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From PROF. W. H. FLOWER, F.R.S.—*Sur la Mensuration de la Capacité du Crane ; Etudes sur les propriétés hygrométriques des Cranes.* By Dr. Paul Broca.

From the EDITOR.—*Revue Scientifique*, Nos. 39–40, 1878.

From the EDITOR.—“*Nature*” (to date).

From the EDITOR.—*Materiaux pour l’histoire de l’homme*, Feb. 1878.

From the SOCIETY.—*Proceedings of the Royal Society*. Vol. XXVII, No. 185,

From the AUTHOR, through the President.—*Illyrian Letters*. By Arthur J. Evans, B.A., F.S.A.

The following paper was read.

On METROLOGY and GEOMETRY in ANCIENT REMAINS, BY
W. M. FLINDERS PETRIE, Esq.

AMONG the various tests of the mental capacity of man, one of the most important, ranking in modern life on an equality with language, is the appreciation of quantity, or the notions of measurement and geometry. This may lay claim to a more important place than language, in some respects ; as it is a more delicate test of exact capability, ebbing and flowing more freely with variations of intelligence, yet present to some extent in the very lowest races, and developing in the highest culture more rapidly than language.

Not only is metrology a test of the powers of separate races, but it serves, like language, to show the connections of races ; and, as the foreign language of a superior and dominant race is imbibed by their inferiors, or acquired for commercial purposes, from just the same causes measures of one race may extend to others by external circumstances, without always implying a fusion of races or a common origin. Thus the possession of the same unit of measurement by different people implies either that it belonged to their common ancestors, or else that a very powerful commercial intercourse has existed between them.

The ideas of numbers are also useful tests, bearing both on the capacities and the connections of races. A mere binary multiplication or division of a quantity is performed by the lowest state of mind ; whereas, a decimal or duo-decimal system shows a much higher power of combining quantities, mentally and mechanically. The use of favourite numbers is also worth consideration, as it is very constant in different races. The Babylonian 6, and the early Jewish 10, are familiar instances.

A good test of the capabilities of a people is the average

accuracy of their workmanship; and though of course there are workmen of very varying character in all countries, yet by taking the averages of large numbers the results will be fairly comparable. It is found, on examination, that there is not as much difference in the average accuracy of different countries and ages as might be expected; Assyrian and prehistoric remains showing both an average error of $\frac{1}{100}$, and the highest class, the Greek and later English remains, having as large an average as $\frac{1}{250}$.

Although the value of units of measure, as tests of races, has been more or less recognized for many years, yet hitherto no methods have been systematically devised to extend this instrument of inquiry to those races whose units of measure are lost to us, but whose constructions executed in terms of those units still remain, and are in many cases the principal traces of their builders. Newton, however, in this, as in the rest of modern science, has laid the foundation; for in his "Dissertation on Cubits" he proceeds on the principle of recovering ancient measures by the relative ratios of the dimensions of buildings.

Now considering, first of all, the remains of those people who are known to have commonly used measures—Egyptians, Greeks, or Romans—if in any buildings erected by such a people we find a dimension continually recurring—there is a strong presumption that this dimension was some simple number of the unit used by the architect, as it would be highly improbable that such a result should occur by mere chance. But we are wholly ignorant of what number of units this length contained and equally ignorant of the length of the unit. If, however, instead of often finding one length repeated, we often find halves, doubles, and quadruples of a length, we have very nearly the same presumption that these multiples of the length are connected with it, as that the repetitions of the same length are connected together.

If, then, on examining the dimensions of a building together, it is found that they all bear simple relations to one another, it is highly probable that this is not the result of chance, but of their being all simple multiples of the unit used in the building. Now, knowing the relations of these lengths to each other, it is evident that we only need to combine all these ratios together, to obtain a series of numbers, one to each length, which shall be the only series of simple numbers bearing the same relations to one another as is borne by the measured lengths; or, in arithmetical phrase, their greatest common measure will be the unit by which they were laid out.

