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FECUNDITY

FERTILITY, STERILITY

AND ALLIED TOPICS

BY

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etc. etc. etc.

SECOND EDITION, REVISED AND ENLARGED.

EDINBURGH
ADAM AND CHARLES BLACK
1871

Printed by R. CLARK, *Edinburgh.*

DEDICATED

TO THE

OBSTETRICAL SOCIETY OF EDINBURGH

BY

ONE OF ITS MEMBERS

PREFACE TO SECOND EDITION.

THE Author publishes this second edition because of the demand which, his publishers tell him, exists for it. He would have wished to reconsider the whole subject, and to have made this edition really a new book, but fresh data, and time also, were awanting. He has therefore adhered closely to the letter of the work as it first appeared. But he has made some slight alterations and numerous additions, especially in footnotes. Professor Tait has revised his chapters of the work, and to them is subjoined a criticism of the first edition. The value and interest of this article seem to justify the unusual step of republication in the middle of a new edition of the work reviewed.

The author is grateful for the very favourable reception of the first edition of the work, and has tried to make it, in this its new form, even more worthy of perusal.

His friend, Dr. Hardie, has given him much assistance in passing the sheets through the press.

30 CHARLOTTE SQUARE, EDINBURGH,
September 1871.

PREFACE TO FIRST EDITION.

In publishing this volume I have to apologise for its many imperfections, arising from its being rather hurriedly thrown together than carefully composed. For such imperfections I can claim the excuse of a busy professional man—that it has been entirely written in scattered *horæ subsecivæ*. Of its positive faults, unknown to me, but probably existing, I must bear the full blame; only, in anticipation, I express my regret that they should find their way into my work.

I have pleasure in calling special attention to Part VI. of the volume, the valuable contribution of Professor Tait, made in the most generous manner, and at much inconvenience to himself.

30 Charlotte Square, Edinburgh,
1866.

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PART I.

ON THE VARIATIONS OF THE FECUNDITY AND FERTILITY OF WOMEN ACCORDING TO AGE.

It has from the earliest times been a matter of philosophical inquiry how much influence the age of women, at the time of commencing to live in wedlock, exerts upon their fertility, and opposite opinions on this point have been embraced by authors of note. The various questions connected with this topic have always been unsatisfactorily treated, on account of the insufficiency and inaccuracy of the data used to settle them, when facts and not mere impressions were the foundations of argument. In the following pages I have attempted to introduce some degree of exactness into the subject, and have used a considerable mass of figures as the almost exclusive basis of my conclusions.

In 1855, when the systematic registration of births in Scotland was established, the schedule in use exacted from the public a variety of interesting details in connection with each return—a circumstance which gives to the registers for that year an extraordinary value. For, in consequence, I believe, of numerous complaints regarding the irksome labour of filling up the document, it was discontinued, and a much less compre-

hensive schedule has been in use ever since. It is from the registers of births for 1855 that I have extracted almost all the data which have yielded the results I am now about to communicate. Similar data cannot be found in the subsequent registers, nor, so far as I know, in any other registers whatsoever.

The exigencies of time, labour, and expense, constrained me to restrict the number of births to be operated on, within moderate limits; and I selected Edinburgh and Glasgow, with their 16,593 children legitimately born in 1855, as the field of operations. It is needless to enter fully upon the reasons of my selecting the conditions of legitimate birth in Edinburgh and Glasgow; my only object was to secure as much as possible of accuracy and completeness in the filling up of the schedules—an object quite incompatible with the inclusion of the data derived from mothers of illegitimate children. It must be noted, that legitimate births, as registered, include only births of living children at the full term of pregnancy, or near it.

The well-known difficulty of handling statistics without infringement of the rules of logic has made me very cautious in my progress in this investigation, and I am all the more bound to be careful, because it will be necessary, in connection with my present topic, to point out great errors made by authors who have entered upon it. But, although I trust no fault will be found with my mode of reasoning, I have to admit the existence of some comparatively few and unimportant errors in the details given in the registers. The

chief of these will be stated in connection with the tables to be brought forward. So far as I know, the errors are all in the original registers; in the elaboration of the details thence derived I have spared nothing that could insure accuracy; and must here mention my obligations to the various intelligent and assiduous gentlemen who have assisted me in the work, especially the late Dr. Craig, Drs. Anderson and Linton, and Messrs. Brown and Slater.

The first part of my investigations is confined to the determination of the comparative fertility or productiveness and fecundity of women at different ages. It is necessary, in order to avoid confusion, here to establish some amount of distinction, which I shall maintain as I go on, between fertility or productiveness and fecundity. By fertility or productiveness I mean the amount of births as distinguished from the capability to bear. This quality of fertility or productiveness is interesting chiefly to the statistician or the political economist. When a population is the subject of consideration, it does not even involve the capability of every individual considered to bear, nor even the conditions necessary for conception. By fecundity I mean the demonstrated capability to bear children; it implies the conditions necessary for conception in the women of whom its variations are predicated. This quality of fecundity is interesting chiefly to the physiologist and physician. In short, fertility implies fecundity, and also introduces the idea of number of progeny; while fecundity simply indi-

cates the quality without any superadded notion of quantity.

In discussing the subject of comparative fertility and fecundity at different ages, I may incidentally afford means for estimating the degree of fertility or fecundity of different ages; but I wish it to be distinctly understood that I have not proposed to myself, in this part, the consideration of the actual degree or amount of fertility or fecundity at any age, but chiefly the variations of fertility or fecundity at different ages as compared with one another. The fertilities of mothers of different ages I shall take up in a subsequent part.

CHAPTER I.

THE ACTUAL FERTILITY OF THE FEMALE POPULATION AS A WHOLE AT DIFFERENT AGES.

THE first law which I propose to establish has reference to the ages of the mothers of legitimate children. In Edinburgh and Glasgow, legitimate births form at least 90 per cent of the whole born. The law therefore regards the ages of the women from whose fertility 90 per cent of the population are recruited.

It must be observed that this law or general statement shows nothing regarding the fecundity of women of different ages, although it has been held as doing so; it merely enunciates a truth in the doctrine of population. I place it first because it is pretty well known, because in my own investigations it was first made out, and chiefly because it is essential, before proceeding farther, to show the facts on which it is founded in their true light, avoiding the great errors of which similar facts have been made the basis.*

The facts or data illustrating this law, with which I am best acquainted, have been derived from reports of lying-in dispensaries, as by Dr. Granville, or from

* See Granville, *Transactions of Obstetrical Society of London*, vol. ii.

similar accounts of lying-in hospitals, as by Dr. Collins, Drs. Hardy and M'Clintock, and others. I here present, as an example, the table of ages of mothers of legitimate and illegitimate offspring, whether born alive or dead, from the *Practical Treatise on Midwifery* of Dr. Collins, master of the Dublin Lying-in Hospital. The data adduced by Dr. Granville in the second volume of the *Transactions of the Obstetrical Society of London* are closely similar. Judging from these data, it would appear that most children are born of women at or near the age of thirty years, or the middle of the child-bearing period of life; and that the offspring of mothers of ages advancing from the commencement of child-bearing to the age of thirty, or the middle of the child-bearing period, gradually increases; that the climax is reached at this age, and that thereafter the offspring of mothers advancing above thirty gradually diminishes. But while the age of thirty forms the climax, there is not an equal fertility on either side of it, a much larger part of the population being born of mothers under thirty than of mothers above thirty. Dividing the number of mothers at thirty years, and adding together those on each side of the division, we have on the side of the younger 12,106, and on the side of the elder women 4279, giving a majority of 7827 in favour of the younger; or, otherwise stated, we have three-fourths of the births among the younger half, and only one-fourth among the elder. The mean age of the mothers in Dr. Collins' table is twenty-seven years.

TABLE I.—SHOWING THE AGE OF EACH OF 16,385 WOMEN DELIVERED IN THE DUBLIN LYING-IN HOSPITAL.

Age.....	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32
No. of Women	3	19	70	237	433	926	682	1142	1023	1089	1174	1295	983	1340	517	2346	242	467

Age.....	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	50	53
No. of Women	378	384	396	379	153	217	65	326	15	21	18	17	11	5	6	5	1

TABLE II.—SHOWING THE AGES OF 16,385 WOMEN DELIVERED IN THE DUBLIN LYING-IN HOSPITAL, ARRANGED IN PERIODS OF FIVE YEARS.

Age	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50 and over.
No. of Women	762	4862	5309	3817	1210	397	22	6
Percentage.....	4·65	29·67	32·40	23·29	7·38	2·42	·13	·03

The next table which I present shows an arranged collection of data, comprising the wives-mothers of living children born at or near the full time in Edinburgh and Glasgow in 1855. The former table has, regarding the use to be made of it, the advantage over this table, of including all mothers bearing children, whether legitimate or not, alive or dead, in the Dublin Hospital. But in every other respect this second table presents what I judge to be more reliable data. The former table contains a class of cases selected according to complicated conditions which it is impossible to state, but which are the result of the correlated circumstances of the Hospital, and of the class from which in Dublin it draws its patients. In the second table, the conditions of selection are fewer and less important, the chief being the legitimacy, life and maturity, or at least viability, of the offspring. Now the limits of the influence of these different conditions are pretty well known, and the proportional differences between the two tables are too great to be accounted for by these differences. The second table is thus shown to be the more trustworthy.

TABLE III.—Showing the Age of each of 16,301 Wives whose Children were registered in Edinburgh and Glasgow in 1855.

Ages .	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
Mothers	4	28	116	228	405	543	828	888	1024	1058	1063	925	1116	875	1214	545	825	645	621

Ages .	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	57	Total
Mothers	691	594	409	426	287	404	142	148	80	66	50	27	6	9	4	–	2	4	1	16,301

TABLE IV.—Showing the Ages of 16,301 Wives-Mothers in Edinburgh and Glasgow in 1855, arranged in Periods of Five Years.

Ages	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	Total
Mothers . . .	376	3688	5037	3850	2407	840	96	6	1	16,301
Percentage .	2·30	22·62	30·89	23·61	14·76	5·15	·58	·03	—	

An inspection of this table shows again that the year of maternal life yielding most recruits to the general population is the thirtieth, and an easy calculation makes out that about three-fifths of the legitimately-born population are derived from women of thirty years and under, while two-fifths are derived from women of thirty years and upwards. For, dividing mothers of thirty years of age, and adding together those on each side of the point of division, we have on the side of the younger 9708 mothers, and on the side of the elder 6593, giving a majority of 3115 in favour of the younger. The mean age of the wives-mothers in this table is above twenty-nine.

From these data I conclude :—

1. That the actual, not the relative, fertility of our female population, as a whole, at different ages, increases from the commencement of the child-bearing period of life, until the age of thirty is reached, and then declines to its extinction with the child-bearing faculty.

2. That the actual fertility is much greater before the climax, thirty years, is reached, than after it is passed.

3. That at least three-fifths of the population are recruited from women not exceeding thirty years of age.

Before leaving these tables, it is expedient to direct attention to a striking lowness of figure at the ages of twenty-nine and thirty-one respectively in Dr. Collins' data. A similar fall on each side of the highest number

occurs in Dr. Granville's table, which has been referred to, and in every similar table which I know. This curiosity has given rise to very natural and ingenious speculation. Dr. Granville suggests that by the earlier decrement nature means to rest awhile, and gather strength for the enormous jump she is to make in the following year, and that by the second decrement she means to evince the exhaustion which invariably follows over-exertion! But I cannot acquiesce in this fanciful hypothesis, believing that really no such decrement, jump, and second decrement, occur. My explanation of this tabular phenomenon is suggested by the occurrence of similar falls on each side of the age of forty years in Collins' table, and in my own and in others. I am too well aware, from ample experience, of the impossibility of getting women's ages stated correctly, especially if they have passed twenty-five years, and have often observed, that when pushed they say thirty or forty, as a round easy number; and the state of the tables appears to me merely to indicate that women about thirty-one and forty-one years of age frequently say they are thirty and forty years old, respectively. In short, these decrements are evidence of the unfortunate element of error which creeps into the most carefully-prepared vital statistics on a large scale.

CHAPTER II.

THE COMPARATIVE FERTILITY OF THE FEMALE POPULATION AS A WHOLE AT DIFFERENT AGES.

HAVING shown the actual fertility of women at different ages in our population, I now proceed to the question of the comparative productiveness of our whole female population at different ages. To settle this, it is only necessary to compare the number of children born of women of different ages with the number of women living at different ages respectively. The result of the calculations involved in this comparison will be to show, not simply (as in Chapter I.) the numbers of children born of women at different ages, but the number of mothers relatively to the number of women living at different ages; in other words, the comparative fertility or productiveness of our female population as a whole at different ages.

TABLE V.—Showing the Comparative Fertility at Different Ages of the Whole Female Population of Edinburgh and Glasgow.

Ages	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Women	31,538	34,631	29,778	24,272	19,362	17,938	13,868
Children	380	3,731	5,096	3,895	2,435	849	97
Proportion of latter to former is 1 in	82·9	9·2	5·8	6·2	7·9	21·1	142·9
Or percentage	1·20	10·77	17·11	16·04	12·57	4·73	·69

Table V. is constructed so as to bring out the desired results, and at the same time be easily compared with Table VI. It must be observed that the fifth table does not exhibit results whose actual amounts are of much value, but results the value of which is very great with a view to determining the question of comparative amounts or productiveness. For, while the numbers of women at different ages include the whole women living at these ages in Edinburgh and Glasgow in 1861, the numbers of children born at different ages, as given in the table, include only children born under the conditions of legitimacy, live birth, and complete or nearly complete maturity, in 1855.

In his work on Annuities,* etc., Mr. Milne in 1815 published a valuable table, which he describes as "showing the fecundity of women at the different periods of life in Sweden and Finland, from 1780 to 1795, both years inclusive." It is taken from a paper by Mr. Nicander, to which he gives a reference, but unfortunately I have not been able to ascertain the exact conditions (if any) under which the table was prepared.

* Vol. ii. p. 582. Large extracts from the Swedish returns, with remarks, are to be found in the English Registrar-General's Sixth Annual Report, 1844, p. 267, *et seq.*

TABLE VI.—Showing the Comparative Fertility at Different Ages of the Female Population of Sweden and Finland. (*From the Table of* Mr. Nicander.)

Ages	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Females	132,765	131,377	121,650	112,250	98,710	89,259	74,002
Deliveries	3,298	16,507	26,329	25,618	18,093	8,518	1,694
Proportion of latter to former, 1 in	40·256	7·959	4·620	4·382	5·456	10·479	43·686
Or percentage . . .	2·48	12·56	21·64	22·82	18·32	9·54	2·28

In the last line of both Tables V. and VI. it will be remarked that the first and last proportional numbers are very low, or that at the beginning of the scale, at the age of from fifteen to nineteen inclusive, fertility is comparatively very small; and that at the end of the scale, at the age of from forty-five to forty-nine inclusive, fertility is again comparatively very small. This no doubt depends to a great degree on the circumstance, that among the women from fifteen to nineteen years of age are included a large proportion of immature girls, and among the women from forty-five to forty-nine years of age a large proportion of women whose child-bearing powers have disappeared. Keeping in view this undoubted partial explanation of the lowness of the figure, or the lowness of fertility at these ages, the tables are seen to yield interesting results. They show that the fertility of the populations of Edinburgh and Glasgow, and of Sweden and Finland, increases gradually till the middle of the child-bearing period of life, or about the age of thirty years, and that then fertility gradually falls off towards its complete extinction.

My knowledge of the conditions under which my own table was framed, as already stated, being exact, as compared with my knowledge of Mr. Nicander's, I shall, in framing conclusions, adopt the results it affords. On like grounds I shall excuse myself from proceeding to compare the easily-remarked differences of the results of the two tables.

In regard, then, to the comparative fertility of our

whole female population at different ages, I conclude—

1. That it increases gradually from the commencement of the child-bearing period of life until about the age of thirty years is reached, and that then it still more gradually declines.

2. That it is greater in the decade of years following the climax of about thirty years of age than in the decade of years preceding the climax.

CHAPTER III.

THE COMPARATIVE FECUNDITY OF THE WHOLE WIVES IN OUR POPULATION AT DIFFERENT AGES.

I NOW proceed to the question of the fecundity, not fertility or productiveness, of the mass of wives of different ages in Edinburgh and Glasgow. In the two preceding chapters the fecundity or comparative power of production at different ages has not been entered on; in them have been considered merely the actual production of children by women of different ages, and the comparative amounts of production by the female population at different ages. It is known that at all ages there is a great mass of spinsters whose productiveness is not tested, and it is of course necessary, in order to determine questions of fecundity, to eliminate all women not living in married life, or not having their fecundity tested in the ordinary way, from our observations and calculations. In this chapter, therefore, the comparison is not of mothers with women living, as in Chapter II., but of wives-mothers with wives.

TABLE VII.*—Showing the Comparative Fecundity at Different Ages of the Whole Wives in Edinburgh and Glasgow in 1855.

Ages	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Wives	756	8,874	14,622	14,579	11,871	10,506	7,537
Wives-Mothers . . .	378	3,709	5,065	3,872	2,421	845	96
Proportion of latter to former is 1 in	2·0	2·4	2·9	3·7	4·9	12·4	78·5
Or percentage . . .	50·00	41·79	34·64	26·56	20·39	8·04	1·27

* In this Table the actual numbers are given as nearly as possible.

The numbers of wives have been arrived at in the following way :—We have the population of Edinburgh and Glasgow in 1851 and 1861, and by calculation estimate the population in 1855. We have the actual numbers of wives of different ages in 1861, and by an easy calculation of proportions we reduce the numbers of wives of different ages to the numbers given for 1855.

The number of wives-mothers extracted from the registers of 1855 is 16,301, bearing 16,500 legitimate children. But the Registrar-General's reports state the number of children as 16,593. Hence it appears that 93 births are omitted in the extracts. These omissions were made on account of manifest carelessness and inaccuracy in the registers. To these 93 births corresponds the number of 92 mothers, one being deducted for a twin case. These 92 mothers have been added proportionally to the others, in order to make up the total of 16,393.

