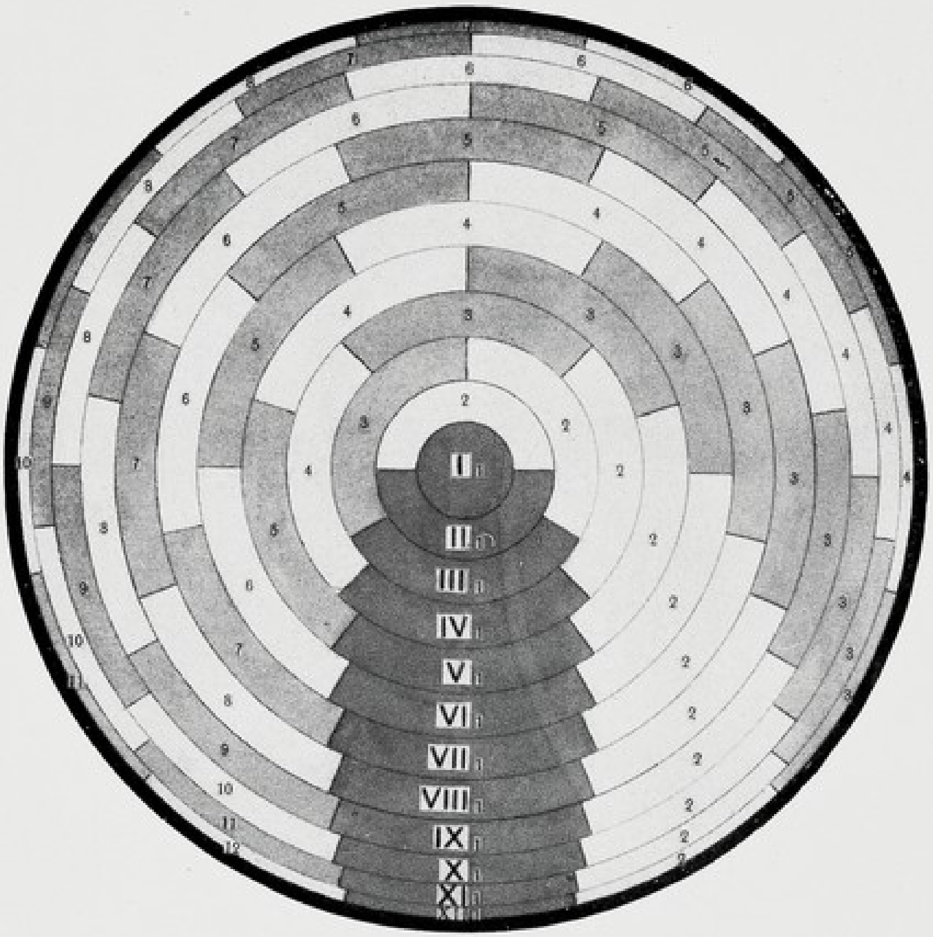

In this 'target' an annular zone by its area represents the relative number of members of families of one, two, three, &c., individuals marked by the roman figures I, II, III, &c. The sections of each annular zone give the relative number of 1st, 2nd, 3rd, &c., born members in each family, marked by the Arabic figures, 1, 2, 3, &c. The dark border is the total frequency of families of thirteen and upwards. The whole is based on the returns for Scottish wives over forty-five years of age. If $f_{M,n}$ represents the annular sector of nth borns in families of M ($n = < M$), and P the whole population; then, supposing Death to shoot *at random* at this target, the chance of his hitting an nth born of a family of $M = f_{M,n}/P$, and the chance of his hitting a nth born at all:

$$= \sum_{M=n}^{M=v} \left(f_{M,n}/P\right) = f_n/P, = p_n \text{ say},$$

where v is the maximum size of family occurring. And the chance that he will not hit an nth born $= 1 - p_n = q_n$. The distribution of nth borns in samples of Q deaths will not, however, follow the binomial $(p_n + q_n)^Q$, unless (1) the death-rate be very small as compared to the population at risk, or (2) we suppose each death to be replaced in some way by a birth. The result otherwise is the hypergeometrical series. But if Death shoots more arrows into an f_n area than elsewhere, that is more among the nth born than any other birth order, it does not follow that all the arrows will be equally distributed over this area. If he aimed with bias to the bull's-eye, he would be certain to hit more first-borns than if he aimed at the target without bias. Thus anything which prejudices a small family prejudices first-borns. Or, Death may have fits of good and bad marksmanship, aiming at the small families and the large families, i.e. the moderate sized families may be least susceptible to certain diseases. In such a case there may be a redundancy of small families without appreciable loss of average size of family.

Frontispiece

On the Handicapping of the First-born

BY

KARL PEARSON, F.R.S.

BEING A LECTURE DELIVERED AT THE GALTON LABORATORY, UNIVERSITY COLLEGE, LONDON
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ON THE HANDICAPPING OF THE FIRST-BORN

(1) Some years ago, in considering Dr. Rivers's data from the Crossley Sanatorium, and following up a suggestion given by him, I considered the incidence of tuberculosis in the family in regard to order of birth. The point is a very important one, because, if the earlier-born members of a family are to any extent inferior physically or mentally, the limitation in the size of families which is spreading throughout the civilized world must in itself tend to increase general degeneracy, for we preserve only the earlier-born members of the community. Since the publication of my *First Study of the Statistics of Pulmonary Tuberculosis*, the collection of material bearing on this problem has gone on continuously in the Galton Laboratory, and we have now much unpublished data touching the same problem in cases of albinism, mental defect, idiocy, insanity, and other pathological or diseased states. The view originally put forth on this point has been supported by confirmatory material of Dr. Rivers himself, of Dr. Hunter, and others, but has been challenged by a variety of medical authorities, Dr. Weinberg in Germany, Dr. Saleeby and Mr. M. Greenwood in this country, and by not a few statisticians, including Mr. G. U. Yule and Mr. Macaulay, Actuary to the Sun Life Office of Canada. As is customary with any fairly novel doctrine propounded by the Galton Laboratory, we hear a good deal in the writings of these various authorities about fallacious theories, incredible blunders, and such-like. That the problem is by no means a simple one was obvious *ab initio*, but it appears to me that several of our critics have themselves disregarded the fundamental principles which must rule any inquiry into the matter.

The problem is further closely associated with the distribution of sizes of family in the different classes of the community and the method by which a census can be taken of such families.

Now when I asserted that the elder-born were more liable to be handicapped by tuberculosis, by insanity, or criminality, I was particularly careful not to give any basis for drawing conclusions beyond the exact wording of my statement. This may be summed up thus: If we consider existing individuals to be classed as first, second, third, &c. born, then the percentage of first-born would be greater in the diseased than in the general population. I was peculiarly cautious not to assert that my diseased population was drawn equally from *all* first-borns, a principle which some of our critics have assumed; I merely asserted that more arrows would fall into the darkest part of the target in the frontispiece, but I by no means thought that they would fall of necessity equally into all parts of this darkest area. I illustrate this on the target in the frontispiece and will ask the reader at this stage to study the letterpress that accompanies it.

I wrote five years ago as follows:

> 'I have simply stated the statistical result, it does not affect my conclusion to be told that it is because the earlier children are born from too young parents; it may be so, or it may not. There is a counter-balancing evil arising from being born of too old parents. Further, the primiparous woman may experience greater stress from the physical changes which precede childbirth—and these may react on the unborn child—than occurs at a second or later birth. And while this stress may diminish with some increase of age, it probably increases rapidly again after the prime of life. At present my point is the statistical fact, we shall learn in good time its cause.'[1]

To explain my point I would now definitely suggest that the growth of the first child is hampered by conditions which exist to a far less extent for the following births; but these conditions will be much harder for the first-born child when its mother is 40 than when she is 25. But the resulting family in the former case is likely to be far smaller than in the latter case. In other words the handicapping of the first-born in small families may be increased by the addition of many small families in which the first-born is also late-born.

There is further another factor weighting small families, namely, they represent very frequently an exhausted virility in the parents. Certain types of parental degeneracy seem incapable of producing more than one or two children at most, and the children of such parents are themselves feeble. But, if any small families are thus selected, we shall increase the number of early-borns in the diseased population, for such small families have no late-borns. I am thus by no means prepared to accept the view that it is sufficient to consider each size of family independently, and consider whether the incidence is the same or unlike for each member. We are dealing with the relative proportions of first- and other-borns in the general and diseased

[1] *The Problem of Practical Eugenics*, p. 18, foot-note.

populations, and a part of this may be due to the greater prevalence of families of one and two among the diseased. We are shooting, so to speak, at the entire population of first-borns, and a bias with regard to selection of weaker families may come in, in much the same way as families up to six or seven may be the sign of healthy parents, and so the offspring will be less liable to disease. This idea cannot be excluded.[1] But in itself it indicates how inadequate is the proposal to treat the problem only within families of constant size.

It is often suggested that asking a series of individuals collected in a lecture hall, a college, an asylum, or a hospital must give a result for *size of families* wholly different from what we should obtain by taking the size of families from the census of the whole population. But a census of the whole population by no means leads to a correct appreciation of the unselected distribution of the size of families. Suppose we take our census by asking the number of brothers and sisters of all individuals, then small families are more likely to have died out than large families, and our record will show a defect of small families. Suppose we ask parents the size of their families, then, if we fix no period for the existence of the marriage, clearly young parents with small families are more likely to be alive than old parents with large families, and the latter will be insufficiently recorded; and again, parents who die early and so miss the census are more likely to have small families.[2] Census returns are not necessarily therefore a safe measure of the existing sizes of families. Limited by a period for the existence of the marriage, they may give us valuable *relative* results for different classes, but hardly absolute distributions. Again, if we take a special class—say, the occupants of an asylum at a given time, or the students of a college—we are told that we shall weight the large families, for such families being more numerous are more likely to have members in asylum or college than a small family. This is the basis of the attack of Yule, Weinberg, and Greenwood, on the principle that the first-born is handicapped. Dealing many years ago with the heredity of fertility or size of family, I took data from family records, where the mother and daughter had been married fifteen years, and correlated the size of their families. But in such a case as this, every daughter in the family—if we work

[1] It does not of necessity contradict the principle that degenerate *stocks* are as a rule very fertile; see frontispiece.

[2] A better plan is to record size of family of each individual of one sex at the death registration.

from the older records—is given the chance of marrying, and apart from small families producing heiresses, or endowed brides with a higher chance of marriage, we may roughly suppose marriage proportional to the number marriageable, and the larger families accordingly to be weighted. Messrs. Yule and Greenwood ask[1] why I 'overlooked' my own principle of the weighting of large families, when I came to the problem of the tuberculous. The answer is that, applied to that case, it led to what I considered absurd results, and that caused me to believe that the method suggested by Weinberg, Yule, and Greenwood to replace our 'fallacious' theories—namely my own theory of dealing with *completed* family records—did not apply to such cases as those of the insane or tuberculous in asylums or sanatoria. The method proposed by these authors is, in its simplest form, very obvious. They say that a family of p offspring is p times as likely to be recorded as a family of one offspring; therefore if in our recorded population we find

n_1 families of 1 child,
n_2 " " 2 children,
n_3 " " 3 children, &c.,

we ought to suppose in the population from which they are drawn:

$q \times n_1$ families of 1 child,
$q \times \frac{1}{2} n_2$ " " 2 children,
$q \times \frac{1}{3} n_3$ " " 3 children,
$q \times \frac{1}{4} n_4$ " " 4 children,

and so on, where q is a constant.

(2) Now let us test this theory in a special case to which it is really applicable, before considering its difficulties in the practical cases to which it has to be applied. From the Backhouse and Whitney Quaker records 1,366 families were taken out completed at the time of issue of the record. Our mark was the occurrence of the initial letter H in the first Christian name. The names occurring in the Backhouse family were Hannah, Harold, Harriet, Harry, Harvey, Henrietta, Henry, Helen, Herbert, Hodgson, Horace, Horatio, and Hugh. So that the selection of a name like Henry left a considerable further field of choice. When a family was 'marked' several times over, however, this family, as in our tuberculosis or insanity material, was only included *once*.

Table I gives the results. Column I gives the actual distribution for the total material of first- to n^{th}-born reduced to permilles.

[1] *Journal of the Royal Statistical Society*, vol. lxxvii, p. 182.

Column II gives the actual distribution of the marked individuals. It agrees fairly closely with the original population. Column III gives the distribution of first- to n^{th}-borns in the sibships of the marked members. It would clearly be a failure, if used to reconstruct the population of Column I. Column V gives the original reconstruction of Pearson, which we shall term the Yule-Greenwood

TABLE I. NUMBER OF EACH ORDER OF BIRTH IN 1,366 COMPLETED QUAKER FAMILIES.

Marked Members: those with initial letter H to their first Christian name. Numbers reduced to permilles.

Order of Birth.	*Total Material.* I.	*Marked Members.* II.	*All Sibships with Marked Members.* III.	*Sibships with first-born Marked Members.* IV.	*Yule-Greenwood Reconstruction.* V.
1	178	172	140	193	188
2	162	159	138	170	167
3	146	168	132	147	149
4	122	136	123	126	128
5	102	106	109	96	102
6	83	69	94	78	80
7	67	60	81	63	65
8	50	58	62	50	46
9	36	24	46	38	31
10	24	20	32	21	20
11	13	11	19	12	11
12	8	11	14	2	7
13	9	6	7	2	4
14	2	–	2	2	2
15		–		–	–
16	1	–	1	–	–
17		–		–	–
Mean Size of Family	5·62	–	7·14	5·18	5·32

reconstruction in this lecture. Column IV gives the order of birth of the sibships which have marked *first-born* members, a method of reconstructing the distribution of first- to n^{th}-born in the original population which has been suggested to me from the actuarial side. Now it will be clear that IV and V represent I, if not very satisfactorily, far better than III, but neither as well as II. This is precisely what we should expect, if the marking were truly at random, and equally likely to attach itself to all members of all families. But the conditions for IV or V accurately representing I are precisely those for II being identical with I. It does not seem to have occurred to Messrs. Yule

TABLE II. H-MARKED QUAKER FAMILIES.

	Size of Completed Family.																	
Order of Birth of H-Marked Members.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	*Totals.*
Non-H-Marked Totals.	105	111	143	102	86	76	57	50	37	34	16	4	6	–	1	1	1	830
1	11	11	10	14	9	7	6	6	8	4	5	–	–	1	–	–	–	92
2		9	11	11	12	7	10	8	5	5	1	4	1	1	–	–	–	85
3			13	13	17	9	11	10	5	5	3	2	1	–	–	1	–	90
4				16	12	9	13	8	6	5	1	1	2	–	–	–	–	73
5					10	8	9	10	6	6	3	3	2	–	–	–	–	57
6						8	10	5	4	4	3	2	–	1	–	–	–	37
7							13	4	6	4	–	3	2	–	–	–	–	32
8								11	9	6	1	2	1	1	–	–	–	31
9									5	4	–	–	4	–	–	–	–	13
10										4	2	3	1	1	–	–	–	11
11											4	2	–	–	–	–	–	6
12												1	3	1	–	1	–	6
13												–	2	1	–	–	–	3
Totals	116	131	177	156	146	124	129	112	91	81	39	27	25	7	1	3	1	1366
H Totals	11	20	34	54	60	48	72	62	54	47	23	23	19	7	–	2	–	536
Frequency of Families for V	11	10	11·3	13·5	12	8	10·3	7·8	6	4·7	2·1	1·9	1·5	·5	–	·1	–	100·7

The last row is obtained by dividing successive entries in the row of 'H Totals' by 1, 2, 3, ... respectively.

and Greenwood to test what changes were made in V, supposing the marking was *not* really at random, and that I and II differed substantially. Would V be any criterion in such a case or lead positively to misinterpretation?

Now, personally, I should never have supposed III to represent I in this illustration. It is not only that when selection is made at random, I know my original method, i.e. that of V, to be approximately correct, but I know that 140 permille of first-borns in completed families corresponds to the class of General Labourers and Hawkers or to Agricultural Labourers, and not to the Professional Classes from which my data are drawn. And here I venture to remark that my critics appear to have overlooked the essence of the method adopted by me. V was discarded because, as we shall see, it leads to grossly exaggerated percentages of early-born when there is any selective action. The method, then, was to compare II with general data for a like class of the community, and it was not till this was found to correspond closely to III in the data under discussion, that III was placed alongside II, *and the comparative data for the like class.* My critics have dropped all reference to this fact. III and not V was found to correspond to the comparative data, and the reasons for this will appear shortly.

I term the above method, as shown in Table I, the 'Long Table' method of comparison. It allows for any weighting of elder-borns whether directly or by selection of the small families. The original data of Table I are given in Table II.

I will illustrate the process of finding column V, so that the reader can follow the argument. I first reproduce the last row to two decimals as (*b*) below.

(*a*)	(*b*)	(*c*)	(*d*)
1	11·00	100·67	188
2	10·00	89·67	167
3	11·33	79·67	149
4	13·50	68·34	128
5	12·00	54·84	102
6	8·00	42·84	80
7	10·29	34·84	65
8	7·75	24·55	46
9	6·00	16·80	31
10	4·70	10·80	20
11	2·09	6·10	11
12	1·92	4·01	7
13	1·46	2·09	4
14	·50	·63	} 2
15	·00	·13	
16	·13	·13	
	100·67	536·11	1,000

This (*b*) gives the hypothetical number of families of the size recorded in (*a*). Add up continuously *from the bottom* and we obtain the number of children of each order of birth in (*c*). Multiply by 1·8652888—i.e. 1000/536·11—and we obtain (*d*) the number of children of each order of birth permille, adjusted in our case to units. The reciprocal of 188, the first entry in (*d*), multiplied by 1000, gives the average size of family.

