LETTERS TO THE EDITOR.

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Average Number of Kinsfolk in each Degree.

WHAT is the average number of brothers, sisters, uncles, nephews, nieces, first cousins, &c., that each person possesses? I had occasion to compute this for a particular collection of persons; the results were so far unexpected as to show that the question deserved a consideration which it has not yet received, so far as I am aware. The problem proved easy enough in the end, but not at first, for there are other ways of attacking it, in which I blundered and lost time.

The simplest conditions that will serve for a general theory are those of a supposed population (1) the numbers of which are statistically constant in successive generations; (2) the generations of which do not overlap; and (3) which are "completed" by having wholly passed into history; and again (4) where every person is taken into account, at whatever age he or she may have died. It will be a further great simplification if it be allowed (5) to suppose the males and females to be equal in number, and in all respects to admit of similar statistical treatment. This need be only a provisional way of looking at the problem, for it will be seen that corrections can easily be introduced if desired.

It will much facilitate matters to begin by dealing exclusively with either the male or the female half of the population, leaving the other half to follow suit. We will begin with the females.

Let d be the average number of female children born of each woman who is a mother, so if there be n mothers in the population the total number of females in the next generation will be nd . How many of these latter will prove fertile of female children? On the supposition of statistical constancy, the number of mothers in the two generations will be the same, therefore d out of the nd will be fertile of female children; conversely, the probability that any one of these female children will herself bear one or more female children $= 1/d$. As a test of this, the average number of fertile daughters to each mother will be $d \times 1/d = 1$, as it should be.

Next, as regards sisterhoods. Each mother bears on the average d female and d male children, or $2d$ individuals in all. Each of these will have $2d-1$ brothers and sisters, and half that number of sisters, namely, $d-\frac{1}{2}$.

The syllable *si* will be used to express "sisters" without regard to age or fertility, and *si'* to express "sisters who are fertile of female children"; similarly *da* and *da'* for daughters.

The number therefore of *si* is $d-\frac{1}{2}$, of *si'* it is $(d-\frac{1}{2})/d$, of *da* it is d , of *da'* it is 1 . The number of *me'*, or of mothers to a child, is, of course, 1 , and there is no occasion for using *me*, as a mother must be fertile.

A few examples of results are given in the following table; it could have been extended indefinitely, but these are quite sufficient for drawing conclusions:—

Specific kinships.	Average number in each	
ANCESTRY—		
<i>me'</i> (mother)	1	1
<i>me' me'</i> (mother's mother)	1 × 1	1
<i>me' me' me'</i>	1 × 1 × 1	1
COLLATERALS—		
<i>si</i> (sisters)	$(d - \frac{1}{2})$	$d - \frac{1}{2}$
<i>me' si</i> (mother's sisters)	$1 \times (d - \frac{1}{2})$	$d - \frac{1}{2}$
<i>me' me' si</i>	$1 \times 1 \times (d - \frac{1}{2})$	$d - \frac{1}{2}$
<i>si' da</i> (sister's daughters)	$(d - \frac{1}{2})/d \times d$	$d - \frac{1}{2}$
<i>me' si' da</i>	$1 \times (d - \frac{1}{2})/d \times d$	$d - \frac{1}{2}$
<i>si' da' da</i>	$(d - \frac{1}{2})/d \times 1 \times 1/d$	$d - \frac{1}{2}$
DESCENDANTS—		
<i>da</i> (daughters)	<i>d</i>	<i>d</i>
<i>da' da</i> (daughter's daughters)	1 × <i>d</i>	<i>d</i>
<i>da' da' da</i>	1 × 1 × <i>d</i>	<i>d</i>

The foregoing remarks and table are equally applicable to males if *bro* (brother) is substituted for *si*, *son* for *da*, *fa* (father) for *me*.

It will, then, be understood that each mother, father, or fertile couple has, on the average, d sons and d daughters, or $2d$ children altogether, of whom 1 is a fertile son, 1 a fertile daughter, and that the others die without issue. In the collection mentioned above, the value of d was about $2\frac{1}{2}$, that is to say, an average family consisted of about 5 children, which is a usual estimate.

It is unnecessary to prolong these remarks by considering the minor corrections to be supplied on account of the hypotheses not being strictly accordant with observation. The two most important of these relate to populations that are not stationary, and to the allowance to be made for inequality in number of the sexes. There are others hardly worth even the trouble of describing, being utterly insensible in rough work.

The general results are that kinships fall into three distinct groups:—(1) direct ancestry, (2) collaterals of all kinds, (3) direct descendants, and that the number of individuals in each *specific* kinship in these classes is respectively 1 , $d-\frac{1}{2}$, and d . Also that $d=2\frac{1}{2}$ may be accepted as a reasonable and not infrequent value. To determine the number of individuals in each *general* kinship, the appropriate tabular number must be multiplied by the number of *species* that the *genus* contains; thus there are two species of aunts, *me si* and *fa si* (mother's sisters and father's sisters), each of which has the tabular number of $d-\frac{1}{2}$; therefore the average number of aunts is twice that amount, or $2d-1$, which, in the above case of $d=5$, is equal to 4.