The seventh table establishes a comparison between the numbers of married women of various ages, and the numbers of such women bearing living children. In Edinburgh and Glasgow, the number of wives within the ages of fifteen and forty-nine inclusive, or who might have born children in 1855, was 68,745, and the number of wives-mothers in the same population, in the same year, was 16,386,* or 1 in 4·2. In the tables these are arranged in columns of different ages, so as to exhibit the comparative fecundity of the whole wives of different ages. It will be seen at a glance that the table shows that the fecundity of the mass of wives is greatest in the first years of the child-bearing period of life; and I regret extremely that the data at my disposal do not permit me to condescend on the circumstances in this respect of each individual year. The table shows that, from the earliest years of child-bearing life onwards, the fecundity of the mass of married women gradually wanes to its extinction. It is also easily made out that while there were 24,252 wives under thirty years of age, and of these 9152 bore children, there were 44,493 wives of ages varying from thirty to forty-nine years inclusive, and of these only 7234 bore children; or, to speak in round numbers,

* The actual number of wives-mothers in Edinburgh and Glasgow in 1855 was 16,393. This figure is in the text reduced to 16,386, and seven wives-mothers omitted, because these seven were altogether exceptional, occurring as they did between the ages of fifty and fifty-seven, and could only damage the statement of results.

the wives under thirty years of age were much more than twice as fecund as the wives above thirty years. But a more interesting and valuable comparison may be made by taking the same number of fifteen years before and after the middle of child-bearing life, a total period of thirty years, which includes the immense majority of child-bearing women. Doing so, we find that of 24,252 wives under thirty years of age, 9152 bore living children, and that of 36,956 wives of ages from thirty to forty-four inclusive, 7138 bore living children. Had the elder women been as prolific as the younger, they would have produced 13,946 children, instead of 7138; that is, the fecundity of the younger women was almost double that of the older.

The data at my disposal enable me to give the figures for each year of age up to twenty. But the numbers are so small that little value can be placed on the results drawn from them. So far as they go, they indicate great fecundity of a mass of wives at seventeen, eighteen, and nineteen.

TABLE VIII.—Showing the Comparative Fecundity of Wives at Ages of 16, 17, 18, 19, and 20, in Edinburgh and Glasgow in 1855.

Ages	16	17	18	19	20
Wives	13	55	232	455	1043
Wives-Mothers . .	4	28	116	228	405
Proportion of latter to former is 1 in .	3·25	1·96	2·00	1·99	2·57
Or percentage . .	30·77	50·91	50·00	50·11	38·83

Table VII. has been prepared so as to give the actual amounts. I found it possible to do this with a near approach to exactness, and it is evident that in this way the results derived are not only comparative statements, with only relative value, but also statements of actual values.

From the data now given I conclude—

1. That the fecundity of the mass of wives in our population is greatest at the commencement of the child-bearing period of life, and after that period gradually diminishes.

2. That the fecundity of the whole wives in our population included within the child-bearing period of life is, before thirty years of age is reached, more than twice as great as it is after that period.

3. That the fecundity of the wives in our population declines with great rapidity after the age of forty is reached.

Some of these conclusions may be stated, with the actual numerical results, as follows :—While of all the wives living in Edinburgh and Glasgow between the ages of fifteen and forty-five, one in 3·8, or 26·3 per cent, bore a living child ; of those between the ages of fifteen and twenty-nine inclusive, one in 2·6, or 38·4 per cent, bore a living child ; and of those between the ages of thirty and forty-four inclusive, one in 5·1, or 19·6 per cent, bore a living child.

It will subsequently be shown that these conclusions regarding a mass of wives are not true, if applied to the individuals forming that mass. A different law governs individuals. Their fecundity is greatest from twenty to twenty-five ; that is, a woman marrying at that age is more likely to demonstrate her fecundity than if she married at any other age. But it will naturally be rejoined, if such is true of individuals, why not of masses ? In the sequel, the explanation will be given : it is founded on the law of intensity of fertility, and may, for the present purpose, be shortly stated thus :—The comparatively greater intensity of fertility of the fertile wives married from fifteen to twenty, over that of those married at from twenty to twenty-five, does more than make up for the sterility of some of the younger. Though less fecund, they are more fertile as a mass.

CHAPTER IV.

THE INITIAL FECUNDITY OF WOMEN AT DIFFERENT AGES.

In commencing the statistical inquiry whose results I am now giving, my object was to discover the fecundity of women at different ages, and I now proceed to address myself to this point.

It is not my object to illustrate the subject of the arrival of young girls at the age of maturity, the change of the girl into the fertile woman. In the case of some peoples, facts might be collected regarding wives so young as to be in a large proportion sterile from immaturity; and their fecundity gradually appearing as age advanced, might produce a column of mothers from ten to twenty years of age, showing a gradually-increasing fecundity of the population at these ages. Even in our tables, derived from the data of wives in Edinburgh and Glasgow, some interesting results are to be found, and allowance must be made for a certain amount of immaturity in the wives of from fifteen to twenty years of age. But this question of the arrival of girls at maturity is foreign to the present topic. In it, all the women are supposed to be mature, and subjected to the conditions essential for procreation.

The fecundity of individual women is known to vary extremely. Some are very frequently pregnant, and repeatedly, or even constantly, have plural births, and thrive with it all. Under like conditions other women are absolutely sterile, or a miscarriage or a dead mature child forms the climax of their fecundity; and this little may be effected at the expense of permanent constitutional exhaustion. Between these extremes of great fecundity and absolute sterility there is an unlimited series of varying degrees of fertility. On this interesting aspect of the subject of fecundity the present research throws little light. It is founded on the result of an aggregate of cases, and can show almost nothing as to individuals. It illustrates the fecundity at different ages of women generally, not the individual fecundity of any.

The table given in last chapter (Table VII.) affords data which cannot be applied to settle the question of the fecundity of women of different ages. For it is evident that among the mass of wives of each succeeding year, or series of years, are included the wives who were once of the former series, or part of them—that is, a class of wives whose fecundity has been at least liable to be increased, diminished, or exhausted, by procreation, before they have come to form part of the wives in any of the columns after the first. In order to arrive at the fecundity of women or wives at different ages, it is necessary to secure that the conditions of the compared women of these different ages be as nearly the same as possible. This is not attempted in the seventh table.

TABLE IX.*—Showing the Initial Fecundity of Women of Different Ages in the First Year of Marriage.

Ages of Wives newly married . . .	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	Total.
No. of Wives newly married . . .	700	1835	1120	402	205	110	46	20	6	2	1	4447
No. of Wives-Mothers within first year of Marriage . . .	96	339	139	46	19	4	...	...	...	...	...	643
Proportion of latter to former is 1 in .	7·3	5·4	8·0	8·7	10·7	27·5	...	...	...	...	...	6·9
Or percentage . . .	13·71	18·48	12·41	11·44	9·27	3·63	...	...	...	...	...	14·46

* The number of wives married at different ages in Edinburgh and Glasgow in 1855 is arrived at in the following manner :—The marriages in Scotland in 1855 were 19,680. The marriages in Edinburgh and Glasgow in 1855 were 4447. The distribution of the 19,680, according to age at marriage, is given at p. 22 of the Registrar-General's Annual Report for 1861. This distribution requires a correction for the number whose ages at marriage were not known. Calculating on the ages of the whole 19,680, the proportional distribution of the 4447 married in Edinburgh and Glasgow is found to be in the table above.

Table IX. is constructed to show the relative initial fecundity of newly-married women of different ages. By the returns of the Registrar-General we calculate how many women at each succeeding year of age contracted marriage in 1855, in Edinburgh and Glasgow. My extracts from the register for 1855 show how many of these women bore living children before they had been a year married. When the two figures are compared for each age, we have the fecundity at the outset of child-bearing at each age. The table reads as follows :—Of 700 women married between fifteen and nineteen years of age inclusive, 96 bore a living child before they had been wives for twelve months, or one in every 7·3 ; and so on.

Table X. is in every respect the same as the former, only it shows the fecundity within twenty-four months of married life ; or the number of women bearing living children in 1855, and before they were two years married, is compared with the number of newly married. The observation that the fecundity within twenty-four months is much more than twice as much as the fecundity within twelve months after marriage, appears to me to give this table more substantial value than the former, as an indication of the actual fecundity of the outset of child-bearing at different ages.

Both these tables show the highest rate of initial fecundity to be between the ages of twenty and twenty-four inclusive, and a gradual declension from that time on either side as age diminishes or increases.

TABLE X.—Showing the Initial Fecundity of Women of Different Ages within the First Two Years of Marriage.

Ages of Wives newly Married . . .	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	Total.
No. of Wives newly Married	700	1835	1120	402	205	110	46	20	6	2	1	4447
No. of Wives-Mothers bearing in 1855, and within two years of marriage	306	1661	849	253	84	17	2	...	...	...	...	3172
Proportion of latter to former is 1 in . .	2·3	1·1	1·3	1·5	2·4	6·4	23·0	...	...	...	...	1·4
Or percentage . .	43·71	90·51	75·80	62·93	40·97	15·45	4·35	...	...	...	...	71·33

The two following tables (XI. and XII.) show that on the side of the women younger than twenty years initial fecundity steadily decreases with age. In regard, however, to these young wives, it may be objected that there is a source of error from immaturity, which is certainly very trifling after the age of twenty is reached. And the objection is, theoretically at least, quite just, for it is absurd to attempt to measure the fecundity of women who have not become sexually mature, and the admixture of immature with mature is a source of error, important, directly according to its amount. It is unsatisfactory merely to allege in answer, that immature girls are not likely to be found among young wives in such numbers as to form a source of great error. I have therefore taken the following means to insure that this source of error be completely excluded.

TABLE XI.—SHOWING THE INITIAL FECUNDITY OF WOMEN UNDER TWENTY YEARS OF AGE IN THE FIRST YEAR OF MARRIAGE.

Ages of Wives newly married	16	17	18	19
No. of Wives newly married	43	108	225	314
No. of Wives-Mothers within first year of Marriage	2	7	31	56
Proportion of latter to former is 1 in	21·5	15·4	7·2	5·6
Proportion after correction for Immaturity is 1 in	15·5	12·8	7·0	5·4
Or percentage	6·45	7·77	14·70	18·30

TABLE XII.—SHOWING THE INITIAL FECUNDITY OF WOMEN UNDER TWENTY YEARS OF AGE WITHIN THE FIRST TWO YEARS OF MARRIAGE.

Ages of Wives newly married	16	17	18	19
No. of Wives newly married	43	108	225	314
No. of Wives-Mothers within two years of Marriage	4	27	98	177
Proportion of latter to former is 1 in	10·7	4·0	2·3	1·8
Proportion after correction for Immaturity is 1 in	7·7	3·3	2·1	1·7
Or percentage	12·90	30·00	46·44	57·84

The commencement of menstruation is generally considered by physicians an indication of the arrival of sexual maturity. It may be true that some are still immature in whom this phenomenon has shown itself, and it certainly is true that some are mature before its appearance. Yet it forms a generally accredited indication of maturity.* The following table (XIII.), framed by Dr. WHITEHEAD, is a large collection of data, showing the age of the appearance of menstruation in 4000 individuals in this country. It is easy to calculate what fraction of the whole 4000 had begun to menstruate at sixteen, seventeen, eighteen, and nineteen years of age respectively; or, in other words, what fraction was believed capable of exhibiting fecundity at these ages. This I have done, and have corrected the numbers of wives in tables eleventh and twelfth accordingly, reducing them to similar fractional parts. After making this correction for immaturity, I have calculated the proportions of wives-mothers to wives, and placed the results in the last line. They remain the same so far as to show a steady diminution of fecundity as age diminishes.

TABLE XIII.—"SHOWING THE AGE AT WHICH PUBERTY WAS ACCOMPLISHED IN FOUR THOUSAND INDIVIDUALS."—(WHITEHEAD *on Sterility and Abortion*, p. 46.)†

At Age of	10	years	9	first menstruated.
„	11	„	26	„
„	12	„	136	„

* See the Discussion on the Age of Nubility. Here, maturity is meant merely to imply ability to bear children, not fitness for married life.

† See also Routh in *London Journal of Medicine*, 1850, vol. ii. p. 244.

At age of	13	years	332	first menstruated.
„	14	„	638	„
„	15	„	761	„
„	16	„	967	„
„	17	„	499	„
„	18	„	393	„
„	19	„	148	„
„	20	„	71	„
„	21	„	9	„
„	22	„	6	„
„	23	„	2	„
„	24	„	1	„
„	25	„	1	„
„	26	„	1	„

From these data I conclude—

1. That the initial fecundity of women gradually waxes to a climax, and then gradually wanes.

2. That initial fecundity is very high from twenty to thirty-four years of age.

3. That the climax of initial fecundity is probably about the age of twenty-five years.*

* See "Note" at the conclusion of this part of the work, p. 43. See also pp. 71, 101, and the chapters on Sterility.

CHAPTER V.

THE FECUNDITY OF WOMEN AT DIFFERENT AGES.

It is plain that many women may prove fecund after two years of married life have passed; and all such women are lost sight of, if we proceed to inquire only in the way adopted in the last chapter, on initial fecundity. The women excluded, by the adoption of the way just named, may be enough to make the laws of initial fecundity deceitful and misleading, if any argument as to fecundity generally be drawn from them.

To make a general table, including all married women, I compare the calculated number of marriages at various ages in Edinburgh and Glasgow in 1855, with the number of first children born in that year of women married at various ages. Although the primiparæ of 1855 will not all be women married in that year, it may be assumed that, if the marriages be nearly alike for some years, the numeration of the primiparæ of one year will give pretty accurately the fecundity of the married women of any year. This process is carried out in the following table :—

TABLE XIV.—Showing the Fecundity of Women married at Different Ages.

Ages of Wives at Marriage .	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50, etc.	Total.
No. of Wives . . .	700	1835	1120	402	205	110	46	29	4447
First Children . . .	649	1905	809	251	96	10	2	...	3722
Percentage Fertile . .	92·7	100	72·3	62·5	46·8	9·1	4·4	...	83·7
Or 1 in	1·08	1·00	1·38	1·60	2·13	11·0	23·0	...	1·19

This table is valuable, in a particular way, confirming the results arrived at in the last chapter, by like results obtained by a different method of analysing the same data. Its results have reference to the average individual woman, not to a mass of women. The conclusions to be derived from it are easily observed.

1. That nearly all women married at from twenty to twenty-five years of age are fecund.

2. That the fecundity of very young (fifteen to twenty) wives, below twenty, is greater than that of wives married at from twenty-five to twenty-nine.

3. That there is a climax of fecundity in women, which is reached between twenty and twenty-five years of age.

A remarkable illustration of the variation of fecundity at different ages is acquired by observation of the fertility of the domestic fowl.

"It has been ascertained (says Mr. Geyelin)* that the ovarium of a fowl is composed of 600 ovula or eggs; therefore, a hen during the whole of her life cannot possibly lay more eggs than 600, which in a natural course are distributed over nine years in the following proportion:—

* *Poultry-Breeding in a Commercial Point of View.* By Geo. Kennedy Geyelin, C.E. London, 1865, p. 18.

TABLE XV.—Showing the Fertility of the Domestic Fowl at Different Ages.

First year after birth	15 to 20		Sixth year after birth	50 to 60	
Second „	100 „ 120		Seventh „	35 „ 40	
Third „	120 „ 135		Eighth „	15 „ 20	
Fourth „	100 „ 115		Ninth „	1 „ 10	
Fifth „	60 „ 80				

"It follows that it would not be profitable to keep hens after their fourth year, as their produce would not pay for their keep, except when they are of a valuable or scarce breed."

At this point my present inquiry is closed. I know of no other way of advancing our knowledge of this subject than by the collection and analysis of statistics. The only quarry for such materials, that I know of, is the Scottish registers for 1855. The tables adduced might be improved by going over a larger field, and increasing the numbers analysed. But I do not see how the matter in the registers could be turned to more account without encroaching on another topic which is at the same time closely connected with that under discussion—viz., the fertility of marriage. Or, as marriage is scarcely admissible as a term in physiology, I should give this subject the title of "sustained fecundity," the degrees of fertility which women of different ages, beginning to live with men, continue to exhibit during the child-bearing period of life.

The views hitherto entertained regarding the influence of age on fecundity have been various. "In regard to age (says Burdach*) fecundity is diminished

* *Physiologie*, tom. ii. p. 117.

in the first and last portions of the continuance of the aptitude for procreation. The elk, the bear, etc., have at first only a single young one, then they come to have most frequently two, and at last again only one. The young hamster produces only from 3 to 6 young ones, whilst that of a more advanced age produces from 8 to 16. The same is true of the pig. This rule appears to be general, since it applies also to the Entomostraca; according to Jurine, the number of the young of the *Monoculus pulex* is at first from 4 to 5, afterwards rising gradually as high as 18. We scarcely ever encounter the births of 3 or 4 children except in women who have passed the thirtieth year. Precocious marriages are not only less fertile, but the children also which are the result of them have an increased rate of mortality. According to Sadler, every marriage in the families of the peers of England yields 4·40 children when the woman was married below sixteen years of age; 4·63 from this age to twenty; 5·21 from twenty to twenty-three; and 5·43 from twenty-four to twenty-seven." The notions here expressed by Burdach are in the main correct; but it is evident that they are very indefinite. They are to be regarded, also, more in the light of happy guesses than of well-founded opinions. Burdach evidently places chief reliance on the evidence afforded by the numbers at a birth. From many quarters I have received corroboration of Burdach's statements regarding the increase and subsequent decrease of the number produced at a birth by pluriparous animals, and I have received

similar information regarding bitches, guinea-pigs, etc. When I first paid attention to this subject, the plural births of women appeared to me to form a simple key for the determination of the fecundity of women at different ages. But I soon became dissatisfied with the materials I quickly collected. Woman is not a pluriparous animal, neither does she produce so regularly, or according to season, as the animals with which she is compared. In her the occurrence of twins and triplets is an exception to the normal rule, and the number of children born by her cannot be so simple and sure a test of fecundity as in the case of animals having multiple litters at stated periods. Indeed, it is apparent that the evidence derived from plural births alone in women may positively mislead, for a woman may be more fertile bearing one child at a time frequently than another bearing twins or triplets more seldom. In this place I shall only say that the numerical study of twins, in reference to the age of the mother, yields interesting results, which do not confirm Burdach's statement regarding them, yet are not hostile to the conclusions here stated. Burdach, in his work, describes an annual rise and fall in the fecundity of some pluriparous animals. This annual variation forms a series of wavelets in the course of the great wave running from youth to old age, and culminating in middle life. This annual rise and fall of fecundity he attributes to the influence of cold.*

* This influence of cold has always been a favourite notion. It has some poetical truth, but it has never, so far as I know, got any

In his *Treatise on Man*, M. Quetelet has, with some care, collected the statistical materials available at the time for advancing the settlement of the question of the relation of age to fecundity. He does not allude

substantial basis. Roberton's labours on the influence of heat and cold upon the commencement of menstruation have ended chiefly in negative results, and have tended rather to overthrow than confirm what may be called the popular opinions. They are to be found in his work on *Physiology and Diseases of Women, and on Midwifery*.

