of $\frac{1}{3}$, and the numerical converse mentioned above of $\frac{3}{2}$; it is four and a half times smaller, since $4\frac{1}{2}$, or $\frac{9}{2}$, being multiplied into $\frac{1}{3}$, is equal to $\frac{3}{2}$.

Let it not be supposed for a moment, that these figures invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair. What it asserts is, that the ablest child of one gifted pair is not likely to be as gifted as the ablest of all the children of very many mediocre pairs. However, as, notwithstanding this explanation, some suspicion may remain of a paradox lurking in these strongly contrasted results, I will explain the form in which the table of data was drawn up, and give an anecdote connected with it. Its outline was constructed by ruling a sheet into squares, and writing a series of heights in inches, such as 60 and under 61, 61 and under 62, etc., along its top, and another similar series down its side. The former referred to the height of offspring, the latter to that of mid-parentages. Each square in the table was formed by the intersection of a vertical column with a horizontal one: and in each square was inserted the number of children out of the 930 who were of the height indicated by the heading of the vertical column, and who, at the same time, were born of mid-parentages of the height indicated at the side of the horizontal column. I take an entry out of the table as an example. In the square where the vertical column headed[1] 69– is intersected by the horizontal column by whose side 67– is marked, the entry 38 is found; this means, that, out of the 930 children, 38 were born of mid-parentages of 69 and under 70 inches, who also were 67 and under 68 inches in height. I found it hard at first to catch the full significance of the entries in the table, which had curious relations that were very interesting to investigate. Lines drawn through entries of the same value formed a series of concentric and similar ellipses. Their common centre lay at the intersection of the vertical and horizontal lines that corresponded to $68\frac{1}{4}$ inches. Their axes were similarly inclined. The points where each ellipse in succession

[1] A matter of detail is here ignored which has nothing to do with the main principle, and would only serve to perplex if I described it.

was touched by a horizontal tangent, lay in a straight line inclined to the vertical in the ratio of $\frac{2}{3}$; those where they were touched by a vertical tangent, lay in a straight line inclined to the horizontal in the ratio of $\frac{1}{3}$. These ratios confirm the values of average regression already obtained by a different method, of $\frac{2}{3}$ from mid-parent to offspring, and of $\frac{1}{3}$ from offspring to mid-parent. These and other relations were evidently a subject for mathematical analysis and verification. They were all clearly dependent on three elementary data, supposing the law of frequency of error to be applicable throughout; these data being 1°, the measure of racial variability; 2°, that of co-family variability (counting the offspring of like mid-parentages as members of the same co-family); and, 3°, the average ratio of regression. I noted these values, and phrased the problem in abstract terms such as a competent mathematician could deal with, disentangled from all reference to heredity, and in that shape submitted it to Mr. J. Hamilton Dickson, of St. Peter's college, Cambridge. I asked him kindly to investigate for me the surface of frequency of error that would result from these three data, and the various particulars of its sections, one of which would form the ellipses to which I have alluded.

I may be permitted to say that I never felt such a glow of loyalty and respect towards the sovereignty and magnificent sway of mathematical analysis as when his answer reached me, confirming, by purely mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope; for the original data ran somewhat roughly, and I had to smooth them with tender caution. His calculation corrected my observed value of mid-parental regression from $\frac{1}{3}$ to $\frac{6}{17.6}$: the relation between the major and minor axis of the ellipses was changed 3 per cent, their inclination was changed less than 2°. It is obvious, then, that the law of error holds throughout the investigation with sufficient precision to be of real service, and that the various results of my statistics are not casual determinations, but strictly interdependent.

