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MARRIAGES *between* FIRST COUSINS *in* ENGLAND *and their* EFFECTS.

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I.—*The Proportion of First Cousin Marriages to all Marriages.*

IT is well known that when the Census Act, 1871, was passing through the House of Commons, an attempt was made by Sir J. Lubbock, Dr. Playfair, and others, to have a question inserted with respect to the prevalence of cousin marriages, under the idea that when we were in possession of such statistics we should be able to arrive at a satisfactory conclusion as to whether these marriages are, as has been suspected, deleterious to the bodily and mental constitution of the offspring. It is unfortunately equally well known that the proposal was rejected, amidst the scornful laughter of the House, on the ground that the idle curiosity of philosophers was not to be satisfied.

It was urged, that when we had these statistics it would be possible to discover by inquiries in asylums, whether the percentage of the offspring of consanguineous marriages amongst the diseased was greater than that in the healthy population, and thus to settle the question as to the injuriousness of such marriages. The difficulty of this subsequent part of the inquiry was, I fear, much underrated by those who advocated the introduction of these questions into the census. It may possibly have been right to reject the proposal on the ground that every additional question diminishes the trustworthiness of the answers to the rest, but in any case the tone taken by many members of the House shows how little they are permeated with the idea of the importance of inheritance to the human race.

In the summer of 1873 the idea occurred to me that it might be, in some measure, possible to fill up this hiatus in our national statistics. In looking through the marriages announced in the "Pall Mall Gazette," I noticed one between persons of the same surname; now, as the number of surnames in England is very large, it occurred to me that the number of such marriages would afford a clue to the number of first cousin marriages.

In order to estimate what proportion of such marriages should be attributed to mere chance, I obtained the "Registrar-General's "Annual Report" for 1853, where the frequency of the various surnames is given. I here find the following table, p. xviii.:—

Number of Persons whose Names were Registered.	Number of Different Surnames occurring in the Whole Register.	Number of Different Surnames to every 100 Persons, *i.e.*, $\frac{275,405}{100} \times 11{\cdot}9 = 32,818$.	Number of Persons to One Surname, *i.e.*, $8{\cdot}4 \times 32,818 = 275,405$.
275,405	32,818	11·9	8·4

The fifty commonest names embraced 18 per cent of all the population. It appears that one in 73 is a Smith, one in 76 a Jones, one in 115 a Williams, one in 148 a Taylor, one in 162 a Davies, one in 174 a Brown, and the last in the list is one Griffiths in 529. Now it is clear that in one marriage in 73 one of the parties will be a Smith, and if there were no cause which tended to make persons of the same surname marry, there would be one in 73^2 or 5,329 marriages, in which both parties were Smiths. Therefore the probability of a Smith-Smith marriage, *due to mere chance*, is $\frac{1}{5329}$; similarly the chance of a Jones-Jones, a Davies-Davies and a Griffiths-Griffiths marriage would be $\frac{1}{76^2}$, $\frac{1}{162^2}$ and $\frac{1}{529^2}$, respectively. And the sum of fifty such fractions would give the probability of a *chance* marriage, between persons of the same surname, who owned one of these fifty commonest names. The sum of these fifty fractions I find to be 0·0009207, or 0·9207 per thousand. It might however be urged, that if we were to take more than fifty of the common names, this proportion would be found to be much increased. I therefore drew a horizontal straight line, and at equal distances along it I erected ordinates proportional to $\frac{1}{73^2}$, $\frac{1}{76^2}$, $\cdots$ $\frac{1}{529^2}$. As I found that the names decreased in value very gradually, I, as a fact, omitted every other name; but had I taken every name the result would have been sensibly the same. The upper ends of these twenty-five ordinates were found to lie in a curve of great regularity,

remarkably like a rectangular hyperbola, of which my horizontal straight line was one asymptote; and the ordinate corresponding to Griffiths was exceedingly short. Observing the great regularity of the curve, I continued it beyond the fiftieth surname by eye, until it sensibly coincided with the asymptote, at a point about where the hundred and twenty-fifth name would have stood, and then cut out the whole (drawn on thick paper), and weighed the part corresponding to the fifty surnames, and the conjectural part. The conjectural addition was found to weigh rather more than one-tenth of the other part (*i. e.* $\frac{124}{920}$); and as the chance of same-name marriages is proportional to the areas cut out, I think I may venture confidently to assert, that in England and Wales, about one marriage in a thousand takes place in which the parties are of the same surname, and have been uninfluenced by any relationship between them bringing them together. Now, it will appear presently that far more than one marriage in a thousand is between persons of the same surname; and, as I do not profess to have attained results of an accuracy comparable to 0·1 per cent., I am entitled to say that same-name marriages, when they take place, are due to the consanguinity of the parties. If it permitted such accuracy, the method pursued would however include a compensation for this disturbing cause.

With the help of an assistant the marriages announced in the "Pall Mall Gazette" in the years 1869-72, and part of 1873, were counted, and were found to be 18,528. Out of these 232 were between persons of the same surname, that is 1·25 per cent. were same-name marriages. The same marriage is occasionally announced twice over, but as there can be no reason to suppose that this course has been pursued oftener or seldomer with same-name marriages than with others, the result will not be vitiated thereby. In order to utilize this result, it now became necessary to determine—

(1). What proportion of this 1·25 per cent. were marriages between first cousins.

(2). What proportion marriages between first cousins of the same surname, bear to those between first cousins of different surnames.

If these two points could be discovered, the percentage of first-cousin marriages *in the upper classes* could be at once determined. I have endeavoured to find out these proportions in several ways.

An assistant was employed to count the marriages of the *men* in the pedigrees of the English and Irish families occupying about 700 pages of "Burke's Landed Gentry," marking every case where the marriage was "same-name." I then tried in every such case

to discover, from a consideration of the pedigree, whether the marriage had been between first cousins. I found that in a certain number of cases I was unable to discover this. The total number of pedigrees in the 700 pages was about 1,300; and of these I had to exclude 71, thinking that by only including family trees where I could discover the relationship of the parties, I should not obtain an unfair selection of the whole. The marriages of the men alone were included, because, had I included those of the women, many marriages would have been counted twice over,—once in the pedigree under consideration, and again in that of the husband. In this way, then, I found that out of 9,549 marriages given by Burke 72 were same-name first cousin marriages, and 72 were same-name marriages not between first cousins. This gives the percentage of same-name marriages as 1·5 (not strikingly different from the 1·25 deduced from the "Pall Mall Gazette"), and of this percentage 0·75 is to be attributed to first cousin marriages.

I further collected in the same way 1,989 marriages from the "English and Irish Peerage," and of these 18 were same-name first-cousin marriages, or 0·91 per cent. The number of same-name marriages not being first cousin marriages was not however compared in this case. It will be observed, that the proportion is nearly 0·2 per cent. higher than with the "Landed Gentry," and as the nobility are known to marry much *inter se*, this was perhaps to be expected; however, 2,000 is too small a number on which to base a conclusion on this head with safety. The Peerage and Burke combined give 90 out of 11,538, or 0·78 per cent., of same-name first cousin marriages.

The next step was to send out a very large number of circulars (about 800) to members of the upper middle and upper classes, in which I requested each person to give me the names of any members of the following classes, who married their first cousins; viz., (1) the uncles, aunts, father and mother of the person; (2) the brothers, sisters and the person himself; (3) the first cousins of the person. I further asked for the names of any persons in the above classes, who contracted same-name marriages *not* with first cousins. I confined my questions to near relations, because, had the more distant ones been included, a risk was run of getting a selected set of marriages,—a risk which I am inclined to suspect was not avoided, as will hereafter appear.

In about 300 of the circulars I further asked for the total number of marriages contracted by the persons included in the Classes 1, 2 and 3. Care was taken to exclude, as far as possible, those persons who had cousins in common, so that each answer should embrace a fresh field. I must here return my thanks to the many persons who so kindly filled in and returned the circulars.

The following result was obtained:—

TABLE A.

Same-Name First Cousin Marriages.	Different-Name First Cousin Marriages.	Same Name *not* First Cousin Marriages.
66	182	29

From 181 circulars returned, in which the total number of marriages in each class was given, the following was the result:—

TABLE B.

Total Number of Marriages.	Total Number of First Cousin Marriages.	Percentage of First Cousin Marriages.	Percentage of Same-Name Marriages, whether Cousin or not Cousin.
3,663	125	3·41	1·38*

* Compare this with 1·25 deduced from "Pall Mall Gazette."