We are now prepared for the 'Short Table' method, which treats each size of family independently, and asks whether the marked members are distributed equally throughout the family. It is convenient to put this in the following form:

TABLE III. DISTRIBUTION OF H-MARKED MEMBERS IN FAMILIES OF DIFFERENT SIZES.

	Size of Family.														*Total observed and expected in Row.*
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	
Expected No.	11·0	10·0	11·3	13·5	12·0	8·0	10·3	7·8	6·0	4·7	2·1	1·9	1·5	0·5	
First-born . . .	11	11	10	14	9	7	6	6	8	4	5	–	–	1	92 / 101
Second-born . .		9	11	11	12	7	10	8	5	5	1	4	1	1	85 / 90
Average of Intermediates					17	9·0	11·0	8·1	5·4	5·0	1·6	2·0	1·4	·4	61 / 55
Average of two Last-born				14·5	11·0	8·0	11·5	7·5	7·0	4·0	3·0	1·5	2·5	·5	71 / 68

It will be seen at once that there is no systematic deviation from the *Expected Number*—i.e. the row of H-Totals of Table II divided by the number in family—in the rows of first-borns, second-borns, average of last two born, or in the average of intermediates. This 'Short Table' method has the advantage of illustrating whether there is special weighting of late-borns as well as, or apart from, first-borns. It misses, however, the point already referred to, that weighting of first-borns may be due to bias against small families.

(3) We are now armed with two methods of approaching the problem—that of the 'Long Table' and that of the 'Short Table'. But to deduce results from the 'Long Table', we must have some standard of reference of what the distribution of first- to n^{th}-borns in any community is. I was very early convinced that this is

much more a matter of social class than of race, and that if we could get reliable data of contemporaneous period collected in precisely the same manner, we should find results for different countries singularly alike. I illustrate this in Table IV. The Danish data are from Rubin and Westergaard's well-known work,[1] the New South Wales data from Powys's paper in *Biometrika*,[2] and the Scottish data from the recently published census of Scotland for 1911.

TABLE IV. FREQUENCY OF EACH BIRTH ORDER IN PERMILLES IN DIFFERENT SOCIAL CLASSES IN DIFFERENT COUNTRIES.

Order of Birth.	*Copenhagen. Marriage lasting at least 15 years.*		*New South Wales. Marriage completed by Death of Father.*			*Scotland. Wife 22–26 at marriage, and marriage lasting at least 15 years.*		
	Professional.	*Industrial.*	*Professional.*	*Industrial.*	*Agricultural.*	*Professional.*	*Industrial.*	*Agricultural.*
1	193	179	186	166	145	218	165	150
2	177	162	162	151	134	200	159	146
3	155	145	140	133	123	170	148	138
4	126	125	118	117	112	132	133	128
5	102	106	98	102	101	98	114	115
6	77	87	79	86	89	70	94	97
7	57	68	64	70	78	48	72	78
8	42	48	51	56	64	30	50	59
9	28	32	37	42	51	17	32	39
10	18	21	25	30	39	9	18	24
11	11	11	14	20	26	4	9	14
12	7	7	10	12	17	2	4	7
13	3	4	7	7	9	1	1	3
14	2	2	4	3	5	}	}	1
15	1	1	2	2	3	}	}	}
16	}	1	1	1	2	} 1	} 1	} 1
17	} 1	} 1	1	1	1	}	}	}
18 and over	}	}	1	1	1	}	}	}
Mean Size of Family	5·18	5·59	5·36	6·04	6·91	4·59	6·05	6·64

In addition to Table IV, I give in Table V the frequencies permille of first-, second-, and third-borns in various social classes. This will serve as a general guide to prevent the reader from accepting permilles of first-borns over 200 as representing something possible or normal in the working classes.

[1] *Statistik der Ehen*, Jena, 1890. [2] Vol. iv, p. 233.

TABLE V. INDEX OF CLASS, FERTILITY, AND PERMILLES OF FIRST-, SECOND-, AND THIRD-BORNS.

Social Class.	*Conditions as to Marriage.*	*Mean size of Family. Fertile Marriages.*	*Permille.*		
			First-born.	*Second born.*	*Third-born.*
Scotland, Professional	Wife at marriage 22–26, at least 15 years	4·59	218	200	170
Scotland, Clerks	Wife at marriage 22–26, at least 15 years	4·72	212	198	169
Copenhagen, Professional	Marriage lasting at least 15 years	5·18	193	177	155
Scotland, Shopkeepers	Wife at marriage 22–26, at least 15 years	5·25	190	180	161
New South Wales, Professional	Marriage completed by death of Father	5·36	186	162	140
Copenhagen, Industrial	Marriage lasting at least 15 years	5·59	179	162	145
British Peerage	,, ,, ,, ,,	5·80	172	163	150
Scotland, Skilled Workmen	Wife at marriage 22–26, at least 15 years	6·00	167	160	149
New South Wales, Industrial	Marriage completed by death of Father	6·04	166	151	133
Scotland, Industrial	Wife at marriage 22–26, at least 15 years	6·05	165	159	148
Selected Marriages (Pearson)[1]	Begun before 35, and lasting 15 years	6·40	152	147	138
Scotland, Builders, Masons, &c.	Wife at marriage 22–26, at least 15 years	6·48	154	149	142
Scotland, Dock Labourers	Wife at marriage 22–26, at least 15 years	6·62	151	144	136
Scotland, Agricultural Labourers	Wife at marriage 22–26, at least 15 years	6·64	151	146	138
Scotland, General Labourers and Hawkers	Wife at marriage 22–26, at least 15 years	6·76	148	143	136
New South Wales, Agricultural	Marriage completed by death of Father	6·91	145	134	123
Scotland, Miners	Wife at marriage 22–26, at least 15 years	7·30	137	134	130
All Scotland	Wives over 45	6·20	161	151	138
All Scotland	All marriages, complete or not	4·63	216	184	150

Except for the Professional Classes, where limitation of family is largely modifying the Scottish returns, we see how like Scotland and New South Wales are, and there is little doubt that England falls into line with these returns.

[1] Including all entries in family histories which fulfilled conditions of second column.

These results illustrate how, for marriages having or approaching completed fertility, the average size and the number of elder-borns are essentially matters of social class. Race and locality are only of secondary importance.

It will be seen that, by aid of Tables IV and V, we can reach a very fair appreciation of what percentages of first-, second-, third-born, &c. are to be expected in any given social class from which our diseased population has been drawn; we shall be able at least to convince ourselves that the distributions provided by Greenwood and Yule are in the bulk of cases wholly incapable of expressing the original distribution. The permilles of first-borns are far in excess of any observed normal population. Now how does this come about? The answer is this, that while the method gives the right result when there is no bias against the elder-born, it exaggerates the percentage of elder-born very markedly when there is bias. My critics have used a criterion which could only work if there were no bias, and which fails the moment there is bias! It was this resulting exaggeration in the cases of bias which led me to discard my original method in these cases, for it applies only to unbiased selection, and adopt other methods for dealing with the problem of the elder-born.

(4) To illustrate the futility of the Weinberg-Greenwood-Yule process as applied to the handicapping of the elder-born, I take the following data:

DISTRIBUTION OF BIRTH-ORDER FOR SCOTTISH WIVES OVER 45 YEARS.

	Order of Birth.															
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Permilles of each born	161	151	138	123	106	89	72	56	41	28	17	9	5	2	1	1
Size of Families . .	10	13	15	17	17	17	16	15	13	11	8	4	3	1	0	1

Now suppose some particular disease to attack 1 % of first-borns, $\frac{1}{2}$ % of second-borns, $\frac{1}{3}$ % of third-borns, and so on.

Then the following table gives the relative frequencies of affected offspring from families of each size for each order of birth.

[1] *Phil. Trans.*, vol. 192 A, p. 257 *et seq.*

TABLE VI. ILLUSTRATING BIAS OF ELDER-BORN.

Size of Family.	Order of Birth. 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Totals.
1	100																100
2	130	65															195
3	150	75	50														275
4	170	85	57	42													354
5	170	85	57	43	34												389
6	170	85	57	42	34	28											416
7	160	80	53	40	32	27	23										415
8	150	75	50	38	30	25	21	19									408
9	130	65	43	32	26	22	19	16	14								367
10	110	55	37	28	22	18	16	14	12	11							323
11	80	40	27	20	16	13	11	10	9	8	7						241
12	40	20	13	10	8	7	6	5	4	4	4	3					124
13	30	15	10	7	6	5	4	4	3	3	3	3	2				95
14	10	5	3	3	2	2	1	1	1	1	1	1	1	1			33
15	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–		–
16	10	5	3	2	2	2	1	1	1	1	1	1	1	1	1	1	34
Totals	1610	755	460	307	212	149	102	70	44	28	16	8	4	2	1	1	3769

Now let us deduce the original population from this by (*a*) the sibships of the marked individuals (Pearson), (*b*) the reduction of those sibships by the factors 1, $\frac{1}{2}$, $\frac{1}{3}$, &c. (Yule-Greenwood), and (*c*) the sibships of the first-borns.

The results are given in the following 'Long Table' (see Table VII, p. 15).

Clearly, as by theory it must, (*c*) gives the absolutely correct result, but it is usually not of service, because our data are enough to get adequate results from first-borns of the marked members only: (*a*) is depressed 17 below the right result, (*b*) raised 39 above it. It is clear that the Yule-Greenwood hypothesis tends to exaggerate more than (*a*) depresses, and cannot be used.

Now let us go a stage further and suppose that a bias towards small families being marked exists. Let families of one and two have a relative weight of 8 in selection for marking, families of three and four a relative weight of 7, families of five and six a relative weight of 6, and so on, . . . up to families of fifteen and sixteen with a relative weight of 1.

TABLE VII. EFFECT OF BIAS TOWARDS ELDER-BORN ON CRITERIA.

Order of Birth.	*Original Population.*	(*a*) *Sibships of Marked Members.*	(*b*) *Yule-Greenwood Reconstruction.*	(*c*) *Sibships of Marked First born.*
1	161	144	200	161
2	151	140	173	151
3	138	132	147	138
4	123	122	123	123
5	106	108	99	106
6	89	94	79	89
7	72	78	60	72
8	56	62	45	56
9	41	46	31	41
10	28	32	20	28
11	17	20	12	17
12	9	11	6	9
13	5	6	3	5
14	2	3	1	2
15	1	2	1	1
16	1			1
Mean Size of Family	6·21	6·94	5·00	6·21

We thus obtain the following distribution of relative frequency :

TABLE VIII. BIAS OF EARLY-BORNS COMBINED WITH BIAS OF SMALL FAMILIES.

Size of Family.	*Order of Birth.* 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	*Totals.*
1	800																800
2	1040	520															1560
3	1050	525	350														1925
4	1190	595	399	294													2478
5	1020	510	342	258	204												2334
6	1020	510	342	252	204	168											2496
7	800	400	265	200	160	135	115										2075
8	750	375	250	190	150	125	105	95									2040
9	520	260	172	128	104	88	76	64	56								1468
10	440	220	148	112	88	72	64	56	48	44							1292
11	240	120	81	60	48	39	33	30	27	24	21						723
12	120	60	39	30	24	21	18	15	12	12	12	9					372
13	60	30	20	14	12	10	8	8	6	6	6	6	4				190
14	20	10	6	6	4	4	2	2	2	2	2	2	2	2			66
15	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–		0
16	10	5	3	2	2	2	1	1	1	1	1	1	1	1	1	1	34
Totals	9080	4140	2417	1546	1000	664	422	271	152	89	42	18	7	3	1	1	19853

This table leads us to the following 'Long Table':

TABLE IX. EFFECT OF BIAS TOWARDS EARLY-BORNS COMBINED WITH BIAS TOWARDS SMALL FAMILIES.

Order of Birth.	*Original Population.*	(*a*) *Sibships of Marked Members.*	(*b*) *Yule-Greenwood Reconstruction.*	(*c*) *Sibships of Marked First-born.*
1	161	166	236	189
2	151	159	196	173
3	138	146	156	151
4	123	130	124	129
5	106	110	93	104
6	89	90	69	83
7	72	69	48	62
8	56	52	34	45
9	41	35	21	29
10	28	22	12	19
11	17	12	6	9
12	9	5	3	4
13	5	2	1	2
14	2	1	} 1 (14–16)	1
15	1	} 1 (15–16)		
16	1			
Mean Size of Family	6·21	6·02	4·24	5·29

It will be seen from this table that the errors introduced by the two kinds of bias which lead to redundancy of early-born tend to cancel in (*a*), so that the final result may be very close to the original population. But they are additive in (*b*); the tendency is always one way, and the elder-borns are exaggerated—a result which, I am surprised, was not noted by Messrs. Yule and Greenwood when they reached such high values. Further, the second type of bias invalidates the use of (*c*) as a criterion, it being also sensibly too large in its frequencies of early-borns in this case. The high values obtained by Yule and Greenwood represent, in my opinion, not that the populations from which our selections were made were very infertile, but that there was actually a great redundancy of elder-born.

(5) In order further to test the theory of weighting propounded by Weinberg, Yule, and Greenwood on something else than artificial marking, I took my own Family Schedules. These were obtained by asking individuals to provide answers to a number of questions on a

schedule. Clearly, the subject asked is a 'marked' individual, and with the reasoning of our critics ought to come from a large family more frequently than a small one. Now Table X gives the distributions of first-, second-, third-born, &c., from my Family Schedules. The 'subject' was always an adult, and the families are all completed.

TABLE X. PEARSON'S FAMILY RECORDS. 'MARKED SUBJECTS.'

Order of Birth.	*Subject's Sibship.*	*Subject's Mother's Sibship.*	*Subject's Father's Sibship.*	*Yule-Greenwood Reconstruction.*
1	170	188	185	236
2	163	177	173	196
3	152	158	157	162
4	136	136	136	132
5	111	111	112	95
6	88	84	87	68
7	64	55	60	44
8	48	37	37	31
9	30	24	23	18
10	18	11	13	9
11	10	8	7	5
12	5	5	5	2
13	3	3	3	1
14 and over	2	3	2	1
Mean Size of Family	5·88	5·32	5·41	4·24

Now, here, the father's and mother's sibships have had no selection for size; they are in excellent agreement with each other and represent distributions such as we might anticipate from the middle classes. It will be seen that the Yule-Greenwood hypothesis has enormously exaggerated the early-borns, and were it correct we should have to assert that there had been a great selection of late-born in our Record subjects. On the contrary, I think that the subjects' distribution actually represents the general population distribution for the class with which we are dealing. My reason is this: The subject knows fairly well the number of his brothers and sisters, but he knows less accurately the number of his parents' brothers and sisters. He or she is more apt to suppose that survivors —although our questionnaire asked pointedly for dead as well as living relatives—form the whole family, and uncles and aunts, who died in extreme infancy, are often overlooked. I have frequently noted corrections made for number of uncles and aunts, when the family

Bible has been consulted. Hence I should anticipate that the mother's and father's sibships would show more early-born than the subject's sibships, owing to a greater omission of infantile deaths. This is precisely what we find, and it is by no means necessary to explain the fall to 170 first-borns as due to selection of subjects from larger families. No professional class even, not even *incomplete* marriages of the whole population, run up to the Yule-Greenwood 236 per-mille of first-borns!

(6) Now the reader will ask: But surely it is correct to assert that you are more likely to ask an individual from a large family? I reply: No, not at all. Let me illustrate my point.

Suppose we asked every freshman who came to college to fill in a schedule, would there be any weighting at all of large families? Each member of a large family is not equally likely to come to college at the same time. They come, say, at 18, and if a family has one representative of this age, it will rarely have a second. Indeed, it is a relatively rare thing, as only those who have sought know, to find two brothers in college together. The births of the members of a large family possibly spread over fifteen to twenty years, and they are not equally likely to become 'subjects' at a given epoch, or during a given short period of any process of recording from college or asylum or sanatorium. The assumption that all members of a large family are equally likely to come into a record is a perfectly arbitrary one, unless the record be based on a completed family history. It is rather curious that our medical critics should have overlooked this point. All members of a family which stretches over 15 to 26 years of age-difference are not equally likely to develop tuberculosis or even insanity. Both these troubles have a modal age, and the chances of any individual rise and fall according to his age. But even here there is danger of confusion; the modal age and the tapering off on either side of it hold for the *population at large*, but in the case of the *individual family* there is an individual modal age and a far more concentrated tapering away of the chances of attack. There are families in which insanity comes on soon after puberty, and others where it appears at change of life; and similar rules hold very largely for tuberculosis and diabetes. As a matter of fact, this individualization of age of onset in the family needs further study, but I append here the chances of any individual being found at different ages in a sanatorium for the tuberculous, in an asylum for imbeciles, in an asylum for the insane, or in a prison.

TABLE XI. (*a*) PROBABILITY AT DIFFERENT AGES OF A MAN BEING ADMITTED TO A SANATORIUM FOR THE TUBERCULOUS.

Age .	11–13	14–16	17–19	20–22	23–25	26–28	29–31	32–34	35–37	38–40
Relative Chance	·07	·37	·49	1·00	·82	·75	·61	·58	·68	·57

Age .	41–43	44–46	47–49	50–52	53–55	56–58	59–61	62–64	Over 64
Relative Chance	·18	·24	·21	·16	·06	·06	·00	·07	·00

(*b*) PROBABILITY AT DIFFERENT AGES OF A MAN BEING FOUND IN AN ASYLUM.