For instance, in a stone cist, found at Jerusalem, the dimensions are 12, 15, and 30 inches (omitting fractions as unnecessary for

illustration), it is evident that 15 contains half the number of units that there are in 30 inches; but 12 inches is two-fifths of 30 inches, therefore 30 must contain 5 or some multiple of 5 units, for 12 to contain a simple number; thus we see that no numbers but 4, 5, and 10 (or double or half of these) can have been the multiples used by the constructor; 4, 5, and 10 times 3 inches being 12, 15, and 30 inches. Three inches then (or more likely double of it) was the unit used in laying out the work. Here, as there are only three measurements, it leaves an uncertainty whether this 12, 15, and 30 inches may be 2, $2\frac{1}{2}$, and 5, or 4, 5, and 10, or 8, 10, and 20, or other numbers of units bearing the same proportions to each other. This is only an uncertainty belonging to cases where but few dimensions are considered; for if we found in a building lengths which bore the proportions 5, $2\frac{1}{2}$, $1\frac{1}{4}$, 8, 12, 16, to each other, these must be the multiples of the unit, for if we halve these proportional numbers (or double the unit) we have on the one hand the less likely fraction of $\frac{5}{8}$ instead of $1\frac{1}{4}$; and if we double these numbers we have on the other hand the less likely multiple 32 instead of 16; thus it is plain that these are much the simplest and most likely series of numbers proportional to the dimensions, and that the lengths in the building must have been originally laid out as containing these numbers of the unit

There are other forms of this method of obtaining the original constructor's unit of a building; but they are all similar in principle, and need not be entered upon in giving a general outline of the investigations.

It is hardly necessary to observe that the whole of this depends on probabilities; it is possible that a large quantity of purely casual dimensions might all have simple relations to one another, but it is highly improbable; if, however, there is not a large number of dimensions to be considered, and if, from roughness of construction, it seems likely that allowance must be made for inaccurate work, then the possibility of casual coincidence becomes such that a portion (say $\frac{1}{16}$ th or $\frac{1}{32}$ th) of the results from a series of monuments are liable to be fallacious.

There are, however, two checks on the liability to casual coincidences, which are most effectual. We may premise that not only may the unit of a monument be ascertained from the dimensions, as already shown, but the mean value of it may of course be found from combining all the dimensions; and, further, the *probable error* of this mean value. The greater the probable error, the greater is the uncertainty as to the result; and the more closely the various lengths agree in showing the unit, the less will be the probable error of the mean. Now, not only may we ascertain the unit of a building, but if a unit was in use in a

country a large number of remains ought to be found, all constructed in terms of that unit; and, accordingly, the average of all the slightly varying results extracted from various buildings may be taken to obtain the average length of the unit throughout the country. Here, the two checks on the fallacious results come into play, for if a mean unit that has been extracted from a building be merely fallacious, it is very improbable that another fallacious result will agree with it, and that three or four fallacious results should all coincide is practically impossible; thus if the mean units of several buildings are found to agree together, it may be taken as certain that they are not mere fallacies.

The second check is, that if a unit be fallacious—that is to say, if several lengths have by chance so nearly simple relations between them as to appear the result of intention—it is highly unlikely that these relations will be all very exact; on the contrary, more and more of these fallacious results will be found on looking at relations which are less and less exact; thus the fallacious relations being on the average far less exact than the intentional relations, they will, as already noticed, have a far larger probable error. Further, as in taking the average of the values of the unit shown in various remains, the units will be weighted inversely according to the square of their probable errors; thus if a fallacious result happens to agree to a group of similar units, it will most likely have a larger probable error, and so have far less weight than the others in taking the mean. From these considerations it is evident that we have the means of not only ascertaining the unit used in a building, but also of checking and almost eliminating the few results of casual coincidence that may occur. And it is well to remember that all the sciences, beyond pure mathematics, rely wholly on the improbability of a series of casual coincidences, in the experimental researches on which all their laws depend. There is a possibility, however remote it may be, that even the law of gravitation is false, and that everything that obeys it does so by casual coincidences.