Persons having no cousin marriages to fill in were asked to return the circular blank, in those cases where the total number of marriages was not asked for. Of such blank returns, together with those where the total number of marriages was not given, 207 came back to me. From Table (B) it will be seen that $\frac{3,663}{181}$, or 20·2 marriages were recorded in each return; to judge, therefore, of the congruity of the 207 blank returns with the others, I impute to each of these 207 circulars, 20 marriages, and therefore add 4,140 marriages to the 3,663; as a grand total, with this conjectural addition, the following is the result:—

TABLE C.

Total Number of Marriages.	Total Number of First Cousin Marriages.	Percentage of First Cousin Marriages.
7,803	248	3·18

It is thus seen that the 207 returns are tolerably congruent with the 181 returns in Table (B); for 3·18 differs but slightly from 3·41.

From Table (A) it is seen that there were 182 different-name cousin marriages to 66 same-name cousin marriages; *i.e.*, for every

same-name cousin marriage there were $2\frac{3}{4}$ different-name cousin marriages.

And again, there were 66 same-name cousin marriages to 29 same-name-not-cousin marriages; that is rather more than two to one. This last result disagrees so much with that obtained from Burke and the Peerage, where the proportion was, as above stated, found to be as 1 to 1, that I am inclined to suspect that I had either a run of luck against me, or more probably that a considerable number of marriages between persons of the same surname, not being first cousins, escaped the notice of my correspondents. This latter belief is somewhat confirmed by what follows. If, however, I combine the results obtained from Burke with those from my circulars, I obtain the following:—

$$\frac{\text{Same-name cousin marriages}}{\text{All same-name marriages}} = \frac{142}{249} = \cdot 57.$$

And in default of anything more satisfactory I am compelled to accept this result as the first of my two requisite factors.

As to the second factor, the proportion $2\frac{3}{4} : 1$ for different-name cousin marriages to same-name cousin marriages is, I fear, also unsatisfactory. But before entering on this point I will indicate the sources of error in my returns:—

(1). The sensitiveness of persons in answering the question in cases where there are cousin marriages, particularly when any ill results may have accrued.

(2). The non-return by persons who had no such marriages to fill in, and who would say, "I have no information, what is the use "of returning this?"*

(3). The ignorance of persons of the marriages of their relations. This ignorance would be more likely to affect the returns of different-name marriages than of same-name ones. I feel convinced that this has operated to some extent, as will be seen hereafter.

(4). In the cases of same-name marriages, persons would be more likely to know of the marriages between first cousins, than of other such marriages. The discrepancy between Burke and my circulars leads me to believe that this too has operated.

I have been much surprised to find how very little people know of the marriages of their relations, even so close as those comprised in my three classes. As it is clear that the marriages contracted by a man's uncles and aunts and by his brothers and sisters, would be less likely to escape his notice than would those contracted by his first cousins, I made an analysis of my circulars, including only

* The circulars were ready stamped for return, which would induce many to return them by saving trouble.

the first two classes, viz.: (1) uncles, aunts, father and mother; (2) brothers and sisters and the person himself, with the following result:—

TABLE D.

Same-Name, First Cousin Marriages.	Different-Name, First Cousin Marriages.	Same-Name, *not* First Cousin Marriages.
42	121	21

And from the returns where the total number of marriages were required, the following is the result:—

TABLE E.

Total Number of Marriages.	Total Number of First Cousin Marriages.	Percentage of First Cousin Marriages.
1,929	81	4·2

It appears then, that

$$\frac{\text{Same-name cousin marriages}}{\text{Different name cousin marriages}} = \frac{1}{3} \text{ nearly,}$$

and

$$\frac{\text{Same-name cousin marriages}}{\text{All same-name marriages}} = \frac{2}{3}.$$

And these results I take to be more trustworthy than those given above, but I think that even here many different-name first cousin marriages and same-name-not-cousin marriages have escaped notice, and that the indirect method, to which I now proceed, is on the whole more reliable.

It is possible to discover the proportion between the same-name and different-name marriages in an entirely different way, and this I have tried to do. A man's first cousins may be divided into four groups, viz.: the children of (*a*) his father's brothers, (*b*) of his father's sisters, (*c*) his mother's brothers, (*d*) his mother's sisters. Of these four groups only (*a*) will in general bear the same surname as the person himself. On the average, the number of marriageable daughters in each family of each of the four groups will be the same. Were the four groups then equally numerous, we might expect that the same-name would bear to the different-name marriages the proportion of one to three. Since however a man cannot marry his sisters, this cannot hold good; for the classes (*a*) and (*d*) are clearly on the

average smaller than (b) and (c), and the proportion we wish to discover is $\frac{(a)}{(b)+(c)+(d)}$, which must evidently be less than $\frac{1}{3}$. To take a numerical example: A's father is one of 3 brothers, who married and have children, and A's father had 2 sisters, who married and have children: A's mother had 1 brother, who married and has children, and was one of 5 sisters, who married and have children. Then clearly the class

(a)	consists of	2 families.
(b)	,,	2 ,,
(c)	,,	1 family.
(d)	,,	4 families.

So that the above fraction becomes $\frac{2}{2+1+4} = \frac{2}{7}$. In this case we may conclude that if A marries a first cousin, it is 5 to 2 that he will marry one of a different surname. In another case the numbers might have been different, and therefore the fraction and the betting also different. And what we wish to discover is the *average* value of this fraction. But, for the various members of a large community, there will be a very large number of such fractions, and some will occur more frequently than others; so that in finding this average value, each fraction should have its proper weight assigned to it. In order to assign the weight to,—say the above fraction $\frac{2}{7}$, we must take a thousand families and find in how many of them there were 3 sons and 2 daughters who married and had children, and in how many there were 1 son and 5 daughters who married and had children. Having sufficiently indicated how the required proportion may depend on probabilities, I may state that I sent out a number of circulars to members of the upper middle, and upper classes, and obtained and classified statistics with respect to 283 families. The following table gives the results, excepting that I have supposed that I had collected 1,000 families, that is, the numbers given in the table are the actual numbers multiplied by $\frac{1{,}000}{283}$.

N.B. (a, b) *means a family in which there are* (a) *sons who married and had children; and* (b) *daughters who married and had children. Only such families are included as have, so to speak, done marrying.*

—	0,1 82	0,2 39	0,3 21	0,4 7	0,5 0	0,6 4	—
1,0 92	1,1 117	1,2 99	1,3 29	1,4 14	1,5 7	1,6 0	—
2,0 64	2,1 78	2,2 43	2,3 46	2,4 11	2,5 14	2,6 0	—
3,0 39	3,1 32	3,2 32	3,3 14	3,4 4	3,5 7	3,6 0	3,7 4
4,0 7	4,1 28	4,2 14	4,3 4	4,4 7	4,5 11	4,6 0	—
5,0 0	5,1 0	5,2 14	5,3 0	5,4 4	5,5 0	5,6 0	—
6,0 0	6,1 7	6,2 0	6,3 0	6,4 0	6,5 4	6,6 0	—

As the number (283) of families collected is so small, the proportions of the rarer order of families will be of course incorrect, thus there are no families of the form (0,5), whilst there are four of the form (3,7). Any small error in these rarer orders of families will have but an infinitesimal effect on my results. I treated the question in four different ways. It might be supposed that a man, who had five families of first cousins in relation to himself, would be five times as likely to marry a first cousin, as a man who had only one such family, or again it might be supposed that he would be only equally likely. The truth, however, will certainly lie between these suppositions. The question, when treated from this point of view, leads to the result that $\frac{\text{same-name cousin marriages}}{\text{different-name cousin marriages}}$ is greater than $\frac{1}{4{\cdot}44}$ and less than $\frac{1}{4{\cdot}12}$. So that the true proportion would be about $\frac{1}{4\frac{1}{4}}$.

The two other methods are founded on the same grouping of families, and depend on the fact that my class (*a*) will on the average be equal in number to class (*d*); and class (*b*) to class (*c*), and all that is necessary is to find what value should be assigned to the ratio (*a*) or (*d*) : (*b*) or (*c*). It would be tedious to indicate the

precise method employed, but suffice it to say, that after a correction for the greater prevalence of the second marriages of men than of women, the result comes out that $\frac{\text{same-name cousin marriages}}{\text{different-name cousin marriages}}$ is greater than $\frac{1}{4{\cdot}23}$ and less than $\frac{1}{4{\cdot}14}$, so that the proportion would be really about $\frac{1}{4\frac{1}{6}}$; a result which differs but very slightly from that given by the two other methods.