Age .	Under 20	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70–74	75 and over
Relative Chance	·32	·77	1·00	·86	·62	·59	·54	·57	·30	·47	·52	·34	·05

(*c*) PROBABILITY AT DIFFERENT AGES OF A PERSON BEING FOUND IN AN IMBECILE ASYLUM.

Age .	1–3	4–6	7–8	9–10	11–12	13–14	15–16	17–18	19–20	21–33	Over 33
Relative Chance	·00	·11	·80	·89	1·00	·91	·81	·42	·15	·01	·00

(*d*) PROBABILITY AT DIFFERENT AGES OF A MAN BEING FOUND IN PRISON.

Age .	10–14	15–19	20–24	25–34	35–44	45–54	55–64	65–74	75–84	85 and over
Relative Chance	·001	·49	1·00	·80	·58	·35	·26	·14	·04	·04

The data are deduced from the age distributions of the inmates of Crossley Sanatorium, Murray's Royal Asylum, Perth, the Royal Albert Asylum, Lancaster, and from Dr. Goring's *The English Convict*, 1913.

All probabilities are expressed in terms of the modal chance, whatever that may be, taken as unit.

As I have indicated, the chance is far more stringently concentrated in the case of a single family. But such results are well known to the medical profession—the concentration of especial liability to attack from certain diseases round special ages,—and it is singular that they should have been entirely overlooked when the hypothesis was put forward in regard to data collected from the occupants for a very limited period of an institution, that all members of a large family are equally likely to be put on the record. Such cases approximate to, although of course they are not exactly the same as, the case of students coming to the university at very nearly a standard age. And this indeed suggests the obvious way to deal with the problem. We ought to consider separately the subjects of each age, and thus avoid the least chance of weighting the larger families. Unfortunately, until there is some really effective system of pooling medical data, or of collecting medical statistics throughout the country,—as might indeed be done under the Insurance Act —the individual investigator cannot limit himself to subjects of one age in such an inquiry. On these two grounds, namely: (i) that the existence of bias itself exaggerates the result deduced by the Yule-Greenwood hypothesis, and (ii) that the fundamental assumption of that hypothesis is only true when we follow the whole life-history of each individual, since each individual at any given time is not equally likely to be 'marked',—I cannot accept the suggestion of Messrs. Yule and Greenwood that my hypothesis of 1898[1] should be applied beyond the limits to which I then legitimately applied it. I neither propose now, nor did I in my early work propose, to use *without control* the sibships of the marked or affected members, but that method gives more and more a correct result, and the Yule-Greenwood method more and more an incorrect one, as fewer and fewer individuals in the family have *equal* chance of being affected. Also, the errors in it tend to balance, when the bias against small families as well as the direct bias against the elder-born in all families are taken into account. In the present investigation I shall use the 'Short Table' method which brings out only one aspect of the question, and also the 'Long Table' method, giving my hypothesis of 1898 under the heading Yule-Greenwood reconstruction, the sibships of the affected as Pearson's reconstruction, and justifying the use of the latter by comparative material from similar social classes.

(7) Before I proceed to the discussion of old and new material on

[1] 'Genetic (Reproduction) Selection', *Phil. Trans.*, vol. 192, App. 257–67.

these lines, I should like to bring further evidence of a different kind to show that the elder-born come to life really handicapped.

I turn first to the dangers which meet the first-born even at birth. Ansell has given us the still-births per 1,000 born alive, in order of birth for 48,843 births.[1] His results are reproduced in Table XII.

TABLE XII. STILL-BIRTHS, PROFESSIONAL AND UPPER CLASSES (ANSELL).

	Order of Birth.				
	First.	*Second.*	*Third.*	*Fourth to Sixth.*	*Seventh and over.*
Still-births per 1,000 born alive	40·0	20·0	15·5	17·4	20·9

It will be seen that still-births for the first birth are *double* those of later births. And this disadvantage follows the first-born into the first year of life. Table XIII gives the infantile mortality of the professional and upper classes for 48,843 births, due to Ansell.

TABLE XIII. INFANTILE MORTALITY, PROFESSIONAL AND UPPER CLASSES (ANSELL).

	First.	*Second.*	*Third.*	*Fourth to Sixth.*	*Seventh and over.*
Deaths in first year per 1,000 born alive	82·2	70·0	69·0	78·3	97·4

Thus we see that not till the seventh child is the death-rate in the first year of life so heavy for any successive child as for the first-born.

I can confirm Ansell's results from our own data for several English towns—for the artisan, not professional classes. Thus we have for Bradford:

TABLE XIV. INFANTILE DEATH-RATE AND DELICACY RATE, BRADFORD.

Order of Birth	1	2–3	4–5	6–7	8–9	10–11	12 and over
Death-rate in first 12 months	16·2	12·4	13·0	14·3	17·4	17·7	33·3
Delicacy Rate in first year .	3·9	4·2	5·7	6·5	6·0	8·3	9·0
Death and Delicacy Rates .	20·1	16·6	18·7	20·8	23·4	26·0	42·3

[1] *Statistics of Families*, London, 1874, pp. 33 and 79.

The Bradford results are based on nearly 3,000 babies. The lowering of the delicacy rate at the year in the case of the first-born is a good illustration of the influence of Natural Selection. Where the delicacy rate is taken at *first* visit, as in Sheffield, the delicacy is a maximum for first-born. My colleague, Miss Elderton, tells me that the high death-rate for families of twelve and over largely disappears if we exclude the mothers of bad habits, who preponderate among the mothers of large families. Of course the material is sparse for these large families.

Again, for Sheffield:

TABLE XV. INFANTILE MORTALITY. LEGITIMATE BIRTHS. BOYS AND GIRLS. DEATHS PER CENT. OF CHILDREN BORN.

Order of Birth	1	2	3-4	5-6	7-8	9-10	11-12	13 and over
Total Births	636	691	1156	843	518	334	143	101
Death-rate in first year of life	12·9	11·6	11·5	10·6	12·6	16·2	11·9	24·8

Thus it is not till we get to the eighth or ninth birth that the mortality is as great as for the first-born. The matter can be further illustrated by examining the reports of the visitors on the health of the baby at their first visit. I give the delicacy rate, i.e. the percentage of children reported as puny or in bad health.

TABLE XVI. SHEFFIELD. HEALTH AT FIRST VISIT.

Order of Birth	1	2	3-4	5-6	7-8	9-10	11-12	13 and over	*Total Cases.*
Delicacy Rate, Boys	12·7	9·5	10·2	6·5	8·7	9·8	9·2	18·5	2,327
,, ,, Girls	10·1	6·5	5·9	6·8	7·3	8·4	6·1	19·4	2,095

Thus not till the thirteenth child do we find as much delicacy as in the first-born.

This inferiority, of course, to some extent wears off with age, but it would still appear appreciable at 12 and 13. The following table gives the number of children diagnosed as definitely pathological (tuberculous or rheumatic) at those ages by Dr. M. H. Williams:

TABLE XVII. WORCESTERSHIRE SCHOOL CHILDREN DIAGNOSED AS DEFINITELY PATHOLOGICAL—TUBERCULOUS OR RHEUMATIC—BY DR. M. H. WILLIAMS IN SCHOOL INSPECTION.

861 Boys and Girls, aged 12 and 13.

Order of Birth	1	2-3	4-5	6-7	8-10	11 and over
Definitely Pathological—Rheumatic or Phthisical	54·6	47·6	52·7	55·0	57·2	55·6

Here the children from 2 to 5 appear to be less affected than the first- or the later-born, but the results are not so marked as in the previous tables for infants. This is probably largely due to the fact that puerile phthisis is a disease through which the great bulk of children must pass.

We can also bring physical evidence of the defect of the first-born from the weight and length of new-born babies. The weights, lengths, order of birth of 2,000 babies were copied for me a number of years ago from the records of the Lambeth Lying-in Hospital. Excluding twins, and confining our attention to legitimate and normal-time infants, we have:

TABLE XVIII. WEIGHTS OF NEWLY BORN BABIES IN ORDER-OF-BIRTH CATEGORIES.

	Weight in lb.							*Total Cases.*	*Mean Weight.*
Birth Order	1	2	3-4	5-6	7-8	9-10	11 and over		
Boys . .	7·01	7·36	7·41	7·70	7·91	7·59	7·92	856	7·40
Girls . . .	6·76	7·03	7·33	7·36	7·32	7·65	7·88	866	7·15

Here the first-born have less weight for both boys and girls than any children subsequently born. Precisely the same point is brought to light in Table XIX for the lengths:

TABLE XIX. LENGTHS OF NEWLY BORN BABIES IN ORDER-OF-BIRTH CATEGORIES.

Birth Order	1	2	3-4	5-6	7-8	9-10	11 and over	*Total Cases.*	*Mean Length.*
Boys . . .	20·62	20·82	20·80	20·95	20·98	20·99	21·14	856	20·81
Girls . . .	20·27	20·33	20·51	20·43	20·36	20·41	20·73	866	20·38

When deducing these results I was unaware that Mathews Duncan [1] had long previously indicated the like facts. He does not, it is true, separate the sexes, but his results, with the exception of an anomalous fall at the fourth birth, are very similar to mine.

TABLE XX. T. MATHEWS DUNCAN. WEIGHTS AND LENGTHS OF NEWLY BORN BABIES. BOYS AND GIRLS TOGETHER.

	Order of Birth.							*All Births.*
	1	2	3	4	5	6	7 and over	
Weight in lb. . . .	7·20	7·31	7·35	7·19	7·45	7·32	7·31	7·26
Length in inches .	19·20	19·24	19·30	18·96	19·27	18·96	18·99	19·19

Mathews Duncan also gives us the age of the mother, which may be an important factor. Unfortunately, the material is not given in a form which would enable us to correlate physique of child and order of birth for constant age of mother.

Order of Birth . . .	1	2	3	4	5	6	7 and over
Mean Age of Mother .	22·79	25·81	27·70	30·32	30·42	32·05	35·56

It follows from this table that age steadily increases with order of birth, as we might anticipate. If we now classify the mothers according to age, we have:

TABLE XXI.

Age of Mother.	*Weight of Baby.*	*Length of Baby.*
15–19	6·98	19·01
20–24	7·22	19·17
25–29	7·40	19·36
30–34	7·27	19·23
35–39	7·27	18·90
40–44	7·16	18·91
45–49	6·92	18·17

Thus we see that weight and length of baby increase from the youngest mothers up to the age 25–29, and after that fall right away to the oldest mothers. The lessened weight and length of the earlier-

[1] *Fecundity, Fertility, and Sterility*, Edinburgh, 1871, pp. 61–2.

born children are thus possibly due in whole or part to the lesser average age of the mother.[1]

We have, however, distinctly guarded ourselves from any expression of the source of the inferiority of the first-born till the data, slowly accumulating, suffices to determine how much the first-born pays for the juvenility and how much for the inexperience of its parents.

It will be seen from the totality of the above results that physically, in the early months of life, the first- or earlier-born babies are inferior to any babies before at least the seventh or eighth. We have now to ask whether this inferiority persists to later life, and whether it shows itself also in congenital defects.

(8) *Imbecility.*

I take first imbecility. The data here were obtained by schedules issued to the parents or relatives of all imbeciles for the time being or recently in the Royal Albert Asylum, and most courteously sent to us by the medical officers. In this way fairly complete family histories were formed. In such material we might expect some approach to the Yule-Greenwood hypothesis, but those authors have, notwithstanding the highly exaggerated value obtained from it, to admit that the first-born is weighted in the matter of imbecility.[2] They also draw attention to the weighting of the last-borns, as if it were a novel point. Had they simply inquired where and when the

[1] *Weights and Lengths as determined from Age of Mother at Birth.*

	Order of Birth.						
	1	2	3	4	5	6	7 and over
Weight observed	7·20	7·31	7·35	7·19	7·45	7·32	7·31
Weight for Age of Mother .	7·23	7·33	7·39	7·33	7·32	7·28	7·27
Length observed	19·20	19·24	19·30	18·96	19·27	18·96	18·99
Length for Age of Mother .	19·18	19·30	19·35	19·29	19·28	19·24	19·03

I am not, however, satisfied with the validity of this method of investigating the influence of mother's age. A much more complete investigation on the weights of newly born babies has recently been made by H. J. Hansen, who allows for class and age of mother (see 'Undersøgelser over nyfødte Børns Vægt', *Meddelelser om Danmarks Anthropologi*, II Bind, 1 Afdeling, 1913, pp. 1–109). He concludes that the weight of a new-born child depends much more, in general, on the number of previous children—i. e. on the birth order—than on the age of the mother.

[2] *Journal of the Royal Statistical Society*, vol. lxxvii, p. 194.

material was originally dealt with, they would have learnt, not only that this point had already been noted, but that it had been several times referred to in the medical literature of the subject. As a matter of fact, it is principally due to the presence of Mongolian idiots.

In Table XXII I give the experience for order of birth for 108 Mongolians. They are drawn in part from Dr. D. W. Hunter's data for the Royal Albert Asylum, and in part from data due to Dr. J. C. Carson of the Syracuse State Institute. Both these authors have already noted the weighting of the latest-born.[1]

TABLE XXII. WEIGHTING OF THE LAST-BORN IN MONGOLIAN IDIOCY.

	Size of Family.															*Totals.*
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
Expected Number in each place.	3·0	6·5	4·7	3·0	3·2	2·5	1·3	·6	·8	·6	·2	·3	·0	·1	·1	(26·9)
Place of Mongolian in Family. 1	3	7	4	1	1	2	1	–	2	–	–	–	–	–	–	21
2		6	5	2	2	1	–	1	–	–	–	–	–	–	–	17
3			5	2	–	2	–	1	–	–	–	–	–	–	–	10
4				7	4	1	1	–	–	1	–	–	–	–	–	14
5					9	1	1	–	–	–	–	–	–	–	–	11
6						8	1	–	–	–	–	–	–	–	–	9
7							5	–	–	–	–	1	–	–	–	6
8								3	1	–	–	–	–	–	–	4
9									4	1	1	–	–	–	–	6
10										4	–	1	–	–	–	5
11											1	–	–	–	–	1
12												2	–	–	–	2
13													–	–	1	1
14														1	–	1
15															–	0
Totals .	3	13	14	12	16	15	9	5	7	6	2	4	0	1	1	108

If distributed at random, the anticipated total in the sum of single cells taken one from each column should be 26·9. The sum of the cells in the first row is 21, or there is a *defect* of about 6 first-borns. The sum of the contents of the diagonal cells is 58, or in the last-borns there is an excess of 31 idiots! Of intermediates we should anticipate on random distribution 57·4; actually there are only 32.

[1] Dr. D. W. Hunter, Paper before British Medical Association, 1910; Dr. J. C. Carson, *Journal of Psycho-Asthenics*, vol. xii, p. 44, 1907-8.

We conclude, therefore, that in Mongolian idiocy there is a marked bias against the latest-born. This may be due to exhaustion of fertility, and correspond to the not uncommon dwarf egg last laid by a hen, or, in whole or part, to an intentional termination of the family after the birth of a Mongolian. At present I should not lay stress on the latter possibility, as bias of last-born has not yet been demonstrated in the same marked manner in the case of other almost equally repugnant congenital defects. I have excluded the Mongolian idiots from the present investigation, as we have perfectly definite evidence that such idiocy is a late-born defect.

Sir Arthur Mitchell was, I think, among the first to draw attention to the bias of the elder-born in the case of idiocy. But his data were inadequate (85 idiots) and his methods of dealing with them hardly in conformity with modern statistical requirements. He had started with 663 idiots, but as 108 were found to be illegitimate children, the material had to be discarded for the problem of bias against the first-born. The following table gives what, I think, is really useful for our present purpose in his material:[1]

TABLE XXIII. DISTRIBUTION OF IDIOT BIRTHS. SIR ARTHUR MITCHELL.