Having thus the means of ascertaining the unit of measure, and the multiples of it employed, the use of special numbers as favourites is shown by the character of the multiples; in one case the multiples may be mainly binary, and in another case 3, or in another 5, may be the favourite number.

This method of extracting the units of measurement having been extensively tested, by applying it to the various remains of the people of the Mediterranean, whose standards of measure are tolerably known, the general results are (carefully avoiding all chances of unconscious bias) that three-quarters of the mean units

found by this purely inductive examination of the ancient remains, are units historically known to have been used; and the remaining quarter are never so widely diffused as the others, and are therefore less important and less likely to be preserved in history. So far then as theory and practice can both give us confidence in a new method, it seems that we may rely on this fresh instrument of inquiry.

In turning to non-historic remains, and trying to recover whatever is possible from these unlettered records, it should be remembered that the inductive methods of metrology do not compel a result to appear, and yield a fallacious answer if there be no true one; if no unit has been used in the construction of a work, usually no unit will be obtained by this method of examination; it is not a conjuring tool to produce something out of nothing, but a key enabling us to unlock whatever may exist. It should not therefore be said that it is useless or impracticable to apply it to remains that may be commonly supposed to be barbarically unmetrical; on the contrary, let us use the reasonable means of inquiry cautiously, and see how far they will lead us, and the results will probably be as already found—that is, that the lower we go in the scale of regularity, the fewer cases do we find of any unit appearing by inductive examination.

With regard to the metrology and geometry apparent in non-historic remains, the proofs of design that are met with only give evidence of a minimum of intelligence in the constructors; how far higher they may have been capable of going, and may have gone, in their unrepresented ideas, is unknown. If the mental capacities of Londoners were to be estimated by the winding and irregular character of a large number of their streets, and the chance angles at which many of them meet, the opinion of them would not be very high.

The best illustrations of the character of the more regular and geometrical remains of non-historic times, will be a few of the plans of such remains that are here shown. These plans are a portion of a series that I have accurately surveyed on purpose to examine the amount of regularity and skill shown in the construction of those earthworks; about 100 such plans are done, and copies are placed in the British Museum Map Department, for the use of all interested in such remains. If they do not give all the details of levels, &c., required in various other inquiries, they will at least serve as accurate bases to which other details can be readily added, as the probable error of the points fixed is mostly $\frac{1}{50}$ th of an inch on the plan.

From accurate plans a great deal can be ascertained as to the methods and order of the construction of earthworks; what points

were laid out with care, and what were left to be filled in by eye; whether works were begun at several points at once, or whether they were all made from one end, and many similar details, which throw light on the ways of the workers. In some few cases, attention to geometric forms, circles, rectangles, and straight lines may be found, without regard to equality or measurement; and in other cases equal lengths may be found laid out, and the work finished by eye without much regard for geometry; but usually measurement and geometry accompany each other to a considerable extent, in the more regular works, which are however only a fraction of the whole. Any one of the following examples if it stood alone might not be above doubt, as to the intentional origin of its regularity; but when so many earthworks are each of a character for which it would be difficult to account by mere chance, their evidence becomes very forcible.

As an illustration of the metrical character of earthworks, we may refer to the East Everley works in Wiltshire; these are banks and ditches of the ordinary style, about a couple of feet high, lying on an open piece of down. On examining the plan it is found that the distances of most of the banks are all simple multiples of one length, many of them being two, three and four times the basis. The first striking objects are three banks at equal distances apart, the distance between the two outer banks being twice repeated in other parts; and on examining the other distances with this basis they are seen to agree to it in a manner beyond all probability of mere casual coincidence. Parts of this work were so evidently added by men who had far less regard for right-angles and regularity than the first workers, that we need not be surprised at their not paying the attention to measurement that is shown by their more careful predecessors. The length of the unit apparently shown in these banks is 691 inches $\pm$ 5, which is 32 times a unit most frequently found in such works; 32 is a likely number to occur in rude works, as it would merely result from a repeated doubling, 2, 4, 8, 16, 32 of the unit, on a rope or cord.