The amount of arithmetical labour was so great that I was obliged in the first two methods to rank all families of a higher order than (3,3) as (3,3), or a family (5,1) as being the same as a family of the form (3,1); in the two latter methods I was able to go as high as (4,4). These higher order of families are of very rare occurrence, and thus the reduction to all families to those of lower orders, would not materially affect the results, but as far as it goes it would make the above fractions too small.

I think on the whole it may be asserted, that the same-name first-cousin marriages are to the different-name first-cousin marriages as 1 to 4. It may perhaps be worth mentioning that a second grouping of families from "Burke's Landed Gentry" led to almost identical results, notwithstanding the bias introduced by the fact that the eldest sons have a constant premium on marriage.

It appears to me on the whole that this latter result is considerably more reliable than that from my circulars, and this as before stated, I can only explain on the supposition that many different-name marriages have escaped notice. The whole is very perplexing, and may perhaps be held to make all my results valueless. My final result for the two required factors then is, that—

$$\frac{\text{Same-name first-cousin marriages}}{\text{All same-name marriages}} = {\cdot}57$$

and

$$\frac{\text{Same-name first-cousin marriages}}{\text{Different-name first-cousin marriages}} = \frac{1}{4}.$$

If this be applied to the percentage 1·25 of the "Pall Mall Gazette," we get 3·54 or $3\frac{1}{2}$ per cent. as the proportion of first cousin marriages to all marriages in the middle classes. If it be applied to the peerage we get $4\frac{1}{2}$ per cent., and for the landed gentry $3\frac{3}{4}$ per cent., and for both combined $3\frac{9}{10}$ per cent.—To sum up, the direct statistical method gives from $3\frac{1}{5}$ to $3\frac{2}{5}$ per cent., or including only the classes (1) and (2), comprising uncles, aunts, brothers, and sisters $4\frac{1}{5}$ per cent.; the indirect method $3\frac{1}{2}$ per cent.; and the partly indirect and partly statistical, founded on the peerage and Burke, gives $3\frac{9}{10}$. There is, however, some reason to suppose that the proportion is really higher amongst the landed classes. There is a

serious discrepancy between the direct and indirect method as to the proportion of same-name and different-name marriages, which goes far to invalidate the results.

Whether, however, these proportions are actually correct or not, there can be little doubt, that if the area taken is large enough, the percentage of first cousin marriages in any class is proportional to the per centage of same-name marriages; so that if the latter is, say only half the former, the cousin marriages are also only half. I therefore obtained from the General Registry of Marriages at Somerset House a return of the proportion of same-name marriages in 1872 in various districts, namely (1) London, (2) large towns, viz., Bradford, Leeds, Manchester, Portsmouth, Southampton, Exeter, Plymouth, Birmingham, Witney, Banbury, Northampton, Wellingborough, Peterborough, Bedford, and (3) Agricultural districts of Hampshire, Devonshire, Middlesex, Herts, Bucks, Oxon, Northampton, Huntingdon, Bedford, and Cambridge, &c. I must take this opportunity of returning my warm thanks to the superintendent of the statistical department, Dr. Farr, for the very great kindness both he and Mr. N. A. Humphreys of the General Registry Office, have shown in helping me in this inquiry by every way in their power. The following tables, in which the third column is introduced for the sake of comparison with the statistics from the "Pall Mall Gazette," give the results:—

	Number of Marriages Registered.	Per Cent. of Same-name Marriages.	Ratio to the Number (1·25) from "Pall Mall Gazette."	Percentage of First Cousin Marriages as deduced by previous Method.
1. London metropolitan districts	33,155	0·55	$\frac{1}{2}$	$1\frac{1}{2}$
2. Urban districts	22,346	0·71	$\frac{7}{12}$	2
3. Rural ,, ...	13,391	0·79	$\frac{2}{3}$	$2\frac{1}{4}$

The numbers in most of the towns and counties, taken individually, were too small to give any trustworthy results.

It thus appears that in London, comprising all classes, the cousin marriages are about half what they are in the upper middle class, that is probably $1\frac{1}{2}$ per cent. In urban districts they are about $\frac{7}{12}$ths of what they are in the upper middle classes, that is, probably 2 per cent. In rural districts they are about two-thirds of what they are in the upper middle classes, that is, probably $2\frac{1}{4}$ per cent. In the middle and upper middle class or in the landed gentry probably $3\frac{1}{2}$ per cent. In the aristocracy probably $4\frac{1}{2}$ per cent. This is in accordance with what might have been expected *à priori:* for the aristocracy hold together very much, the landed gentry

slightly less, the business class again less. And beginning from the other end, London is an enormous community, recruited from every part of England; the large towns form communities, only one degree less heterogeneous; and the country is still less heterogeneous. I am, however, somewhat surprised at finding the proportion in the rural districts so small, for one would imagine that agricultural labourers would hold together very closely.*

Persons accustomed to deal with statistics will be able to judge better than myself, what degree of reliance is to be placed on the previous results. My own *impression* is that there is not an error of 1 per cent. in asserting that amongst the aristocracy the proportion of first-cousin marriages to all marriages is 4½ per cent., and that for the upper middle classes, and the urban and rural districts, the error in the percentages is somewhat less, and lastly for London decidedly less. But this is an impression that I hardly know how to justify, and I therefore leave an ample field for adverse criticism.

II.—*Inquiries in Asylums.*

I now pass on to the second part of my inquiry, namely, the endeavour to discover, by collecting statistics in asylums, whether first cousin marriages are injurious or not.

The method I intended to pursue was as follows: to get the superintendents of asylums to ask each one of the patients under their charge, either personally or through their subordinates, the question, "Were your father and mother first cousins or not?" In the case of the insane, I thought, in my ignorance, that those who

* I may mention that Mr. Clement Wedgwood made very careful inquiries for me concerning 149 marriages of skilled artisans in the potteries, and did not find a single case of first cousin marriage, and only three where there was any kind of relationship between the husband and wife. He was further assured that such marriages never take place amongst them. It may be worth giving the following table of consanguineous marriages out of "Italia Economica nel 1873," kindly sent to me by Dr. Farr.

Consanguineous Marriages Contracted in Italy from 1868-70 *inclusive.*

Marriages.	1868-70.	Per Annum.	Per 100.	Percentage to all Marriages.
Between brothers-in-law and sisters-in-law	2,392	797	33·27	0·413
Uncles and nieces	292	97	4·05	0·050
Aunts and nephews	50	17	0·70	0·009
Cousins	4,455	1,485	61·98	0·769
Total	7,189	2,396	100·0	1·24

It must be borne in mind that in Roman Catholic countries, a dispensation is requisite to permit the marriage of cousins.

had charge of them would have so intimate a knowledge of the character of each individual case, as to be able to sift those whose answers could be depended on, from those who were quite untrustworthy. In this it appears that I was mistaken, as will be shown by the remarks sent me by the various gentlemen who so kindly took up this inquiry. I cannot help thinking, however, that they undervalue the statistics which they have collected for me. I must take this opportunity to return my warm thanks to all the gentlemen mentioned below for the immense pains they have been at in collecting these results. I could hardly have believed that so many men, much occupied by their business, could have shown a stranger so much kindness, more especially as many of them seemed convinced that their labours were almost in vain. To Dr. W. Lauder Lindsay, Dr. Crichton Browne, Dr. Maudsley, and Dr. Scott, I must return my especial thanks for the really extraordinary vigour with which they took up the subject, and gave me every help in their power.