In the lecture at the Royal institution to which I have referred, I pointed out the remarkable way in which one generation was succeeded by another that proved to be its statistical counterpart. I there had to discuss the various agencies of the survival of the fittest, of relative fertility, and so forth; but the selection of human stature as the subject of investigation now enables me to get rid of all these complications, and to discuss this very curious question under its simplest form. How is it, I ask, that in each successive generation, there proves to be the same number of men per thousand who range between any limits of stature we please to specify, although the tall men are rarely descended from equally tall parents, or the short men from equally short? How is the balance from other sources so nicely made up? The answer is, that the process comprises two opposite sets of actions, one concentrative and the other dispersive, and of such a character that they necessarily neutralize one another, and fall into a state of stable equilibrium. By the first set, a system of scattered elements is replaced by another system which is less scattered; by the second set, each of these new elements becomes a centre, whence a third system of elements is dispersed. The details are as follows: In the first of these two stages, the units of the population group themselves, as it were by chance, into married couples, whence the mid-parentages are derived; and then by a regression of the values of the mid-parentages the true generants are derived. In the second stage, each generant is a centre whence the offspring diverge. The stability of the balance between the opposed tendencies is due to the regression being proportionate to the deviation, — it acts like a spring against a weight.

A simple equation connects the three data of race variability, of the ratio of regression, and of co-family variability; whence, if any two are given, the third may be found. My observations give separate measures of all three, and their values fit well into the equation, which is of the simple form, —

$$v^2 \frac{p^2}{2} + f^2 = p^2,$$

where $v = \frac{2}{3}$, $p = 1.7$, $f = 1.5$.

It will therefore be understood that a complete table of mid-parental and filial heights may be calculated from two simple numbers.

It will be gathered from what has been said, that a mid-parental deviate of one unit implies a mid-grandparental deviate of $\frac{1}{3}$, a mid-ancestral unit in the next generation of $\frac{1}{9}$, and so on. I reckon from these and other data, by methods that I cannot stop to explain, that the heritage derived on an average from the mid-parental deviate, independently of what it may imply, or of what may be known concerning the previous ancestry, is only $\frac{1}{2}$. Consequently, that similarly derived from a single parent is only $\frac{1}{4}$, and that from a single grandparent is only $\frac{1}{16}$.

The most elementary data upon which a complete table of mid-parental and filial heights admits of being constructed, are, 1°, the ratio between the mid-parental and the rest of the ancestral influences; and, 2°, the measure of the co-family variability.

I cannot now pursue the numerous branches that spring from the data I have given, as from a root. I will not speak of the continued domination of one type over others, or of the persistency of unimportant characteristics, or of the inheritance of disease, which is complicated in many cases by the requisite concurrence of two separate heritages, the one of a susceptible constitution, the other of the germs of the disease. Still less can I enter upon the subject of fraternal characteristics, which I have also worked out. It will suffice for the present to have shown some of the more important conditions associated with the idea of race, and how the vague word 'type' may be defined by peculiarities in hereditary transmission; at all events, when that word is applied to any single quality, such as stature. To include those numerous qualities that are not strictly measurable, we must omit reference to number and proportion, and frame the definition thus: 'The type is an ideal form towards which the children of those who deviate from it tend to regress.'

The stability of a type would, I presume, be measured by the strength of its tendency to regress; thus a mean regression from 1 in the mid-parents to ⅔ in the offspring would indicate only half as much stability as if it had been to ⅓.

The mean regression in stature of a population is easily ascertained, but I do not see much use in knowing it. It has already been stated that half the population vary less than 1.7 inches from mediocrity, this being what is technically known as the 'probable' deviation. The mean deviation is, by a well-known theory, 1.18 times that of the probable deviation, therefore in this case it is 1.9 inches. The mean loss through regression is ⅓ of that amount, or a little more than .6 inch. That is to say, taking one child with another, the mean amount by which they fall short of their mid-parental peculiarity of stature is rather more than six-tenths of an inch.

With respect to these and the other numerical estimates, I wish emphatically to say, that I offer them only as being serviceably approximate, though they are mutually consistent; and with the desire that they may be reinvestigated by the help of more abundant and much more accurate measurements than those I have had at command. There are many simple and interesting relations to which I am still unable to assign numerical values for lack of adequate material, such as that to which I referred some time back, of the superior influence of the father over the mother on the stature of their sons and daughters.