More recently Dr. Stark, in an official report to the Registrar-General for Scotland, has, by means of a comparison of the Swedish returns with those of Scotland, arrived at a definite conclusion regarding the influence of cold. "The comparison," he says, "would seem to indicate that cold does delay the child-bearing period of life in women, as has been often supposed, but never till now proved by trustworthy facts" (*Eighth detailed Annual Report*, 1866, p. xv.) Speaking of Sweden, he remarks that—"it would appear that the child-bearing period of life in that country is, as it were, delayed for a period of about four years later than in this country. Thus, instead of the greatest number of women bearing children between the ages of twenty-five to thirty, as in this country, in Sweden the greatest number bear children between the ages of thirty to thirty-five years. Then, instead of the next most prolific age being, as it is in Scotland, from twenty to twenty-five, and from thirty to thirty-five years of age, in Sweden it is from twenty-five to thirty, and from thirty-five to forty years of age, while nearly the same number of women bear children above forty as between twenty and twenty-five years of age. These facts are so singular, that in Table VIII. are given the number and proportion of mothers at different ages in Scotland and in Sweden, that the difference in the prolific periods of the females of each country may be seen at a glance. By comparison of the facts, it will be observed that, at all ages under twenty-five years, only half the number of women bear children in Sweden as compared with Scotland; whereas, at the other end of the child-bearing period, only half the number of

to the opinions of Burdach, probably because they have no sufficient foundation, but he refers to Milne, Malthus, Sadler, Granville, Finlayson, and several foreign authors, who have more or less directly tried to throw light on the topic. Quetelet's whole chapter

women above forty years of age bear children in Scotland, as in Sweden."

These striking facts demand the study of physiologists if they have the bearing upon the influence of cold which Dr. Stark ascribes to them. But we cannot, in the present state of our knowledge, admit the validity of this bearing. We hesitate before accepting the doctrine of Dr. Stark regarding cold. The differences between the two countries may be explained by cold, but we do not think this probable, for they appear to us far too considerable at all ages, and specially too great at the late ages (above forty), to be accounted for by the influence of cold. Besides, in order that such tables as Dr. Stark adduces be admitted as proving anything, it is necessary first of all to collate with them a table of the ages of Swedish women at marriage for comparison with a similar Scottish table. Difference in the ages at marriage may account for all the striking facts in the table. Indeed the observation has repeatedly forced itself upon me, that comparisons of this kind made between two countries are dangerous foundations for argument, so great is the risk of error from the conditions of the statistics differing in the countries. [See foot-note, p. 109.]

Whatever value may be ascribed to the opinion of Burdach and Stark as to the influence of cold, or to the statement of Burdach, unsupported as it is by data, that there is an annual rise and fall in the fecundity of some pluriparous animals, I think the observation of the size of a yearly series of hen's eggs lends some shadow of confirmation to the supposed existence of an annual rise and decline of fecundity. For I am informed by more than one henwife that the first and last of a hen's yearly series of eggs are smaller than the eggs laid when the process in undisturbed operation. But I have found no satisfactory

on the influence of age on the fecundity of marriages is very unsatisfactory. It is at least difficult to reconcile with one another the conclusions arrived at in various parts of this chapter, and I shall not attempt to do it. It is only fair to say that he seems conscious of the numerical deficiency of data sufficient for a basis of any conclusion, and as an example of the state of matters, the table of Sadler, which he and Burdach both quote, may be mentioned; in it the number of marriages analysed is under 500, and they are all selected according to extraordinary conditions. The final conclusion which M. Quetelet announces, is, that it is before the age of twenty-six years that we observe the greatest fecundity in woman.

The latest writer on this topic, whom I know of, is Dr. Granville, who, in an interesting paper in the *London Obstetrical Transactions*, returns to the description of his former labours in the same field. In this paper, production or fertility is confounded with productive power or fecundity, and the table to which I have alluded in Chapter I. he describes not as showing the fertility at different ages of the industrial classes

ground of connection of this phenomenon with degrees of temperature.

It has, indeed, been surmised that heat may retard or diminish fecundity. "Azara" (*Quadrupèdes du Paraguay*, tom. ii. p. 360), says Darwin (*Animals and Plants under Domestication*, vol. i. p. 82), "has remarked that in the temperate regions of La Plata the cows conceive when two years old; whilst in the much hotter country of Paraguay they do not conceive till three years old. 'From which fact,' as he adds, 'one may conclude that cattle do not succeed so well in warm countries.'"

of the metropolis, but erroneously, as showing the alternations in the productive power of women at different ages.

In this part, then, I have, *inter alia*, shown that the great majority of the population is recruited from women under thirty years of age; but that the mass of women in the population, of from thirty to forty years of age, contribute to the general fertility a larger proportional share than the mass of women of from twenty to thirty years of age.

Further, that the wives in our population, taken collectively as a mass, show a gradually decreasing fecundity as age advances; but that the average individual wife shows a degree of fecundity which increases till probably about the age of twenty-five, and then diminishes.

The fecundity of the average individual woman may be described as forming a wave which, from sterility, rises gradually to its highest, and then, more gradually, falls again to sterility.

NOTE.—Notwithstanding the ingenious and valuable criticism of the writer in the *North British Review* for December 1867, I have, after much hesitation, resolved to reproduce the text of the parts criticised almost destructively, just as it was in the first edition. While I gratefully recognise the truth of the critic's main assertions as to the faultiness of the table of initial fecundity, I still am disposed to regard my conclusions as correct. This course is so extraordinary as to

require some account to be given of it. And firstly, I may state that I shall mislead no one, for I shall, in a subsequent part of this book, avail myself of permission, kindly granted to me, to republish the whole review that I have referred to.

I fear I lay myself open to the charge of clinging too tenaciously to views once expressed; and certainly I am guilty of not attaching so much importance to the destructive criticism of one of my tables as I attached to its evidence when I supposed it invulnerable. But there are several sources of knowledge as to the question involved besides my injured table. Again, my table on initial fecundity, though fairly criticised, yet embodies a set of true statements which give it a certain value and weight. As I published Professor Tait's notes, which were not exactly consistent with my views, so I shall give the learned reviewer's criticism, which attacks them. Lastly, I may express a hope, at some future time, and perhaps with better data, to reconsider the whole matter, and arrive at final results regarding it.

Since my first edition was published I have perused an elaborate paper by Dr. Routh, "On Procreative Power."* This author produces an analysis of Irish statistics which confirms, so far as I can make out from them in their somewhat imperfect state, the view I entertain as to the existence of a climax of fecundity in women. I here give Dr. Routh's tables, and the conclusions bearing on my subject which he draws.

* *London Journal of Medicine*, 1850, vol. ii. p. 240.

"TABLE (XVI.) OF FECUNDITY FOR FEMALES, FOR THE YEARS 1839 AND 1840.

Age.	No. of deliveries to 600 married.			To 100 population.	To a maximum of 100.
	1840.	1839.	Total.		
Under 17	104	114	218	36·33	44·96
17—25	230	240	470	78·33	96·94
26—35	185	265	450	75·00	90·28
36—45	129	118	247	41·17	50·95
46—55	81	33	114	19·00	23·51
Above 55	0	37	37	6·16	7·06

giving as a maximum age 26·66, and maximum ordinate 80·80. The maximum age for 1840 alone is 25·13.

"This result would seem to fix the maximum of male and female about the same for average physiological power, and, if anything, rather higher for females than males. The single year 1840 alone makes it higher for males. This is manifestly an error, owing to the accident before noticed. Yet, as the deduction of average procreative power from fecundity tables may be useful for reference, it is perhaps convenient to consider the maxima in both males and females to be 26, at the same time, as by reducing to the same unit the numbers deduced from the marriage tables, other numbers for the ages $21\frac{1}{2}$, 31, and 41, may be annexed in another column for comparison, and this is what has been done in the annexed table for physiological procreative power.

"That there is a curve representing the exact inclination of procreative power, cannot be doubted. Its exact determination, however, is impossible, when such distant

intervals as ten years are given as ordinates between the ages. The circle would perhaps be the nearest curve that could be selected, but it very much increases the amount of arithmetical labour necessary. Fortunately, for practical purposes, such accuracy, however desirable, is not absolutely needed. If we suppose these several ordinates joined by a straight line, the procreative power of every age may in this manner be calculated, which, if not exactly correct, may yet be very useful in its application. Accordingly, in the following table, the procreative power for every age has been so calculated.

"TABLE (XVII.) OF AVERAGE PHYSIOLOGICAL POWER, CALCULATED FROM THE IRISH TABLES.

Age.	Males.		Fem.	Age.	Males.		Fem.	Age.	Males.		Fem.	Age.	Male. Fem.	
	Mar.	Fec.	Fec.		Mar.	Fec.	Fec.		Mar.	Fec.	Fec.		Fec.	Tab.
15	...	...	22	28	82	80	96	41	60	51	51	54	30	5
16	...	31	44	29	87	70	94	42	...	49	48	55	29	...
17	...	36	54	30	93	60	92	43	...	47	45	56	28	...
18	...	40	63	31	100	51	90	44	...	45	42	57	28	...
19	...	45	73	32	95	51	86	45	...	43	39	58	27	...
20	...	49	82	33	91	51	82	46	...	41	37	59	26	...
21	...	54	92	34	87	51	78	47	...	39	34	60	26	...
22	47	61	97	35	84	51	74	48	...	37	31	61	25	...
23	53	70	97	36	80	51	70	49	...	35	28	62	19	...
24	59	80	98	37	76	51	66	50	...	33	26	63	12	...
25	65	90	99	38	72	51	62	51	...	32	23	64	6	...
26	70	100	100	39	68	51	58	52	...	31	17	65	...	...
27	76	90	98	40	64	51	54	53	...	30	11			

"The age of greatest fecundity in males is from thirty-one to thirty-three, in females twenty-six.

"But the procreative value of a person between fifteen and sixteen is greater than at any preceding or succeeding age."

PART II.

ON THE WEIGHT AND LENGTH OF THE NEWLY-BORN CHILD.

Inquiring into the influence of the age of the mother upon fecundity, I desired to find out if any light could be thrown upon the subject by the variations, if any, of the weight and length of mature children born of women of different ages; intending to assume that the weight and length of the child might increase or diminish with the high or low state of the fecundity of women, or of the vigour of the generative functions.*

The observations, upon which all my conclusions are founded, have been drawn from the records of the Edinburgh Royal Maternity Hospital. They amount to 2070 pregnancies, with 2087 children. They are not nearly so numerous as I could desire, but no more are available to me.

* For some curious remarks and references, see Tanner, *Signs and Diseases of Pregnancy*, 2d edition, p. 199; also Joynt, Case of protracted Utero-gestation—*Dublin Quarterly Journal*, Nov. 1866, p. 388; also Bonnar, on Superfœtation—*Edinburgh Medical Journal*, vol. x., 1864-65, p. 593; also Spæth—*Edinburgh Medical Journal*, vol. vii., 1862-63, p. 846; also Cazeaux, *Traité de l'art des Accouch.*, 6me ed., p. 210.

CHAPTER I.

ON THE INFLUENCE OF PRIMOGENITURE ON THE WEIGHT OF THE NEWLY-BORN CHILD.

Professor Hecker of Munich, in a recent work,* publishes some calculations made to show the mean weight of the children of primiparæ as compared with those of multiparæ, and he finds that the latter exceed the former in weight. His figures are as follows: —378 children of primiparæ weighed, on an average, 7·07 lbs. each; 718 children of multiparæ weighed, on an average, 7·38 lbs. each; among these 1096 infants, the average weight of the mature children of primiparæ was less than that of the mature children of multiparæ by ·309 lb.

My observations on 2053 children confirm those of M. Hecker. The average weight of 1011 children of primiparous females was 7·170 lbs.; the average weight of 1042 children of multiparous females was 7·277 lbs.; the average weight of mature children of primiparæ being less than that of the mature children of multiparæ by ·107 lb.

* *Klinik der Geburtskunde.* Von Dr. C. Hecker und Dr. L. Buhl. S. 46. Leipzig, 1861.

If these results are subjected to some study, their apparent value almost entirely disappears. Hecker evidently would imply that primogeniture is the circumstance which determines the comparative lightness of first-born children, and *vice versâ*. But it is evident that, in order to a just comparison of the weights of children of primiparæ with those of multiparæ, the children compared must be born in circumstances as nearly identical as possible. Especially, care must be had that the now known influence of age of the mother be taken into account, and this care M. Hecker has altogether omitted, an omission for which he is not in any degree blamable, seeing that, when he wrote, the influence of age was not discovered.

In the following investigation it will, I think, be established that some connection exists between variations in the weight of the newly-born child, and, not primiparity or multiparity, but the age of the mother at the time of the birth. No doubt, any statistic of a population or of an hospital may show greater weight in second and subsequent births than in first, because the great majority of primiparæ are young, and their age, anticipating the arrival of the climax of fecundity, may tell upon the size of their offspring. The following considerations seem to me almost to prove the nullity of influence on weight exercised by primiparity.

1. The weight of the children of primiparæ is not nearly uniform, but varies according to the law of the age of the mother. (See Table XXII.)

2. The weight of the children of all mothers,

whether primiparæ or multiparæ, varies according to the same law of mother's age. (See Table XIX.)

3. The following Table (XVIII.) of the mean weights of the children of first and subsequent pregnancies shows no increase or decrease according to the number of the pregnancy. No doubt the children of first pregnancies are lightest, but this may be accounted for by age.* After the first pregnancy there is no variation according to any law. There is, indeed, great uniformity in pregnancies after the first. I have appended a line of mean ages to show that even the average woman of seventh and subsequent pregnancies has not passed into the ages in which the decline of fecundity is strongly marked. It is perhaps on this account that no gradual diminution of weight is observed in the more advanced pregnancies. I may add that I have an impression that, were data forthcoming to extend this table to eighth, ninth, tenth, eleventh, twelfth pregnancies, and so on, a diminution of the mean weight of the children would appear, and that it would correspond to the average age of the woman, advancing in these pregnancies into years of decided decline of fecundity.

* Perhaps the lightness and shortness of first-born children confirms Mr. Shandy's opinion, that the eldest son is the greatest blockhead of the family (Sterne's Works, vol. ii., chap. xix.), and the Scotch proverb that not the laird, but the laird's brother, is the clever member of the house. On this subject, see Galton, *Hereditary Genius*, p. 87.

TABLE XVIII.—SHOWING THE AVERAGE WEIGHT OF CHILDREN BORN IN FIRST AND SUBSEQUENT PREGNANCIES.

No. of Pregnancy	1	2	3	4	5	6	7, etc.	Total.
Weight of Child .	lbs. oz. 7 3·157	lbs. oz. 7 4·897	lbs. oz. 7 5·597	lbs. oz. 7 3·046	lbs. oz. 7 7·223	lbs. oz. 7 5·076	lbs. oz. 7 4·991	lbs. oz. 7 4·109
Age of Mother .	22·787	25·806	27·701	30·321	30·424	32·045	35·562	25·625

Before advancing, I may here interpolate a remark regarding the difficulty and danger of labour in primiparæ. The popular notion of the increased difficulty and danger of a first labour in a woman no longer young may find its explanation, in part at least, in the increased bulk of the child as indicated by its increased weight and length in such women. I have often heard that an old primiparous woman passes through labour more easily than one somewhat younger, and we may possibly find this also accounted for by the comparatively small size of children born in the latest periods of fecundity.

CHAPTER II.

THE VARIATION OF THE WEIGHT OF THE NEWLY-BORN CHILD ACCORDING TO THE AGE OF THE MOTHER.

I HAVE already shown that the fecundity of the average individual woman increases to about the age of twenty-five years. This conclusion receives very interesting corroboration from the following Table (XIX.) of the average weights of mature children born of mothers of ages gradually rising to twenty-five years. The weight of the child gradually increases to its climax in the age of from twenty-five years to twenty-nine. After this age the weight of the child declines, and the diminution goes on by very slow degrees. This slowness is entirely out of proportion with the rapid diminution of fecundity of women of similar ages. But, in the present deficiency of data, it will be well not to attempt to push the discussion farther.

TABLE XIX.—Showing the Average Weight of Children born of Mothers of Different Ages.

Age of Mother.	Number of Pregnancies.	Total Weight.		Average Weight.	
		lbs.	oz.	lbs.	oz.
15-19	209	1459	10	6	15·741
20-24	832	6008	7½	7	3·547
25-29	570	4220	4	7	6·463
30-34	278	2020	6	7	4·281
35-39	139	1011	2	7	4·388
40-44	38	272	0	7	2·526
45-49	3	20	12	6	14·666

The decrease of children's weight may perhaps not be justly compared with the decrease of fecundity, for this last decrease is produced chiefly by women entirely deserting the category of the fecund, and bearing no children for comparison. The decrease of the fecundity of the average woman is not produced by diminished fecundity of the individual woman, but by the arrestment or disappearance of fecundity in women previously fertile.