The following tables give the results collected from lunatic and idiot asylums in England and Wales, Scotland and Ireland. Besides the results tabulated below, Dr. Wilkie Burman, of the Wilts County Asylum, Devizes, informed that he could collect no statistics worth giving; Dr. Bacon, of the Fulbourn Asylum, Cambridge, whilst kindly offering to persevere, expressed his conviction of the uselessness of the attempt; Dr. Shuttleworth, of the Royal Albert Asylum, Lancaster, estimated that, out of 200 patients, 5 per cent. were the offspring of first cousins; and Dr. Clouston, of the Royal Edinburgh Asylum, tells me that, out of 750 patients, two *said* that they were offspring of first cousin marriages, but most could not answer. The table of results is as follows:—

English and Welsh Asylums.	Doctors.	Number of Patients.		Answers to "were Parents First Cousins?"	
		Males.	Females.	Males.	Females.
1. West Riding, Wakefield (lunatics and idiots)	Crichton Browne	700	707	337	318
2. Hanwell (lunatic)	Rayner	166	214	110	145
3. Warneford, Oxford (lunatic)	Byewater Ward	30	29	20	
4. Mickleover, Derby (lunatic)	Murray Lindsay	174	190	99	99
5. Metropolitan District, Caterham (lunatic)	Adam	877	1,038	434	126
6. Glamorgan County (lunatic)	Yellowlees	254	238	102	116
7. Chester County (lunatic)	Lawrence	About 450		115	110
8. County Lunatic, Snenton, Nottingham	Phillimore	184	206	97	103
9. Grove Hall, Bow	Mickle	427	—	181	—
10. Hatton, Warwick	Oscar Woods	537		258	
11. Earlswood, Surrey (idiot)	Grabham	—		1,388	
12. Broadmoor Criminal (lunatic)	Orange	370		150	
Totals for England and Wales	—	8,170 very nearly.		4,308	
SCOTCH ASYLUMS.					
1. Montrose (lunatic)	Howden	179	227	49	92
2. Crichton Royal Institution, Dumfries	Gilchrist	87	59	31	20
3. Southern Counties, Dumfries	Anderson	178	140	108	92
4. Murray Royal Institution, Perth	Lauder Lindsay	42	38	28	16
5. Perth District, Murthly	McIntosh	99	130	37	41
Totals	—	585	594	253	261
		1,179		514	
IRISH ASYLUMS.					
1. Maryborough	Through Dr. Courtenay	217		—	
2. Limerick District	Courtenay	434		—	
Totals	—	651		—	

Untrustworthy, or unable to answer.		Offspring of First Cousins.		Observations.
Males.	Females.	Males.	Females.	
363	389	14	17	Examination conducted with great care; cases of doubt excluded. Almost all who gave answers were lunatic and not idiotic.
56	169	2 or 1	1	Only those are given as trustworthy where the history of the patient could be ascertained. Amongst the males there were twelve cases of doubtful consanguinity, but whether first cousins or not, is not stated.
39		0		Patients of the farmer and tradesmen class.
75	91	2	2	Dr. Lindsay thinks these statistics worth little.
433	912	8	12	Statistics very imperfect; trustworthiness of answers uncertain. The total number of patients is overstated by nine, but whether males or females I know not.
152	122	5	4	Statistics worth little. Of those who did not answer, 137 were ignorant, and 137 incapable.
about 225		1	2	Patients of the labouring class.
87	103	2 or 5	2 or 4	Statistics to be little depended on.
246	—	8	—	Patients old soldiers.
277		9 or 8		Patients, labourers and artisans. The offspring of first cousins belonged to seven families. Examination conducted with great care.
—		53		Facts derived from parents, and therefore tolerably trustworthy.
220		2		Dr. Orange places little reliance on these results.
about 3,860		149 or 142		Between 3·46 and 3·29 per cent. of the patients who answered said they were offspring of first cousin marriages.
130	135	2	6	Dr. Howden thinks the inquiry useless. No inquiry was made of the idiots in this asylum.
56	39	2	2	
70	48	4	4	
14	22	4	0	Dr. Lindsay thinks the results *very* doubtful. The failure to get answers was due to incapacity and refusal.
62	89	1	2	Patients paupers.
332	333	13	14	5·25 per cent. of the patients who answered said that they were offspring of first cousin marriages.
665		27		
—		2		Patients agricultural labourers.
—		3		Twenty patients of better class; the rest labourers.
—		5		No information as to numbers who failed to answer. Dr. C. considers these statistics of little value. Roman Catholics do not marry first cousins. 0·77 per cent. of *all* the patients say they are offspring of first cousin marriages.

The columns of observations show how very unsatisfactory the collectors consider these results. From various circumstances, it appears that the results from Earlswood, Hatton, and the West Riding Asylums are considerably more trustworthy than the others.

Including, then, only these three asylums, it appears that, out of 2,301 patients, 90 or 91 were offspring of first cousins, that is 3·9 per cent. The fact that this agrees pretty closely with the 3·4 per cent. deduced from the whole table, leads me to think that the trustworthiness of the results collected has been under-estimated by the collectors themselves.

At Hanwell, where also there were some circumstances leading one to believe in tolerable accuracy, the percentage is very small, and this agrees well with what I should have been led to expect, from the small percentage of cousin marriages I found in London, by the methods of the first part of this paper. It is to be observed, however, that there were twelve cases reported of doubtful *consanguinity*.

It will be seen that the percentage of offspring of first cousin marriages is so nearly that of such marriages in the general population, that one can only draw the negative conclusion that, as far as insanity and idiocy go, no evil *has been shown* to accrue from consanguineous marriages.

From the high percentage ($5\frac{1}{4}$) of offspring of first cousin marriages in the Scotch asylums, I should be led to believe that such marriages are more frequent in Scotland than in England and Wales, and from the mountainous nature of the country this was perhaps to be expected.

The methods of the first part of this paper throw no light on the question as far as concerns Scotland.

From the two Irish asylums no results whatever can be deduced.

But whatever the value of these statistics may be, the opinion of prominent medical men, who have had especial advantages of observation, and are many of them also men of science, cannot be without interest.

Dr. Crichton Browne writes to me that the investigation was impossible in the case of idiots, except through the medium of the parents. "It has always seemed to me that the great danger "attending such marriages consists in the intensification of the "morbid constitutional tendencies, which they favour. Hereditary "diseases and cachexiæ are much more likely to be shared by "cousins than by persons who are in no way related . . . . (and "these) are transmitted with more than double intensity when they "are common to both parents. . . . They seem to be the square or "cube of the combined volume. . . . Even healthy temperaments, "when common to both parents, often come out as decided cachexiæ

" in the children." He adds, that persons of similar temperaments ought not to intermarry. Elsewhere he tells me that he did not at first make sufficient allowance for the ignorance " and stupidity of " my patients." In such an investigation, congenital effects, he says, should be distinguished from the acquired. I fear, however, that I must leave this to some hands more skilful than mine.

Dr. Rayner, of Hanwell, says that amongst the fishermen of Whitstable there is much intermarriage. The results seem to show that the prevalent diathesis is developed, whether it be strumous, rheumatic, or otherwise. He says that it was very difficult to discover the facts from his patients.

Dr. Howden, of Montrose, says: " As regards insanity, my " own impression is, that unless there exists a hereditary predispo- " sition the marriage of cousins has *no effect* in producing it. . . . . " Neither in insanity nor in any other abnormal propensity do two " plus two produce four; there is always another factor at work " neutralising intensification and bringing things back to the " normal." Dr. Howden thus disagrees with Dr. Crichton Browne, who, I take it, would maintain that, in insanity, two plus two make more, and not less than four.

Dr. Lauder Lindsay is of opinion that the ill effects of cousin marriage, including insanity, are much less than represented. He quotes " Stonehenge " (Mr. J. H. Walsh), " On the Dog " (" Field " newspaper, p. 188, 1859), to the effect that in-and-in breeding sometimes reduces dogs " to a state of idiocy and delicacy of constitution, " which has rendered them quite useless . . . full of excitability " . . . with a want of mental capacity." He also urges the " impos- " sibility " of obtaining trustworthy answers from the patients themselves; and even the results of personal inquiries from the nearest relatives of the patients would be liable to much error. Several of my correspondents expressed a belief that consanguinity of parents was more potent in producing idiocy than insanity. The results from Earlswood do not seem, however, to confirm this, and here the results sent seemed peculiarly trustworthy.

I had intended to pursue my inquiries in hospitals and asylums for other diseases, but the attempt which I made with respect to deaf mutes has shown me that the difficulties which arise are so great, that it is almost useless to persevere in this course any further. I will now give the results which I have collected.

The first return relates to the College for the Blind at Worcester. The results were communicated through the kindness of the Rev. Robert Blair and Mr. S. S. Foster. The college is small, and only 20 cases are recorded, and particulars of each case were sent. Of these 20, the offspring of first cousins were one, and of second cousins a case of two brothers. Of the 20 cases, 2 were due to

accidents. Thus, out of 17 families, there was one offspring of first cousins.

Dr. Scott, of Exeter, has informed me that out of 241 families, in which there were children born deaf and dumb, there were 7 cases of first cousin marriage. In three or four of these families there were more than one child so afflicted.

Dr. Scott also kindly offered to place me in communication with the superintendents of a number of institutions for the deaf and dumb, and having availed myself of his kindness, I have collected the following answers.