In the earthworks at Hill Deverill, Wilts, considerable regularity of dimensions is also to be seen; and the banks are connected with a singular semi-circular earthwork of very true form, the mean difference from a true semi-circle being only 16 inches on a work 380 feet across. The radius of the semi-circular bank is exactly equal to four of the units of the straight banks outside it, which points to a connection between them. The form of it, and the ditch being placed inside the bank, where no strategical reasons exist for such an arrangement, both seem to show that it was not intended for defence.

The accuracy of other circular earthworks is greater than at Hill Deverill; a circular bank and ditch, commonly called a flat barrow, at West Everley has a mean error of only 7 inches from a true circle, on a diameter of 150 feet, or less than $\frac{1}{2\frac{1}{10}}$ error; and this 7 inches includes my errors of estimating the true centre of the bank (15 feet wide), and of surveying and plotting. The circle at Chilham, Kent, has also under 4 inches mean error on 130 feet diameter. The great earth circle of Stonehenge is about as true as these other circles, the mean error being only 9 inches (including my own errors of survey) on a diameter of over 300 feet, or $\frac{1}{4\frac{1}{10}}$; the mean error of the outer circle of sarsen stones from a true circle is 3·2 inches on a diameter of about 100 feet or $\frac{1}{4\frac{1}{10}}$; the outer circle of blue stones is much less accurate, having a mean error of 9 inches on about 75 feet, or $\frac{1}{1\frac{1}{10}}$; and the inner semi-circle of blue stones has 3 inches mean error on about 40 feet on $\frac{1}{1\frac{1}{10}}$ th. We will not pause now to enter into the various conclusions that may be drawn from an accurate survey of Stonehenge, with regard to the relative date of various parts, and of works connected with it; but the figures just mentioned show that the mean errors from true circles are small in all the parts, and least of all in the great earth circle and sarsen circle; the errors are all ascertained by a survey of Stonehenge which I made on purpose for accurate examination, and in which over a thousand measures were made correct to a fraction of an inch, so as to make it certain that modern errors of survey were not attributed to the ancient constructors.

Circles are of course the easiest forms to make; but the care taken in throwing up banks and ditches 15 or 20 feet wide each, and yet maintaining the centre of the bank true to a few inches, shows that much importance was attached to making accurate and regular figures. In fact, with so much care given to regularity, it would be intrinsically probable that some regular unit of length would be, either transiently or generally, adopted; even if there was not such strong evidence for it in other works containing equal and proportional lengths. In a square at Upavon, care has been given to make the sides equal, as their mean difference is under a foot on a length of 110 feet; but in this, as in all the other earthworks, there is a lamentable deficiency in good right-angles; very few are to be found with less than a degree of error, and 5 degrees of error are often found.

In a very remarkable, and probably quite unique set of works at Steeple Langford, there are some of the best right angles; a raised plateau on the west is truly rectangular, with an average error of only 5 inches; and considering that it has been exposed for perhaps thousands of years, and that my errors of

estimating its form, and of fixing the positions of the corners in a survey extending about $\frac{1}{4}$ of a mile, are all included in this 5 inches, it would be rather hard to charge the original workers with any perceptible amount of the error. The long and short sides are in the ratio of 5 to 3, with an average error of only $4\frac{1}{2}$ inches on 70 feet; and this again is so small an error that it can scarcely be charged on the constructor. The unit used in laying it out, 5 for the side and 3 for the end, is just 10 of a unit commonly found in other earthworks; the sides being thus 50 and the ends 30 measures in length.