FRANCIS GALTON.

The Mendelian Quarter.

A FEW weeks ago we heard in Section D at the Cambridge meeting of the British Association a paper by Mr. A. D. Darbishire on the bearing of his experiments in crossing Japanese waltzing and albino mice on Mendelian theory. He told us that on that theory we should expect a quarter of the offspring of the hybrids to be albinos—and we found them albinos—and a quarter of the offspring of the hybrids to waltz—and they did waltz. Somebody protested *sotto voce*, and Mr. Darbishire added "a rough quarter." Since that meeting I have been looking up the matter, for the point seems to me of great interest, and this is what I find in a recent paper by Mr. Darbishire in the *Manchester Memoirs*, "On the Bearing of Mendelian Principles of Heredity on Current Theories of the Origin of Species," vol. xlviii. p. 13:—"Let us consider the offspring of hybrids . . . Secondly with regard to their progression, we should expect to find 25 per cent. waltzing mice: this is very roughly what happens; . . . Now let us look at the offspring of hybrids from *both* points of view at the same time: one mouse in every four is an albino; one in every four is a waltzer, so we should expect one in every sixteen to be an *albino waltzer*. Now these albino waltzers are new things . . ." and then Mr. Darbishire tells us that he has been unable to get offspring from them.

Here, from a quarter, we have got to a quarter "very roughly," but still "one mouse in every four is a waltzer." I must confess that Mr. Darbishire's "rough quarter" excited me to look further, and these are the words I find describing some actual experiments on these mice:—"Waltzing occurs in only 97 out of the 555 individuals resulting from the union of hybrids. When we compare this with the number of pink eyed individuals (131-134) or of albinos (137) we see that the proportion of waltzing individuals *cannot be regarded as a possible quarter*. The probable error of the expectation that a quarter of the individuals will waltz is, on the Mendelian hypothesis, $0.6745\sqrt{\frac{1}{4} \times \frac{3}{4} \times 555} = 6.88$ only, and the observed deviation is $138.75 - 97 = 41.75$, the odds against so great a deviation being rather more than 50,000 to 1. As the result here obtained differs from Mendelian expectation in the same direction as that already obtained by von Guaita and to an extent consistent with the agreement of both, *the evidence that the waltzing character does not segregate in Mendelian proportions is very strong*."

The sentences in italics are not in italics in the original,

but I want to emphasise them. The words quoted are from an article on the "Result of Crossing Japanese Waltzing with Albino Mice" in *Biometrika*, vol. iii. p. 20. The writer appears to be a Mr. A. D. Darbishire of Oxford, not of Manchester. The one Mr. Darbishire considers that the proportions cannot be regarded as a possible quarter, the other that a rough quarter, or "one mouse in every four," is waltzing. Mr. Darbishire of Manchester expects that one in every sixteen of the offspring of the hybrids will be an albino waltzer, and then proceeds to state that he has so far been unable to breed from these albino waltzers. Reading his paper, I presumed he would have told us had he not found albino waltzers to be 1 in 16. Consulting, however, Mr. Darbishire of Oxford, I find he had 20 instead of 35 albino waltzers among his 555 offspring. I presume that $20=35$ is a "rough" sixteenth to our Manchester author, while he of Oxford would doubtless have been able to tell us that the odds against such an underestimate were two or three hundred to one! Which writer shall a member of the inquiring general public trust? Or, if the two writers should be the same, must we assume that in Oxford, under the influence of some recessive biometer, Mr. Darbishire failed to see that 97 in 555 was a reasonable quarter, or 20 in 555 a reasonable sixteenth, but that he has learnt in Manchester, or perhaps in Cambridge from some dominant anæsthetist, that these things really are so?

But if 97 be not even roughly 139, or 20 approximately 35, would it not be well at once to admit that the waltzing habit corresponds to a compound allelomorph, one element of which, the *chorophore*, may be credited to any mouse, but only becomes patent when combined with the *chorogen* to form the true waltzing habit? I am not sure this will work, but perhaps Mr. Darbishire will give it a trial. Should this in turn fail, a metaphysician might help him out of these procrustean difficulties by analysing straightforward advance into right-handed and left-handed elements, each with its own *chorophore* and *chorogen*—but I must not anticipate the details of such a remarkable progression at present.

KARL PEARSON.

The *n*-Rays.