Mr. Arthur Hopper, of the Deaf and Dumb School near Birmingham, conducted an inquiry with the utmost care. He tells me that out of 122 pupils, he has received information about the parentage of all but 9. The 113 pupils, whose parentage is known, belonged to 109 families; of these 113, there were deaf from accident or disease 37, and of 10 the cause of deafness was unknown. Of these 10 pupils and the 66 congenitally deaf, not one was the offspring of a *consanguineous* marriage. Of the 37 who became deaf from disease, one was the offspring of first cousins. I am not informed whether the cases, where several were deaf in a family, belonged to the congenital cases, but it is almost certain to be so, and in any case I will assume (as the most unfavourable assumption) that it is so. Thus, out of 62 congenitally deaf families, not one was the offspring of even a consanguineous marriage. If we were to assume the 10 other cases to be cases of congenital deafness, it would be, not one in 72 congenitally deaf families was the offspring of a consanguineous marriage.

Mr. Patterson, of the Manchester School for Deaf Mutes, kindly informs me that his 130 pupils belonged to 123 families. Concerning 8 of these families no information could be obtained; in 67 such families the deaf-mutism resulted from disease; in 63 it was congenital; and only one family was the offspring of first cousins.

Mr. Neill, of the Northern Counties Institution, at Newcastle-on-Tyne, says, "350 have been admitted into this institution, and I "do not think more than 6 of the parents were cousins. In one "family whose parents were cousins there were 4 deaf mutes."

I have thus accurate information with respect to 366 families (*i.e.*, 241 + 62 + 63), and out of these 8 were offspring of first cousins; that is to say, nearly 2·2 per cent. were offspring of first cousins. And, including the 350 cases at Newcastle, the percentage is $\frac{1400}{7116}$, or 1·9 per cent. It is curious to notice that I deduced $2\frac{1}{4}$ per cent. as the proportion of first cousin marriages in urban districts, other than London. Thus as far as these meagre results go, no evil in the direction of deaf-mutism would appear to arise from first cousin marriages. The failure to collect more statistics of this kind does

not arise from any inability to get at the best sources of information; on the contrary, I have on all hands received the kindest assurances of willingness to help me.

Mr. David Buxton, of the Liverpool School, says the mode of investigation is simply impracticable; but he has sent me several pamphlets on the subject, his own excellent paper amongst the number.*

Mr. William Sleight, of the Brighton School, tells me that the children know nothing, and the parents are unwilling to communicate the fact inquired after, and says, "As far as I have been able "to ascertain, about 7 per cent. of born deaf children are the "offspring of parents who were cousins." (Query, first cousins?)

Mr. Patterson also writes to me that he is of opinion that, "though the result of the marriage of near relatives may not be "seen in the deafness of their immediate offspring, yet the result is "a deterioration of the constitution of the offspring, which may "show itself in deafness in a few generations."

Mr. Neill, who has been engaged in the tuition of the deaf and dumb for forty years, thinks the cases of offspring of cousins so afflicted are fewer than is supposed. He also gives me facts showing how strongly heritable congenital deafness is where both parents are deaf mutes; marriages are, moreover, by no means uncommon between pupils of these institutions.

* Since my paper has been in print, Mr. Buxton has sent me additional statistics, and from these I make some extracts.

Mr. Buxton himself collected the following cases twenty years ago.

Twenty-six families, offspring of first-cousin marriages, gave 54 deaf-mutes, 1 deaf of one ear, 1 semi-mute, and 1 idiotic. 17 of these families contained 85 children, of whom 42 were afflicted and the remaining 43 sound.

In another family each of the parents had had families by previous marriages, but neither of these previous families were affected by deaf-mutism.

Mr. Buxton has also sent me a number of extracts from the reports of American Institutions for the Deaf and Dumb; they refer in general to consanguineous marriages, but the following refer to first-cousin marriages.

In Illinois, out of 893 children, 42 (or 4·7 per cent.) were offspring of first cousins.

Out of 36 children who entered the Pennsylvanian Institution in 1872, one was the offspring of first cousins.

Mr. Buxton also quotes a return made to the French Academy. A *résumé* of M. Boudin's paper is given in the "Comptes Rendus," Tom. liv, 1862. The statement is as follows:—

Two per cent. of all French marriages are consanguineous. Deaf-mute offspring of consanguineous marriages equal, at Lyons, 25 per cent. of the congenitally afflicted, at Paris 28 per cent., and at Bordeaux 30 per cent. "Taking the ordi-"nary risk of deaf-mute offspring as 1, there are 18 such births from unions of "first cousins, 37 from those of uncles and nieces, and 70 from those of nephews "and aunts. Healthy parents, if related in blood, may have deaf-mute children, "while deaf and dumb parents not so related rarely have any."

The paper is given *in extenso* in the "Annales d'Hygiène Publique," tom. xviii pp. 5—82.

To sum up the results of the whole investigation: It seems probable that in England, among the aristocracy and gentry, about 4 per cent. of all marriages are between first cousins; in the country and smaller towns between 2 and 3 per cent.; and in London perhaps as few as 1½ per cent. Probably 3 per cent. is a superior limit for the whole population. Turning to lunatic and idiot asylums, probably between 3 and 4 per cent. of the patients are offspring of first cousins. Taking into account the uncertainty of my methods of finding the proportion of such marriages in the general population, the percentage of such offspring in asylums is not greater than that in the general population, to such an extent as to enable one to say positively, that the marriage of first cousins has any effect in the production of insanity or idiocy, although it might still be shown, by more accurate methods of research, that it is so. With respect to deaf-mutes, the proportion of offspring of first cousin marriages is precisely the same as the proportion of such marriages for the large towns and the country, and therefore there is no evidence whatever of any ill results accruing to the offspring in consequence of the cousinship of their parents.

III.—*Literature on the Subject.*

For the sake of any persons who may desire to make investigations in this subject at any future time, I will append a short sketch of what I know of the literature on the subject. I cannot, however, pretend to have studied previous writings at all deeply.

To the best of my belief, the most thorough investigation ever made is contained in some papers* "On Blood Relationship in "Marriage," by Dr. Arthur Mitchell, a Deputy Commissioner in Lunacy for Scotland.

In 1860, Dr. Mitchell collected the histories of 45 consanguineous marriages, and amongst these found 8 cases of no evil results, 8 cases of sterility, and 29 cases of evil resulting to the offspring. He feels sure, however, that these cases do not present the rule, but that they were really selected from their striking nature, although the observers had doubtless no intention of making any such selection; an equal number of marriages, where no kinship existed between the parties, might easily be collected, presenting a yet sadder picture. He observes that some families seem to have a tendency to cousin marriages, and I have noticed the same thing in my investigation. He points out that the most satisfactory mode of investigation appears to be—

1st. Take a large number of instances of any defect, and ascertain how many are the offspring of kinsmen; then compare the result

* "Edinburgh Medical Journal," March, April, and June, 1865.

with the proportion of cousin marriages to other marriages in the same community. This is, in fact, the method I have tried to pursue.

2nd. Take a locality and collect the family history of every marriage there, and compare results. Such an inquiry, if sufficiently wide to be accurate, is almost beyond the power of a private individual.

In applying the former of these methods, Dr. Mitchell states that certain Scotch counties have an aggregate population of 716,210; and that he investigated the cases of 711 idiots from these counties. Of 84 of these the parentage was unknown, and yet of the whole 711, 98, or 13·6 (query 13·8) per cent. were *shown* to be offspring of blood relations. Marriages of blood relations are notoriously not 13·6 per cent. of all marriages, but Dr. Mitchell thinks that it may be regarded as certain that such a ratio is about ten times the reality, or that the actual percentage of consanguineous marriages is about 1·3 per cent. I here venture to differ with him and I should not be greatly surprised if the marriages of *first* cousins alone in Scotland were as many as 4 per cent. Dr. Mitchell has elsewhere shown that illegitimacy tends to produce defective children; deducting, therefore, the illegitimate children (of whom, of course, the parentage was unknown) from the above 711 idiots, the percentage of the offspring of consanguineous marriages rises to 18·9 per cent.

The 98 cases of blood relations, whose offspring were idiots, were first cousins in 42 cases; second cousins in 35 cases; third cousins in 21 cases. It is probable, he says, that more second and third cousins marry than first cousins, so that these statistics show that the nearer the alliance the greater the danger.

Out of 177 insane persons from the counties of Ross and Wigtown, he found 23 per cent. were offspring of first, second, or third cousins; or, including those about whose parentage no information could be obtained, out of 260 patients 16 per cent. were such offspring.