CHAPTER III.

ON THE INFLUENCE OF PRIMOGENITURE ON THE LENGTH OF THE NEWLY-BORN CHILD.

It will be interesting now to inquire if Hecker's opinion regarding the influence of primogeniture be confirmed by a study of the varying lengths of children. Hecker himself has, in his clinical work, made no observations on this point, and it must, I think, be admitted that length of mature infants is not nearly so good a test of growth and nourishment as weight. Yet it will immediately appear that interesting corroboration of other allied laws, if not of Hecker's, may be drawn from a study of lengths; and it was not to be expected that the estimation of such measurements should be omitted by an obstetrician who has laid great stress on the value of length as an indication of maturity, enunciating the doctrine that good evidence of maturity cannot be obtained so satisfactorily by weighing as by measuring.

Among 2053 mature single children there were 1011 born of primiparæ; the average length of these was 19·213 inches;—there were 1042 born of multiparæ; the average length of these was 19·202 inches; the average length of mature children of primiparæ

exceeding that of the mature children of multiparæ by ·011 inch. The difference here in favour of primiparæ is so slight that it may be disregarded. No notable difference in this respect, therefore, is made out between primiparæ and multiparæ, a circumstance which shows that weights and lengths of children have no direct or certain relation, one to another, if the figures adduced have been carefully ascertained and are numerous enough to justify such a statistical conclusion.

The remarks, which I have already made in connection with the corresponding observations on the weight of children of primiparæ and multiparæ, are so closely applicable, *mutatis mutandis*, to the present topic also, that I shall not repeat them. I shall only here give a table of the lengths of children born in first and succeeding pregnancies, to show that there is no increase or decrease of length according to the number of the pregnancy; that length does not seem to be under any law connected with the first or subsequent occurrence of pregnancy.

TABLE XX.—Showing the Average Length of Children born in First and Subsequent Pregnancies.

No. of Pregnancy .	1	2	3	4	5	6	7, etc.	Total.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
Length of Child .	19·197	19·239	19·304	18·959	19·273	18·962	18·991	19·188
Age of Mother . .	22·787	25·806	27·701	30·321	30·424	32·045	35·562	25·625

Although, however, the facts here adduced do not show the influence of age in conjunction with primiparity, I have little doubt that a large enough collection would show comparative shortness of first-born children, just as comparative lightness has been shown, and for the same reason—namely, because primiparous women are, in a very large proportion, young.

CHAPTER IV.

THE VARIATION OF THE LENGTH OF THE NEWLY-BORN CHILD ACCORDING TO THE AGE OF THE MOTHER.

ALTHOUGH the observations I have tabulated bear no evidence in regard to the influence of primiparity or of multiparity, yet when thrown together so as to be questioned regarding their relation to maternal age, they support the doctrine which I have elsewhere maintained. Length of the newly-born child is shown in Table XXI. to increase as the mother gets older until the period from 25 to 29 is reached: after this, the length of the child gradually diminishes.

TABLE XXI.—SHOWING THE AVERAGE LENGTH OF CHILDREN BORN OF MOTHERS OF DIFFERENT AGES.

Age of Mother.	No. of Children.	Total Length.	Average Length.
		Inches.	Inches.
15–19	209	3,972½	19·007
20–24	839	16,082¼	19·168
25–29	574	11,109½	19·355
30–34	280	5,384¼	19·229
35–39	142	2,683¾	18·899
40–44	39	737½	18·910
45–49	3	54½	18·166

It appears, then, that a careful study of the weights and lengths of newly-born mature children lends some support to the doctrine that the vigour of the female reproductive system waxes till the age of about 25 years is reached, and then wanes.

I here append two general tables, containing numerous details, which speak for themselves :—

TABLE XXII.—Showing the Average Weight of Children born at various Ages and Pregnancies of Mothers.

No. of Pregnancies.	Age of Mothers.	Pregnancy 1.	Pregnancy 2.	Pregnancy 3.	Pregnancy 4.	Pregnancy 5.	Pregnancy 6.	Pregnancy 7, etc.	Total.
		lbs. oz.	lbs. oz.	lbs. oz.	lbs. oz.	lbs. oz.	lbs. oz.	lbs. oz.	lbs. oz.
209	15-19	6 15·772	6 10·733	11 5·000	...	...	...	...	6 15·741
832	20-24	7 3·455	7 4·486	6 13·735	7 12·273	6 15·273	8 4·000	5 8·000	7 3·547
570	25-29	7 4·836	7 7·481	7 8·451	7 1·447	7 13·565	7 9·400	7 2·125	7 6·463
278	30-34	7 5·283	7 1·733	7 2·971	7 1·864	7 6·097	7 3·520	7 10·270	7 4·281
139	35-39	7 0·846	7 5·846	7 13·444	6 15·364	7 5·167	7 2·765	7 4·289	7 4·388
38	40-44	8 8·000	6 7·200	7 12·000	7 10·750	7 14·000	6 13·333	6 15·000	7 2·526
3	45-49	...	...	6 0·000	...	...	...	7 6·000	6 14·666
1	50	...	...	...	8 8·000	...	...	...	8 8·000
	Average Total.	7 3·157	7 4·897	7 5·597	7 3·046	7 7·223	7 5·076	7 4·991	7 4·109
	Mean Age of Mother.	22·787 Years.	25·806 Years.	27·701 Years.	30·321 Years.	30·424 Years.	32·045 Years.	35·562 Years.	25·625 Years.

TABLE XXIII.—Showing the Average Length of Children born at various Ages and Pregnancies of Mothers.

Number of Children.	Age of Mothers.	Pregnancy 1.	Pregnancy 2.	Pregnancy 3.	Pregnancy 4.	Pregnancy 5.	Pregnancy 6.	Pregnancy 7, etc.	Total.
		Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
209	15-19	18·992	18·966	22·500	...	...	...	...	19·007
839	20-24	19·142	19·171	19·321	19·682	19·636	18·000	18·000	19·168
574	25-29	19·399	19·382	19·455	18·697	19·435	19·225	19·687	19·355
280	30-34	19·739	19·250	18·819	19·125	19·218	18·640	19·487	19·229
142	35-39	19·115	19·086	19·600	18·250	19·028	18·941	18·685	18·899
39	40-44	19·750	18·500	18·750	19·250	18·500	20·333	18·605	18·910
3	45-49	...	...	18·500	...	...	...	18·000	18·166
1	50	...	...	...	21·000	...	...	...	21·000
	Average Totals.	19·197	19·239	19·304	18·959	19·273	18·962	18·991	19·188
	Mean Age of Mother.	22·787 Years.	25·806 Years.	27·701 Years.	30·321 Years.	30·424 Years.	32·045 Years.	35·562 Years.	25·625 Years.

CHAPTER V.

PROFESSOR HECKER'S OBSERVATIONS.

My inquiries were first published in the *Edinburgh Medical Journal*, December 1864. That article called forth from Professor Hecker of Munich a very complete and elaborate article on the same topic.* It appeared in the *Monatsschrift für Geburtskunde und Frauenkrankheiten*, November 1865. The Bavarian professor's results are not identical with mine, though they tally with them in one of the chief conclusions. While mine are founded on 2087 observations, Hecker's have for a basis the far larger number of 4449, and on this account alone demand a higher degree of confidence. But I have preferred to adhere in the meantime to the form of my own original statements, and to add to them here the conclusions of Hecker. For neither Hecker's nor my numbers show a satisfactory or assuring amount of steadiness in the increase and decrease of the figures; and, considering the small differences which the figures show as they increase or decrease,

* See also an elaborate paper by Hecker's pupil Wernich, Ueber die Zunahme der weiblichen Zeugungsfähigkeit. *Beiträge zur Geburtshülfe und Gynäkologie.* I. Band. I. Heft, Berlin, 1870. Consult also Abegg. *Zur Geburtshülfe und Gynäkologie*, Berlin, 1868; also Frankenhæuser, *Jenaische Zeitschrift für Medicin*, etc., 1867, S. 182.

it is necessary, in order to have considerable assurance as to the reliability of the results, to possess a much larger statistical basis than even the 4449 observations of Hecker. I have already said that Hecker's numbers give his conclusions a value superior to that of mine, and I must add that I hold Hecker to have demonstrated an increase of weight and length of the child with the number of the pregnancy. Only it appears to me that this conclusion of Hecker must be applied, not generally to all pregnancies, but only as far as the fifth; for his figures are given only for the pregnancies numbering from first to fifth.

I now give the conclusions of this author—

"1. The view of Matthews Duncan, that increase of weight and length of the child is in direct dependence on the age of the mother has been shown to be quite correct.

"2. On the other hand, it is not established that there is, within the child-bearing period of woman's life, a distinct climax of increase of its weight and length; it is rather shown that increase, on the whole, goes on till the end of fruitfulness.

"3. Age is not the only factor of this increase, but, as Duncan implies, an influence must be distinctly recognised as exerted by the number of the pregnancy."

The results of Hecker are not without support in the general history of fruitfulness of women, as will be shown hereafter. They may, if verified, stand beside the law of the intensity of fertility of women as age at marriage advances, or as families increase in number.

On the other hand, if my conclusions come to be confirmed, they may take their place with the law of the rise and decline of the fecundity of women.*

In leaving this topic, I wish to point out another subject for investigation, from whose elucidation some explanation of the variations of the weight and length of the newly-born child may arise. Hecker, Clay, Montgomery, and Joulin, mention the comparative shortness of the pregnancy of primiparæ. This may account for the smallness of first-born children; and a similar relation may be established between the duration of pregnancies of different numbers, and of women of different ages, and the relative weight and length of the resulting offspring.

Dr. Montgomery's† opinions that there is no good foundation for any such rule as would affirm that the duration of human gestation is directly proportioned to the age of the woman, and that there is no relation between the duration of pregnancy and the size of the child produced, need not discourage the inquirer, for they are founded, as a perusal of his work shows, more upon individual instances than upon a large collection of data. I shall refer to the latter of these opinions

* A study of the domestic fowl's eggs seems to lend some confirmation to my statistical results. The small eggs of the young hen are generally known. I am informed by two experienced hen-wives that the old hen also lays a comparatively small egg. An observation of my own shows that these eggs are sometimes entirely without yolk, or with an imperfect one.

† See Montgomery, *Signs and Symptoms of Pregnancy*, 2d ed. p. 535, etc.

when I discuss the protraction of pregnancy, in the latter part of this volume. But I may here mention that Frankenhæuser, founding on the data of a few cases published by Hecker, has by means of them been able to find some support in actual facts for the explanation of the smallness of first-born children, which I had suggested, namely, that the pregnancies might be shorter.* Dr. Clay † also has, in a formal manner, and in direct opposition to M. Tessier,‡ laid down the proposition, that, as age increases, the term of utero-gestation is lengthened; and, so far as they go, his observations confirm it. But they are too few for a satisfactory basis, and they require to be corrected for the influence of the number of the pregnancy. The probability that age might exercise an influence on this term was long ago indicated by Condorcet.§

* *Jenaische Zeitschrift für Medicin*, etc., 1867. S. 185.

† *Observations on the Term of Utero-Gestation*. An interesting essay by Charles Clay, M.D. London, 1855.

‡ *Mem. de l'Acad. Royale des Sciences*, 1817, p. 16.

§ *Ibid.* p. 3.

PART III.

ON SOME LAWS OF THE PRODUCTION OF TWINS.

HITHERTO the variations of fecundity have been most frequently and most easily observed in animals which have ordinarily multiple births, and the number of whose young, born at different litters, varies. Such difference in number at different times cannot fail to strike the observant eye, and little ingenuity is needed to make the number of young a test of the degree of fecundity. To such an argument I can find no objection.*

Among women, the birth of twins occurs once in about eighty deliveries. Triplets, quadruplets, quintuplets, and even higher figures, are occasionally observed; they are very uncommon, and the rarity is progressive with the number. The normal or ordinary rule in woman is to bear one child at a time; and the next most frequent condition is temporary or persistent sterility—two points in which she signally differs from what is generally believed of the animals subjected to the observations referred to.

It is easy to understand how a regularly-bearing animal, the number of whose young born at different

* For much valuable relative information, see Kehrer; *Beiträge zur vergleichenden und experimentellen Geburtskunde.* II. Heft, S. 2. Giessen, 1868.

litters varies, may in its individual history give good illustration, if not evidence, of some law of the variation of fecundity. In irregularly-bearing uniparous woman no such illustration or evidence of any law can be got by observing an individual, and recourse must be had to statistics, to the analysis of the details regarding the pregnancies of large numbers of women subjected to the conditions essential to fecundity.

The rarity of a plural birth in woman, and the increased danger to both mother and offspring in these circumstances, render such an event, in a certain limited sense, a disease, or an abnormity.* This abnormity, if used alone, is not a good test of fecundity, and it has been resorted to for this purpose in consequence of the enticement of its apparently simple and easy application to woman as to the lower animals.

It is, at first sight, evident that a woman, even repeatedly bearing twins, may be surpassed in fecun-

* On this subject, consult a valuable paper by Dr. Arthur Mitchell, published in the *Medical Times and Gazette* of November 15, 1862. In that paper Dr. Mitchell demonstrates the abnormality of twin-bearing in the human female; and to his other proofs he might have added the frequency of abortion, of hydramnios, and the occurrence of acephalous monsters only, among twins. His conclusions are:—

"1. Among imbeciles and idiots a much larger proportion is actually found to be twin-born than among the general community.

"2. Among the relatives of imbeciles and idiots twinning is also found to be very frequent.

"3. In families, when twinning is frequent, bodily deformities (of defect and of excess) likewise occur with frequency.

dity by another bearing single children more rapidly or for a longer period. Neglect of these and other such truths has led to errors; and I may cite a popular hasty conclusion regarding the comparative fertility of races in illustration. The greater frequency of plural births in one race than in others, as in the Irish compared with the English, has been represented as showing that the former is more prolific than the latter. Whether the conclusion be true or not, I do not here propose to inquire;* certainly, the grounds of the conclusion are insufficient to establish it, and unsatisfactory, seeing that an opposite conclusion is not absolutely inconsistent with them, as I have just shown. And a little ingenuity easily discovers other arguments against the popular view; for example, it might be said

"4. The whole history of twin-births is exceptional, indicates imperfect development and feeble organisation in the product, and leads us to regard twinning in the human species as a departure from the physiological rule, and therefore injurious to all concerned.

"5. When we pass from twins to triplets and quadruplets, everything we know regarding these latter gives support to the general conclusions in question."

The curious relation of malformation to twinning has not been sufficiently studied in woman and in the lower animals. On this subject consult Simpson, *Obstetric Works*, vol. ii. p. 349.

Perhaps it may be unsafe, in these days of vindication of the rights of women, to draw any argument in favour of the doctrine that twinning is a disease, from the numerical excess of females among twins. See Simpson, *Obstetric Works*, vol. i. p. 402.

* The facts as they appear in Collins' data, and in my statistics of Edinburgh and Glasgow in 1855, are as follows:—In the Dublin Lying-in Hospital 16,385 women produced 480 twin children. In Edinburgh and Glasgow in 1855, 16,301 wives produced 396 twin

that twins, as an unnatural and excessive strain upon the female reproductive powers, may exhaust the fecund energies, and lead to comparative barrenness subsequently.

In illustration of the treatment of this subject by physiologists, I cite the following passage from the works of Burdach :*—"In regard to age (says he), fecundity is diminished in the first and last portions of the continuance of the aptitude for procreation. The elk, the bear, etc., have at first only a single young one, then they come to have most frequently two, and at last again only one. The young hamster produces only from three to six young ones, whilst that of a more advanced age produces from eight to sixteen; the same is true of the pig. This rule appears to be general, since it applies also to the Entomostraca; according to Jurine, the number of the young of the *Monoculus pulex* is at first from four to five, afterwards rising gradually as high as eighteen. We scarcely ever encounter the births of three or four children

children. As already often repeated, the two sets of cases are not suited for exact comparison. The result (including all sources of error) is, that the women delivered in Edinburgh and Glasgow should have produced 477 twin children instead of 396, if they were not to allow themselves to be surpassed in double births. The variation of the frequency of twin births in different countries is so great (Oesterlen, *Handbuch der medicinischen Statistik*) as to remove all probability from the notion or belief that the greater or less frequency of twins shows greater or less general fertility. See also Boudin, *Geographie et Statistique Médicales*, tome ii. p. 62.

* *Physiologie.* Tome ii. p. 117.

except in women who have passed the thirtieth year.* Precocious marriages are not only less fertile, but the children also which are the result of them have an increased rate of mortality. According to Sadler, every marriage in the families of the peers of England yields 4·40 children when the woman was married below sixteen years of age; 4·63 from this age to twenty, 5·21 from twenty to twenty-three, and 5·43 from twenty-four to twenty-seven."

In support of Burdach's statements generally, I here quote an extract from a letter on this subject from the highly intelligent gamekeeper of the Earl of Southesk. It forms a very favourable example of the kind

* There is a general deficiency of evidence for Burdach's statements. In order to test his assertion concerning the ages of women bearing triplets and quadruplets, I have hastily collected the following ten authentic cases of triplets from the works of Collins, M'Clintock and Hardy, Braun Chiari and Spæth, Hugenberger, and from my note-book. They speak for themselves, and it will be observed that triplets are by no means exclusively confined to women above thirty years of age. Yet it is noticeable that not one occurs among the younger child-bearing women, and not one in a first pregnancy.

TABLE XXIV.—Table of Ten Cases of Triplets.