The circle divided into equal squares is apparently not so accurate, the mean error being 7 inches on 130 feet, but it is so very faint that the whole of it cannot be traced, and much of the error may be due to misinterpretation in surveying; in any case it is a strange piece of work, quite unaccountable on any simple utilitarian hypothesis, and shows a considerable love of geometry. With regard to this and other cases, it is well to state that the method of surveying adopted was such that I was wholly ignorant of the forms or equalities of any of the works, until the plotting was done some weeks afterwards; and thus there was no chance of *cooking* the survey, or assuming that any lines were equal, parallel, or at right-angles without proof. The other works around this circle are also peculiar, but do not call for notice in our present subject.

The most remarkable earthwork yet surveyed is one on the open down near Orcheston, Wilts; remarkable not for the detail of it, but for its form, which is a true ellipse, with an average error of only 11 inches on about 400 feet: and when it is considered that it has been all ploughed over, and unhappily a quarter of it obliterated, it is plain that the errors of determining its original form will swallow up most if not all of this 11 inches. The outer ellipse has apparently been made by eye or rough measurement from the inner, after that was formed. Such a regular outline as this inner ellipse might occur by chance, and we may say that in thousands of chance forms one ellipse would probably occur as accurate as this; but the improbability of finding it among only a couple of dozen of circular figures yet surveyed is such as to place its intentional construction beyond any reasonable doubt.

An ellipse shows far more than other figures the method of the constructors; remembering that there are three practical ways of describing an ellipse: first, by lengthening all the parallel chords of a circle in one direction; second, by a rectangular trammel, and a beam with points traversing the trammel; third, by a cord fastened at the two ends to the two foci, and a point traversing within the cord, by examination it can be toler-

ably settled in which of these three ways it was drawn. Not that I would suggest that this shows the constructors to have understood conic sections, or the properties of an ellipse; but merely that they knew by some means how to draw an elegant and regular oval figure, which was probably all they cared for. In the first two methods, by elongating a circle and by trammel and beam, the major and minor axes of the ellipse are the measured points; whereas by the third method by string, which is easier, the major axis and the distances of the foci are the measured points. Now a man who cared enough for an unusual figure to take such trouble about it, would certainly lay it out by measure, especially as we see that far less regular and intellectual works have considerable uniformity of dimensions. On looking then at the proportions of it, we find that the major axis has no simple ratio to the distance of the foci, therefore the third or string method was not adopted in laying it out; but the major and minor axes have exactly the ratio of 12:10, and from this the only conclusions are that either, *first*, the ellipse was laid out by elongating a circle drawn on the ground, which would require the worker at least to be capable of dividing any line into five parts and then adding a fifth to it; or else, *second*, that some modification of the beam and trammel method was adopted. This last is more probable, as two lines at right angles in the ground, a cord, and three men, is all that would be absolutely necessary. But though the means may be thus simple, the work shows an amount of appreciation of geometry, and of skill in execution, that would be rather surprising to find even in Greece or Egypt; and, when we look at the ground inside the work, and find it strewn with hundreds of chipped flints, which are only found perhaps once in a mile's walk outside the enclosure—the inferences, either that the flint workers appreciated and drew an ellipse, or else that we have here a series of coincidences which seem beyond all probability, are such as to warrant an exhaustive examination of these remains.

It might be suggested that a more intellectual people subsequently occupied a flint workers' site, and there laid out the ellipse; but there is nothing to give colour to such an idea, as there is no natural peculiarity of the ground to induce a man to settle there in preference to any other part of the gently sloping downs all around it; and it would be very unlikely that a subsequent people should exactly pitch on the spot strewn with flint flakes, and exactly enclose it with this ellipse, without having any of the flint-strewn ground outside it. As before mentioned, in this as in all the experimental sciences, everything rests on what is likely and unlikely, and until equally distinct evidence to the contrary can be given, the only reason-

able conclusion seems to be to accept the facts as they stand, and say that at present it appears to be 10 to 1, 100 to 1, or 1,000 to 1, that such and such things are true, whatever may be said by preconceived ideas apart from facts.