The influence of these marriages, he says, in producing insanity is clear. It appears also that its influence is more felt in producing imbecility and idiocy, than in insanity acquired late in life. It does not, of course, follow that blood relationship of parents is the *cause* of mental weakness in the children.

A valuable collection of references will be found in this paper, and I have myself consulted some of the originals, but as I am unable to improve on Dr. Mitchell's abstract, I will only give the outline of what he says, and refer readers for more accurate details to the orignal sources.

In 1846, Dr. Howe showed that in Massachusetts, the parentage

of 359 idiots, out of 574 cases, was ascertained, and that there were 17 cases certainly, and probably 3 more, of offspring of consanguineous marriages; that is, about 5 per cent.

Again the reports of the Commissioners on idiocy to the General Assembly of Connecticut showed that in 1856, out of 310 cases, consanguinity of parents was the probable cause of idiocy in 20 cases, or nearly 7 per cent. of the whole number. But Dr. Mitchell finds that the question as to consanguinity of parents was only answered in 160 cases, so that we ought to count the 20 cases as $12\frac{1}{2}$ per cent. Of these 20 cases, 12 were the offspring of first cousins.

With respect to deaf-mutism, Mr. Buxton, of the Liverpool Institution for the Deaf and Dumb, in a paper in the "Medico-"chirurgical Journal" (January, 1859), says that he found one deaf mute in ten to be the offspring of first cousins.

Dr. Peet, of the New York Institution, gives the same proportion, and adds, that in that part of the United States hardly one family in fifty is the offspring of a cousin marriage.

Dr. Mitchell himself instituted an inquiry into institutions for deaf mutes, and found that in English institutions, out of pupils representing 323 families, 15 families were the offspring of consanguineous marriages; and that in Scotland the corresponding numbers were 181 and 9. Making a deduction of 25 per cent. for cases of acquired mutism, Dr. Mitchell finds that 1 case in 17 of congenital mutism is the offspring of a consanguineous marriage. The numbers, however, collected by me are larger than these, and show a very different result, unless indeed the marriage of more distant cousins is more fruitful of this evil than those of first cousins—a very unlikely result.

Dr. Peet says in his "Thirty-fifth Annual Report," in analysing Sir C. Wilde's "Status of Disease in Ireland,"* that it appears that in Ireland about 1 in 16 of deaf mutes were offspring of first, second, or third cousins.

From the Irish statistics, Dr. Mitchell deduces the result that deaf-mutism, as it appears among the children of cousins, seems to be more congenital than in marriages between persons not akin.

Dr. Mitchell carried out his second plan by collecting family histories, as complete as possible, of the whole populations in the islands of St. Kilda, Scalpay, the parish of Berneray in Lewis, and some small fishing villages on the south-east coast of Scotland. In all these places he had been led to suppose that cousin marriages were frequent. It should be mentioned that in every case the frequency was found less than was supposed, although it some-

* "Blue Book," containing Report of the Census (for Ireland), 1864.

times rose as high as one marriage in four. These districts were chosen because the populations are much isolated. Very full details of the status of disease are given; but Dr. Mitchell was disappointed in the degree of accuracy to which he was able to attain. He sums up the general result of his second method as follows:—

"The facts which I have detailed appear to show a great unsteadi-
"ness in the character of this influence (consanguineous marriages).
"Sometimes we seem to find little or no proof that it is an evil
"influence. At other times this bloodship in the parentage appears
"to be the origin of much injury to the offspring. More frequently
"still the facts admit of various interpretations, and are not very
"clear or satisfactory in their teaching. It is of importance, how-
"ever, to know that these differences or seeming differences may
"occur, and to learn that it is necessary to widen the field of
"observation, and carefully to inquire into all those circumstances
"by which it is quite clear the results may be, and often are,
"exaggerated, modified, or concealed. . . . . If taken *as a*
"*whole* and fairly interpreted, it appears to me that they lead to
"the same conclusion as that drawn from the first line of inquiry,
"viz., that consanguinity in parentage tends to injure the offspring."

Dr. Mitchell came to the conclusion that, under favourable conditions of life, the apparent ill effects were frequently almost nil, whilst if the children were ill-fed, badly housed and clothed, the evil might become very marked. This is in striking accordance with some unpublished experiments of my father, Mr. Charles Darwin, on the in-and-in breeding of plants; for he has found that in-bred plants, when allowed enough space and good soil, frequently show little or no deterioration, whilst when placed in competition with another plant, they frequently perish or are much stunted.

The general conclusions, drawn from the whole investigation, may be shortly stated as follows:—

1st. Consanguinity of parents is injurious to the offspring.

2nd. Where the children seem to escape, the injury may show itself in the grandchildren.

3rd. In many isolated cases, and even groups of cases, no injurious result can be detected.

4th. These unions influence idiocy and imbecility more than the forms of insanity acquired later in life.

5th. The frequency of these unions in Scotland (although not so great as supposed) somewhat increases the amount of idiocy there.

All who are interested in the subject should certainly refer to the originals of these papers.

It will be observed that my investigation, so far as it is worth anything, tends to invalidate Dr. Mitchell's results; but perhaps

the apparent invalidation is due to the fact, that a large majority of Englishmen live under what are on the whole very favourable circumstances.

The next paper to which I will refer is by Signore Paolo Mantegazza, Professor at Pavia, and is entitled "Studj sui "Matrimonj Consanguinei."* The Professor begins with an interesting historical sketch of the legislation against consanguineous marriages, which has obtained in the various ages of history. He says it would be useless to repeat all that has been written on the subject, and refers to the works of Chipault,† Reich,‡ his own work,§ and to Dr. Mitchell's paper above referred to. He gives a list of fifty-seven authors who have opposed these marriages, and of fifteen who have defended them, but says that we ought not to lay stress on the great inequality of these numbers He also gives a long list of experiments made on the in-breeding of animals.

He says that in 1863 an inquiry was made by the Government of France as to the proportion of consanguineous marriages, but that he failed to obtain (at least at that time) an inquiry of a like nature in Italy. The professor therefore gives the cases of 512 consanguineous marriages (inclusive of Mitchell's) from all countries collected by him; and of these he found 409 cases of bad results, and 103 with no ill results. It cannot, however, he observes, be asserted from this that it is 4 to 1 that the result of such a marriage will be ill. They are selected cases, for they naturally caught the attention of observers. Many of the evils recorded in the list are doubtless quite independent of consanguinity.

The only fact which can be safely deduced from these numbers is the effect in producing sterility; and he finds that 46, or 8·9 per cent., of the marriages were sterile.

The results, which he deduces from his consideration of the cases, may be shortly summed up as follows:—

1. Consanguineous marriages are, on the whole, more unfavourable to the offspring than others.

2. The proportion of 4 : 1, deduced from his table, is not a correct view of the case.

3. The injury arises from the multiplication of pathological germs of the same nature.

4. This influence alone would weaken the offspring of relations. This is confirmed by the frequency of sterility, and of miscarriages, and the appearance of diseases new to the family.

* Milan, Gaetano Brigola, 1868, price 75 centimes.
† "Étude sur les mariages consanguins," Paris, 1863.
‡ "Geschichte, Natur," &c., Cassel, 1864.
§ "Elementi d'Igiene," Milan, 1868, p. 437.

5. The best proved results of these unions are, failure of conception, abortive conception and miscarriage, monstrosities, disposition to nervous complaints, arrested mental development, scrofulous and tubercular diathesis, lowered vitality, high rate of mortality, especially amongst infants, dysmenorrhœa, small generative power, pigmental retinitis.

6. The nearer the kinship, the greater the danger.

7. He gives the obvious conclusions as to choice in marriage.

8. It is tolerably probable that the danger is greater in cases of uterine kinship: first, because more evil or good is heritable from the mother; and secondly, because "we are entirely sons of our mother, but are not equally so, nor always, the sons of our father." This last sentence can hardly refer to conjugal infidelity; and yet, if it does not so, how does the second cause differ from the first?

Amongst the references given in this paper, is one to "a very "recent work" by Dr. Loubrieu on "Deaf-mutism," and others to Dr. Down's "Marriages of Consanguinity," &c. ("London Hospital "Report III," pp. 224 and 236), to Saint-Lager's "Études sur les "causes du Crétinisme," &c., Paris, 1867, and to "Studj sui sordo-"muti e rendiconto degli Istituti ecc," Milan, 1864.