The limits of deviation beyond which there is no regression, but a new condition of equilibrium is entered into, and a new type comes into existence, have still to be explored. Let us consider how much we can infer from undisputed facts of heredity regarding the conditions amid which any form of stable equilibrium, such as is implied by the word 'type,' must be established, or might be disestablished and superseded by another. In doing so I will follow cautiously along the same path by which Darwin started to construct his provisional theory of pangenesis; but it is not in the least necessary to go so far as that theory, or to entangle ourselves in any questioned hypothesis.

There can be no doubt that heredity proceeds to a considerable extent, perhaps principally, in a piecemeal or piebald fashion, causing the person of the child to be to that extent a mosaic of independent ancestral heritages, one part coming with more or less variation from this progenitor, and another from that. To express this aspect of inheritance, where particle proceeds from particle, we may conveniently describe it as 'particulate.'

So far as the transmission of any feature may be regarded as an example of particular inheritance, so far (it seems little more than a truism to assert) the element from which that feature was developed must have been particulate also. Therefore, wherever a feature in a child was not personally possessed by either parent, but transmitted through one of them from a more distant progenitor, the element whence that feature was developed must have existed in a particulate, though impersonal and latent form, in the body of the parent. The total heritage of that parent will have included a greater variety of material than was utilized in the formation of his own personal structure. Only a portion of it became developed: the survival of at least a small part of the remainder is proved, and that of a larger part may be inferred by his transmitting it to the person of his child. Therefore the organized structure of each individual should be viewed as the fulfilment of only one out of an indefinite number of mutually exclusive possibilities. It is the development of a single sample drawn out of a group of elements. The conditions under which each element in the sample became selected are, of course, unknown; but it is reasonable to expect they would fall under one or other of the following agencies: first, self-selection, where each element selects its most suitable neighbor, as in the theory of pangenesis; secondly, general co-ordination, or the influence exerted on each element by many or all of the remaining ones, whether in its immediate neighborhood or not; finally, a group of diverse agencies, alike only in the fact that they are not uniformly helpful or harmful, that they influence with no constant purpose: in philosophical language, that they are not teleological; in popular language, that they are accidents or chances. Their inclusion renders it impossible to predict the peculiarities of individual children, though it does not prevent the prediction of average results. We now see something of the general character of the conditions amid which the stable equilibrium that characterizes each race must subsist.

Political analogies of stability and change of type abound, and are useful to fix the ideas, as I pointed out some years ago. Let us take that which is afforded by the government of a colony which has become independent. The individual colonists rank as particulate representatives of families or other groups in the parent country. The organized colonial government ranks as the personality of the colony, being its mouthpiece and executive. The government is evolved amid political strife, one element prevailing here, and another there. The prominent victors band themselves into the nucleus of a party: additions to their number, and revisions of it, ensue, until a body of men are associated capable of conducting a completely organized administration. The kinship between the form of government of the colony and that of the parent state is far from direct, and resembles in a general way that which I conceive to subsist between the child and his mid-parentage. We should expect to find many points of resemblance between the two, and many instances of great dissimilarity; for our political analogy teaches us only too well on what slight accidents the character of the government may depend when parties are nearly balanced.

The appearance of a new and useful family peculiarity is a boon to breeders, who by selection in mating gradually reduce the preponderance of those ancestral elements that endanger reversion. The appearance of a new type is due to causes that lie beyond our reach; so we ought to welcome every useful one as a happy chance, and do our best to domicile and perpetuate it. When heredity shall have become much better, and more generally understood than now, I can believe that we shall look upon a neglect to conserve any valuable form of family type as a wrongful waste of opportunity. The appearance of each new natural peculiarity is a faltering step in the upward journey of evolution, over which, in outward appearance, the whole living world is blindly blundering and stumbling, but whose general direction man has the intelligence dimly to discern, and whose progress he has power to facilitate.

A NEW THEORY OF COHESION.