THE inability of a large number of skilful experimental physicists to obtain any evidence whatever of the existence of the *n*-rays, and the continued publication of papers announcing new and still more remarkable properties of the rays, prompted me to pay a visit to one of the laboratories in which the apparently peculiar conditions necessary for the manifestation of this most elusive form of radiation appear to exist. I went, I must confess, in a doubting frame of mind, but with the hope that I might be convinced of the reality of the phenomena, the accounts of which have been read with so much scepticism.

After spending three hours or more in witnessing various experiments, I am not only unable to report a single observation which appeared to indicate the existence of the rays, but left with a very firm conviction that the few experimenters who have obtained positive results have been in some way deluded.

A somewhat detailed report of the experiments which were shown to me, together with my own observations, may be of interest to the many physicists who have spent days and weeks in fruitless efforts to repeat the remarkable experiments which have been described in the scientific journals of the past year.

The first experiment which it was my privilege to witness was the supposed brightening of a small electric spark when the *n*-rays were concentrated on it by means of an aluminium lens. The spark was placed behind a small screen of ground glass to diffuse the light, the luminosity of which was supposed to change when the hand was interposed between the spark and the source of the *n*-rays.

It was claimed that this was most distinctly noticeable, yet I was unable to detect the slightest change. This was explained as due to a lack of sensitiveness of my eyes, and to test the matter I suggested that the attempt be made to announce the exact moments at which I introduced my hand into the path of the rays, by observing the screen. In no case was a correct answer given, the screen being announced as bright and dark in alternation when my hand was held

motionless in the path of the rays, while the fluctuations observed when I moved my hand bore no relation whatever to its movements.

I was shown a number of photographs which showed the brightening of the image, and a plate was exposed in my presence, but they were made, it seems to me, under conditions which admit of many sources of error. In the first place, the brilliancy of the spark fluctuates all the time by an amount which I estimated at 25 per cent., which alone would make accurate work impossible.

Secondly, the two images (with *n*-rays and without) are built of "instalment exposures" of five seconds each, the plate holder being shifted back and forth by hand every five seconds. It appears to me that it is quite possible that the difference in the brilliancy of the images is due to a cumulative favouring of the exposure of one of the images, which may be quite unconscious, but may be governed by the previous knowledge of the disposition of the apparatus. The claim is made that all accidents of this nature are made impossible by changing the conditions, *i.e.* by shifting the positions of the screens; but it must be remembered that the experimenter is aware of the change, and may be unconsciously influenced to hold the plate holder a fraction of a second longer on one side than on the other. I feel very sure that if a series of experiments were made jointly in this laboratory by the originator of the photographic experiments and Profs. Rubens and Lummer, whose failure to repeat them is well known, the source of the error would be found.

I was next shown the experiment of the deviation of the rays by an aluminium prism. The aluminium lens was removed, and a screen of wet cardboard furnished with a vertical slit about 3 mm. wide put in its place. In front of the slit stood the prism, which was supposed not only to bend the sheet of rays, but to spread it out into a spectrum. The positions of the deviated rays were located by a narrow vertical line of phosphorescent paint, perhaps 0.5 mm. wide, on a piece of dry cardboard, which was moved along by means of a small dividing engine. It was claimed that a movement of the screw corresponding to a motion of less than 0.1 of a millimetre was sufficient to cause the phosphorescent line to change in luminosity when it was moved across the *n*-ray spectrum, and this with a slit 2 or 3 mm. wide. I expressed surprise that a ray bundle 3 mm. in width could be split up into a spectrum with maxima and minima less than 0.1 of a millimetre apart, and was told that this was one of the inexplicable and astounding properties of the rays. I was unable to see any change whatever in the brilliancy of the phosphorescent line as I moved it along, and I subsequently found that the removal of the prism (we were in a dark room) did not seem to interfere in any way with the location of the maxima and minima in the deviated (!) ray bundle.

I then suggested that an attempt be made to determine by means of the phosphorescent screen whether I had placed the prism with its refracting edge to the right or the left, but neither the experimenter nor his assistant determined the position correctly in a single case (three trials were made). This failure was attributed to fatigue.

I was next shown an experiment of a different nature. A small screen on which a number of circles had been painted with luminous paint was placed on the table in the dark room. The approach of a large steel file was supposed to alter the appearance of the spots, causing them to appear more distinct and less nebulous. I could see no change myself, though the phenomenon was described as open to no question, the change being *very* marked. Holding the file behind my back, I moved my arm slightly towards and away from the screen. The same changes were described by my colleague. A clock face in a dimly lighted room was believed to become much more distinct and brighter when the file was held before the eyes, owing to some peculiar effect which the rays emitted by the file exerted on the retina. I was unable to see the slightest change, though my colleague said that he could see the hands distinctly when he held the file near his eyes, while they were quite invisible when the file was removed. The room was dimly lighted by a gas jet turned down low, which made blank experiments impossible. My colleague could see the change just as well when I held the file before his face, and the substitution of a piece of wood of the same size and