I have to thank Mr. Buxton, of the Liverpool School for Deaf-mutes, for sending me a pamphlet by Mr. J. Scott Hutton.* At the Halifax (Canada) School Mr. J. Scott Hutton found that out of 54 families (with 100 children), with respect to which information could be obtained, 15 families (with 37 children) were offspring of first cousins. He sums up by saying:—"Thus out of 110 deaf-"mute children of whom we have definite information, 56 are the "offspring of cousins . . . . an expressive example of the melan-"choly consequences flowing from cousin-marriages." It should be added, that there are two apparent discrepancies between the figures given in this part of the paper. The statistics of other countries are not so striking. "In England, the deaf and dumb in "marriages within the limits of consanguinity are in the proportion "of 6 per cent., in France 25 to 30 per cent., in Kentucky 20 per "cent., in Illinois 12 per cent., and in Ontario 28 per cent. No authority is given for these figures, nor is the phrase "limit of consanguinity" defined.

Mr. Buxton's admirable paper on "Deaf-mutism" has been already referred to in the sketch of Dr. Mitchell's paper.

Sir W. Wilde in an Appendix to his "Aural Surgery," gives a very complete account of the history, and tuition of deaf-mutes, and the causes which produce the disease. He says consanguinity may be looked on as paramount. "Many conjectures have been offered

* "American Annals of the Deaf and Dumb," January, 1869. Washington, D. C., pp. 15—17.

" on the subject, but the question has been set at rest by the results " of the Irish census." This appendix embodies the results of the inquiry carried out in the Irish census, and is referred to in Dr. Mitchell's papers.

So general a consent as to the ill effects of cousin-marriages must certainly have far greater weight than my purely negative results. But it strikes me that in no case has the investigation been free from flaws, for in no case has it been really determined, what is the proportion of consanguineous marriages in the whole population. The exceedingly various estimates which different people have given me of the frequency of cousin-marriages (from 10 per cent. down to 1 in 1,000, if my memory serves me right), leads me to believe that general impressions on this point are almost valueless. Every observer is biased by the frequency or rarity of such marriages amongst his immediate surroundings.

My paper is far from giving anything like a satisfactory solution of the question as to the effects of consanguineous marriages, but it does, I think, show that the assertion that this question has already been set at rest, cannot be substantiated.

The subject still demands attention, and I hope that my endeavour may lead more competent investigators to take it up from some other side.

*Marriages between Cousins in Relation to Infertility and a High Death-rate amongst the Offspring.**

Professor Mantegazza states in his paper† that he may conclude with tolerable safety, from his collection of 512 cases of consanguineous marriage, that consanguinity tends to cause sterility; for he found that between 8 and 9 per cent. of the recorded marriages were sterile. It is not clear, however, how he is entitled to draw this conclusion, unless he knows what is the proportion of sterile marriages in the general population, and he admits that he has no statistics on this point. M. Boudin, who wrote at an earlier date, is of the same opinion, and considers, further, that even where sterility does not afflict the consanguineous marriage itself, it is apt to affect the offspring‡. Dr. Balley is also of opinion that the ill effects of such marriages are liable to appear in the second generation.§

Since reading my paper on " Cousin Marriages in England," on the 16th of March, before this Society, a method has occurred to me of settling these points pretty satisfactorily. This method is by a comparison between the fertility of the marriages of first cousins

* Sent in by Mr. Darwin subsequent to the reading of his paper.
† "Studj Sui Matrimonj Consanguinei." Milan, 1868.
‡ "Annales d'Hygiène Publique," tom. xviii, pp. 5–82.
§ "Comptes Rendus," tom. lvi, p. 135.

and of the marriages of their offspring, as recorded in the pedigrees in "Burke's Landed Gentry" and the "Peerage," with the fertility of marriages between persons not akin.

I had already got a large number of marriages marked as being between first cousins, and I accordingly proceeded to count the number of children arising therefrom. The marriages made within the twenty years immediately preceding the publication of those works were excluded; so that only complete families were counted. It soon became evident that the lists of the daughters were very incomplete, and that the daughters were perhaps sometimes omitted altogether; the sons dying in infancy are also frequently omitted (especially in the "Landed Gentry"); and when such occurred I excluded them. I think that the lists of the sons surviving infancy are however, pretty complete, and any incompleteness will clearly affect the record of marriages between persons not akin as much as it does the first cousin marriages. The comparison to be made must, therefore, be only between the numbers of sons. I shall use the words *sterile* or *infertile* to mean the absence of children surviving infancy. The number of daughters recorded will be given, so as to show the extent of incompleteness.

In this manner 116 families, offspring of first cousins, were collected. In all but 12 of them the marriages were between children of brothers. In 11 of the 116 it is merely stated that there was issue of the marriage, and in 8 others there is no information as to whether there was issue or not. I found in a subsequent inquiry, by cross references to other pedigrees, that where there was no information there was nevertheless often a family; so that the absence of information is no indication of sterility, and indeed is perhaps some slight indication of fertility, because the family is omitted in order to economise space, and d. s. p. (*decessit sine prole*) is frequently added where there *was* no issue. In this case, however, cross references were of no avail, because the family would be recorded in the pedigree under consideration or not at all. The absence of information is here then a slightly greater indication of sterility than in my later inquiry, where it is no indication at all.

The cases where issue was recorded may clearly be disregarded in making the comparison, since they might be matched by similar cases amongst the non-consanguineous marriages.

Subtracting, then, the 11 recorded cases of issue and the 8 cases of no information, we are left with 97 families; these gave 202 sons and 153 daughters. It is probable that about 212 daughters should have been recorded. Now 202 sons to 97 marriages is at the rate of 2·07 sons to each marriage; or, supposing the 8 cases of doubt to have been all sterile, we get 105 marriages as giving 202 sons, that is at the rate of 1·92 sons to each marriage.

Thus the average number of sons who survive infancy, arising from a marriage of first cousins amongst the gentry of England is between 1·92 and 2·07.

The next step was to collect the non-consanguineous marriages. In order to secure myself from bias, I opened my book by chance and counted all the marriages in the pedigree which fell under my eye. I then did the same in another place, and so on. In this way 217 families arising from persons not akin were collected, and found to give 416 sons and 340 daughters. Here, as before, the daughters are deficient, and about 437 daughters ought probably to have been given. Now 416 sons to 217 marriages is at the rate of 1·91 sons to each marriage. Thus the average number of sons who survive infancy, arising from non-consanguineous marriages, is 1·91.

The balance of fertility is therefore slightly on the side of the cousins, but the small difference is probably due to chance.

In order to feel greater confidence in this result, a second method of analysis was carried out. If cousin marriages tend to cause sterility, they probably tend to cause partial sterility. Now amongst the 97 cousin marriages, 14 were sterile (in the sense defined), and amongst the 217 non-consanguineous marriages 33 were sterile. Thus we have 83 fertile cousin marriages and 184 fertile non-consanguineous marriages; the former gave 202 sons, the latter 416 sons. It will be observed that this course entitles me to disregard the 8 cases of "no information" before referred to, for if they were sterile they are to be subtracted *ex hypothesi*, and if there was issue, they could be matched by similar cases amongst the non-consanguineous. Thus fertile first cousin marriages produce sons at the rate of 2·43 sons to each marriage, and fertile non-consanguineous marriages produce sons at the rate of 2·26 sons to each marriage.

Therefore the analysis leads to a similar slight balance in favour of the fertility of the first cousins, just as did the former one.

I offer the following suggestion as a possible explanation of the greater fertility of the cousins, although mere chance is the more probable cause of the difference. Marriages between first cousins will be more apt to take place where there is a large group of persons who bear that relationship to one another. In such families fertility will be hereditary; hence it is possible that the comparison is to some extent being effected between abnormally fertile families, and those in which fertility is only normal.

The next point to investigate is as to whether the offspring of first cousin marriages are themselves affected by sterility.

To test this, recourse was again had to the "Peerage" and "Landed Gentry," and 136 marriages of the offspring of first cousins were collected. Concerning 29 of these no information could be obtained, and, for the reasons before assigned, these may be set aside.

Of the 107 remaining marriages, it is recorded that 14 had issue. Subtracting these, we are left with 93 marriages, and these gave 180 sons and 157 daughters. It should be mentioned that some few of the marriages were recent, so that the families would be not quite complete in these cases. Now 93 marriages giving 180 sons is at the rate of 1·93 sons to each marriage.