Age of Mother . . .	27	27	29	30	30	33	35	35	36	37
No. of Pregnancy. .	3	3	2	3	11	2	3	6	3	2

An interesting fact in connection with this subject is mentioned in Hugenberger's report of the St. Petersburg Midwives' Institute (1863). Three women admitted there between 1845-59 in their fifteenth pregnancies had triplets, and each had triplets three times in succession. Leopold (*Arch. f. Gynaek.* II. Bd. S. 285) records a quadruplet birth in the seventh pregnancy of a woman æt. 34. He mentions another case in an eighth pregnancy.

of information I have been able to collect on this topic. "My observation has led me to believe that, as a general rule, a bitch has fewer puppies the first, and gradually increases year by year till her strength begins to fail. Then a rapid diminution follows, ending in one or two. This rule, however, is very materially affected by circumstances, and one may come to very erroneous conclusions by overlooking these. Such, for instance, is the state of the health, condition, amount of work. According to the state of the health, there may be many or few (puppies), weak or strong production; according to the condition, there will be few or many. If the animal is in a good fair condition, there will be many; if overfed, few. And as regards the amount of work a dog has to perform, so will the decline be rapid or gradual; and hence, if a bitch is worked hard year after year, she will fail rapidly, and the diminution of her puppies will be accordingly; but if worked moderately, and well kept, she will fail gradually, and the diminution will be less rapid.

"The above rule holds good in reference to breeding sows, but it is a common thing in their case for condition (fatness) to interrupt the rule. When a sow or bitch in an overfed state is brought in contact with the male, and afterwards increases in fat, which in this state they are very liable to do rapidly, consequently there must be a pressure upon the womb, and hence the embryo will be crushed, and thus production will be prevented. I believe it is well known how easily

the embryo can be injured or destroyed, but I do not think it is well known how much it will endure in the fœtal or more advanced state. We had an instance of this last year, when the great spate was in the month of May, when so many of our pheasant eggs were carried away by the flood and covered in water, many of which we lifted from under one and two feet of water, and many we got floating about in the ditches, which had been a night and a good part of a day in the cold water. Now, we knew that many of these eggs had been a good while sat upon; and being anxious if possible to save some of them, got them set under common hens, and many of them hatched wonderfully well: some of them came out a few days after we set them. Now, had these eggs been in the embryo state a little shake and being in cold water an hour or two would have rendered them quite useless.

"I have never observed any cause that led to a greater proportion of males or females in a litter. From what I have observed, I do not think that age or strength on either side has anything to do with it."

Bischoff, in his work on the development of the roe-deer, has the following passage, which has evident bearings on the relation of age to plurality of births:—"Häufiger finden sich zwei Corpora lutea an einem Eierstocke, als eines an beiden, bei Schmalrehen oft überhaupt nur eines, bei älteren Gaisen fast immer zwei, sehr selten drei, und nur ein einziges Mal fand ich vier."*

* *Entwicklungsgeschichte des Rehes.* Giessen, 1854, S. 10.

CHAPTER I.

THE NUMBER OF TWINS BORN OF WOMEN OF DIFFERENT AGES.

I HAVE made a collection of 1512 twins from the sources indicated in the footnote,* and have arranged them so as to show the various ages of their mothers in quinquennial periods. This table shows that in the general population it so happens that the number of twins born increases with the age of the mother, until the age from twenty-five to twenty-nine inclusive is reached, and that after this age is passed the number of twins born regularly diminishes,—a result which accords with what is observed of the fertility generally of the whole female population; the actual fertility of the female population increasing from the commencement of the child-bearing period of life until the age of thirty is reached, and then declining to its extinction with the child-bearing faculty.†

* Collins, *Practical Treatise;* private letter from Dr. M'Clintock, giving extracts from the Dublin Hospital Register; M'Clintock and Hardy on *Midwifery and Puerperal Diseases;* Chiari Braun und Spæth, *Klinik der Geburtskunde;* my own extracts from the Registers of Edinburgh and Glasgow for 1855.

† *Transactions of the Royal Society of Edinburgh* for 1863-64, p. 479; and Part I. of this volume.

TABLE XXV.—Showing the Ages of 756 Mothers of Twins.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45	Total.
Collins	3	53	76	71	28	8	1	240
M'Clintock	2	23	45	41	17	1	...	129
M'Clintock and Hardy	1	20	34	26	12	2	...	95
Chiari Braun and Spæth	1	25	36	26	4	2	...	94
Statistics of 1855	3	28	46	58	52	11	...	198
Total	10	149	237	222	113	24	1	756

But while there is this general accordance of results, an examination of the table at once reveals to the observer great differences between the general fertility of women and their fertility in twins. These differences will be the subject of further description; in the meantime I shall only adduce sufficient evidence to show that there is great difference, so much indeed as at once to demonstrate that the production of or fertility in twins is in woman not subjected to the same laws as fertility generally.

The mean age of 16,385 paturient women included in Dr. Collins' tables is 27 years. The mean age of 240 women in the same lists bearing twins is 29. The twin-bearer is older than the general run of bearers.

The number of twins born by women under and above thirty years in Dr. Collins' lists is 153 and 87, showing a majority of 66 on the side of the younger women, and thus a smaller proportional number of twins born of the young women than of all children born of the same.

But the data of Dr. Collins are not the best I can adduce to elucidate this point. His are derived from a class of cases submitted to selection, the conditions being all those connected with admission to the Lying-in Institution of Dublin. I bring forward data derived from an analysis of the whole legitimate births in Edinburgh and Glasgow in 1855. These show still more markedly and satisfactorily than the data of Collins, that a far larger proportion of twins than of children generally is born of elderly women. It is

easy to imagine reasons for the greater though similar difference shown by my statistics than by those of Collins; and there can scarcely be a doubt that mine are, in regard to this point, far more reliable than his. The mean age of 16,301 mothers of legitimate children in Edinburgh and Glasgow, in 1855, was above twenty-nine years. The mean age of 198 wives bearing twins was thirty-one years. The twin-bearer is here again older than the general run of bearers. In the same population the number of twins born by women under thirty years of age was 86; the number born by women above thirty years was 112,—showing a majority of 26 on the side of the elder women. Or, while of all births among these 16,301 wives three-fifths occurred among women under thirty years of age, there occurred only two-fifths of the twins among these younger women.*

* It is necessary to remark that the statements here given are not absolute or exact. For while the Dublin statistics include all the births at or near the full time, the statistics of Edinburgh and Glasgow include the same, with the exception of those born dead. But it is evident that were the figures to be exactly true, not only should the dead-born be included, but also all born in miscarriages or abortions. Until such comparative statistics are procured as include all births, mature and premature, living and dead, no statement, even of the comparative frequency of twin births, can be absolutely relied on; for fewer plural pregnancies come to maturity than pregnancies with single children. Chiari Braun and Spæth have shown that abortions are comparatively more frequent in plural pregnancies than in ordinary pregnancies.

CHAPTER II.

THE INFLUENCE OF AGE ON WOMAN'S FERTILITY IN TWINS.

I HERE first produce a table formed by adding the data of Dr. Collins to my own, and showing the very remarkable result that, speaking generally, the older a mother is the more likely is she to have twins.

While, of mothers from fifteen to nineteen years of age, only every 189th had twins at a birth, mothers rising in age were more prolific in twins, till at the age of from thirty-five to thirty-nine years the climax of fertility in twins was reached, every forty-fifth woman producing two at a birth, or fully four times as many as the women under twenty. This statement of gradually-increasing fertility in twins does not, so far as the table shows, appear to hold good after the age of forty is reached. And I shall have presently to point out an analogous divergence from the same statement, but in an opposite direction, when I treat of the influence of primiparity in increasing twin-births.* I venture, therefore, founding on the above

* Regarding twins as an abnormal birth, and the children as comparatively feebly organised or imperfectly developed, it may

TABLE XXVI.—Showing the Actual Fertility in Twins of Mothers of Various Ages.*

Ages . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59	Total.
Mothers . . .	1138	8550	10,346	7667	3617	1237	118	12	1	32,686
Mothers of Twins .	6	81	122	19	80	19	1	...	...	438
Proportion of latter to former is 1 in	189·6	105·5	84·8	59·4	45·2	65·1	...	...	...	74·6

* The numbers in this table are got by adding together the numbers given by Collins and those extracted by myself from the Registers of Edinburgh and Glasgow for 1855.

The following table shows the like facts ; only it is confined to the legitimate viable births in Edinburgh and Glasgow in 1855. The results also are similar, with this difference, that twin-births are more frequent in the first than in the second quinquennial period :—

TABLE XXVII.—Showing the Actual and Comparative Fertility in Twins of Wives-Mothers of Various Ages

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
Wives-Mothers	376	3688	5037	3850	2407	840	103	16,301
Wives-Mothers of Twins . .	3	28	46	58	52	11	...	198
Proportion of latter to former is 1 in	125	132	109	66	46	76	...	82

observations, to state the law, that from the earliest child-bearing period, till the age of forty is reached—that is, till a period when fecundity has become extraordinarily diminished—the fertility of mothers in twins gradually increases.

This twenty-sixth table, showing a fertility in twins gradually increasing with age, is almost exactly opposed to what our knowledge of the fecundity of women generally would lead us to expect. The general productiveness of a mass of wives is greatest at the commencement of the child-bearing period of life, and after that epoch gradually diminishes.

In the twenty-sixth and twenty-seventh tables twin-bearing mothers are compared with the whole mothers. I here produce a table of twin-bearing mothers compared with married women of corresponding ages. This twenty-eighth table is, in more respects than one, not very satisfactory. It cannot, I think, be expected to yield much. Yet its evidence is to the effect that when woman generally is most fecund she is least likely to produce twins. Between the ages of twenty and thirty years, fewest wives have twins. Before and after that period of high general fecundity the special productiveness in twins increases. And this result is in

be found that their frequency in first births confirms an opinion, held not without reason, that a first-born child (not in a twin-birth) is, in general, more feeble than its followers. A first pregnancy is short. A first child is of comparatively light weight. A first egg of a fowl is smaller than those which follow. (See pages 50 and 66.)

general accordance with what we have already shown regarding productiveness of twins—that is, it is the opposite of what we know of general fecundity ; moreover, it may find some special support from the evidence of the twenty-seventh table, and of the thirty-first.

TABLE XXVIII.—Showing the Fertility in Twins of Wives of Various Ages.*

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Wives	756	8874	14,622	14,579	11,871	10,506	7537
Wives-Mothers of Twins . .	3	28	46	58	52	11	
Proportion of latter to former is 1 in	252·0	316·9	317·8	251·3	228·3	955·1	

* The wives are estimated for Edinburgh and Glasgow for 1855. The mothers of twins are those of Edinburgh and Glasgow in 1855.

CHAPTER III.

INITIAL FERTILITY IN TWINS AT DIFFERENT AGES.

THE results arrived at in the former chapter are confirmed by a comparison of the initial fecundity of wives generally with the special initial fecundity in twins of the same women. The twenty-ninth table is unfortunately not large enough in numbers to afford results of a high degree of reliability. I believe that a more extensive collection will probably show a regular increase of initial productiveness of twins with increasing age. I leave it as it stands, showing that the wives married youngest have the fewest twins, and that there is an increase as age advances.

TABLE XXIX.—Showing Comparison between Wives newly married, and Mothers of Twins within Two Years of Marriage.*

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
No. of Wives newly married . . .	700	1835	1120	402	205	110	75	4447
No. of Mothers of Twins within two years of marriage	2	15	16	4	32	...	...	39
Proportion of latter to former is 1 in . .	350	122	70	100	102	...	...	114

* Here not only are the twins comparatively few, but also the numbers of wives are not real or actual, but estimated for Edinburgh and Glasgow in 1855. See page 26. The mothers of twins within two years of marriage are extracted from the Registers of Edinburgh and Glasgow for 1855.

Fertility in twins is better or more justly contrasted with the fertility of fertile women than with the fecundity of a mass of wives both sterile and fertile. In the former case all the women brought into comparison bear children, and thus show their fecundity and fertility, and their adaptation for comparison, while in the latter case women fertile in twins are (as in Table XXIX.) compared with both women who are fertile and with those who are sterile. In the thirtieth table I establish a comparison between two sets of fertile women, the one bearing single children, the other bearing twins. What does this table show? It remarkably confirms the law already stated as to the increase of twins as fertile women grow older. And there is here seen a regular increase up till the age of forty is reached. Every 153d woman among the youngest fertile women bears twins (within two years after marriage); among the older women, from thirty-five to forty years of age, every forty-second woman bears twins within two years after marriage, or nearly four times as many.

TABLE XXX.—Showing Comparison between Mothers within Two Years of Marriage, and Mothers of Twins within Two Years of Marriage.*

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
No. of Mothers within two years of marriage . . .	306	1661	849	253	84	17	2	3172
No. of Mothers of Twins within two years of marriage .	2	15	16	4	2	...	...	39
Propn. of latter to former is 1 in .	153	111	53	63	42	...	...	81

* In this Table the figures may with justice be regarded as the actual numbers (not estimated) in Edinburgh and Glasgow in 1855.

TABLE XXXI.—Showing Comparison between Mothers Two Years married and upwards, and Mothers of Twins Two Years married and upwards.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
No. of Mothers two years married and upwards . . .	70	2027	4188	3597	2323	823	101	13,129
No. of Mothers of Twins two years married and upwards .	1	13	30	54	50	11	...	159
Propn. of latter to former is 1 in .	70	156	139	66	46	75	...	82

I here interpolate another (thirty-first) table, which is supplementary to the thirtieth. It is composed of all mothers except those who have borne children within the first two years of marriage. Its evidence is to the same general effect as that of the preceding, only it seems to show the increased fertility in twins of the youngest women—a circumstance to which I have already called attention.

The especial value of these tables of initial fertility, and of subsequent fertility at various ages, lies in their eliminating entirely, or almost entirely, the disturbing element of the number of the pregnancy, and leaving evidence of the effects of age simply. In short, they contribute to the demonstration of the law of the influence of age stated in the second chapter of this part.

CHAPTER IV.

THE RELATION OF THE FREQUENCY OF TWINS TO THE NUMBER OF THE MOTHER'S PREGNANCY.

If it be true that the older a woman is who still retains a degree of fecundity, the more likely is she to bear twins, then we should, without further data, guess that twins were comparatively more frequent in late than in early pregnancies. And this is confirmed by an investigation of the subject. In the following table are given the number of children born in Edinburgh and Glasgow in 1855 in first and subsequent pregnancies, and beside them are placed for comparison the number of twins born in the same. A glance at the table shows that up to the ninth pregnancy, far beyond the average number of pregnancies, and as far as we have considerable numbers to guide us, the proportional frequency of twins increases with the number of the pregnancy. To this general statement there is an exception in the case of first pregnancies. Woman has apparently an increased chance of bearing twins in her first pregnancy, which leads to a disorder of the general rule above stated. With this notable exception, the rule holds manifestly good, at least till the ninth pregnancy is passed. After the ninth pregnancy, the table, from the smallness of the numbers and the irregularity

of the results, cannot be much relied on. Yet it shows, on an average, a still increasing fertility in twins as the number of the pregnancy advances. According to Table XXXII., among a thousand child-bearing women pregnant for a first, second, or third time, as these pregnancies happen to occur in our population, nine

TABLE XXXII.—Showing the Actual and Comparative Number of Twins born in First and Subsequent Pregnancies.*

No. of Pregnancy.	No. of Children.	No. of Twins.	Percentage of latter to former.
1st	3722	45	1·21
2d	2893	19	·66
3d	2534	24	·94
4th	1982	19	·96
5th	1543	19	1·23
6th	1221	18	1·47
7th	848	17	2·00
8th	641	14	2·18
9th	425	14	3·29
10th	222	3	1·35
11th	152	2	1·31
12th	61	3	4·92
13th	34		
14th	11		
15th	6		
16th	2	1	50·00

* It is necessary to note a source of error that exists in this table. In the third column are given the twins registered in 1855, and no other twins are supposed to have been in the family previously. The Registers only give the number of previous viable children, whether twins or not; they do not give the number of pregnancies. These are taken directly from the number of viable children.

only will produce twins; among a thousand similar women in fourth, fifth, and sixth pregnancies, there will be twelve twin births; among a thousand similar women in seventh, eighth, and ninth pregnancies, there will be twenty-four twin births; and among a thousand similar women in tenth, eleventh, and twelfth pregnancies, there will be twenty-five double births.

It may then be stated that, after passing the first pregnancy, a woman's chance of bearing twins increases with each subsequent pregnancy.

The broad statement, that multiparous women are more likely to have twins than primiparous, has been made by Chiari Braun and Spæth, and by Hugenberger. But this is only an imperfect development of the general statement just enunciated.

I have hitherto carefully abstained from giving this general statement the dignity of a law; for it may be only a coincidence resulting from the circumstance that age of mothers increases as the number of the pregnancy increases. The law of increased frequency of twins with advancing age may afford the explanation of the increased frequency as the number of the pregnancy advances. It remains to be determined, then, whether this general statement be a law or only a corollary to the law of age.

I now present a table (XXXIII.) which is so constructed from the data at my command as to avoid error from the influence of age, women of the same age, but of different pregnancies, being compared. In it various adjacent pregnancies and ages are thrown

TABLE XXXIII.—Showing the Comparative Frequency of Twins in Different Sets of Pregnancies of Wives of the Same Ages.

Mother's Age.	25 to 39.			30 to 34.			35 to 39.		
	No. of Children.	No. of Twins.	One in	No. of Children.	No. of Twins.	One in	No. of Children.	No. of Twins.	One in
Pregnancies 2d, 3d, and 4th .	3235	20	162	1628	19	86	568	9	63
Pregnancies 5th, 6th, and 7th	766	6	128	1568	27	58	993	17	58
Pregnancies 8th, 9th, and 10th	28	1	28	283	7	40	616	19	32

together in order to attain considerable numbers, with a view to reaching a trustworthy conclusion, and these collocations diminish the value of the table.

Yet an inspection of it will, I believe, convince the observer of the influence of the number of the pregnancy. And a comparison of this table with those previously given demonstrating the influence of age, leaves no doubt that the increase of twins with the number of the pregnancy is greater than could be accounted for by the error introduced by using quinquennial periods—that is, by the possible accumulation of the more advanced pregnancies in the last years of the quinquennial periods contrasted.

The increased frequency of twin-bearing as the number of the pregnancy increases may therefore be now regarded as a law of the production of twins.