SINCE a great part of the relations discussed in a paper by Dr. H. Whiting, on a new theory of cohesion (*Proc. Amer. acad.*, xix. 353), are determined by the equation between the pressure, volume, and temperature of a given quantity of the substance considered, a comparison of the form of this equation as given in this paper with forms previously proposed affords the readiest means of comparing the author's results with those of previous investigators. The equation proposed in 1873 by Van der Waals has the form

$$\left(p + \frac{a}{v^2}\right)\left(1 - \frac{b}{v}\right)v = Rt; \qquad (1)$$

that of the present paper (see p. 376, third equation) may be written

$$\left(p + \frac{a}{v^2}\right)\left(1 - \sqrt[3]{\frac{c}{v}}\right)v = Rt. \qquad (2)$$

In both equations, p, v, and t denote pressure, volume, and absolute temperature: the other letters denote constants, to be determined by the nature of the substance considered.

We may get some idea of the numerical difference in the indications of these equations, if we observe that the ratio of the volume of the critical state to that which would be required by the laws of Boyle and Charles is 0.375 by the first equation, and 0.556 by the second (the experiments of Dr. Andrews give something like 0.414 for carbonic acid). Again: the ratio of the volume of the critical state to that at absolute zero would be 3 by the first equation (which, however, was not intended to apply to such a determination), and 3.58 by the second.

The equation of Dr. Whiting has an important property in common with that of Van der Waals. If we use the pressure, volume, and temperature of the critical state as units for the measurement of the pressure, volume, and temperature of all states, the constants will disappear from either equation, and we obtain a relation between the pressure, volume, and temperature (thus measured), which should be the same for all bodies. From this property of his equation, independently of the particular relation obtained, Van der Waals has derived a considerable number of interesting conclusions, which would equally follow from the equation of Dr. Whiting (see the twelfth and thirteenth chapters of the German translation of the memoirs of the former, by Dr. Roth, Leipzig, 1881). One of these is mentioned in Dr. Whiting's treatise, p. 427.

It is well known that the equation of Van der Waals agrees with experiment to an extent which is quite remarkable when the simplicity of the equation is considered, and the complexity of the problem to which it relates. But it was not intended to be applied to states as dense as the ordinary liquid state. Dr. Whiting's equation, on the other hand, seems to have been formed with especial reference to the denser conditions of matter, and, from the numerical verifications which are given, would appear to represent the ordinary liquid state, in some respects at least, much better than the equation of Van der Waals. The principal verifications relate to the coefficient of expansion and the critical temperature. When the pressure may be neglected, as in the ordinary liquid state, equation (2) gives

$$\frac{de}{dt} = \frac{7}{3}e^2 + \frac{4}{3}te^3,$$

where e is the coefficient of expansion $\left(\frac{dv}{vdt}\right)$. A very elaborate comparison is made between this equation and the experiments of Kopp, Pierre, and Thorpe. An empirical formula of Dr. Mendelejeff is also considered, which gives

$$\frac{de}{dt} = e^2,$$

a value of de/dt about one-third as great as Dr. Whiting's. We may add that the equation (1) of Van der Waals would give

$$\frac{de}{dt} = 3e^2 + 2te^3,$$

a value of de/dt about one-third greater than Dr. Whiting's. The result seems to be that the indications of experiment lie between the formulae of Dr. Whiting and Dr. Mendelejeff (pp. 424 ff.). We may conclude that they would not agree so well with that of Van der Waals.

Each of the equations (1) and (2) will give the critical temperature when we know the coefficient of expansion for a given temperature. Dr. Whiting has calculated the critical temperature, by means of his equation, for twenty-six substances for which this temperature has been observed. The calculated and observed values generally differ by less than ten degrees Centigrade. An equation derived by Thorpe and Rücker, in part from the formula of Mendelejeff above mentioned, and in part from a principle of Van der Waals, gives about the same agreement with experiment. We may add that the general equation of Van der Waals, taken alone, would give for the critical temperature t_c the formula

$$t_c = \frac{8(2te + 1)^2}{27e(te + 1)},$$

which does not seem, from the test of a few cases, to agree so well with experiment.