Order of Birth.	*All Births permille.*	*Idiot Births permille.*
1	228	330
2	177	188
3	155	176
4	121	24
5	94	24
6	74	24
7	52	70
8	39	35
9	26	24
10	13	70
11	9	35
12 and over	12	—

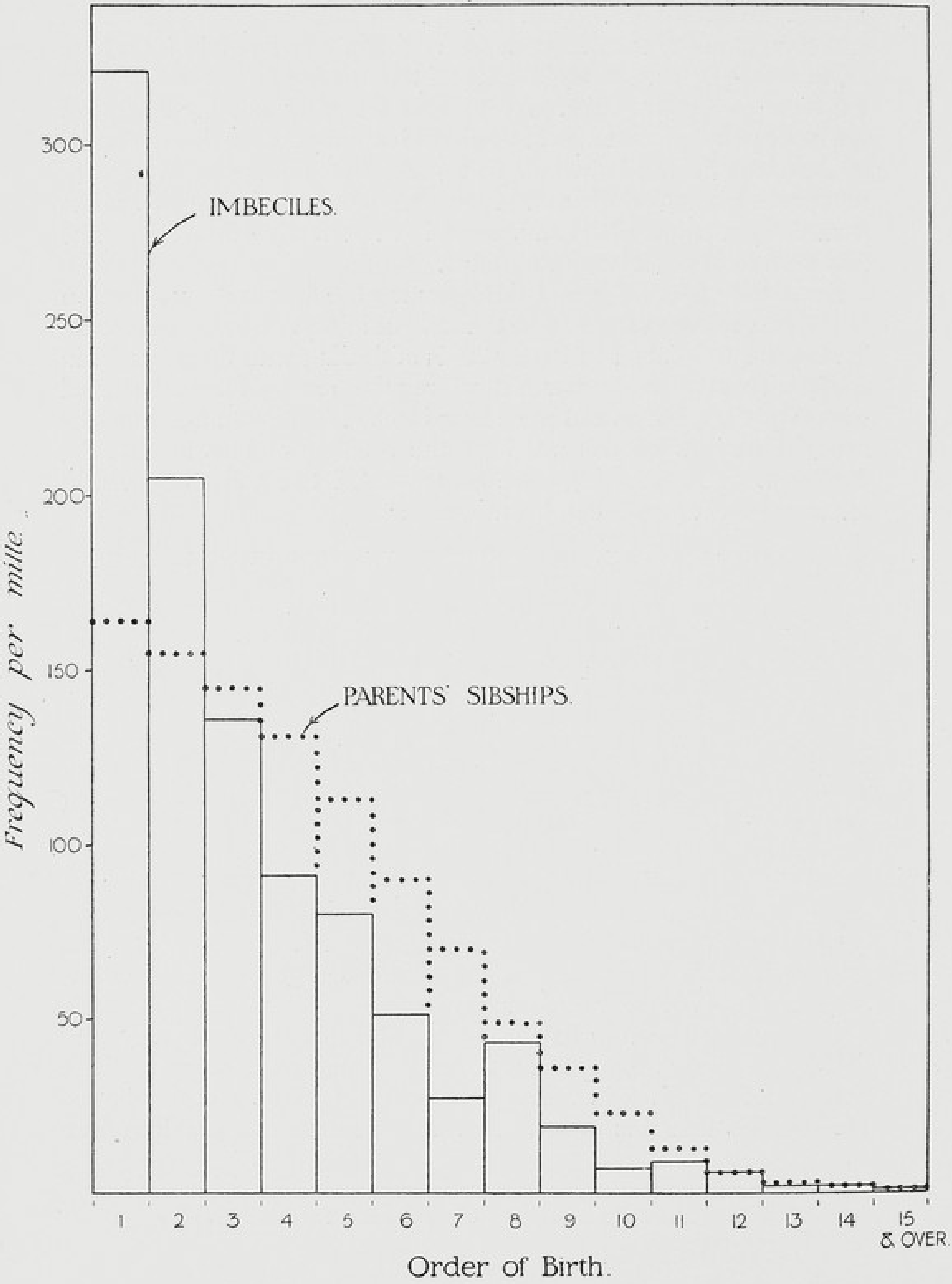
'All births permille' are those in Glasgow and Edinburgh for the year 1855. It will be seen that the data, although very inadequate, show emphasis of early and late births.

G. Langdon Down in 1887[2] reported also that there was a greater liability to idiocy among the children of primiparous women. The only proof he gives is that 240 permille of idiots are first-borns. He assumes that this is in excess of the number of children first-

[1] *Edinburgh Medical Journal*, vol. xi, pp. 639-45, 1866; earlier inquiries, vol. viii, p. 1142, 1863.

[2] *On Some of the Mental Affections of Childhood and Youth*, London, 1887, pp. 45, 56.

Handicapping of First-born. Imbecility.



born in the community, but gives no numbers. We have seen that in Edinburgh and Glasgow it is given by Mitchell as 228, so that there is not much margin, unless we suppose the idiots to belong to *completed* families. Langdon Down writes:

'Curiously enough, while 24% of idiots generally are primiparae [? offspring of primiparae], no less than 40% of resuscitated idiots come under this category. I have long held and taught that suspension of animation must be regarded as one of the pre-efficients of idiocy.'

Elsewhere he points out that the transit of the child is more likely to be delayed to a perilous extent by the smallness of the internal passages and the rigidity of the perinaeum usually associated with a primiparous birth.

We now turn to our own investigations and examine first the 'Long Table'. The average age of the idiots dealt with (Mongolians excluded) is 15·96, 85% being at least 10 years of age. The families therefore are in the main, but not entirely, completed. The class consists of the small shopkeeper, clerk, artisan, right downwards. Of such a population we might anticipate on completed families 160 to 170 permille of first-borns (see Table V), and if we allow for certain of the families being incomplete, an average family of, say, 5·5, or 180 to 185 permille of first-borns. I have tested the distribution in the class from which these imbeciles are drawn by taking out the sibships of the parents, and exhibit the whole in the following table:

TABLE XXIV. IMBECILES (WITHOUT MONGOLIANS) FROM THE ROYAL ALBERT ASYLUM DATA. PERMILLES TO EACH ORDER OF BIRTH.

Order of Birth.	*Imbeciles.*	*Parental Sibships.*	*Sibships of Imbeciles (Pearson).*	*Yule-Greenwood Hypothesis.*
1	321	164	181	286
2	205	155	163	190
3	136	145	151	155
4	91	131	131	117
5	80	113	108	86
6	51	90	86	62
7	27	69	64	42
8	43	49	44	26
9	19	36	30	17
10	7	23	19	9
11	9	13	11	5
12	6	6	6	3
13	2	3	3	1
14	2	2	2	} 1
15 and over	1	1	1	
Mean Size of Family	—	6·08	5·53	3·50

Now it is quite certain that the Yule-Greenwood hypothesis here grossly exaggerates the number of first-born, and that exaggeration measures nothing but the exaggeration of the first-borns among the affected. The reconstructed population deduced by this hypothesis cannot be used as a criterion of whether there is or is not bias against the first-born. Much more reasonable is the measure obtained from the parental sibships, but we have to recognize that the offspring sibships while in the bulk complete are not wholly so. It seems to me that my use of the sibships of the imbeciles gives here the most suitable comparative distribution, but we might run up to 190 permille of first-borns without affecting in the least the statement that idiocy has a very marked bias against the elder-born, involving not only the first- but the second-born.

I now proceed to the 'Short Table', which, as we have seen, neglects the weighting of small families:

TABLE XXV IMBECILES (WITHOUT MONGOLIANS) FROM THE ROYAL ALBERT ASYLUM.

	Size of Family.												*Observed and Expected in Row.*
Order of Birth . .	1	2	3	4	5	6	7	8	9	10	11	12	
Expected Number	25	23·5	18	15·5	13·2	8·7	7·7	3·2	2·8	2·3	·9	·4	—
First-born . . .	25	25	18	16	17	15	9	2	3	–	–	–	130 121
Second-born . .	–	22	21	15	13	8	10	2	2	2	–	–	95 96
Average of Intermediates	–	–	–	–	9	6·5	7·7	2·7	2·4	2·7	1·3	·5	33 57
Average of two Last-born	–	–	–	15·5	8·5	8	6	5·5	4	2·5	·5	·5	51 55

It will be observed that there is considerable excess of first-born above expected, but the difference is not so marked as in the 'Long Table', i.e. there is a marked selection of small families. There is now no preponderance of second-born idiots, and, the Mongolians being excluded, there is even a defect in late-born; actually, the numbers show defect in the case of families under eight, and slight excess for large families of eight and over.[1] There is marked defect of idiots among the intermediates.

[1] Of course, as Dr. Hunter has remarked (*The Child*, January, 1914, p. 254), late-born defect is of relatively little importance, because the last-borns in big families are so relatively few in number as compared with first-borns in general.

The subject of imbecility has been also considered by Søren Hansen.[1] But his data are open to considerable doubt. No average age is given, so that we do not know whether the family was approximately complete. The material recorded is spread over a score of years, so it is quite possible that the same family was several times represented. No data are provided for forming a 'Short Table'.

TABLE XXV *bis*. IMBECILE PERMILLES. DANISH DATA. HANSEN.

Order of Birth.	*Imbeciles.*	*Sibships of Imbeciles (Pearson).*	*Danish Census. Complete Families.*	*Danish Industrial Class (15 years at least).*	*Yule-Greenwood Reconstruction.*
1	234	168	173	179	236
2	159	162	159	162	201
3	149	149	143	145	160
4	114	131	124	125	125
5	100	109	103	106	93
6	72	88	85	87	68
7	53	68	66	68	46
8	43	49	50	48	31
9	35	31	35	32	18
10	16	19	23	21	10
11	3	11	15	11	5
12	7	6	9	7	2
13	5	4	6	4	2
14	1	3	3	2	1
15	1	2	6	1	1
16 and over	2	2		2	1
Average Size of Family	—	5·95	5·78	5·59	4·24

Unfortunately, I have no data for the Danish Agricultural Classes, which, to judge by other nations, would be more fertile than the Industrial Classes and so nearer in accord with the results from 'Sibships of Imbeciles'. But it is clear that the latter far more closely resemble corresponding data for Denmark than the Yule-Greenwood reconstruction.[2] It seems to me incontestable that my process

[1] *Meddelelser om Danmarks Anthropologi*, II Bind, 1 Afdeling, p. 116.

[2] There are 994 imbeciles, so to preserve whole numbers we may take the permilles as given in the table above and test the goodness of fit. Comparing the 'Sibships of Imbeciles' with the Danish Industrial Classes, we find $\chi^2 = 2{\cdot}388$ (throwing 15 and over into one class) and find $P = {\cdot}9996$. Comparing the Yule-Greenwood Reconstruction with the Danish Industrial Classes, we have $\chi^2 = 68{\cdot}538$, and $P = {\cdot}000,000$. It is thus quite impossible that the Yule-Greenwood reconstruction should represent the distribution in completed families (marriages 15 years and upwards) of the Danish Industrial Classes in Copenhagen. The only answer to this must be either (i) that the Imbeciles are not drawn from completed

reproduces closely the distribution of order of birth in the class from which the material is drawn, and that the Yule-Greenwood process certainly does not.

(9) *Epilepsy.*

Closely allied to imbecility is epilepsy, and a table connecting size of family and order of birth of epileptic member was given by Dr. D. F. Weeks in 1912,[1] and is reproduced by Hansen (*loc. cit.*, p. 120). I will look first at this from the 'Short Table' standpoint.

TABLE XXVI. EPILEPTIC DATA. DISTRIBUTION IN FAMILIES OF EACH SIZE. 391 CASES; DUE TO WEEKS.

	Size of Family.														*Observed and Expected Totals.*
Order of Birth . .	1	2	3	4	5	6	7	8	9	10	11	12	13	14 and over	
Expected Number	19	11	13·3	11·8	9·2	7	6	5·1	2·9	2·6	1·2	1·0	0·5	0·6	—
First-born . . .	19	10	12	14	6	5	5	6	5	3	2	–	1	2	90 91·2
Second-born . .	–	12	16	15	8	9	7	6	4	4	–	–	–	–	81 72·4
Average of Intermediates	–	–	–	–	14	6·5	7·3	5·5	2·2	2·5	1·0	1·1	2·2	·7	43 36·1
Average of two Last-born	–	–	–	9	9	7·5	4	3·5	3	2	2	1·5	1·5	–	43 47·9

It is clear that here there is no excess of the eldest-born in the individual families; if there be any excess it is in the intermediates. Thus, if we may trust these data, which are slender, there is no weighting of the first-born in the case of epilepsy, unless it arises from the weighting of small families (see p. 5).

We now turn to the 'Long Table'; here we have no direct material to compare with these American distributions, and in a mixed population like the American, it would be hazardous to make comparisons. Below is the table, however, for what it is worth:

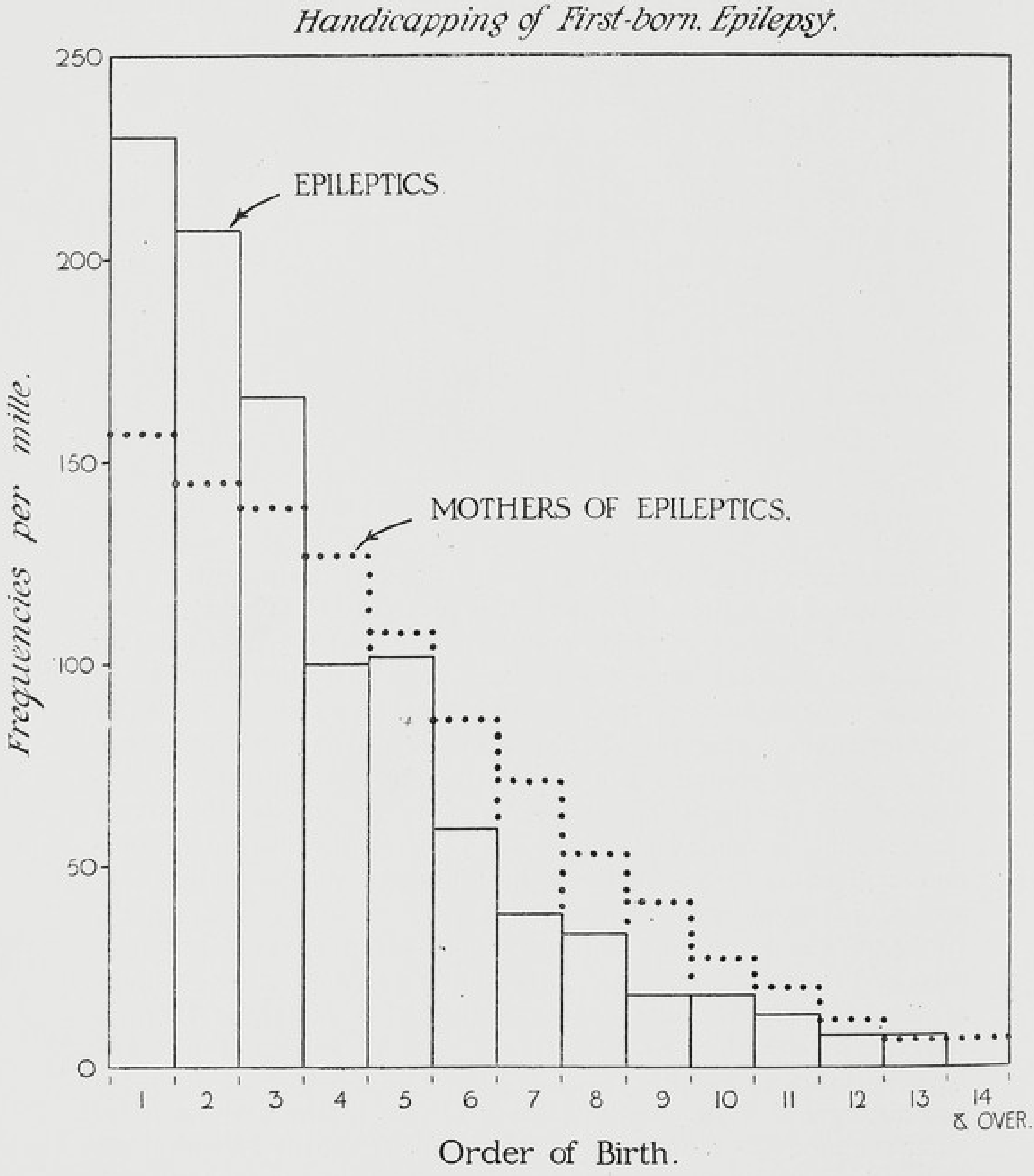
families, or (ii) that they are drawn from a far less fertile class than the Danish artisan. I think the latter hypothesis cannot be true, as many must be drawn from the far more fertile classes of agriculture and general labour. On the other hand it is probable that the families may not be quite complete, even if the average age be, as at the Royal Albert Asylum, nearly 16. When we recollect that the fertility of the insane and of the chronic alcoholist are respectively, although both incomplete, 5·2 and 6·1 in Great Britain, it seems utterly unreasonable to attribute to the parents of imbeciles in Denmark a nearly *completed* fertility of 4·2, as Messrs. Yule and Greenwood's process does.

[1] *Problems in Eugenics*, London, 1912, p. 95. I publish these results with reservation, as there are many instances of contradictions in Dr. Weeks's paper.

TABLE XXVI *bis*. EPILEPTICS, PERMILLES. AMERICAN DATA. WEEKS.

Order of Birth.	*Epileptics.*	*Sibships of Epileptics (Pearson).*	*Sibships of Mothers of Epileptics.*	*Sibships of Fathers of Epileptics.*	*Yule-Greenwood Reconstruction.*	*Sibships of First-born Epileptics.*
1	230	159	157	184	233	219
2	207	151	145	171	185	172
3	166	142	139	151	157	148
4	100	126	127	132	122	119
5	102	107	108	109	92	85
6	59	88	86	86	69	71
7	38	71	71	64	51	58
8	33	54	53	39	36	46
9	18	38	41	24	22	32
10	18	27	27	15	15	19
11	13	16	20	12	8	12
12	8	11	12	7	5	7
13	8	6	7	3	3	7
14 and over	–	4	7	3	2	5
Average Size of Family	—	6·29	6·37	5·44	4·29	4·58

I must say a word here about the sibships of the parents of the epileptics. I took them from the tables given in Dr. Weeks's paper. But a very little examination convinced me that the data were in great bulk obtained from the mothers, and that they were more reliable informants as to their own brothers and sisters than as to their husbands'. It appears to be almost an equal chance whether the father's sibship contains 4, 5, 6, or 7 members, while the distribution of the mother's sibships follows fairly closely the usual shape. On the other hand, there are 54 cases in which no information is given as to the mother's brothers and sisters, while there are 71 in which no information is entered as to the father's. In 14 of the former 54, information is given as to other relatives of the mother, and in only 3 of the 71 as to other relatives of the father. In only *one* case, that of a father, is there a record of an *only* child. Clearly no distinction is made in the record between cases in which nothing is known of the parents' brothers and sisters and those in which there are no brothers and sisters. After averaging all the cases in Table IV (p. 11 above), I determined 72 in 1,000 completed families to be families of a single member. 26·6 families should occur in the mothers' group and 25·9 in the fathers'. I have given them each 28 as a reasonable estimate. The result for



Handicapping of First-born. Epilepsy.
250
200
150
100
50
0
Frequencies per mille.
EPILEPTICS
MOTHERS OF EPILEPTICS.
1
2
3
4
5
6
7
8
9
10
11
12
13
14
& OVER.
Order of Birth.

the mothers' sibships is remarkable; it agrees completely with the data for the sibships of the epileptic themselves. The fathers' give nearly a child less, but I have no hesitation in describing it as far less reliable. Both show the Yule-Greenwood reconstruction as hopelessly exaggerating the number of first-born. We must, I think, conclude by recognizing that, while there is a weighting of the elder-born even in epilepsy, this is due to a selection of families rather than to a selection of the elder-born in each individual family.