In order to the more complete discussion of the influence of the number of the pregnancy on the frequency of twin births, I produce another table (XXXIV.) whose interesting results throw light on the subject.

The third column of this table shows how a hundred twin births are distributed according to the number of the pregnancy. It is evident that, speaking generally, twinning becomes rarer as the number of the pregnancy increases; and at the top of the list, far surpassing all the rest, is the first pregnancy with the large number of nearly 23 per cent of all twin births. Actually, then, twinning diminishes as the number of the pregnancy increases.

TABLE XXXIV.—Showing the Frequency of Twin Births and of all Births in First and Subsequent Pregnancies.*

Number of Birth in Family.	Number of Twin-bearing Mothers.	Percentage of Twin-bearing Mothers.	Percentage of Mothers.
1st	45	22·7	22·8
2d	19	9·6	17·7
3d	24	12·1	15·5
4th	19	9·6	12·1
5th	19	9·6	9·4
6th	18	9·1	7·4
7th	17	8·6	5·2
8th	14	7·1	3·9
9th	14	7·1	2·6
10th	3	1·5	1·3
11th	2	1·0	·9
12th	3	1·5	·3
13th			
14th			
15th			
16th	1	0·5	·01

But we must go farther into the matter, and we find that, as the number of the pregnancy increases, so the number of all births, single and plural, diminishes. The fourth column in Table XXXIV. shows how a hundred births are distributed among the pregnancies in some of which twins occurred. There is a regular diminution as the number of the pregnancy increases; and, in order to comprehend the relation of twin-bearing to child-bearing generally, this column must

* This table reads as follows:—22·7 per cent of twin-bearing mothers are bearing their first viable children; 22·8 per cent of all mothers are bearing their first viable children; and so on.

be contrasted with the preceding. Doing so, we find the first four pregnancies forming a contrast to the subsequent pregnancies. In the former, or first four pregnancies, the proportion of twinners in a hundred twinners is smaller than the proportion of child-bearers in a hundred: in the latter, or the fifth and subsequent pregnancies, the proportion of twinners in a hundred twinners surpasses the proportion of child-bearers in a hundred, and the preponderance goes on increasing from the fifth at least as far as the ninth pregnancy.

In this chapter I have shown that—

1. The actual number of twins born in different pregnancies decreases as the number of the pregnancy increases.

2. The comparative number of twins born in different pregnancies increases as the number of the pregnancy increases.

3. The increase of the comparative number of twins with the number of the pregnancy does not appear to hold good with the first pregnancy as compared with the three immediately subsequent pregnancies; women in their first pregnancies being more likely to bear twins than in those immediately subsequent.

CHAPTER V.

THE SIZE OF FAMILIES IN WHICH TWINS OCCUR.

It is very desirable to know what influence the having of twins has upon women's fertility. Do women having twins bear larger families than those never getting but one at a birth? To this question one naturally gives an affirmative answer.* Of course, if women's subsequent fertility be not affected by twin-bearing, there will be just an excess above ordinary families of a unit for every pair born in a family, and I daresay I am right in saying this is the popular belief. It receives some sort of support from the circumstance that twins are relatively more frequently additions to an already existing considerable family than they are either the first of a family or additional to an already existing small family. Moreover, twins occur with greater proportional frequency in elderly

* In this chapter no consideration is taken of the survival of the children forming the family. No doubt, twins in a family will diminish the chances of survival. Although it is probable that races may differ as to amount of twinning without difference in general fertility, it appears to me that twinning may come to be a good test of excessive fertility or of the imperfection of the children.

than in younger women, and are therefore certainly less likely, and perhaps less liable, to interfere with further or subsequent productiveness than if they occurred chiefly among the younger women.

To contribute to the solution of this interesting question, I have framed the following table (XXXV.) It also appears to support the affirmative response to the question just given. It shows that the average size of families of women married the same number of years is greater in the twin-bearing than in ordinary families, counting down to the birth of the twins. This is all that I can say in favour of the view or supposition that twin-bearing women have larger families than their neighbours. But the view is very far from being so demonstrated true.

TABLE XXXV.—SHOWING THE AVERAGE SIZE OF FAMILIES AFTER DIFFERENT DURATIONS OF MARRIAGE, IN MOTHERS GENERALLY, AND IN MOTHERS BEARING TWINS.

No. of Years married	Under 5.	5-9.	10-14.	15-19.	20-24.	25-29.
Average size of Families . .	1·699	3·940	6·063	7·967	9·868	13·075
Average size of Twin Families .	2·523	4·936	7·397	9·793	9·533	

The thirty-fifth table seems to me certainly to show that twinning has retrospectively no connection

* The mothers in this table are those only who continue fertile up till the different durations of marriage.

with a diminished degree of fertility of woman; that a woman destined to have twins at some future time is in the meantime as fertile as any other; that twinning does not occur to supplement pre-existing deficiency. So far from this being the case, the table shows that twin-bearing women have on the whole been already more fertile than their neighbours; that the twins come as additions to families already numbering above the average. For, keeping in mind that twins are comparatively most frequent in the latter pregnancies of women, it is easily seen that while, in the first column, that is among women under five years married, or in the earlier pregnancies of women, twinning does not add so much as a unit to the average size of the family in which the twins occur; in the next column, or among women from five to ten years married, twinning does add a unit; in the third and fourth columns, containing the greater proportional number of twin-bearers, the twinning adds more than a unit; or, in other words, the women, even if they did not have twins, have borne larger families than their neighbours. The table shows then that the great majority of women having twins are already more prolific than usual. The last column of women is evidently exceptional; it relates to women from twenty to twenty-four years married, and who have passed (as shown in other ways) their ninth pregnancy; and, as already shown, it is founded on a very narrow foundation of figures.

But all this is only nearly a complete demonstration

of the affirmative, that twin-bearers are more fertile than others. No doubt a twinner is in that birth more fertile than a uniparous woman. No doubt, as just shown, a twin-bearer is, counting up till the time of twinning, more fertile than a woman bearing one at a time. But the real question is not answered. Does a woman who has finished bearing a family, and has in that family had twins, produce more or fewer than a woman always uniparous? In other words, do twin-bearing women, who have shown all the fertility of their lives, produce larger or smaller families than women uniformly uniparous? This is the real question. To it the above-mentioned arguments do not supply a conclusive answer; and I regret to say I must leave it unanswered. Only I admit that the affirmative is probably the true answer. To procure a reliable solution, mothers must be compared who have borne their last children. I have no such data. Table XXXV. is not a table of women who have borne their last children. It carries the women of a population down only to their children born and registered in 1855.

In bringing this part to a conclusion, I may remark that the chief results of it appear to me to be well established by the evidence. Yet I cannot but feel that a larger accumulation of data would have added to their security and firmness.

It is interesting, first of all, to note that twin-bearing is not an accident, that it is subjected to laws

of which we now have a glimpse. A philosopher might have fancied twinning to be the result of the act of conception taking place on the rupture of a Graafian vesicle fortuitously containing a double ovum or two ova, or on the rupture of two Graafian vesicles accidentally matured simultaneously; in other words, that twinning was the result of some transcendental primordial energies in the ovary, whose products might be attributed to chance, because their origin was not only unknown but inscrutable by any known means of investigation. The data, as here arranged, compared, and reasoned upon, seem already to carry us so far as to remove twin-bearing from the category of the accidental, and to indicate to us laws of their occurrence which may be steps in the ladder of ascent to higher knowledge and wonderful discovery.

In the course of this part I have attempted to demonstrate the following conclusions:—

(1.) The largest number of twins is produced by women of from twenty-five to twenty-nine years of age; and on each side of this climax of fertility in twins there is a gradually increasing falling off in their number as age diminishes on the one side and increases on the other. (Table XXV.)

(2.) Twins are not regularly distributed among births generally; their production, therefore, is not subjected to the same laws as govern ordinary fertility.

3. The mean age of twin-bearing mothers is greater than that of mothers generally.

4. Twins increase in frequency as mothers become older (Table XXVI.) This forms a striking contrast to the fecundity of a mass of wives (not mothers), which diminishes as their age increases. It accords, however, with the law of intensity of fertility of fertile women.

5. Newly-married women are more likely to have twins the older they are. (Tables XXIX. and XXX.)

6. While the fecundity of the average individual increases with age till twenty-five is reached, and then gradually diminishes, there is some probability that the opposite is true, so far as regards twins alone, fertility in twins being greatest when fecundity is least, and *vice versa.* (Tables XXVII. XXVIII. XXXI.)

7. The actual number of twins born of a mass of women in different pregnancies decreases as the number of the pregnancy increases. (Table XXXII.)

8. The number of twins, relatively to the number of children born in different pregnancies, increases with the number of the pregnancy. In other words, a woman is more likely to have twins in each succeeding pregnancy than in the former pregnancy. The first pregnancy forms an exception to this rule. (Tables XXXII. XXXIII. and XXXIV.)

9. In an individual, twin-bearing is of course a sign of high fertility at the time. It also, in a mass of women, shows a high amount of fertility, at least

till the time of the birth of the twins. (Table XXXV.)

10. It is probable, though not proved, that twin-bearing women have larger families than women uniformly uniparous

PART IV.

ON THE LAWS OF THE FERTILITY OF WOMEN.

When concluding my account of fecundity, including the question of the age at which women are most likely to have children after marriage, I said that I could not advance farther without encroaching on another topic —viz. the fertility of marriage; or, as marriage is scarcely admissible as a term in physiology, the subject may be designated "sustained fecundity" or the laws of the fertility of women cohabiting with men during the child-bearing period of life. It is this subject which I now propose to enter upon. So far as I know, very little is ascertained in this department of physiology. The writings upon it are for the most part to be found in the works of political economists, and are chiefly confined to the single question of the rate of increase of a population under varying circumstances. To illustrate this topic, which is one of little interest to the physiologist, data are numerous and abundant. But when the writers referred to attempt to go deeper into the fundamental laws of the fertility of women, having very scanty materials and using them without care, they arrive at scanty results, which are either positively erroneous or of little value.

"The statistics," says Major Graham, registrar-

general for England, "of a country in which the age of a mother at marriage, and at the birth of her children, is not recorded, must always remain imperfect, and leave us without the means of solving some of the most important social questions."* These data were secured for the first year of the registrations in Scotland. The results to be now described are derived from a study of a part of these registers—namely, those of Edinburgh and Glasgow for 1855, and are founded on an analysis of 16,301 families of wives.

* *Registrar-General's Report* for 1845, p. xiv. (England.)

CHAPTER I.

THE FERTILITY OF THE WHOLE MARRIAGES IN A POPULATION.

On this subject much has been written, in latter times chiefly by Malthusians and anti-Malthusians, to whose works I refer generally. Elaborate comparisons are made between the fertilities of marriage in different countries; and there are exhibited variations to so great an extent, that they appear themselves to show the worthlessness of the data and of the comparisons instituted, at least in a physiological point of view. In illustration, I may refer to the variations described by M. Benoiston de Chateauneuf,* in a paper on the intensity of fecundity in Europe at the commencement of the nineteenth century. The highest figure is derived from some villages in Scotland, where there are asserted to be six or seven children to a marriage, while his lowest figure is 2·44, the alleged productiveness of some marriages in Paris.

We shall restrict our view to Great Britain; and we find the method, generally followed, of estimating the fertility of marriage, to be the very old and simple one of dividing the number of legitimate births in any year by the number of marriages. "In 1861," says

* *Annales des Sciences Naturelles*, tome ix. 1826.

Dr. Stark,* "for every marriage which occurred in Scotland there were born 4·64 legitimate children; that is to say, 464 legitimate children were born to every 100 marriages. During the same year, in England, only 3·89 legitimate children were born to every marriage, or 389 legitimate children to every 100 marriages." This is an exemplification of the ordinary method of calculating; and it is evident that the result derived is of not the slightest value as a contribution to the science of fertility. For, besides including marriages of all durations and at every fecund age, also second and third marriages, it includes many marriages at ages when fecundity has entirely disappeared. It is impossible, indeed, to state what is the exact relation between the number of marriages in a population in any year and the number of legitimate children born in the same year, with a view to any physiological result. This aspect of the statement is, however, well worthy of being pointed out, because authors of respectability, whom it is needless to name, refer to and use these figures as exhibiting the fertility of continued married life in England and Scotland. Malthus was well aware of the real meaning of these figures—of the fact that they merely show the relative frequency of marriage-ceremonies and births in a population. "The rule," he says,† "which has been here laid down, attempts to estimate the prolificness of

* *Seventh Detailed Annual Report for* 1861, published in 1865, p. xviii. (Scotland.)

† *Essay on the Principle of Population*, vol. ii. p. 6.

marriages, taken as they occur; but this prolificness should be carefully distinguished from the prolificness of first marriages and of married women, and still more from the natural prolificness of women in general, taken at the most favourable age. It is probable," he adds, "that the natural prolificness of women is nearly the same in most parts of the world; but the prolificness of marriages is liable to be affected by a variety of circumstances peculiar to each country, and particularly by the number of late marriages."

As a corollary from the preceding data, of value only in proportion to their value, it may be stated that the average duration of fertility in married women (including those who do not bear children) is about $7\frac{1}{2}$ years. For, as the intervals between marriage and the birth of a child, and between the births of successive children, is, on an average, 20 months, and as there are about $4\frac{1}{2}$ children to each marriage, we have about $7\frac{1}{2}$ years, counting from marriage, spent in producing that number.

British authors, as Graunt, Short, Malthus, Sadler, Senior, and those of later date, name 4, $4\frac{1}{2}$, or 5, as the fertility of marriage. Malthus, founding on such data, gives a wife four children produced within eight years, a statement which cannot be passed over without the obvious remark that Malthus, so calculating, utterly neglects the force of the wise words which we have just quoted from his work.

Making use of the Swedish returns, Major Graham

has, in his last annual report, published results obtained in a novel manner. I quote his words :— *

"The marriages in a calendar year give rise to births, which are registered year after year for 20 years. The births to the 167,723 marriages in the year 1859 could only be determined by following the families and counting all the children unto the end. The division of the sum of the children by the marriages would accurately express the fecundity, as it has been called, of marriages. If the annual marriages do not increase or decrease in number through a series of years, the division of the annual births by the annual marriages of the same years expresses the fecundity pretty accurately ; but the marriages in England are increasing rapidly ; consequently the 740,275 births registered in the year 1864 must be divided by the marriages of some earlier year to get an approximation to the fecundity. As the age of the mothers is unfortunately not recorded, the interval in England is unknown which intervenes between the mean age of marriage and the mean age of the mothers when their children are born ; otherwise that interval would indicate the calendar years with which the births of the year 1864 should be compared.

"But the interval in Sweden between the mean ages of mothers at marriage (25·8 years) and their mean age at the births of their children (31·7) is six years ; and the interval in England cannot differ much

* *Twenty-Seventh Annual Report of the Registrar-General* (England), p. xix.

from six years.* Hence, if the legitimate births of given years are divided by the marriages of six years' earlier date, the quotient will be the proportion of children to a marriage within close limits. In England the births thus determined to a marriage were 4·255, 4·301, 4·304, in the years 1862, 1863, and 1864. In Scotland the births in 1862 to the average marriages of six years' earlier date (1855, 1856, and 1857) were 4·694."

"For the present (says M. Husson) it cannot be concealed that the population, which is the primary wealth of civilised countries, and the principal power of great nations, is diminishing in France, or remaining stationary. Formerly the average was five children for each marriage. At the commencement of the present century there were more than four (4·20) for each

* The table published by Dr. Stark, in the *Eighth Detailed Annual Report* (Scotland), with a view to show the influence of cold on fecundity, seems to me to show that Major Graham's argument from Sweden to England is not well founded. The differences there shown to exist between the two countries are probably greater than Major Graham knew when he wrote the passage quoted above. The mean age of first marriages in England is 24·6 years for females (*Census of Great Britain for* 1851, vol. i. p. xxxi.) The mean age at the birth of children is, in Collins' collection, 27 years; in Edinburgh and Glasgow, in 1855, a little above 29 (see pages 6 and 10). The average age of wives bearing first children, in Edinburgh and Glasgow in 1855, is 24 years. In France the age of women at marriage is 26 years (*Annuaire du Bureau des Longitudes.* See *Pall Mall Gazette*, quoted in *Scotsman* of February 26, 1867). The average age of marriage of females in the city of Providence is 24·67 years (Snow's *Twelfth Annual Report*, 1867, p. 14).

legitimate union. Now it is the utmost if there are three throughout all France, and in Paris the average is only two."*

* *Lancet*, 1866, Nov. 10. Vol. ii. p. 528.

CHAPTER II.

FERTILITY OF THE WHOLE FERTILE MARRIAGES IN A POPULATION AT A GIVEN TIME.

I HAVE nothing satisfactory to offer as to prolific marriages, to contrast with the statements given concerning all marriages. Dr. Lever* says, that "the average number of children consequent upon a prolific (not every) marriage is shown to be rather more than $5\frac{3}{4}$, but not amounting to 6." This is given without any authority stated or evidence detailed, and I know not what value to ascribe to it. In a physiological point of view, its value must be scarcely appreciable; for no allowance is made for the duration of the marriage, nor for the age of the woman at the time of the ceremony.

In St. George's-in-the-East, London, the average number of children consequent on the prolific marriages was 5·33 to each marriage.† That is, 5·33 is the average number of children that has been born in all the families in a place at a given time. It tells

* *On Organic Diseases of the Uterus*, p. 5.

† *Quarterly Journal of the Statistical Society of London*, vol. xi. 1848, p. 235.

nothing concerning the average number in completed families, or in still growing families,* or in existing or still undispersed families.

Franklin says that the females in America have, "one with another, eight children to a marriage;" † almost certainly a great exaggeration, especially as he does not even state, as a condition, that the marriages included only the prolific.

* Some interesting facts regarding the fertility of Esquimaux women are to be found in Roberton's *Essays and Notes on Physiology and Diseases of Women*, p. 53.

† Sadler. *Law of Population*, vol. ii. p. 495.

For some remarks on the fertility of prostitutes, see Lasègue, *Archives Générales de Med.* November 1869.