In establishing his fundamental equations, Dr. Whiting, like Van der Waals, treats the molecules as elastic spheres which attract one another when not in contact. The cohesive effect of the molecular attraction is regarded by both as proportional to the square of the density. It is, in fact, represented by the same term $\left(\frac{a}{v^2}\right)$ in equations (1) and (2). This effect is deduced by Dr. Whiting from the hypothesis of a molecular attraction varying inversely as the fourth power of the distance, by supposing a body to expand so that every distance is increased in the same ratio; but such an expansion is entirely unlike any which actually occur in fluids, since it increases the distance within which the centres of molecules do not approach one another. We shall probably come much nearer to the case of nature, if we suppose that the average number of molecules in a fluid, which are between the distances r and $r + dr$ from a given molecule, varies as the density of the fluid. This supposition will evidently make the cohesive effect of the molecular attraction vary as the square of the density. It would seem that any agreement of experiment with the indications either of equation (1) or of equation (2) should be regarded as confirmatory of this law of the distribution of the molecules rather than of any particular law of attraction.

THURSTON'S FRICTION AND LOST WORK.

THIS volume combines characteristics not too often found in a work on this or kindred subjects. It is thoroughly scientific in method, as well as in the treatment of separate problems. It is eminently practical in results, as well as in the selection and range of the problems considered. It is clear, accurate, and minute in the details which give completeness to its discussions, and make them readily available for actual use. It is not merely or principally a compilation. While it brings together the formulae and results of the standard writers and experimenters upon friction, its laws, modifications, and effects, it also includes the author's own elaborate experiments, made with a view to their bearing upon questions of daily and vital importance to the engineer and the student. The conclusions drawn from these experiments, being always subject to comparison with the facts and knowledge gained by the author in a wide and extensive engineering practice, are rational and reliable. The book comprises eight chapters. The first explains the object of mechanism, the manner of computing work and power, the laws of the persistence and transformation of energy, and the relation of lost work to the efficiency of mechanism. In the second chapter, the theory and laws of friction are developed. The problems which arise in practice are taken up one by one, clearly analyzed, mathematically solved, and the applications of the resulting formulae pointed out.

Friction and lost work in machinery and mill-work. By R. H. THURSTON. New York, *Wiley*, 1885. 12+365 p., illustr. 8°.

The next three chapters form an exhaustive treatise on the lubricants used for reducing friction: their nature and relative values; the means of applying and using; methods of analyzing, inspecting, and testing them. Cuts of the best lubricators in use, and also of the apparatus used in making physical tests; tables giving physical and chemical properties of oils, their color reactions, density, specific gravity, and viscosity; and diagrams showing the relations of viscosity and lubrication, and effects of temperature upon viscosity, accompany the text. Oleography and electrical conductivity are noticed as methods of identifying various oils. The nature and effects of friction, and the kinds and properties of lubricants, having been thus fully discussed, the author proceeds to the subject of experiments, from which must be obtained the values of constants which enter into all the formulae. Upon the correctness of these values depends the accuracy of results obtained by calculation from the formulae developed by the theoretical investigations.

The sixth chapter relates to experiments of two kinds: First, those designed to ascertain the relative amounts of friction between different surfaces under varying conditions; to determine constants, or suggest the value and form of empirical formulae, applicable to friction of both solids and fluids. Second, experiments with machines for testing lubricants, with cuts and descriptions of oil-testing machines. The mathematical theory and method of using Thurston's machine are given in detail, together with tables showing records of oil-tests made by the author with his machine. The seventh chapter gives results of experiments with lubricants, showing their effects in modifying friction; their endurance under different conditions of pressure and velocity; and the effect of changes of pressure and velocity upon the coefficient of friction.

It is impossible to give in a brief review an adequate idea of the minuteness of detail with which the wide range of problems and experiments are discussed. The reader may expect to find, substantially, all that is known upon these subjects through the investigations of earlier writers, supplemented by the results of