(10) *Insanity.*

Here unfortunately we have not the original data any longer accessible to form a correlation table between size of family and order of birth. We are thus compelled to deal with the matter from the 'Long Table' standpoint. But what shall we put against the distribution of first-, second-, ... n^{th}-born among the insane? Clearly the families are all completed. As material coming from the same population, we have the offspring of the insane themselves. But their families are in many cases incomplete. Hence in this case 193 permille of first-borns must be considerably in excess of the required number. The modal age of the insane at onset is 36 years; many are of course younger, and it is well known that those who recover only too frequently return to increase their families. The point also is not whether as insane they can or will increase their families, but whether normal members of the same class and same age would do so; for we are endeavouring to measure the fertility of the population from which our insane material is drawn. According to the *Reports* of Murray's Royal Asylum, Perth, 20·9% of the inmates are of the professional classes, 12% are of independent means, 27·1% are commercial, that is of the shopkeeping class, 26·1% of the industrial or artisan class, and 13·9% of the agricultural class. The difficulty is to know what to do with the 12% of 'independent means', who may belong as well to the shopkeeping as the professional classes. Further, the professional classes appear to include clerks. I have accordingly given this group, which is 32·9% of the whole, 209 permille of first-borns; the commercial group has 190, the industrial 165, and the agricultural 151. We find as a result of the combination, that the Scottish population in question would have 184 permille of first-borns. I expect—to judge by the offspring of the insane—that this is an exaggeration, but we will let it stand, and will adopt the series from Table X (p. 17 above) as fitting to describe it. We then find:

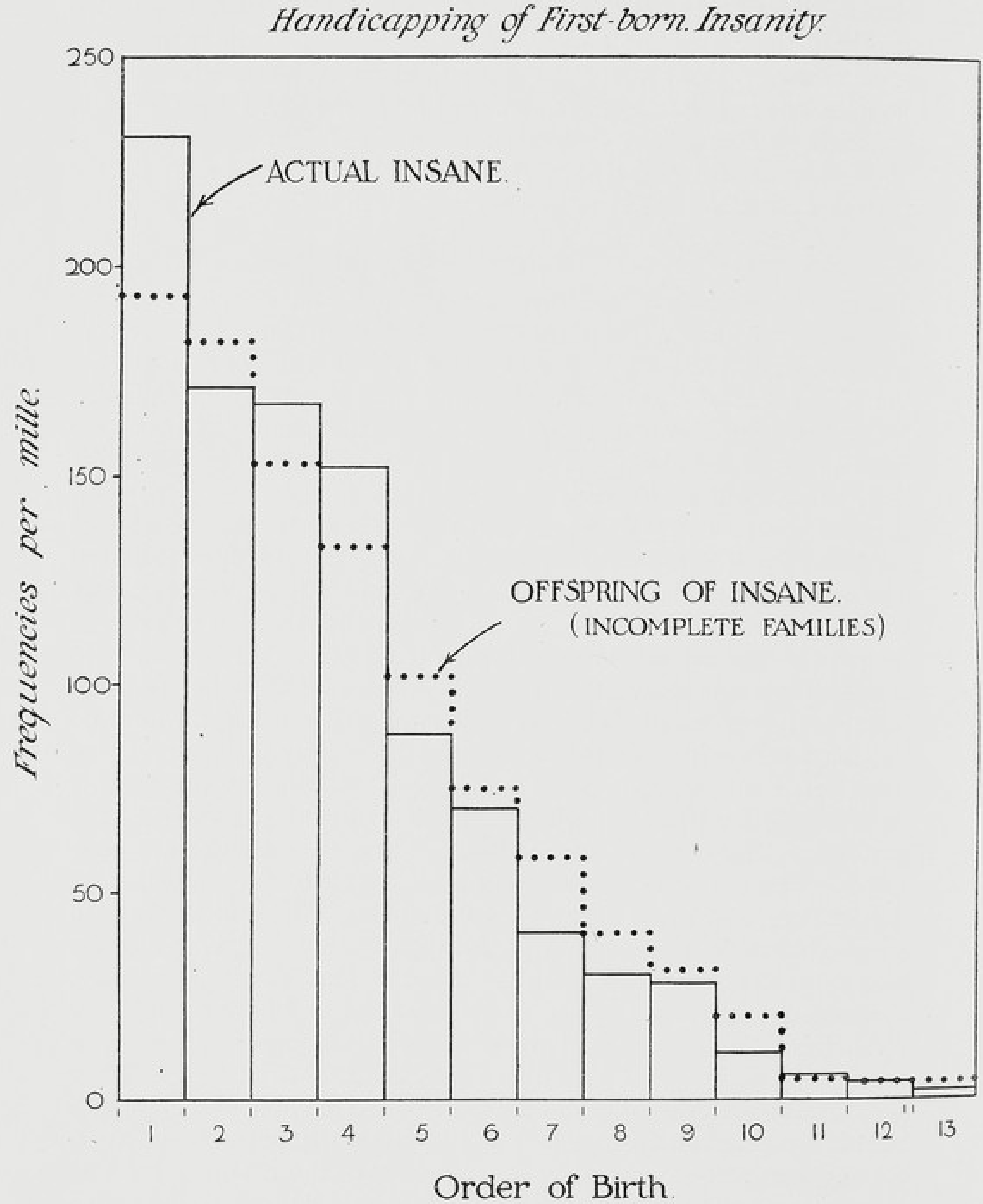

Handicapping of First-born. Insanity.
ACTUAL INSANE.
OFFSPRING OF INSANE.
(INCOMPLETE FAMILIES)
Frequencies per mille.
250
200
150
100
50
0
1
2
3
4
5
6
7
8
9
10
11
12
13
Order of Birth.

TABLE XXVII. INSANITY. DR. URQUHART'S DATA FROM MURRAY'S ROYAL ASYLUM, PERTH, REDUCED BY DR. HERON. PERMILLES OF EACH BIRTH ORDER.

Order of Birth.	*Actual Insane.* I.	*Offspring of Insane. Incomplete Families.* II.	*Sibships of Insane (Pearson).* III.	*Distribution from Inmate Occupations (see p. 36).* IV.	*All Scottish Marriages Incomplete.* V.	*Yule-Greenwood Reconstruction* VI.
1	231	193	168	185	216	223
2	171	182	159	173	184	191
3	167	153	150	157	150	169
4	152	133	130	136	121	125
5	88	102	112	112	95	101
6	70	75	86	87	73	70
7	40	58	69	60	55	51
8	30	40	49	37	40	32
9	28	31	29	23	27	17
10	11	20	19	13	18	10
11	6	5	13	7	10	6
12	4	4	8	5	6	4
13	2	4	5	3	3	1
14	–	–	2		1	–
15	–	–	} 1	2	1	–
16	–	–			–	–
Mean Size of Family	—	5·18	5·97	5·41	4·63	4·48

Now this table shows us at once that had we taken all the marriages at present in Scotland, *disregarding the incompleteness of large numbers of them*, we should not have got to the 223 permille of first-borns of the Yule-Greenwood reconstruction. I will allow the possibility that the sibships of the insane may here give too low a value, but it is far nearer to the 185 permille of the most probable comparative distribution than Messrs. Greenwood and Yule's 223! We know 193 to be a maximum exaggerated by the incompleteness of the families, by the check which confinement in an asylum places on fertility, and by the fact that smaller families are more customary now than in the generation of the parents of the insane. It is singular that our critics pass over the bearing of this distribution of the offspring of the insane, and here as elsewhere disregard the practically impossible values their reconstruction gives for the distribution in completed families of first-, second- to n^{th}-born in the population from which the material is drawn.

It is as well to illustrate this exaggerating tendency from data drawn from my Family Schedules. Those histories were selected by the choice of a 'subject', who filled in the schedule; such subject was, as a rule, by the nature of the case, a *normal* adult. Among the families thus recorded occur 281 cases of neuropathic sibships, i.e. sibships in which at least one person was neuropathic. These, of course, were reached independently of the neuropathic individual. They belong to the middle and professional classes, and probably on the average to a higher social class than the inmates of Murray's Royal Asylum, Perth. Of the neuropathic sibships, 89 have insane members, 5 feeble-minded members, 35 have nervous defect, 1 has a member with the drug habit, 96 have members with chronic alcoholism, 28 with epilepsy, and 27 with hysteria. The following table is based on this data. I have added to the neuropathic distributions the sibships of the Record subject, i.e. the 'selected' individual, and those of his parents, and the result of the Yule-Greenwood reconstruction from the neuropathic sibships. See Table XXVII*bis*.

TABLE XXVII *bis*. PERMILLE DISTRIBUTION OF ORDER OF BIRTH IN NEUROPATHIC STOCKS.

Order of Birth.	*All Neuropathic Sibships.*	*Insane Families.*	*Alcoholic Families.*	*Parents of 'Subject'.*	*Record Subject's Sibship.*	*Yule-Greenwood Reconstruction.*[1]
1	156	149	160	186	170	194
2	154	147	158	175	163	187
3	149	147	148	158	152	171
4	132	129	130	136	136	133
5	108	108	107	112	111	96
6	94	99	87	86	88	77
7	74	72	77	57	64	56
8	58	58	60	37	48	41
9	36	42	33	23	30	24
10	18	20	13	12	18	10
11	11	15	12	8	10	6
12	5	8	7	5	5	2
13	3	3	6	3	3	2
14 and over	2	3	2	2	2	1
Mean Size of Family	6·45	6·73	6·25	5·37	5·41	5·15

Now we have already seen how the Yule-Greenwood hypothesis exaggerates if applied to the sibships of the 'Record subject' (Table X: it gives 236 permille of first-borns!). The peculiar fact

[1] Based on the column of 'All Neuropathic Sibships'.

that is emphasized by Table XXVII^{bis} is again the exaggeration of first-borns in the population reconstructed by the Yule-Greenwood hypothesis from the neuropathic sibships alone (the reader must bear in mind that these give the order of birth of siblings of the neuropathic and not of the neuropathic population only, which unfortunately was not recorded). This hypothesis gives 194 permille of first-borns, whereas the general population from which the neuropathics are drawn has probably about 180.

I think we must support Dr. Heron in his view that there is weighting of the elder-born in the case of insanity, and that Messrs. Yule and Greenwood have simply weakened the contrast between the distribution of the insane and the distribution of the population from which they are drawn by the use of a fallacious method.

Here seems also a fitting point to refer to another matter: If when there is bias towards the firstborn, if when all members of a family are not equally likely to be affected, the Yule-Greenwood hypothesis markedly exaggerates the number of earlier-borns, it is clear that it can likewise give no safe measure of the fertility of the population from which the affected members are drawn. The exaggeration produced by the method used by Yule and Greenwood for the permille of first-borns shows itself again in the depreciated fertility these authors deduce for insane and tuberculous stocks. Here again there are new difficulties which they appear to have entirely overlooked. They state that if the marking were at random, and every individual had an equal chance of being marked, then their hypothesis would reproduce the sizes of family of the *original* population. But this is not what we want; we desire to know whether the section of the original population affected with the deformity or disease is more or less fertile than the original population at large. Albinism, insanity, and imbecility are not in our opinion distributed at random; they are confined to certain stocks which may be represented by large or by small families in any generation, and when we take a census of insanity for a given district, as we practically do by examining the records of Murray's Royal Asylum, Perth, we are deducing something approximating to the real fertility of the insane stocks, and not to the exaggerated fertility of random marking. The chance of a given individual being an imbecile is very small, but this smallness arises from the chance of his belonging to an imbecile stock being very small; if he does belong to such a stock, then his chance of being an imbecile is considerable; it is still greater, if we consider his chance of being insane, if he comes of the relatively few insane stocks.[1] Finally, if we are to reduce the observed fertility in one case, we must do it in all, and we cannot reduce the insane and omit to do this with the normal material which has been in many cases collected in precisely the same manner, i.e. by asking an individual the size of his stock.[2] Instead of venturing, however, into any such field of endless hypothesis, it appears to us far more reasonable to compare the distribution of first-, second-, ... n^{th}-borns who are affected, with the nearest distribution of a corresponding social character to that of the population from which the affected are drawn. This is undoubtedly the '*Distribution from Inmate Occupations*' of Table XXVII (Column IV) in the case of the insane. And this is sufficient to argue upon in demonstrating the bias of the first-born. But even here I must confess to prejudice in favour of the '*Sibships of the Insane*' (Column III), for the simple reason that I do not believe 0·2 children to represent all that the insane with a modal age of 36 would require to 'complete' their families. I should hold 0·8 to 1·0 children a far more reasonable addition. Of course this is on the assumption that children of the insane are representative of the fertility of the stocks from which the insane are themselves drawn. If this be denied, then it has at least to be admitted that the fertility of the insane is in excess of that of the population from which they are themselves drawn, the point which our critics set out to disprove.

(11) *Albinism.*

I now pass to the subject of Albinism, and shall consider only albinos of European race. I have omitted all cases of defective pedigrees, and deal finally with a population of 952 albinos, of whom something like 90% are due to England, Scotland, and Norway. The great majority of these albinos belong in the first place to the agricultural, and in the second place to the artisan classes.

[1] With Mendelian values we should expect 100%, 50%, or 25% of the members of a tainted sibship to be affected. Taking 50% and supposing such random marking, the distribution for tuberculous stocks would not reduce, as Messrs. Greenwood and Yule suggest, from 5·68 to 4·06, but only to 5·36.

[2] Even the data from the *Peerage* must be 'corrected'. For take the case of a peer with a family of one or two: if these die, the peerage may come to an end and will disappear from the record, and thus chance of non-disappearance will be proportional to the size of family. In the same way with the census returns. Parents who die early leave few children, and thus small families will be omitted more frequently than large ones by deaths of parents.

Handicapping of First-born. Albinism.

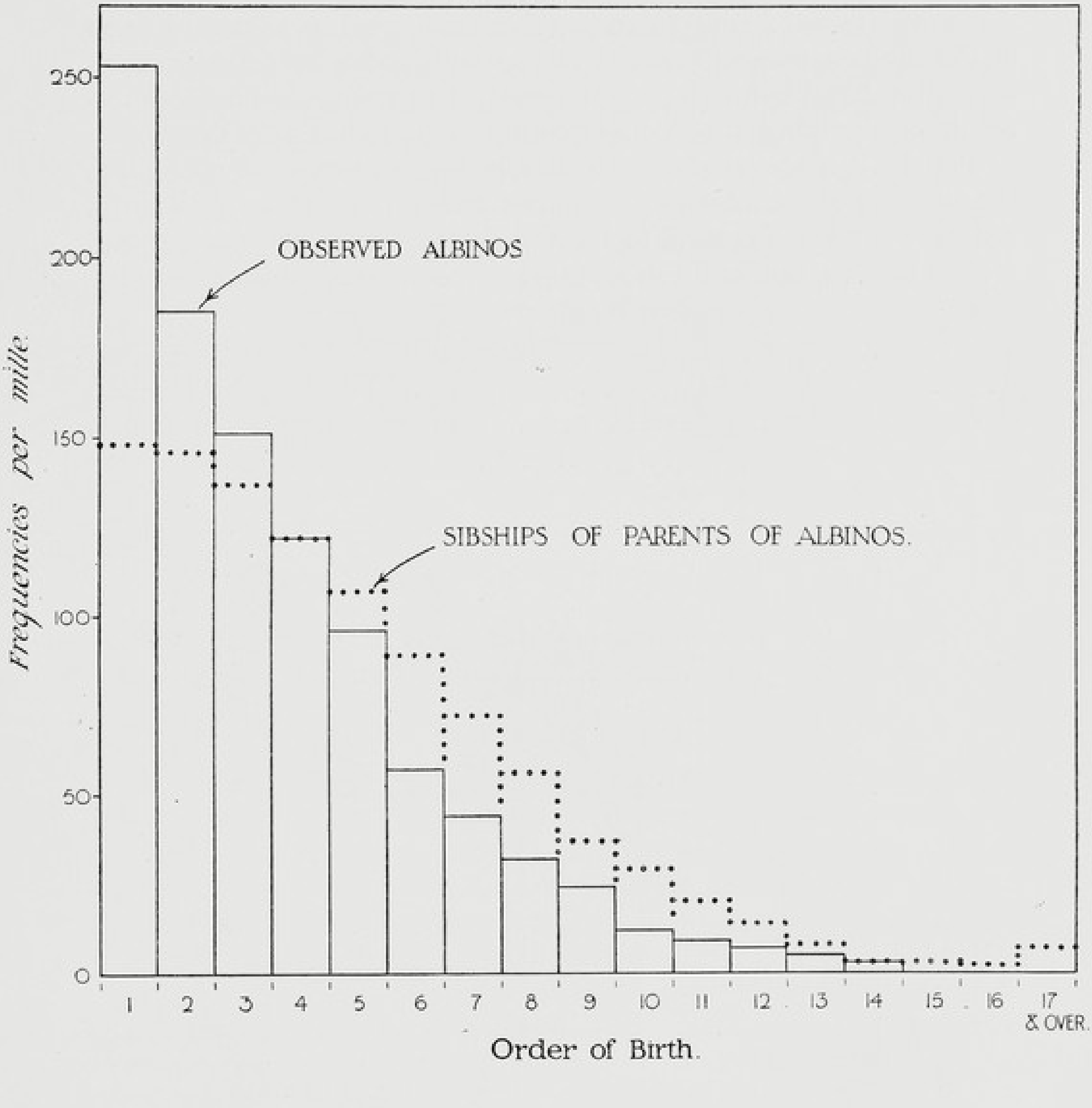


Table XXVIII is the usual form of 'Long Table'. In the first column we have the observed albinos arranged for each order of birth in permilles. In the second column I give the sibships of the parents of the albinos arranged in order of birth. This is a fair sample of what the population from which the albinos are drawn has for its unselected fertility. In the third column I give the sibships of the albinos themselves, and in the fourth and fifth the Scottish agricultural and industrial populations. The sixth column gives the result of applying the Yule-Greenwood hypothesis to the albino sibships. We observe at once that it exaggerates wildly, and that the sibships of the albinos themselves (Pearson) are closely in accord, not only with the results to be anticipated in the agricultural and artisan classes, but with the sibships of the albinos' parents.