For some remarks on the infertility of men of genius, see Galton, *Hereditary Genius*, p. 330.

CHAPTER III.

ANNUAL FERTILITY OF THE MARRIED WOMEN OF CHILD-BEARING AGE IN A POPULATION.

SEEING the inexactness of the statements of which those just given are an example, Dr. Stark has adopted another method of arriving at the comparative prolificness of marriages in England and Scotland. "In 1861," says he, "when the census was taken in England, the number of wives at the child-bearing ages—viz. 15 to 45—was 2,319,649; and as the number of legitimate children born during the year amounted to 652,249, this gives the proportion of one legitimate child for every 3·55 wives at the ages of 15 to 45 in the population; or, in other words, every 355 wives in England, at these ages, gave birth to 100 children during the year. In Scotland, during the same year, there were 305,524 wives between the ages of 15 and 45 years; and as 97,080 legitimate children were born during the year, this gives the proportion of one legitimate child for every 3·14 wives at these ages in the population; or, in other words, every 314 wives in the population of Scotland, at these ages, gave birth to 100 legitimate children during the year." *

* *Seventh Detailed Annual Report* (Scotland), p. xix.

While for every marriage in 1861 there were born in the same year in Scotland 4·64 legitimate children; every 3·15 wives between 15 and 45 in Scotland in the same year produced one legitimate child. Of 54,408 wives in Edinburgh and Glasgow in 1855 between 15 and 44 years of age inclusive, 16,290 bore children fit for registering; or, one child was born to every 3·3 wives aged from 15 to 44.

If we adopt these latter statements, we must take care to note that they do not give the fertility of the whole marriages in a population, as the older and former statements in Chapter I. do. These latter give the annual productiveness of a mass of married women in our populations. The results of the two methods of computing the fertility of marriage cannot be contrasted, for each is concerned with an entirely different topic from the other.

CHAPTER IV.

THE SIZE OF THE FAMILIES IN A POPULATION AT A GIVEN TIME.

In order to prevent confusion with calculations which are found in writings on population, I here insert an extract from Major Graham's writings on the subject of this chapter. Analysing a part of the English returns, he comes to the conclusion that the average number of children to an existing family is 2·26.*

"The number (says he) of children resident with their parents was 93,788; and there were 2·26 children on an average to each family, or 4·26 children and parents, including the father and mother, to each family of this class. Striking off the families consisting of husband and wife, sole, there remain 31,896 pairs, having with them at home 93,788 children; that is 2·94 children to a family, or 4·94 children and parents to a family. A fourth part of the families had four children or more at home, and these families of parents and children consisted of seven persons on an average."

* *Census of England and Wales*, 1861—*General Report*, vol. iii. p. xi.

CHAPTER V.

FERTILITY OF THE WHOLE MARRIAGES IN A POPULATION THAT ARE FERTILE AT A GIVEN TIME.

In Edinburgh and Glasgow in 1855 there were 16,393 wives who bore first or subsequent children. Of these the necessary data are given in 16,301 cases. These 16,301 mothers had produced 60,381 children; or 3·7 children constituted the average production of each mother. In other words, excluding the large class of wives sterile in 1855, we have 3·7 as the average number of children (surviving or not surviving) in each family that increased in 1855.

To compare with the above result, we may observe 16,414 women delivered in the Dublin Lying-in Hospital during Dr. Collins's mastership, who had borne 53,458 children, whose families, on an average, numbered 3·25; also 6634 women delivered in the same hospital during the period reported on by Drs. M'Clintock and Hardy, who had borne 20,680 children, whose families, on an average, numbered 3·12.

As there can be no doubt that these 16,301 families are a fair sample of all the growing families in Edinburgh and Glasgow, it appears that the average size of growing families existing at a particular time

in our population is between 3 and 4 ; and if it be true that, on an average, children are born with an interval not exceeding twenty months, then all mothers child-bearing at any particular time have been on an average less than seven years fertile. It is to be remarked that this statement concerns only the families of wives-mothers child-bearing at a particular time (*i.e.* in 1855), and is not to be compared with the corollary to Chapter I., which includes all families, and especially the mass of completed families.

The accompanying Table (XXXVI.) shows the data upon which these statements are founded. It, in addition, gives the percentage of children (surviving or not) in families of different numbers, that increased in 1855.

TABLE XXXVI.—Showing the Number and Percentage of Mothers bearing respectively 1st, 2d, and 3d Children, and so on ; also Percentage of Children in still growing Families of Different Numbers.

Number of Child.	Number of Wives-Mothers.	Percentage of Wives-Mothers.	Percentage of Children.
1	3722	22·83	6·16
2	2893	17·74	9·58
3	2534	15·54	12·59
4	1982	12·16	13·13
5	1543	9·46	12·77
6	1221	7·49	12·13
7	848	5·20	9·83
8	641	3·93	8·49
9	425	2·60	6·33
10	222	1·36	3·67
11	152	·93	2·77
12	61	·37	1·21
13	34	·20	·732
14	11	·06	·255
15	6	·03	·149
16	2	·01	·053
17	2	·01	·056
18	1	·006	·029
19	1	·006	·031

CHAPTER VI.

THE FERTILITY OF FERTILE MARRIAGES LASTING DURING THE WHOLE CHLD-BEARING PERIOD OF LIFE.

This subject may be stated in the form of a question: How many children does a fertile woman produce, living in wedlock from fifteen to forty-five years of age? The only collection of data known to me, which can throw light on this point, is that published in the "Report to the Council of the Statistical Society of London, from a Committee of its Fellows, appointed to make an investigation of the state of the poorer classes in St. George's-in-the-East."* In that district there were found eighty mothers married at ages varying from fifteen to nineteen, and who had lived in wedlock at least thirty-one years. These fertile wives, having lived nearly all the child-bearing period of life in wedlock, had borne on an average 9·12 children.

There are evident sources of inexactness in the above very limited data which tend to diminish the average fertility; and it will be as near the truth to state ten as the average fertility of fertile marriages lasting during the whole child-bearing period of life.

* *Quarterly Journal of the Statistical Society*, August 1848, vol. xi.

The conclusions given in further parts of this paper will show that the figure of ten children, for thirty years of child-bearing life, is not indicative of each mother having borne a child every third year. The fertility, while it lasts, will be shown to be much intenser than this. The average interval between births of successive children is hereafter shown to be twenty months, which gives about seventeen years as the average duration of fecundity in a fertile woman living in the married state all the child-bearing period of life.

In his work on *Abortion and Sterility* Dr. Whitehead gives no data which I can properly collate with those just given. After stating his belief that the actual duration of the child-bearing period in the female of this climate is about twenty years, he adds that a woman, under favourable circumstances, has in that period twelve children. But as this includes abortions and premature deliveries, which he estimates at $1\frac{1}{2}$ for each individual, the figure 12 has to undergo that reduction for comparison with 10, and the approximation is very close.*

Sadler states as a fact, "that marriages, on the average, are only fruitful for about a third part of the term of possible fecundity."† But he nowhere, so far as I know, affords any evidence of this statement, and I therefore attach to it no importance.

* On the Frequency of Abortion. See Hegar, *Monatsschrift für Geburtskunde.* Band xxi. 1863. Supplement Heft, S. 34.

† *Law of Population*, vol. ii. p. 276.

CHAPTER VII.

THE FERTILITY OF PERSISTENTLY FERTILE MARRIAGES LASTING DURING THE WHOLE CHILD-BEARING PERIOD OF LIFE.

THIS subject may also be conveniently stated in the form of a question: How many children does a fertile woman produce, living in wedlock from fifteen to forty-five years of age, and bearing children periodically up to the end of that time?

To this question I cannot give at once an answer founded on sufficient data; and I shall invert my usual mode of proceeding, stating the conclusion—namely, that fifteen at least is the average number of children borne by a persistently fertile female in thirty years—before giving the reasons for it. These are as follows:—A persistently fertile woman, at all ages, is found to have borne one child about every two years; the average fertility of fifteen mothers who have had each twenty-six years of persistently fertile life is thirteen; the thirty-ninth table, to be hereafter given, showing an excess of fertility on the part of those long persistently fertile, or bearing children in the year of counting, would give sixteen as the proportional fertility of thirty years of persistently fertile marriage,

calculating from the actual values given for the other results in the table. The deficiency of actual facts for settling this point is to be seen in the next Table (XXXVII.), where the number of women bearing children when above twenty-six years married is only seven.

On this subject Allen Thomson makes the following statement, which is remarkably accurate, seeing that it is apparently not founded on any analysis of documents. "A healthy woman," says he,* "bearing during the whole time, and with the common duration of interval, may have in all from twelve to sixteen children, but some have as many as eighteen or twenty."

The following extract from a daily newspaper may be inserted as curious histories or fables :—

Strange Recommendation for a Portrait.—Bronzino's celebrated portrait of Dianora Frescobaldi has unquestionably high merits as a work of art; but the high price which it fetched at the late sale of the San Donato collection (£600) was in a measure due to the inscription at its foot, which asserts that Dianora was the mother of "at least fifty-two children." She had never less than three at a birth, says the inscription, and we may add that there is a tradition in the Frescobaldi family that she once had six. Brand, in his *History of Newcastle*, mentions as a well-attested fact, that a weaver in Scotland had, by

* *Todd's Cyclopædia of Anatomy and Physiology*, vol. ii. p. 478.

one wife, sixty-two children, all of whom lived to be baptized; and in Aberconway Church may still be seen a monument to the memory of Nicholas Hooker (whom we might term the Injudicious), who was himself a forty-first child, and the father of twenty-seven children by one wife.

CHAPTER VIII.

FERTILITY OF PERSISTENTLY FERTILE WIVES AT DIFFERENT YEARS OF MARRIED LIFE.

The following Table (XXXVII.), from the 1855 Edinburgh and Glasgow data, gives at a glance the rate of yearly-increasing production of wives-mothers who are still fertile—that is, who produced a living child in the year of our census or counting. It is framed by adding together the whole children born of mothers having different durations of marriage, and dividing the sum by the number of mothers corresponding to each duration of marriage. The results will be found, on the whole, to tally pretty closely with those given in Table XLI. It is easy to account for the differences between the two tables. In the latter table the wives arrived at different numbers of progeny are collated and compared, while in the former the wives arrived at different durations of marriage are collated and compared. The table requires no further explanation; it is easily read.

TABLE XXXVII.—Showing the Average Number of Children that have been Born at the Completion of each Year of Persistently Fertile Marriage.

Duration of Marriage.	Number of Wives-Mothers.	Number of Children.	Average to each Mother.
	16,301	60,381	3·70
1 year married and under	3,172	3,336	1·06
2 years "	1,223	2,090	1·70
3 " "	1,540	3,195	2·07
4 " "	1,248	3,229	2·58
5 " "	1,193	3,645	3·05
6 " "	1,122	3,959	3·53
7 " "	870	3,414	3·92
8 " "	733	3,225	4·40
9 " "	719	3,447	4·79
10 " "	761	4,021	5·28
11 " "	624	3,502	5·61
12 " "	520	3,134	6·03
13 " "	441	2,878	6·53
14 " "	393	2,698	6·86
15 " "	372	2,659	7·15
16 " "	293	2,248	7·67
17 " "	240	1,918	7·99
18 " "	198	1,647	8·32
19 " "	177	1,541	8·71
20 " "	142	1,303	9·17
21 " "	115	1,116	9·70
22 " "	80	790	9·87
23 " "	56	557	9·95
24 " "	39	415	10·64
25 " "	8	95	11·87
26 " "	15	195	13·00
27 " "	2	25	12·50
28 " "	3	42	14·00
29 " "	1	14	14·00
30 " "	1	13	13·00

CHAPTER IX.

FERTILITY OF FERTILE WIVES AT DIFFERENT PERIODS OF MARRIED LIFE.

With a view to comparison with the results given in Table XXXVII., I have prepared the following Table (XXXVIII.) from the data of St. George's-in-the-East, already referred to. The circumstances in which these data were collected, and their paucity, do not justify me in ascribing to them a value equal to those given in Table XXXVII., nor do I think they are well adapted for the purpose of the comparison for which they are adduced. But I know no other to refer to.

As in the Report of the Committee of the Statistical Society, the periods are counted from the birth of the first child; I have added to them 17 months ($1\frac{5}{12}$ths year), the average interval between marriage and birth of a first child, with a view to make the table more easily contrasted with Table XXXVII.

The direct results of this table are given in the figures, and require no statement. But comparing it with the preceding table, we observe that, as is easily understood, the differences between the fertile and the persistently fertile increase as the duration of marriage increases; and that, while the numbers of the children

of fertile women are about a third of the years of duration of marriage, the numbers of the children of persistently fertile women are about a half of the years of duration of marriage. In other words, if these tables are at all trustworthy, we may guess that the number (surviving or not) of a fertile married woman's family is about a third of the number of years since her marriage. But if, in addition to knowing that the married woman has a family, we know that she has just had an addition to her family, then we may guess that the number of her family is about a half of the number of years since her marriage.

TABLE XXXVIII.—SHOWING, FROM THE DATA OF ST. GEORGE'S-IN-THE-EAST, THE FERTILITY OF FERTILE WIVES AGED FROM 15 TO 45 YEARS.

Years Married.	Mothers.	Children.	Average of each Mother.
$2\frac{5}{12}$	56	59	1·05
$3\frac{5}{12}$	60	88	1·46
$4\frac{5}{12}$	54	99	1·83
$5\frac{5}{12}$	66	184	2·79
$6\frac{5}{12}$	57	163	2·86
$7\frac{5}{12}$	60	196	3·26
$8\frac{5}{12}$	76	269	3·54
$11\frac{5}{12}$	254	1178	4·64
$16\frac{5}{12}$	215	1319	6·13
$21\frac{5}{12}$	148	1075	7·26
$26\frac{5}{12}$	44	353	8·02

From the same London data I have also framed the following table, without doing any apparent violence to them, and with a result which is extremely interesting. The student will observe that beside the

data from St. George's-in-the-East I have placed corresponding data extracted from the Edinburgh and Glasgow registers of 1855. The comparison of the fertility of a set of fertile wives—that is, all wives who have borne children some time during their still-continuing married lives—with that of a set of persistently fertile wives—that is, exclusively of wives bearing at the ends of the periods under consideration (that is, in this table, the end of their child-bearing lives)—is, as already said, marred and loses value on account of the two sets being of very different numbers, different localities, and different populations. Taking it as it stands, we find that fertile women generally, living with husbands for sixteen years before the conclusion of child-bearing life, have an average family of about $4\frac{1}{2}$; while persistently fertile wives—that is, wives bearing children at the end of their child-bearing lives—have an average family of $11\frac{1}{2}$. While fertile wives, married twenty-one years, before and up to the age of forty-five, have an average family of about 6; persistently fertile wives have an average family of $10\frac{1}{2}$. While fertile wives married for twenty-six years, before and up to the age of forty-five, have an average family of 8; persistently fertile wives, in the same circumstances, have an average family of about 14. While fertile wives, married for thirty-one years, before and up to the age of forty-five years, have an average family of 9; persistently fertile wives, in the same circumstances, have an average family which may be estimated at 16.

TABLE XXXIX.—Showing a Comparison of the Fertility of Mothers and of persistently Fertile Mothers.

Age at Marriage.	Duration of Marriage.	St. George's-in-the-East. Wives-Mothers. Number of Mothers.	St. George's-in-the-East. Wives-Mothers. Number of Children.	St. George's-in-the-East. Wives-Mothers. Average Fertility of each Mother.	Edinburgh and Glasgow in 1855. Wives-Mothers bearing Children at the end of Child-bearing Life. Number of Mothers.	Edinburgh and Glasgow in 1855. Wives-Mothers bearing Children at the end of Child-bearing Life. Number of Children.	Edinburgh and Glasgow in 1855. Wives-Mothers bearing Children at the end of Child-bearing Life. Average Fertility of each Mother.
15-19	At least 31 years	80	730	9·12	...	...	16
20-24	At least 26 years	179	1418	7·92	6	83	13·83
25-29	At least 21 years	100	630	6·30	7	74	10·57
30-34	At least 16 years	25	115	4·60	4	46	11·50

In this Table (XXXIX.) it will be observed that the differences between the fertile and the persistently fertile are much greater than in the former (XXXVII. and XXXVIII.), a circumstance which is easily explained. For, in the latter, all the women have been long married, and the persistently fertile have had time to far outrun the average fertility of all the fertile. It must also be noted that all the women in the table are fertile at or near the end of the child-bearing period, a time at which, it will be hereafter shown, the intensity of fertility is greater than at any other.

CHAPTER X.

DEGREES OF FERTILITY OF WIVES-MOTHERS OF FAMILIES OF DIFFERENT NUMBERS.

Under this head the first question that raises itself relates to the interval between marriage and the birth of the first child. In Table XL. this question is found fully answered. In fertile marriages generally there intervene about 17 months (1·38 year) between the ceremony and the birth of the first child. But in women of all ages this interval is far from being identical. As age increases above 25 years, the interval increases; the birth of a living child is longer deferred. The table does not confirm this statement for wives married at 40 and upwards; but this is almost certainly a mere result of the paucity of the data at these ages. The whole tenor of the table confirms the law of greatest fecundity according to age, meaning by fecundity likelihood of having children. For it is observed that not only are wives most fecund from 20 to 24, but also that they begin the career of fertility sooner than their younger or elder sisters.

TABLE XL.—Showing the Interval between Marriage and the Birth of a First Child in Wives married at Different Ages.