Again, 16 of these marriages were sterile, so that 77 fertile marriages gave 180 sons, that is at the rate of 2·34 sons to each marriage. If these two numbers, viz., 1·93 and 2·34, be compared with the corresponding numbers, viz., 1·91 and 2·26, for the non-consanguineous marriages, it is clear that there is again no evidence of want of fertility in the offspring of first cousin marriages.

The results with respect to fertility may be summed up in the following table:—

Parentage.	Average Number of Sons to each Marriage.	Percentage of Sterile Marriages.*	Average Number of Sons to each Fertile Marriage.
Not consanguineous	1·91	15·9	2·26
Parents first cousins	between 2·07 and 1·92	between 14·7 and 20·9	2·43
One parent the offspring of a marriage between first cousins.....	1·93	17·2	2·34

* Sterility means absence of children surviving infancy.

The comparison may be best effected by means of the numbers in the last column. The figures in the second column are not of much value, since in some cases it was difficult to decide whether the entry should be made as being a case of "no information" or of sterility.

The comparison of the figures in the first and last columns show, without much room for doubt, that the alleged infertility of consanguineous marriages, whether direct or indirect, cannot be substantiated.

I now pass on to the question of the youthful death-rate.

It has been stated by M. Boudin and others that the offspring of consanguineous marriages suffer from an excessively high rate of infant mortality. I have tried to put this to the proof as follows:—

I recurred to the families in the "Peerage" which were offspring of first cousins, and marked every case where it is recorded that a son or daughter died in infancy or youth. Where the age of the child was mentioned, ten years was taken as the standard of youth. "Burke's Landed Gentry" was of no avail in this inquiry, because I found that children dying in infancy were never, or very rarely, mentioned therein.

From the "Peerage" I could only obtain 37 fertile first cousin

marriages; in two of these there were no children surviving youth. The 37 gave 86 sons, who survived infancy, 15 children (boys and girls) who died in infancy or youth, and 4 more as to whom the period of death was doubtful. Besides this, it is stated of one family, that "all died young except one daughter." Now in the previous part of this paper it is shown that the average number of sons to a fertile first cousin marriage is nearly $2\frac{1}{2}$; so that it may not be unreasonable to credit this family with 4 infants who died.

On this supposition we should have 37 fertile marriages of first cousins giving 86 sons, who survived, and between 23 and 19 boys and girls, who died early. Reducing these numbers to percentages, I find that—

One hundred fertile marriages of first cousins would give from 51 to 62 children who die young, and that for every 100 sons, offspring of first cousins, who survive youth, there are from 22 to 27 boys and girls (their brothers and sisters) who die early.

These numbers cannot be used as giving the actual infant death rate, on account of the imperfections in the pedigrees in the "Peerage," but they may be used in a comparison with other statistics deduced from the same source.

Now 89 fertile non-consanguineous marriages (collected by chance from the "Peerage") gave 197 sons, and 44 sons and daughters who died young. Reducing these numbers to percentages as before, I find—

That 100 fertile non-consanguineous marriages would give 49 children who die young, and that for every 100 sons, offspring of fertile non-consanguineous marriages, who survive infancy, there are 22 boys and girls (then brothers or sisters) who die early.

The numbers to be compared are therefore 51 or 62 with 49, and 22 or 27 with 22.

These are merely two different ways of consulting the facts, and it appears that both methods give some evidence of a slightly lowered vitality amongst the offspring of first cousins.

Thirty-seven cases form, however, far too small a total on which to base satisfactory statistics. The numbers thus collected are far scantier than those collected by others, but as far as I am aware this is the only occasion in which the method of collection has been one in which the unconscious bias of the collector could not operate. In all these inquiries I was ignorant as to whither the figures were tending until I came to add up the totals.

This last inquiry is, I fear, worth but little, but so far as it goes it tends to invalidate the alleged excessively high death-rate amongst the offspring of cousins, whilst there remains a shade of evidence that the death-rate is higher than amongst the families of non-consanguineous marriages.

Discussion *on* Mr. Darwin's Paper.*

The President said that the paper was obviously of that order which had to be very carefully studied and considered before it could be properly discussed; but still there might be some gentlemen present who would like to offer some observations on the subject. He then called upon Dr. Farr who, he said, had acted as a referee in this matter, to express his opinions on the subject.

Dr. Farr said he was sure that those present would agree with him that the Society were indebted to Mr. Darwin for taking up a subject the importance of which could not be questioned. He had paid a good deal of attention to the reading of the paper and to the important results that had ensued from the inquiries made by the author. It must, he thought, have required great courage on the part of Mr. Darwin to undertake a subject so difficult. At first sight, the question of the marriage of cousins appeared very easy, but when the subject came to be dealt with it, would be found very difficult. He had had occasion to go over "Blackstone" for a particular purpose. He (Blackstone) started of course with the assumption that each person had two parents, and he worked up by degrees until he arrived at the twentieth degree, proving that each person has a million ancestors, and that up to the fortieth degree they had a million million ancestors. It was pretty evident, he thought, that Blackstone had fallen into an error there, and that he had overlooked the fact that cousins intermarried, which would narrow the stream considerably. He (Dr. Farr) was quite puzzled in following Mr. Darwin in his analysis of cousins. That was one source of difficulty. If the number of persons who married their first cousins in England could be ascertained, with the ages at which they married, the number of children they had, and the state of health of these children, more important results would, he thought, be arrived at. He trusted that Mr. Darwin's paper would lead some of the Fellows of the Society to see the importance of the subject and be the means of pressing an investigation of the real effect of cousin marriages on the proper authorities. It was somewhat anomalous that a man should be prevented from marrying his deceased wife's sister, and yet allowed to marry his cousin. In Italy aunts were allowed to marry their nephews, and among the Jews marriages of that kind were allowed. It would be interesting to know the effects of intermarrying, but what he would specially like to ascertain was the effect of marriages at different ages. There had been some inquiries on that subject by Dr. Duncan and others. It was found, for instance, that women, if they produced children at all, produced them every twenty months. He merely mentioned this to show what an important inquiry Mr. Darwin had opened by his paper.

Mr. Cornelius Walford said he had the honour of assisting Mr. Darwin in collecting statistics in this matter, but he found a great deal of difficulty in getting returns from persons who, he

* The discussion refers exclusively to Mr. Darwin's original paper.

thought, would be able to give them. In reference to this subject he had read, some twenty-five years ago, "The Constitution of Man;" by George Combe—a man who was very much preached against, but who gave some very vital information on the subject, and was strongly opposed to cousin marriages. He thought Mr. Darwin would have alluded to the Society of Friends, as that Society, some years ago, had taken this subject into consideration; and one of their reasons for allowing marriages outside the Society was that they found that marriages with cousins within the Society were very detrimental. The author could have found also a great deal of information by reference to Jewish families. He thought, however, the inquiry instituted by Mr. Darwin had a good deal more in it than at first sight appeared, and he hoped it would be the beginning of a series of inquiries that would throw some light on the subject. Some years ago, Mr. Fox read a paper before the Society, which would give Mr. Darwin and other members of the Society some useful information on the matter.

Mr. H. T. Humphreys (of the "Morning Post") said that as a member of the Society of Friends, he wished to correct the remark made by the last speaker with reference to that body. Although they had to some extent opened their gates, they had always had a rule against first-cousin marriages.

Mr. Galton thought that Mr. Darwin was somewhat too modest as to the results of his investigation, as he had undoubtedly swept away, to some extent, an exaggerated opinion which was current as to the evil resulting from first-cousin marriages. He thought, however, there might be some error on the part of the author in dealing with the "Pall Mall," results due to the particular class of advertisers in that paper, which, if rectified, would bring the result more in harmony with the others. In reference to the remarks of previous speakers as to intermarrying among the Quakers and the Jews, he instanced the sect of the Samaritans, who were visited by Mr. George Grove in his travels a few years ago. He found only eighty men, all of whom had a magnificent bodily presence, and were apparently free from any local disease. He (Mr. Grove) saw no reason why that race should not continue for a length of time, unless the falling off were due to their practice of fasting on particular days, during which the infants which were being suckled might be deprived of sufficient nourishment. There was, however, as a matter of fact, a good deal of mortality among them. There were many other similar cases to be found. In the southern valleys of the Alps, where there were small communities and the natives intermarried, there was a remarkably fine class of men.

Mr. Kennelly said he was among the Samaritans in 1865 and 1866. He found there were then 162 men and women, all more or less in very good health; and he believed that, to a great extent, they intermarried. There were, however, signs of falling off, and it was only a matter of time that they would cease to be a people upon the earth. So that there must be among that race some agency at work other than intermarrying.