TABLE XXVIII. PERMILLE DISTRIBUTION OF ORDER OF BIRTH IN THE CASE OF ALBINISM.

Order of Birth.	*Observed Albinos.*	*Sibships of Parents of Albinos.*	*Sibships of Albinos (Pearson).*	*Scottish Agricultural Class.*	*Scottish Industrial Class.*	*Yule-Greenwood Hypothesis.*
1	253	148	156	150	165	227
2	185	146	149	146	159	186
3	151	137	139	138	148	152
4	122	122	125	128	133	122
5	96	107	107	115	114	95
6	57	89	90	97	94	72
7	44	72	69	78	72	50
8	32	56	51	59	50	33
9	24	37	41	39	32	25
10	12	29	27	24	18	15
11	9	20	20	14	9	10
12	7	14	12	7	4	6
13	5	8	6	3	1	3
14	3	3	4	1		2
15	–	3	2		1	1
16	–	2	2	1		1
17 and over	–	7	–			–
Mean Size of Family	—	6·75	6·43	6·64	6·05	4·40

It will be noticed that there were no less than 0·7% of children born in the 17th or higher places among the sibships of the albinos' parents. This marks the heavy fertility of the Scottish and Norwegian peasantry from which so much of our material was drawn.

Actually, in the districts of Scotland and Norway, chiefly dealt with, we had rather a census than a sample of albinism. It might be supposed that here, with a congenital defect, something like the Yule-Greenwood assumptions would hold. They do not, because (i) there is no pretence really of random marking; all families are not equally likely, and where the stock is albinotic, there albinism will almost certainly appear; and (ii), because there is weighting of the first-born, the Yule-Greenwood hypothesis exaggerates the resulting early-borns.

I now turn to the 'Short Table' method:

TABLE XXIX. ALBINOS. DISTRIBUTION IN FAMILIES OF EACH SIZE.

	Size of Family.																*Observed and Expected Totals.*
Order of Birth . .	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	
Expected Number	39·0	32·5	28·3	26·2	21·6	21·3	15·4	7·9	9·2	4·8	4·5	2·7	1·1	·8	·1	·7	—
First-born . . .	39	33	36	28	28	25	21	7	11	6	2	1	2	1	0	1	241 216
Second-born . .	–	32	28	25	21	20	17	10	9	5	3	2	1	2	0	1	176 177
Average of Intermediates	–	–	–	–	21	25	17·3	8	8·8	5·1	4·9	3·1	·9	·6	0	·8	96 90
Average of two Last-born	–	–	–	26	19	16·5	8·5	7	9·5	3	5	2	1·5	1	1	1·5	102 116

It will be seen that there is marked excess of albinos among the first-born—241 cases instead of 216; that the second-born have no excess, and that if anything there is defect in last-born. Here as before, the first row gives the size of family, and the second row the expected number if the albinos were distributed equally through each order of birth.

I think there cannot be the least doubt of a quite significant weighting of the first-born in the matter of albinism.

(12) *Criminality.*

I do not agree with Dr. Goring's treatment of the problem of the elder-born in his recent invaluable report on the English convict.[1]

[1] *The English Convict, a Statistical Study*, Wyman & Sons, 1913, pp. 278-9.

In fact, until Messrs. Yule & Greenwood drew attention to the matter, I had not noticed that his method diverged from that adopted by myself, or I should have thrashed the point out with him. My objections here are precisely those which occur in the case of insanity: Every member of a large family is not equally likely at any given time to be in a convict prison; and, further, every family of the class from which the bulk of convicts are drawn is by no means equally likely to contribute to the convict population. Further, Dr. Goring, in antedating Messrs. Yule and Greenwood in the application of my method of 1898 to the problem of the elder-born, has also overlooked the fact that that method, if there be bias towards the elder-born and bias towards small families, will much exaggerate the percentage of elder-born sons. In the case of criminals it gives 216 permille of eldest sons, which is practically identical with that of the Scottish Professional Classes, or, indeed, with the incomplete families of the whole of Scotland! Now about 71% of criminals are drawn from the class of general labourers, hawkers, and inferior artisans, and, since their parents' families are practically complete,[1] we should expect only 140 to 150 permille of eldest-born at a maximum. Yet Messrs. Yule and Greenwood reach 216 permille, and find nothing remarkable about it! Even with such a number, the observed 269 permille of eldest-born criminals has such excess over the hypothetical number, that the only conclusion must be that 'the elder members of a family, especially the first and second, are liable to become criminal at a greater rate than are the younger brothers' (Goring, *loc. cit.*, p. 280).

Dr. Goring's suggestion (*loc. cit.*, p. 280) that possibly the later-born have greater infant mortality, and so do not survive in the same numbers to become criminals, is not, I think, borne out by the statistics of infant mortality (see p. 21 above), except in the case of the extreme members of very large families, who are too few in number to produce any such marked effect as that observed.

In order to show what really a permille of 216 eldest-born means, I have taken 4,000 births of the poorest Glasgow population, and tabulated in the families which were thus *all definitely incomplete* the number of first-, second-, ... n^{th}-born. There results:

[1] 90% of the prison population are 20 years and over, and under ·05% are below 17 years of age.

TABLE XXX. PERMILLE OF FIRST-, SECOND-, -n^{th} BORN OF THE POORER CLASSES IN GLASGOW.

Order of Birth.	*Permille.*	*Order of Birth.*	*Permille.*
1	225	9	19
2	197	10	11
3	163	11	6
4	127	12	3
5	98	13	1
6	70	14	
7	48	15	1
8	31	16 and over	

It seems impossible to assert that the *completed* fertility of the stocks from which criminals are drawn shows practically the same percentage of first-borns, i.e. 22 %,[1] as the fertility of families from practically the same class in which every family *is still incomplete* for the source of the record is the occurrence of a birth! The 'Long Table' is as follows:

TABLE XXXI. DISTRIBUTION OF ORDER OF BIRTH IN CRIMINAL STOCKS. PERMILLES OF FIRST-, SECOND-, ... n^{TH}-BORN.

Order of Birth.	*Criminals Observed.*	*Sibships of Criminals (Pearson).*	*Scottish General Labourers and Hawkers.*	*Scottish Miners.*	*Scottish Dock-hands.*	*Scottish Professional Classes.*	*Yule-Greenwood Hypothesis.*
1	269	143	148	137	151	218	216
2	235	137	143	134	144	200	176
3	137	130	136	130	136	170	150
4	94	117	125	123	128	132	119
5	77	104	113	114	113	97	96
6	52	85	97	102	99	70	70
7	40	68	80	86	80	48	50
8	24	54	62	68	60	30	37
9	22	42	43	47	39	17	25
10	12	33	26	30	24	10	19
11	8	26	14	16	14	4	14
12	9	19	8	8	7	2	10
13	8	14	3	3	3	1	7
14	4	10	1	1	1		4
15	2	6				1	2
16	3	4	1	1	1		2
Over 16	4	8					3
Mean Size of Family	—	7·0	6·8	7·3	6·6	4·6	4·6

[1] My result, 225 permille of first-borns for material of 1911, is singularly close to the 228 for 1855 given on p. 28.

Messrs. Greenwood and Yule give to the stocks from which criminals are drawn a completed fertility comparable only with that of the family-limiting Professional Class, and quite out of keeping with those rougher types of the community which prison statistics show are largely drawn upon for the criminal population; labourers, miners, and the lower types of artisan provide 71 % of this population.

The 'Short Table' for Criminals entirely confirms the result from the 'Long Table':

TABLE XXXII. CRIMINALS. DISTRIBUTION IN FAMILIES OF EACH SIZE. EXPECTED AND OBSERVED.

	Size of Family.																*Observed and Expected Totals.*
Order of Birth . .	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	
Expected Number	57	36·5	45·3	32·5	37·2	29·2	18·7	16·1	9·2	7·8	5·6	4·2	3·5	2·8	·1	1·3	—
First-born . . .	57	38	57	39	53	42	16	17	18	11	15	4	6	4	2	5	384 307
Second-born . .	–	35	47	47	50	37	33	33	15	7	6	7	7	4	3	2	333 250
Average of Intermediates	–	–	–	–	29	23	17·3	11·2	5·6	6·7	4	3·9	2·4	2·3	·8	·8	107 136
Average of two Last-born	–	–	–	22	27	25	15	17	11	10	6·5	4	5·5	4	1	1·5	150 166

The actually observed first- and second-born criminals amount together to 717 as against 557 which would be anticipated if the tendency to crime were divided equally among all members. There is a defect of both intermediates and of last-born criminals. The general bias against the elder-born appears amply substantiated on these data.

(13) *Tuberculosis.*

I turn in the next place to the disease on which my first investigation was based. My data in that case were provided by Dr. Rivers, then of the Crossley Sanatorium. The modal age at onset was 29 for men and 25 for women, and only very few patients indeed were admitted as young as 15. The families can therefore be looked upon as practically completed. With what series shall we compare them? This sanatorium draws on the lower middle and working classes, chiefly skilled artisans. I have placed against the observed results, (i) the distribution for Skilled Artisans, (ii) that for all families in Scotland for which the wife is over 45, (iii) a series, which I obtained from the ages of the patients, by giving them a mother just 20 years older than they were at time of onset; this was done approximately, only six age-groups for the tuberculous being used. The result came surprisingly close to that for the sibships of the tuberculous (Pearson). It will be seen that in this case the Yule-Greenwood hypothesis attributes to the population from which the tuberculous are drawn a fertility no greater than that of the incomplete Glasgow families, and less than that of the Scottish Professional Classes. The actuarial proposal to deal with the sibships of the first-born affected comes off still worse than the Yule-Greenwood scheme. Both these plans must fail, if there be real bias against the first-born. My own reconstruction from the siblings of all the affected again seems to give a result far more compatible with the actual population than either the medical or actuarial reconstruction. It has not now, any more than it had when I first used it, a theoretical justification; its value is based solely on what I showed in my first paper and what I have repeatedly indicated here, namely, it agrees reasonably closely with the distribution which we learn by experience to associate with a given social type. One may feel absolutely confident that 160 to 180 permille of first-borns corresponds to the actuality in the class from which this sanatorium draws, and equally confident that 220 to 240 eldest sons per 1,000 is impossible. Table XXXIII is the 'Long Table' corresponding to this case, and we see at once that even if the Yule-Greenwood hypothesis were correct, there would still be ample ground for the assertion that tuberculosis is biased against the elder-born.

The reader is requested to examine carefully this table. The New South Wales data are what I originally selected for comparative purposes, and to justify the use of the sibships of the phthisical. They agree extraordinarily closely with the Scottish Skilled Artisans, who are, of course, principally Lowland Scottish, and nearly allied in race to the North Country English. The sibships of the affected give almost identical results with the series of completed families found by paying attention to the ages of the mothers of the patients.

The whole of the first four comparative series are of a totally different order to those provided by the Yule-Greenwood hypothesis or the actuarial proposal to select the sibships of the first-born affected. We see that these hypotheses give fertilities which are less than that of the completed families of the Professional Classes, and less than that of the incomplete families of the Glasgow Working Classes. I think we may safely conclude that these hypotheses cannot in any

TABLE XXXIII. PERMILLES FOR EACH ORDER OF BIRTH. CROSSLEY SANATORIUM DATA AND COMPARATIVE AND HYPOTHETICAL SERIES.

Order of Birth.	*Actual Phthisical Patients.*	*Sibships of Phthisical Patients (Pearson).*	*Scottish Skilled Artisans.*	*New South Wales Industrial Classes.*	*Scottish Data. Mothers' ages fixed by those of Patients.*	*Scottish Professional Classes.*	*Incomplete Scottish. Working Classes.*	*Yule-Greenwood Reconstruction.*	*Distribution from First-born Phthisical Patients.*
1	297	176	167	166	174	216	225	227	234
2	207	169	160	151	164	184	197	193	204
3	108	153	149	133	148	150	163	154	166
4	136	134	133	117	130	121	127	121	122
5	102	114	114	102	108	95	98	97	102
6	47	86	93	86	88	73	70	68	71
7	47	58	71	70	68	55	48	45	39
8	24	40	49	56	49	40	31	32	33
9	8	26	31	42	33	27	19	24	17
10	8	16	17	30	20	18	11	18	8
11	8	10	9	20	10	10	6	15	2
12	3	7	4	12	5	6	3	3	2
13	3	4	2	7	2	3	1	1	–
14	2	3	} 1	3	} 1	1	} 1	1	–
15	–	1		} 5		1		} 1	–
16 and over	–	3				–			–
Mean Size of Family	—	5·68	5·99	6·04	5·74	4·63	4·44	4·41	4·27

way represent the state of affairs among the population from which the Crossley patients were drawn. And the reasons for this are precisely those we have previously given: all families are not equally likely to be tainted, and all members of families which are tainted are not equally likely to appear in a sanatorium population. The fundamental hypotheses of the Greenwood-Yule theory have no application to anything but the pure random-marking as illustrated in our example of the H-marked offspring of Quaker-families (p. 7).

The 'Short Table' for this material is as follows:

TABLE XXXIV. PHTHISICAL PATIENTS. DISTRIBUTION IN FAMILIES OF EACH SIZE. EXPECTED AND OBSERVED.

	Size of Family.											*Observed and Expected Totals.*
Order of Birth . .	1	2	3	4	5	6	7	8	9	10	11 and over	
Expected Number	15	17	14·3	10·5	12·4	9·8	5·7	3·6	2·4	1·4	1·6	—
First-born . . .	15	18	21	10	15	15	3	8	4	3	1	113 93·7
Second-born . .	–	16	13	13	8	9	11	3	3	–	3	79 78·7
Average of Intermediates	–	–	–	–	11	8·5	4·3	3·0	2·0	1·7	1·5	32 36·9
Average of two Last-born	–	–	–	8·5	14	9	6·5	3	2·5	·5	4	48 47·4

Hence we see that, even if we neglect the weighting of small families and consider families of each size only, there is a bias of tuberculosis against the first-born.

Dr. Rivers has put together data from Riffel's papers, and I reproduce here the distribution of phthisical members in the series (S + K) with comparative distributions. The families may be taken as completed, and a rural community is the source. I have no distribution of the size of families in German rural districts, and can only put against the phthisical distribution those of agricultural populations in Scotland and New South Wales. It seems to me very improbable that the fertility of German rural completed families is less than those of British race. See Table XXXV.

Here, again, the sibships of the affected give a result in accordance with other experience; the Yule-Greenwood reconstruction erroneously exaggerates the percentage of first-born. Indeed, it represents here much the permille of first-borns that we should obtain for the births in a given year, rather than the permille of first-borns in the completed families of a rural district.

Brehmer[2] has also published data for order of birth in cases of

[1] *Lancet*, Oct. 7, 1911, p. 1001.

[2] Hermann Brehmer, *Die Aetiologie der chronischen Lungenschwindsucht vom Standpunkt der klinischen Erfahrung*, Berlin, 1888, p. 178, &c.

tuberculosis, but, on close inspection, I found it impossible to use his material for present purposes. It has been much selected and is divided into classes to illustrate certain 'principles', and we cannot be sure that his classes, if combined, would represent the general population. Thus his first series contains 100 cases which are supposed to justify the conclusion that: 'It is probable that the last

TABLE XXXV. PERMILLES FOR EACH ORDER OF BIRTH. RIFFEL'S DATA WITH COMPARATIVE AND HYPOTHETICAL SERIES.

Order of Birth.	*Phthisical Members.*	*Sibships of Phthisical (Pearson).*	*Scottish Agricultural Class.*	*Scottish Industrial Class.*	*New South Wales Agricultural Class.*	*Yule-Greenwood Reconstruction.*
1	203	164	150	165	145	239
2	158	157	146	159	134	198
3	134	142	138	148	123	154
4	139	125	128	133	112	117
5	104	106	115	114	101	89
6	99	90	97	94	89	69
7	54	71	78	72	78	50
8	49	53	59	50	64	34
9	25	38	39	32	51	23
10	15	24	24	18	39	13
11	10	14	14	9	26	7
12	10	8	7	4	17	4
13	–	4	3	1	9	1
14	–	2	1	} 1	5	1
15 and over	–	2	1		7	1
Mean Size of Families	—	6·10	6·64	6·05	6·91	4·18

offspring of a numerous family whose parents are themselves strong and healthy will be phthisical, although the earlier children are sound.' Accordingly, we find 100 big families, the last or nearly last children in which are phthisical. We then have 100 families taken to represent another point, and so on. It is impossible to combine series of round numbers like this and suppose them components of a general population. I therefore have excluded Brehmer's data entirely.