The principles of inductive metrology are not to be judged by their least certain application to the ruder and non-historic remains, of which we have looked at a few examples; but by their results when applied to the works of people whose history and measures are not open to such doubts and uncertainties. There, as far as researches have gone, the conclusions are closely in accord with what is already known, though many important and interesting facts have also appeared; such as the much wider extension of the use of the various ancient cubits and feet, beyond what is historically recorded; the recovery of many measures altogether lost in history; and the identity of some measures in countries between which no such connection was expected. For the details of these researches, and for full particulars of the working methods of examination, and of the application of probable error to all such numerical results, I can only refer to the published essay, "*Inductive Metrology*," in which I have stated the subject, in order to obtain the opinions and guidance of those versed in such inquiries.

I will only add that there are two important requirements connected with this subject, which can only be forwarded by such an institution as this: first, to urge on travellers to make accurate measurements of all regular and symmetrical remains which they describe—a dozen accurate measurements, of only two or three feet long, are worth more than any rough paces and guesses at general dimensions; secondly, to organize some plan of ensuring the accurate survey of remains in England, especially such as are in danger of destruction, or are already slightly ploughed over. The remarkable details that are brought to light by careful survey of these works will sufficiently recommend the necessity of rescuing them by recording their forms, even where their unimposing nature may deprive them of the legal protection now recognised as necessary for the principal remains of this country.

DISCUSSION.

Mr. TYLOR said that, on reading Mr. Petrie's work, he had suggested to him the desirability of bringing his method and results before the Anthropological Institute. The introduction of the exact method of mathematical comparison into the study of culture was in his opinion a step of much importance. Mr. Petrie's method of ascertaining the standard measures in use in any country by taking dimensions of structures as probably multiples in round numbers

of the unit, is based on that of Newton in his "Dissertation on Cubits," but makes an advance upon it by subjecting the probable errors of measurement, &c., to calculation according to the theory of probabilities. Mr. Tylor hoped Mr. Petrie's results would be subjected to careful criticism, as if found correct they would afford great help in tracing the lines by which civilization has travelled. To students of barbaric culture, Mr. Petrie's attempt in his book to prove the existence among the mound-builders of North America of a unit closely approaching a measure used in the Old World deserved careful consideration. With reference to the elliptic enclosure described by Mr. Petrie, Mr. Tylor thought the evidence tending against the notion of its having been drawn with a line from two set foci, and that the elongation or diminishing of the ordinates of a smaller or larger circle was rather more likely.

On the GAME of PATOLLI in ANCIENT MEXICO, and its PROBABLY ASIATIC ORIGIN. By E. B. TYLOR, Esq. D.C.L., F.R.S.

THE group of games to which our *backgammon* belongs is ancient and widely spread over the world. In it a number of pieces are moved on a diagram or board, not at the player's free choice as in draughts or chess, but conformably to the throws of lots or dice. One can hardly doubt, from the peculiar combination of chance and skill here involved, that all the games coming under this definition must be sprung from one original game, though this cannot now be clearly identified, and may indeed have disappeared many ages since. The closeness of correspondence between the abacus or reckoning-board with its little stones or calculi moved on its lines or spaces, and the board and pieces for ancient backgammon, which were even called by the same names, strongly suggests the idea that the original backgammon arose out of the sportive use of the calculating-board. Its descendants, the backgammon family, fall into two groups of games: those played with numbered dice, and those played with two-faced lots which can only fall in two ways, as we say "head or tail." These two groups of games may be conveniently called *dice-backgammon* and *lot-backgammon*. Dice-backgammon makes its appearance plainly in classic history. The game of the "twelve lines" (*duodecim scripta*) was played throughout the Roman Empire, and passed on with little change through mediæval Europe, carrying its name of *tabulæ*, *tables*; its modern representatives being French *trictrac*, English *backgammon*, &c. Among ancient Greek games, the *kubeia* or "dice-playing" is shown by various classical passages to have been of the nature of backgammon. It appears from Plutarch that in early times