In the same article in which Rivers considers Riffel's material, he prints the following table; this table giving the causes of death of the many individuals discussed by Riffel seems to me suggestive and worthy of reproduction.

TABLE XXXVI. DR. RIVERS'S TABLE BASED ON RIFFEL'S DATA FOR THE RELATIVE DEATH-RATES OF EACH BIRTH ORDER.

	Order of Birth.											
Cause of Death .	1	2	3	4	5	6	7	8	9	10	11	12
Accident . . .	·1	·7	·6	·4	·6	·5	·6	·9	·4	–	–	2·0
Typhus . . .	1·2	·7	1·2	·7	·5	1·3	·4	·6	1·7	1·3	1·1	–
Cancer . . .	1·2	·8	1·1	·9	·4	·5	·6	·3	1·2	1·3	–	–
Pneumonia . .	1·8	2·5	2·0	1·5	1·3	2·1	2·1	1·8	·8	2·6	–	–
Tuberculosis .	6·3	5·0	5·2	5·7	5·2	5·7	5·0	4·8	3·4	3·3	3·3	6·2

Perhaps no very great stress can be laid on this table, but as far as it goes it shows: (i) that accidental deaths and deaths from typhus or pneumonia do not weigh more heavily on the first-born than on later-born children; indeed accidents appear to be a minimum for the first-born, probably because they have the mother's more undivided attention; and (ii) the death-rates from phthisis, and just possibly cancer, do appear to be in excess for the first-born.

Quite recently a Danish investigator, Søren Hansen, after dismissing with contumely my original reference to the weighting of the elder-born in the Tuberculosis memoir, has then proceeded to demonstrate the same fact from Danish material.[1] I pardon him his criticism for the more ample material he brings to the discussion, and from which he deduces the conclusion that it may be taken as certain that pulmonary tuberculosis is considerably more frequent among the earlier than the later children. It will interest my readers to put his material into the 'Long Table' and 'Short Table' forms we have used in this paper. The population with which Hansen deals is that of the consumption section of Øresund Hospital in Copenhagen. The average age of the males may be taken as 29·6, and of the females 27·4.[2] Accordingly, it is reasonable to consider the

[1] *Meddelelser om Danmarks Anthropologi*, II Bind, 1 Afdeling, pp. 112–52, 'Om de førstfødte Børns ringere Kvalitet.'

[2] Averages from Boserup Sanatorium, merely rough numbers to indicate practical completion of the family. I do not know how Hansen has tabled his ages on p. 134. If done in the usual English manner, his mean ages would be 30·1 and 27·9 respectively, and not 29·6 and 27·4 as he states. He seems to think that my results (see his p. 115) would be invalidated

families as completed, and we may use the data for the Industrial Class of Copenhagen in Table IV. Hansen himself takes marriages of 25 years' duration, and suggests 171 permille of first-borns. Table XXXVII is the 'Long Table', with the comparative and hypothetical data:

TABLE XXXVII. PERMILLES FOR EACH BIRTH ORDER. SØREN HANSEN'S TUBERCULOSIS DATA.

Order of Birth.	*Phthisical Members.* I.	*Sibships of Phthisical (Pearson).* II.	*Danish Census. Completed Marriages.* III.	*Copenhagen Industrial Class.* IV.	*Scottish Industrial Class.* V.	*Yule-Greenwood Reconstruction.* VI.	*Sibships of Firstborn Phthisical.* VII.
1	281	171	173	179	165	253	243
2	202	162	159	162	159	202	199
3	161	148	143	145	148	160	161
4	121	127	124	125	133	120	119
5	77	105	103	106	114	87	88
6	56	84	85	87	94	63	61
7	32	63	66	68	72	42	43
8	23	47	50	48	50	29	31
9	13	31	35	32	32	17	19
10	14	22	23	21	18	11	13
11	7	14	15	11	9	7	8
12	7	10	9	7	4	4	6
13	2	6	6	4	1	2	4
14	1	4	3	2		1	2
15		2		1		1	1
16	3	2	6	1	1		1
17		1		1		1	1
18 and over		1					
Mean Size of Family	—	5·85	5·78	5·59	6·05	3·95	4·12

I presume Hansen has avoided, as I did, taking two individuals from the same family; at any rate, the agreement between the second and third columns shows that the influence of two members of the same family upon the results must be very small. Hansen only tells

if first-born were on the average older than the second-born. I did not think it needful to demonstrate such an obvious result as that; unless there was a very marked death-rate, the average age of all offspring would be the same. Hansen (p. 135) appears to think that he has shown that the early-born male is slightly younger than the later-borns, and gives the result 1–4-born 29·5 years, 4 and over 29·8 years. But he has not calculated the probable error of his results. He should have given 1–4-born $29{\cdot}5 \pm {\cdot}25$ years, which shows that no significant difference exists.

us that he took the material 'fra en forholdsvis kort Periode',[1] which may possibly explain why he has been protected from the multiplication error. We are able to put Hansen's material in the 'Short Table' form, as in Table XXXVIII below.

TABLE XXXVIII. PHTHISICAL PATIENTS. DISTRIBUTION IN FAMILIES OF EACH SIZE EXPECTED AND OBSERVED. DANISH DATA. HANSEN.

	Size of Family.															*Observed and Expected Totals.*
Birth Order . .	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15 and over	
Expected Number	178	150·5	141·3	114·5	86·0	72·5	47·7	40·8	21·2	14·9	8·0	7·2	3·2	2·1	3·1	—
First-born . . .	178	155	170	128	107	74	52	45	26	19	10	9	6	3	6	988 891
Second-born . .	–	146	134	106	100	77	44	45	22	12	9	11	5	1	1	713 713
Average of Intermediates	–	–	–	–	85	74·5	50·3	37·8	21·2	14	7·9	5·4	2·6	1·9	3·0	303·6 306·7
Average of two Last-born	–	–	–	112	69	67·5	43·5	42·5	18·5	17	7	11·5	3·5	3·5	·7	396·2 421·2

We see at once that, considering each family individually, there is heavy preponderance of tuberculosis in the case of the first-borns; there is also defect in the case of the late-borns. Hansen gives a second series of data from the *Boserupsanatoriet* of 2,113 cases; here fewer than at Øresund come into the 15 and under group — indeed only 74 out of the whole number; while there appears to have been a larger but unstated number at Øresund. The results would in this respect approach more nearly the Crossley Sanatorium data. The following is the 'Short Table':

[1] *loc. cit.*, p. 120. Westergaard's criticisms (*Meddelelser om Danmarks Anthropologi*, II Bind, 1 Afdeling, pp. 155-61) seem to me to be wide of the point, and confuse number in completed family with birth order in the community at large reckoned on actual births. I am unable to follow what he says about surviving children, because he does not state, as far as I can see, for what period they have survived; and in cases of congenital defect, like albinism, we have, of course, considered all dead albinos. I do not see that the comparison between new-born children, 'surviving children', and the insane order of birth on his p. 157, demonstrates anything. The order of birth in completed families, as in the sibships of the insane, differs wholly from the other two.

TABLE XXXIX. PHTHISICAL DISTRIBUTION IN FAMILIES OF EACH SIZE. DATA FROM BOSERUPSANATORIET. HANSEN.

	Size of Family.															*Observed and Expected Totals.*
Order of Birth . .	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15 and over	
Expected Number	48	69	76·7	64·7	53·6	45	34·1	24·6	15·8	9·9	6·7	6·3	2·1	1·8	1·4	—
First-born . . .	48	74	91	84	54	46	33	27	19	10	6	5	2	3	4	506 459·7
Second-born . .	–	64	66	66	49	45	30	24	26	11	9	12	1	4	1	408 411·7
Average of Intermediates	–	–	–	–	70	44	34·3	25·8	14	10·2	6·4	4·6	2·1	1·6	1·1	214·1 201·3
Average of two Last-born	–	–	–	54·5	47·5	45·5	36·5	21·5	13·5	8·5	7	10·5	2·5	1	1·5	250 266·0

The excess of first-borns is again demonstrated; the slight excess of intermediates appears to be entirely due to an anomalous 70 cases in families of 5.

We will now consider the 'Long Table' method of investigating this material. I have added here for comparison the usual 'Sibships of the affected', the 'Danish Census, completed Families', the 'Yule-Greenwood Reconstruction', with the 'Sibships of the First-born affected'.

The material in this case ought to give lower values permille of first-born than that from Øresund, because there are fewer non-adults. The Danish census of course includes all social grades, and it may well be doubted whether all dead infants have been as fully recorded as in the case of an individual and personal inquiry. We should anticipate, therefore, something below the 171 of the second column of Table XXXVII. The resulting 159 of the phthisical sibships may be somewhat depressed, but is, I feel sure, closer to the result for nearly completed families than the 'Yule-Greenwood' or the 'Sibships of First-born Phthisical' columns. These give much too low a fertility. To illustrate this, I put in column IV a new series,[1] namely the permilles in each order of birth of the births in a Danish

[1] From a valuable paper by H. T. Hansen, 'Undersøgelser over nyfødte Børns Vægt', *Meddelelser om Danmarks Anthropologi*, II Bind, 1 Afdeling, p. 41. See also the same journal, p. 157, for Ditzel's data.

TABLE XL. PERMILLES OF EACH BIRTH ORDER. BOSERUPSANATORIET. HANSEN.

Birth Order.	*Phthisical Members.* I.	*Sibships of Phthisical (Pearson).* II.	*Danish Census. Completed Families.* III.	*Danish Births.* IV.	*Yule-Greenwood Reconstruction.* V.	*Sibships of First-born Phthisical.* VI.
1	239	159	173	220	217	218
2	193	155	159	191	195	198
3	171	145	143	159	162	165
4	121	128	124	117	126	126
5	88	108	103	93	95	90
6	65	88	85	64	70	66
7	46	68	66	49	49	47
8	28	50	50	37	32	33
9	19	35	35	26	21	21
10	9	24	23	20	13	13
11	10	17	15	12	9	8
12	5	11	9	7	5	6
13	4	5	6	3	2	4
14	} 2	3	3	1	2	3
15		2	} 6	} 1	1	} 2
16 and over		2			1	
Average Size of Family	—	6·29	5·78	[4·54]	4·60	4·59

rural district, including, however, a small town of 2,100 inhabitants. The following results are also given by Ditzel, who takes both legitimate and *illegitimate*[1] births:

DANISH BIRTH ORDER AND PERMILLES (DITZEL).

1	209	(220)	4–6	309	(274)
2	180	(191)	7–10	134	(132)
3	154	(159)	11 and over	14	(24)

corresponding to an *incomplete* family average of 4·78. Thus Ditzel's values support those of Hansen, and demonstrate that the values reached by the Greenwood-Yule treatment are such as could only arise if the case of *incomplete* fertilities were under discussion. I think we may safely conclude that Hansen's new data entirely confirm my original view that the eldest-born is weighted, and very sensibly weighted, for phthisis.

But the method I have here introduced of taking the birth order in growing families seems to me worth discussing at greater length.

[1] This should, of course, tend to exaggerate, not depress, the number of first-borns.

Let us suppose we are dealing with a stable population, and that per thousand wives still in their child-bearing ages $m_1, m_2, m_3, \ldots m_s$ are the numbers of children born in a given period in first-, second-, third- ... s^{th}-birth orders. These numbers can be easily ascertained by asking the number of the pregnancy in the case of each recorded birth during a given period of the class under consideration.[1] Then m_1, m_2, m_3, &c., must represent the relative total numbers of first-born, second-born, third-born ... children in such a community if there be no differential death-rate. They are the steady inflow of offspring of each of these orders, and must represent, if the community be stable, the relative numbers existing of these orders. Now, in this community certain of these offspring belong to families which are completed, and certain to families which are still continuing to grow. Here is where the difficulty arises; we do not select our insane or tuberculous from the community at large, but as a rule from the *completed* families, and therefore we cannot compare, except as a limit, m_1, m_2, m_3, &c., with the observed numbers drawn from completed families. Let $n_1, n_2, n_3, \ldots n_s$ be the numbers of first-, second-, third- ... fifth-born in 1,000 growing families; then clearly

$$n_s = m_s + m_{s+1} + m_{s+2} + \ldots$$

and is known from the m's. Also it is obvious that $n_1 = 1{,}000$. Let $n_1', n_2', n_3', \ldots n_s'$ be corresponding numbers for the completed families. Now let M_1 be the number of growing and M_2 the number of completed families; then we have

$$\frac{\frac{M_1}{1000}n_1 + \frac{M_2}{1000}n_1'}{m_1} = \frac{\frac{M_1}{1000}n_2 + \frac{M_2}{1000}n_2'}{m_2} = \ldots = \frac{\frac{M_1}{1000}n_s + \frac{M_2}{1000}n_s'}{m_s}.$$

Accordingly: $$n_s' = \frac{m_s}{m_1}\frac{M_1 + M_2}{M_2}1000 - \frac{M_1}{M_2}n_s.$$

Thus, if we can find the ratio M_1/M_2, we shall be able to determine the series n_s'.

In the following table I give m_s for Denmark, the resulting n_s, v_s the value of the n_s series in permilles, and the resulting n_s' reduced

[1] If we take a long enough period to be uninfluenced by trade depressions, &c., m_1 will be $> m_2$, $m_2 > m_3$, and so on. Where a depression has caused few marriages in a year, there m_2 may be $> m_1$ for that year.

as v_s' to permilles for $M_1/M_2 = 0{\cdot}5$, $1{\cdot}0$, $1{\cdot}3$ and $1{\cdot}4$. These correspond to $n_s' = 6{\cdot}8080\ m_s - 0{\cdot}5\ n_s$; $n_s' = 9{\cdot}0773\ m_s - n_s$; $n_s' = 10{\cdot}4388\ m_s - 1{\cdot}3\ n_s$; and $n_s' = 10{\cdot}89275\ m_s - 1{\cdot}4\ n_s$ respectively.

TABLE XLI. THEORETICAL DEDUCTION OF COMPLETED DANISH FAMILIES.

Order of Birth.	m_s	n_s	v_s	v_s'			
				$M_1/M_2 = 0{\cdot}5$	$M_1/M_2 = 1{\cdot}0$	$M_1/M_2 = 1{\cdot}3$	$M_1/M_2 = 1{\cdot}4$
1	220	1000	267	203	188	180	177
2	191	780	208	185	180	177	176
3	159	588	157	160	160	161	161
4	117	429	115	117	118	118	118
5	93	313	83	97	100	102	102
6	64	220	59	66	68	68	69
7	49	156	42	52	54	55	56
8	37	107	28	41	44	45	46
9	26	69	18	28	31	32	32
10	20	44	12	24	26	28	28
11	12	23	6	14	16	17	17
12	7	11	3	8	9	10	10
13	3	5	1	4	4	5	5
14	1	2	} 1	} 1	1	1	2
15 and over	1	1			1	1	1
Mean Size of Family	[4·54]	—	3·75	4·93	5·33	5·56	5·65

It will be seen that the completed families in this district must give between 177 and 220 permille of firstborns according to the ratio taken for M_1/M_2.

I do not enter at length here into the problem of the best method of determining the ratio M_1/M_2. If we consider the 'stable population' under consideration to be that of families, i. e. father, mother, and offspring, then M_1/M_2 would be the ratio of wives in their fertile to wives past their fertile stage. Of course this could only be a rough measure, where fertility is not only as now much a question of choice, but also depends on age at commencement of the family. Still possibly some approach to the ratio M_1/M_2 may be obtained by considering the ratio of the number of married women under 45 to the number over 45 years of age. This varies considerably with the nature of the district, rural or urban, emigratory or immigratory. Thus we find:

	M_1/M_2
Liverpool City	2·40
Lanark (County)	2·22
London (County)	2·01
Renfrew	1·99
Bradford	1·81
Yorkshire, North Riding (with Middlesbrough)	1·79
Edinburgh (County)	1·79
Yorkshire, North Riding (without Middlesbrough)	1·59

	M_1/M_2
Aberdeen	1·55
Forfar	1·54
Suffolk, East	1·38
Perth	1·33
Westmoreland (County)	1·29
Yorkshire, North Riding, Rural Districts	1·26
Inverness	1·10
Ross and Cromarty	·98

It will be seen that the values come approximately what one would anticipate, but the whole matter requires and will receive much further study than is possible in this lecture. But in many districts the completed families go far lower than 180 permille of first-borns. Thus from the record of the number of children of wives of the poorer classes in Sheffield, 4,368 births, I deduce:

TABLE XLII. GROWING FAMILIES IN SHEFFIELD. PERMILLES OF BIRTH-ORDER.

	Permilles.																	*Mean Size of Family.*
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17 and over	
All Fertile Wives	213	185	150	120	94	71	53	39	28	18	11	7	5	2	2	1	1	4·69
Wives under 35	271	226	170	124	86	54	33	19	10	4	2				1			3·69
Wives 35 and over	121	120	118	113	107	98	85	70	57	40	27	17	12	6	4	3	2	8·26

It is striking to see how these results for incomplete families run up as the mother gets older, i. e. from 3·69 to 8·26 per family. Of course, no one would suppose 8·26 to be the mean size of completed families. In the working classes, even when there is no direct limitation of the family, many wives' fertility is exhausted before 35. But any one, who has studied at all the fertility of the poorer classes, knows how such values as those reached by Messrs. Yule and Greenwood, ranging from 195 to 286 permille of first-borns in completed families, in no way represent the true state of the case, which may be taken to be from a minimum of 140 to a maximum of 180 permille of first-borns.

I have lastly some data for the order of birth of the tuberculous most kindly sent to me by Dr. Fanning, formerly of the Kelling Sanatorium, Norwich. I am in some doubts whether they ought to be used for the present purpose, because they were not directly collected from inmates of the sanatorium, but by inquiries of patients, who had left the sanatorium for some years before the inquiry was made. Dr. Fanning had most kindly sent me the results of his annual investigation into the history of his former tuberculous patients, and he took the opportunity this year of asking some details as to size of family, order of birth, and affected brothers and sisters. It will be clear, as there is a heavy death-rate each year of the former sanatorium patients, that if this death-rate be in any way differential, then the full bias against the elder-born will not appear; the elder-born may not only be more liable to tuberculosis, but more liable, owing to general delicacy, to have less power of resistance to the disease when acquired. In this case many of the patients had left the sanatorium for a period considerably longer than the average duration of life after onset of the disease, and thus even a small differential death-rate would be of significant influence. Unfortunately we have not particulars as to the parental sibships, but we have a record of the occupations of the patients. Of the women the bulk are domestic servants; there are a few dressmakers, shop assistants, and shop clerks; the remainder are chiefly described as 'housewives', but there are a very few school teachers and factory hands. Of the men the majority are artisans; there are some farm workers, shop assistants, and clerks; there are street porters, tradesmen's messengers, and male domestic servants; the professional classes appear to be represented by one clergyman and one medical student. The labour does not appear to be of the highest skilled class, but such as we might expect to find in a country town with no very important manufactures. I should put the permille of first-borns for completed families at 165, about that for the Scottish industrial classes, although it is conceivable that many of these workers were born in the agricultural districts with a possible permille of 150 first-borns in their sibships (see Scottish data, p. 11). The 'Long Table' on p. 60 results.

It will be seen that while the 'Sibships of the Phthisical' are 7 to 22 below probable comparative series, the 'Yule-Greenwood Reconstruction' gives values 30 to 45 above. But until we know more of the birth-rate in the eastern counties of England with special reference to the workers in Norwich, it is impossible to fix the real number of first-borns at either 165 or 150. It must also be borne in mind that the differential death-rate, if it applied especially to the class of family commented on in the last paragraph of p. 4, would not only lower the permille of first-borns in column I, but column II might be produced from column III (see Table VII) without the counter-balancing influence of the bias towards smaller families (see Table IX), which in this case would be excluded.

TABLE XLIII. PERMILLES OF EACH BIRTH ORDER IN THE CASE OF THE TUBERCULOUS. KELLING SANATORIUM.[1]

Order of Birth.	*Phthisical Members.* I.	*Sibships of Phthisical (Pearson).* II.	*Scottish Industrial Class.* III.	*Scottish Agricultural Class.* IV.	*Yule-Greenwood Reconstruction.* V.
1	205	143	165	150	195
2	183	140	159	146	170
3	137	134	148	138	152
4	112	123	133	128	126
5	106	109	114	115	101
6	91	94	94	97	80
7	49	77	72	78	60
8	41	60	50	59	44
9	32	47	32	39	32
10	22	30	18	24	19
11	13	17	9	14	10
12	5	9	4	7	4
13	1	6	1	3	3
14	2	5	1	1	2
15	1	3		1	1
16		2			1
17 and over		1			
Mean Size of Family	—	6·98	6·05	6·64	5·13

I have printed these data because, although their method of collection seems to me unsuited to our present purpose, it still in my opinion shows evidence of the handicapping of the first- and second-born members in the case of tuberculosis.

(14) *Congenital Cataract.*

I am able to give results for fifty families with congenital cataract from pedigrees in my possession. The material is not very ample, but it is sufficient to suggest that this disease, which is markedly hereditary in character, has a bias against the two elder-born.

[1] This table was formed by the same method as that for Congenital Cataract : see p. 61.

Working with pedigrees we have in the majority of cases no record of the 'subject', and must thus include all affected members of a sibship. This would clearly, however, weight the big families. In order to avoid this, if a family, say of eight, had second-, fourth-, and fifth-born members affected, I counted that family as providing $\frac{1}{3}$ of a second-born, $\frac{1}{3}$ of a fourth-born, and $\frac{1}{3}$ of a fifth-born member. Thus the family was not more weighted than a family of three with one member affected. In a pedigree which covers four or five generations it is often impossible to say where the 'subject', 'patient', or first-observed member occurs. The history develops

TABLE XLIV. PERMILLES OF EACH BIRTH ORDER IN THE CASE OF CONGENITAL CATARACT.

Order of Birth.	*Cataractous Members.*	*Sibships of the Cataractous (Pearson).*	*Sibships of Parents of Cataractous.*	*Yule-Greenwood Reconstruction.*
1	308	182	184	266
2	238	168	171	185
3	103	140	158	144
4	122	130	136	124
5	64	116	111	103
6	52	89	66	71
7	40	65	57	47
8	40	38	44	23
9	11	24	29	13
10	12	21	22	11
11	–	10	10	5
12	5	10	6	5
13	5	4	3	2
14	0	3	3	1
Mean Size of Family	—	5·49	5·43	3·76

until innumerable cases have been added to the pedigree. Care was taken to select only practically completed sibships, as judged by the age of the youngest child. In these pedigrees we had also the parental sibships, but of course each parental sibship was only reckoned once. On the other hand, a parental sibship would be reckoned as an offspring sibship if it also contained affected members. The material was of rather wide range, stretching from the middle and professional classes right down to the agricultural labourer. *A priori* I should have anticipated about 170 permille of first-borns;

the actual number comes out somewhat greater than this. There is a very close agreement between the sibships of the affected and of their parents. The Yule-Greenwood reconstruction is quite impossible, and this is what we should anticipate; for, working with long pedigrees, there is really no sensible selection of large families, and further, congenital cataract is so markedly hereditary that the whole hypothesis of random marking is really inapplicable. The data appear to illustrate the principle that the less robust members of a tainted stock—and such are the elder-born—appear more likely to be affected.

(15) *Conclusions.*

In the following table I give a *résumé* of our chief results:

TABLE XLV. *RÉSUMÉ* OF THE PERMILLES OF ORDER OF BIRTH AND OF THE SIZE OF FAMILY, AS OBSERVED, AS FOUND BY SIBSHIPS OF AFFECTED, AND BY THE YULE-GREENWOOD RECONSTRUCTION, TOGETHER WITH THE MOST PROBABLE COMPARATIVE RESULTS.

	Permilles.			*Size of Family.*
	First-born.	*Second-born.*	*Third-born.*	
A. *Imbecile Stock* (Hunter). (Table XXIV.)				
Observed Affected	321	205	136	–
Sibships of Affected (Pearson)	181	163	151	5·53
Most Probable: Sibships of Parents	164	155	145	6·08
Yule-Greenwood Reconstruction	286	190	155	3·50
B. *Imbecile Stock* (Hansen). (Table XXV *bis.*)				
Observed Affected	234	159	149	–
Sibships of Affected (Pearson)	168	162	149	5·95
Most Probable: Danish Census	173	159	143	5·78
Yule-Greenwood Reconstruction	236	201	160	4·24
C. *Epileptic Stock* (Weeks). (Table XXVI *bis.*)				
Observed Affected	230	207	166	–
Sibships of Affected (Pearson)	159	151	142	6·29
Most Probable: Mother's Sibships	157	145	139	6·37
Yule-Greenwood Reconstruction	233	185	157	4·29

	Permilles.			Size of Family.
	First-born.	*Second-born.*	*Third-born.*	
D. *Insane Stock* (Urquhart). (Table XXVII.)				
Observed Affected	231	171	167	–
Sibships of Affected (Pearson)	168	159	150	5·97
Incomplete Families of Affected	193	182	153	5·18
Most Probable: Scottish Census	185	173	157	5·41
Yule-Greenwood Reconstruction	223	191	169	4·48
E. *Albinotic Stock* (Pearson). (Table XXVIII.)				
Observed Affected	253	185	151	–
Sibships of Affected (Pearson)	156	149	139	6·43
Most Probable: Sibships of Parents	148	146	137	6·75
Yule-Greenwood Reconstruction	227	186	152	4·40
F. *Criminal Stock* (Goring). (Table XXXI.)				
Observed Affected	269	235	137	–
Sibships of Affected (Pearson)	143	137	130	6·99
Most Probable: Scottish Labourers	148	143	136	6·76
Yule-Greenwood Reconstruction	216	176	150	4·63
G. *Tuberculous Stock* (Rivers). (Table XXXIII.)				
Observed Affected	297	207	108	–
Sibships of Affected (Pearson)	176	169	153	5·68
Most Probable: Skilled Artisans	167	160	149	5·99
Yule-Greenwood Reconstruction	234	204	166	4·27
H. *Tuberculous Stock* (Riffel). (Table XXXV.)				
Observed Affected	203	158	134	–
Sibships of Affected (Pearson)	164	157	142	6·10
Most Probable: Scottish Agricultural Class	150	146	138	6·64
Yule-Greenwood Reconstruction	239	198	154	4·18
I. *Tuberculous Stock* (Hansen, I). (Table XXXVII.)				
Observed Affected	281	202	161	–
Sibships of Affected (Pearson)	171	162	148	5·85
Most Probable: Danish Industrials	179	162	145	5·59
Yule-Greenwood Reconstruction	243	199	161	4·12

	Permilles.			*Size of Family.*
	First-born.	*Second-born.*	*Third-born.*	
J. *Tuberculous Stock* (Hansen, II). (Table XL.)				
Observed Affected	239	193	171	–
Sibships of Affected (Pearson)	159	155	145	6·29
Most Probable: Danish Census	173	159	143	5·78
Yule-Greenwood Reconstruction	217	195	162	4·60
K. *Tuberculous Stock* (Fanning). (Table XLIII.)				
Observed Affected	205	183	137	–
Sibships of Affected (Pearson)	143	140	134	6·98
Most Probable: Mean of Scottish Industrial and Agricultural	157	152	143	6·64
Yule-Greenwood Reconstruction	195	170	152	5·13
L. *Cataractous Stock* (from Pedigrees). (Table XLIV.)				
Observed Affected	308	238	103	–
Sibships of Affected (Pearson)	182	168	140	5·49
Most Probable: Parental Sibships	184	171	158	5·43
Yule-Greenwood Reconstruction	266	185	144	3·76

It will be seen from this table that:

(i) wherever we have reliable data for the size of family in the stocks from which the affected are drawn, e. g. among the imbeciles A, epileptics B, albinos E, the permille of first-borns and the corresponding fertility are respectively below and above even those provided by the sibships of the affected.

(ii) the Yule-Greenwood hypothesis gives permilles of first-born and corresponding fertilities of the order of those of incomplete families, and often respectively much above and below those values.

Messrs. Greenwood and Yule's theory would apply, as I have shown, to chance marking such as is found in Christian names with a capital H, or less completely in the chance that a given individual will marry deduced from completed family histories. It does not apply in the least to actual data of disease, or even when we circularize individuals. This is well shown in Table X, where the 'subjects' of a circular, which might be supposed to reach members of large families in proportion to their number, are shown to have a birth order in their sibships closely similar to that of their non-selected parents, and probably identical with the latter series, if we could be equally certain of the completeness of the parental records. Yule and Greenwood provide a hopelessly exaggerated result.

The reasons for the failure of the theory put forward by them lie in the facts (*a*) that they have overlooked the principle that all families are not equally liable to these markedly hereditary affections, and (*b*) that in the same relatively short interval of time all members of even the same family are not equally likely to be affected, or, if affected, to appear in the same sanatorium or prison record.

The value I adopted for comparative purposes,—the Sibships of the Affected—was only selected after some consideration of the problem, and by comparison of its results with those for material for allied social classes. The Weinberg, Yule, and Greenwood hypothesis—that of my original memoir on the heredity of fertility—was known to lead to impossible results in the case of disease.[1]

Messrs. Yule and Greenwood's statement as to the fertility of abnormal stocks, that 'it seems to us clear that the size of family is not, as has been stated, abnormally large' (*loc. cit.*, p. 196), must also fall to the ground if their permille of first-borns is obtained from an erroneous hypothesis. The one varies inversely as the other, and if the number of first-borns is markedly exaggerated, the size of family will be markedly depreciated. We can illustrate this at once by taking cases where we know the actual size of family in the stocks from which the affected came. Thus we have:

TABLE XLVI. SIZE OF FAMILY IN AFFECTED STOCKS.

Character.	*Actual Sibships of Affected.*	*Sibships of Parents.*	*Offspring of Affected.*	*Yule-Greenwood.*
Imbecility . . .	5·53	6·08 (both)	–	3·50
Epilepsy	6·29	6·37 (mother)	–	4·29
Insanity	5·97	–	5·18 (incomplete !)	4·48
Albinism	6·43	6·75 (both)	–	4·40
Chronic Alcoholism	–	–	6·05 (incomplete !) *	–
Criminality . . .	6·99	–	6·05 †	4·63

* Married women living with their husbands. Heron: *Eugenics Laboratory Memoirs*, xvii: 'Extreme Alcoholism in Adults', p. 79.

† Not necessarily complete. The difficulty about criminals in regard to offspring is their long confinement in prison and their desertion by their wives. To obtain this result I have taken first offenders whose marriages have lasted 25 years. This is a natural fertility not interrupted by constraint.

[1] About a year ago Mr. Greenwood told me he had doubts as to my treatment. I informed

The criticism of Messrs. Yule and Greenwood on this point is also invalid. Judged not by the sibships of the affected but by the non-selected sibships of their parents, or by those of their children, abnormal individuals do appear to come of very fertile stocks; and, as we have indicated before, if Messrs. Yule and Greenwood's values were correct instead of incorrect, then, for comparative purposes, we must reduce in the same way the values of the normal stocks with which they were compared, and this would have more reason, for they were obtained by a still more direct method of marking, in which the heredity of the stock played no part.

It must, I hold, be concluded that the criticisms raised against the handicapping of the first-born are not valid. The first-born is very significantly handicapped, and this statistical result will coincide with a good deal of personal and individual experience. The data collected in this paper can and will be extended, but they are, I think, sufficient to justify the general statement I have made,[1] that the small family is detrimental to race progress. That is the reason why I have approached this subject at all. After this lecture was delivered, I was asked by an anxious mother: 'Why, even if the doctrine be true, should it be published to the world, as it would only alarm and so further injure a class of the community already asserted to be handicapped?' My reply to that question is: 'Study in the first place the incidence rates of these abnormalities we are discussing, and you will see that it is only in mass-statistics that the handicapping becomes sensible.' Further, I must add that in the science of National Eugenics we have to consider what profits the nation at large, and I feel strongly convinced that the present tendency (exhibited so markedly in France), to make the first-born 50% instead of something less than 22% of the whole number of births, spells degeneracy. The individual feelings of the first-born, even if the handicapping were far more substantial than it is, cannot be con-

him that a paper on the subject had been long in hand and would be published. The present lecture is not that paper, but it has been needful to issue a part of it in a popular form in order to show that the charge of 'fallacious method' can be met. It is needless to point out that the discussion in my First Study of Tuberculosis was ancillary to other matter. The paper by Messrs. Yule and Greenwood does not seem actuated by desire to reach the truth on an important problem, but it aims rather at making petty points, a process which was relatively easy, as all this Laboratory had issued on the subject was contained in incidental references in lectures or memoirs on quite different topics.

[1] *The Scope and Importance to the State of the Science of National Eugenics* (Dulau & Co.), p. 43.

sidered to outweigh the national importance of the problem. If this principle of the handicapping of the first-born be true, as I have little doubt that it is—and if a similar principle holds for the last-born (to a lesser degree it is true) for some conditions like Mongolian imbecility—what must be the moral of the present lecture? Surely that the better born are the intermediates in families from 5 to 8, and that when families are restricted to twos or threes, or extended to twelves and thirteens, there may be a quite appreciable tendency to increase the proportion of the less efficient in the community. I make no pretence at present to associate inferiority at beginning or end with too young parents or too old parents. I am only too aware that we want much fuller data, so that we can correct for parental ages at marriage, and for period after marriage of the birth of each child. We want to study not only the order and number of children, but the interval between their births.

The handicapping of the first-born is not, as some of my correspondents have supposed, subversive of any faith in heredity. It would not even be an argument against an hereditary Upper Chamber, except in as far as such a chamber is based on primogeniture. Statistics of the failure of the eldest-born of peers and of the success of their younger brothers might from this standpoint be of much interest.[1] The real argument against an hereditary chamber is the customary want of hereditary power in its members, i.e. the neglect of the fact that a man has sixteen great-grandparents, and, possibly, only one of them may be of distinction—the man who won the title. As Galton wrote: 'An old peerage is a valueless title to natural gifts, except so far as it may have been furbished up by a succession of wise intermarriages. . . . I cannot think of any claim to respect, put forward in modern days, that is so entirely an imposture, as that made by a peer on the ground of descent, who has neither been nobly educated, nor has any eminent kinsman within three degrees.'[2] The dearth of ability in the 'hereditary' peers of the present day is largely due to their neglecting marriage into able stocks, and in some

[1] I do not see any error in Galton's method of approaching the problem (*Hereditary Genius*, 1869, p. 87), beyond the paucity of his material. There are always more eldest-born than second-born and more second- than third-born, and hence, if the number of judges first-born is significantly fewer than those second-born, it is evidence of the greater judicial power of the later-born. I do not grasp Søren Hansen's somewhat contemptuous dismissal of Galton's method with a reference to the fairy tales of the younger sons who were the pioneers.

[2] *Hereditary Genius*, 1869, p. 87.

cases quite possibly to a succession of eldest-son inheritance,—an evil which the whole community may bring upon itself, if it selects its surviving offspring in the same restricted manner. To criticize primogeniture is not to discard heredity.