		Mother's Age at Marriage.							Total.
		15–19.	20–24.	25–29.	30–34.	35–39.	40–44.	45–49.	
Years married.	Less	94	325	126	44	15	4	...	608
	1	409	1259	533	135	49	3	2	2390
	2	83	202	88	45	17	2	...	437
	3	25	50	35	12	10	1	...	133
	4	8	31	13	8	1	...	...	61
	5	13	10	3	3	3	...	...	32
	6	5	14	6	1	1	...	...	27
	7	5	3	1	3	...	...	...	12
	8	1	3	1	...	...	...	...	5
	9	2	3	...	...	...	...	...	5
	10	...	1	...	...	...	...	...	1
	11	...	1	2	...	...	...	...	3
	12	2	1	1	...	...	...	...	4
	13	1	1	...	...	...	...	...	2
	14								
	15	1	...	...	...	...	...	...	1
	16								
	17								
	18	...	1	...	...	...	...	...	1
	Total	649	1905	809	251	96	10	2	3722
Average interval between Marriage and Birth of first Child.	Year.	1·516	1·329	1·350	1·510	1·594	1·400	1·000	1·385
		or	or	or	or	or	or	or	or
	Months.	18·2	15·9	16·2	18·1	19·1	16·8	12·0	16·6

It is noteworthy, that while the average interval between marriage and the birth of the first child is seventeen months, the average interval between the births of successive children, however numerous, is a little under twenty months; the two intervals approxi-

mating one another so closely as to destroy all probability of the truth of the explanations usually offered for the delay of impregnation after a recent childbirth, and of the efficacy of continued lactation in retarding the occurrence of a new conception. And we shall soon see, in a quotation from Sadler, that he finds that women who do not suckle their offspring have as long an interval between conceptions as others. But, while Sadler by this demonstration destroys the only physiological foundation for his invective against the rich who do not suckle, he nevertheless proceeds enthusiastically, as if the dictum of physiologists were valid, even after their argument was ruined.

Speaking of the interval between marriage and a first birth, Sadler gives the following indefinite statement:—"Married females do not become fruitful, on the average, during the first year of their nuptials, but nearly so. A great number of cases which I have collected, with a view of determining this point, give three-fourths of them as producing their first child at the average of one year after marriage." *

Whitehead,† founding on the observation of 541 married women of the average age of twenty-two years, makes out the average interval between marriage and the birth of a first child to be 11½ months.

Quetelet‡ admits, with sufficient probability, as an average term, that the birth of the first-born takes place

* *The Law of Population*, vol. ii. p. 30.

† *On Abortion and Sterility*, p. 242.

‡ *Treatise on Man*, p. 15.

within the first year which follows marriage. His error, as those of the others, depends upon the acknowledged want of documents.

"Ainsi (dit M. Courty*), mème chez les femmes fécondes ou aptes à le devenir, l'aptitude à l'impregnation ne se développe ou ne se révèle qu' après une pratique suffisante de la copulation. D'après M. Spencer Wells, sur 7 mariages féconds, l'accouchement ne survient que 4 fois avant 18 mois de mariage. D'après M. Puech, sur 10 mariages féconds, l'accouchement survient 5 fois au bout de la première année, 4 fois au bout de la seconde, une fois au bout de la troisième."

* *Traité Pratique des Maladies de l'Utérus*, 1866, p. 1014. On this subject consult also M. Villermé, *Annales d'Hygiène Publique*, etc., tome i. p. 86: 1831.

TABLE XLI.—SHOWING THE AVERAGE DURATION OF MARRIAGE AT BIRTH OF EACH SUCCESSIVE CHILD; AND THE AVERAGE INTERVAL BETWEEN THE BIRTHS OF THE SUCCESSIVE CHILDREN.*

Number of Children.	Number of Mothers.	Duration of Marriage in Months.	Average interval between successive Births.
1	3722	17	17·0
2	2893	38	19·0
3	2534	64	21·3
4	1982	90	22·5
5	1543	115	23·0
6	1221	137	22·8
7	848	162	23·1
8	641	181	22·6
9	425	203	22·5
10	222	225	22·5
11	152	235	21·4
12	61	246	20·5
13	34	263	20·2
14	11	281	20·1
15	6	280	18·7
16	2	336	21·0
17	2	252	14·8
18	1	252	14·0
19	1	204	10·7
		Average	19·9

* This is not a correct statement of the contents of this table. This last column does not directly give the average interval between the births of successive children, but the average interval between marriage and the birth of the child, divided by the number of the children born. For brevity's sake, the title is left as it stands.

It next comes to be inquired at what rate children succeed each other in families. This interesting topic is developed from the data given in Table XLI. It is formed by dividing the whole years of duration of sets of marriages, of different durations, by the number of children born in the corresponding marriages; and it must be remembered that as our data all spring from women who were fertile on the year of our census or counting, no women are included who, although fertile formerly, have now ceased to be so; and it is evident that, for the purposes of our argument, this is just.

The first conclusions deducible from the data are—

1. That the mass of early or first children, up to the third or fourth, come into the world in more quick succession than those that immediately follow.

2. That a mass of children, numbering from the fourth or fifth on to the tenth, succeed one another more slowly than those of the first category, and of the third.

3. That a mass of children, following the tenth, come into the world hurrying after one another with a gradually-increasing rapidity, which excels that of all their predecessors (a circumstance which may, in part at least, account for the great mortality of women bearing children after the ninth).*

While all these propositions are true of a large number of children, it must not be supposed that they

* *Edinburgh Medical Journal*, September 1865, p. 209; and Part VII. of this volume.

directly indicate laws regulating the fertility of women. But the table bears important information relative to this last topic. And it appears to me that the first of the three conclusions given above can be explained only by supposing what may therefore be held as equally well demonstrated—

(1.) That wives bearing their early children, up to the third or fourth, breed more rapidly than they subsequently do.

For the average fertility of all wives is at least 4 children; and the great mass of fertile wives is therefore included in the calculation. All the wives destined to bear large families, and furnish data for the second and third conclusions, are included in the data for first 4 children. The mass of children born in families numbering 11 and more is not large enough to have great influence on the data, should it be the case that they are proportionately very quick breeders from the first.

If we now regard the mothers whose children have afforded the data for the second conclusion as to the rapidity of the succession of a mass of children, we shall have, I think, no difficulty in accepting the proposition—

(2.) That wives produce their children, numbering from the third or fourth on to the tenth, at greater intervals than their earlier progeny.

For, in the calculations, the earlier and more rapidly-succeeding progeny are included, and have their full influence, and diminish the periods given in

the table opposite children numbering from 4 to 10, reducing them below what they would be were pregnancies from 4 to 10 alone counted, exclusive of those from 1 to 4.

Regarding now the mothers of families numbering 11 or more, it is evident that their paucity, though not such as to destroy all their value, is such as to prevent their having a paramount influence upon the figures of the two preceding categories. It might therefore appear necessary to leave undecided whether their specially rapid bearing were a consequence of their great fertility, and therefore an acquired or secondary rapidity, or were an original condition true of even their earlier pregnancies. That the latter is to be accepted to the exclusion of the former supposition is evident, if we observe that the married life of the women with families above 10 is not long enough to admit of their having gone through the series of lengths of pregnancies given in the table opposite each successive child. It is thus shown—

(3.) That wives bearing more than 10 children, or wives bearing very large families, breed more rapidly than others during their whole child-bearing lives.

Wives, therefore, who bear numerous progeny, do so in virtue of two differences from other women. They bear their children more rapidly, and they continue fertile longer than their neighbours.

Were the third conclusion just given not before us, it might be supposed that the rapid bearing of earlier children was a result of youth and vigour. This sup-

position is not only inconsistent with the third conclusion, but with the law to be hereafter demonstrated, that the oldest women, who are continuedly fertile, bear children more rapidly than any other.

The average length of interval between all successive children is (19·9) nearly 20 months.

I have frequently heard it said that a fertile woman bears a child every two years. Some authors have made careful statements on this point. Whitehead * says that fertile women produce children every 20 months; but "this includes abortions, false conceptions, so-called premature deliveries, and all having an unsuccessful issue, the average amount of which will be rather more than $1\frac{1}{2}$ for each individual." Sir William Petty long ago laid it down that "every teeming woman can bear a child once in two years." Malthus † adopts the same period, and refers to the *Statistical Account of Scotland* as confirming it. The number and exactness, however, of the data here adduced, and the circumstance that they include only children born alive (excluding still-born and abortions), leave no room for doubt that all the authors referred to under-estimate the rate at which married women bring children into the world. ‡

* *On Abortion and Sterility*, p. 245.

† *An Essay on the Principle of Population*, vol. ii. p. 3.

‡ See also Roberton's *Essays and Notes on the Physiology of Women*, p. 185. His conclusions (p. 193) are as follow :—

"The *first* corollary which I would draw from the facts collected in Manchester and in York, is, that in 7 out of 8 women

On this point Sadler is so full and distinct that I quote his words:—"The interval of time," says he, "at which the fruitful couples produce their children, calculated from the period of their marriage to the birth of their last child, including the greater prolificness of the first year, exceeds 2 years. It extends to between $2\frac{1}{4}$ and $2\frac{1}{2}$ years, if calculated from the first birth."* In this calculation, as in that of the interval between marriage and the birth of a first child, Sadler evidently errs, making the former too long and the latter too short. For both he gives no data; yet, in regard to the interval between the births of successive children he says:—"All the tables are constructed upon the presumption of its certainty, and happily it is one which, on this very debatable question, has never been made the subject of controversy, and which does not admit of it. Nothing," he continues, "is more certain or better ascertained than the average period at which the human female, in a state of prolificness, repro-

who suckle for as long a period as the working classes in this country are in the habit of doing, there will elapse an interval of from 12 to 15 months from parturition to the commencement of the subsequent pregnancy.

"*Second*, That in a majority of instances, when suckling is prolonged to even 19 or 20 months, pregnancy does not take place till after weaning.

"*Third*, That lactation having this influence on the generative function, we are warranted in regarding the secretion of milk as the cause which regulates the periods of conception in mankind, as instinct operates to the same end in graminivorous quadrupeds, and probably in all other animals."

* Vol. ii. p. 30.

duces. Were we, indeed, to form our general rules from particular exceptions, we should in this, as in all other cases, be grievously misled. We might conclude, for instance, that she would continue to multiply within the year; but general computations will rectify any such error, and conduct us to conclusions which are not only reconcilable with philosophy and truth, but resolvable into the ordinations of a merciful Providence. The human mother has to feed her infant for a period pretty nearly corresponding in length to that of gestation (I speak now as regards the necessity of the great mass of the community, with whom the question evidently rests); nature, therefore, has kindly ordained, as a general rule, that the period of impregnation shall be postponed till that essential duty is discharged, and for a period somewhat beyond it; and he must be ignorant indeed who does not see most clearly that the health, and indeed frequently the existence, of both mother and offspring are secured by this physical regulation of the common parent of mankind. The human being, in reference to the term of existence, multiplies later, and at longer intervals, and ceases to be prolific sooner, than any other animated being with whom we are acquainted; hence we find, on the average, that in the maternal state, during its period of fruitfulness, the births are not so frequent as once in two years. Even in the rank of society which is absolved from the necessity (though not from the duty) of fulfilling one of the most important of the maternal offices, that of feeding from their own bosoms their infant offspring, and who

too often avail themselves of that unnatural immunity, consequently removing what our physiologists regard as one of the physical impediments to an accelerated prolificness,*—even in this rank, I find the births are at intervals of about, but rather exceeding, two years. That period, therefore, as it respects the mass of the community, who are differently circumstanced in this respect, cannot be shorter. But arguments and proofs on this point are unnecessary, no writer having ever ventured upon supposing a shorter period than two years possible; and even Sir William Petty, when labouring to prove the possibility of a doubling every ten years for a century after the flood, amongst his other suppositions, so extravagant if applied to the present era, only lays it down that every teeming woman can bear a child 'once in two years.'"

* On this subject the work of Roberton, already cited, may be consulted; also a paper by Professor Laycock, quoted by Roberton.

CHAPTER XI.

FERTILITY OF WIVES-MOTHERS MARRIED AT DIFFERENT AGES.

BEFORE discussing this and the next topics, it is necessary to remark that fertility may be maintained in degree in two ways—either by long continuance, or by intensity while it lasts. At present I omit entirely the consideration of the intensity of fertility while it lasts, taking up this in the next Part. But I shall show that, of a mass of fertile women, the younger are on the whole more fertile than the older. To demonstrate this I first adduce a table drawn from the data of

TABLE XLII.—SHOWING THE FERTILITY OF WIVES-MOTHERS MARRIED AT DIFFERENT AGES, FROM THE DATA OF ST. GEORGE'S-IN-THE-EAST.

Mother's Age at Marriage.	$11\frac{5}{12}$ Years married. Average Number of Children.	$21\frac{5}{12}$ Years married. Average Number of Children.
15-19	5·0	7·7
20-24	4·5	7·0
25-29	4·4	6·4
30-34	3·4	3·0

St. George's-in-the-East. It is evident here that the

younger women 11 years married, and also those 21 years married, have on an average larger families than the elder, of whatever respective ages. It must be observed that the table includes all wives who, in a small selected population, have shown any fertility; and it must be added that the committee of the Statistical Society have enunciated the same conclusion. I quote their own words:—"The following abstract will show the average number of children to each marriage, at the respective periods of 10, 20, 30, and 40 years after the birth of the first child, for each class of marriages formed at the four different quinquennial periods of life:—

TABLE XLIII.

Years elapsed since Birth of first Child.	Average number of Children to each Marriage formed at Ages			
	16-20	21-25	26-30	31-35
10	5·05	4·51	4·42	3·44
20	7·68	7·01	6·43	3·00
30	8·41	7·89	6·80	7·00
40	10·85	8·24	5·00	4·00

"It is thus obvious that marriages formed under the age of twenty-five are more prolific than those formed after that age, and that those formed between sixteen and twenty years of age are still more so than those at any of the superior ages."*

* *Journal of the Statistical Society of London*, vol. xi. p. 223.

As doctrine is still taught exactly the opposite of that here sustained, it is important to establish the latter, if possible, by further proof. At another place I shall show the erroneous interpretation of the data which have been adduced in support of the opposite doctrine—namely, that marriages formed late in life are more prolific than those formed earlier.

The figures now to be adduced not only confirm the doctrine that early marriages are more fruitful than late marriages; they also explain it, showing that the younger married have a longer continuance of fertility than the older married, allowing to both the same duration of marriage, and all within the child-bearing period of life. So far as the demonstration has hitherto gone, we have shown that the younger are more fertile than the elder; that, excluding those who have no children, the younger will bear larger families than the elder. We have not shown which bear their children most rapidly—that is, which have the greatest intensity of fertility while it lasts—leaving this topic for another chapter. We now proceed to show that, among the fertile, the younger have a longer continuance of fertility than the elder. It is this last circumstance which accounts for the greater fertility of the marriages of the younger. The following table demonstrates this. It needs no explanation. The details are given in the footnote.*

* The Table XLIV. may be easily seen to be made up from the following five tables—XLV. XLVI. XLVII. XLVIII. XLIX. In these five tables of the fertility of married life at different

TABLE XLIV.—Showing the Amount of Continuance in Fertility of Wives married at Various Ages (as shown within Twelve Months.)

Age of Mother at Marriage	15-19	20-24	25-29	30-34	35-39	Total.
The number Child-bearing in the 5th year of Married Life is 1 in	2·6	2·7	4·1	4·9	10·5	3·2
The number Child-bearing in the 10th year of Married Life is 1 in	3·2	4·0	5·9	8·7	...	4·4
The number Child-bearing in the 15th year of Married Life is 1 in	4·6	6·8	18·2	37·4	...	8·0
The number Child-bearing in the 20th year of Married Life is 1 in	8·5	14·6	129·8	...	...	16·3
The number Child-bearing in the 25th year of Married Life is 1 in	68·0	480·5	...	...	...	171·0

epochs, the number of wives-mothers at the respective epochs is the actual registered number in Edinburgh and Glasgow in 1855. The number of wives of different ages is got by estimating, and

In order to derive from Table XLIV. more information as to the relative numerical value of the fertility of a mass of wives in the fifth, tenth, and fifteenth years of married life, and so on, I have framed the following Table (LVI.) I have freely pointed out

the Carlisle Table of Mortality is used. The estimate is not made in the exactest way, but the errors will not injure the comparison of the figures with one another, as the same (perhaps unavoidable) error is introduced into all. The results probably give a near approach to the true degrees of fertility; for, while among the child-bearing there are some omitted, there are probably fewer marriages omitted, and the number of wives as estimated would be too large were not a very high percentage taken off (1 in 100) for the special mortality of first confinements. (See Part VII. of this volume, and Dr. Stark's Report in the *Seventh Annual Report of the Registrar-General for Scotland*, p. xxxii.)

To find how many women, 5, 10, and 15 years married, are alive and not widowed in 1855, it would strictly be necessary to have the numbers married in 1850, 1845, and 1840, from which the estimates should be made. Instead of doing this, I have estimated from the number married in 1855. As the population is increasing not greatly, this error thus introduced will not be great.

It is partly with a view to correct this error that I have taken off an extravagantly high percentage for the mortality of first labours.

In making the estimate I have doubled the mortality in order to exclude the widowed.

TABLE XLV.—Fertility of Wives in the Fifth Year of Married Life.

Ages at Child-bearing.....	20-24	25-29	30-34	35-39	40-44	Total.
Number of Wives	644	1686	1008	358	179	3875
Number of Wives-Mothers ...	247	611	244	72	17	1191
Number Child-bearing, 1 in...	2·6	2·7	4·1	4·9	10·5	3·2

the sources of error in the fundamental figures of Table XLIV.; and after all I flatter myself that in these fundamental figures there is an approach to truth such as to justify the further deduction of Table LVI.; only it is necessary to mention that in this table there

TABLE XLVI.—Fertility of Wives in the Tenth Year of Married Life.

Ages at Child-bearing.....	25-29	30-34	35-39	40-44	Total.
Number of Wives	594	1528	902	313	3337
Number of Wives-Mothers.....	186	381	153	36	756
Number Child-bearing, 1 in	3·2	4·0	5·9	8·7	4·4

TABLE XLVII.—Fertility of Wives in the Fifteenth Year of Married Life.

Ages at Child-bearing.....	30-34	35-39	40-44	45-49	Total.
Number of Wives	532	1360	782	262	2936
Number of Wives-Mothers	116	200	43	7	366
Number Child-bearing, 1 in	4·6	6·8	18·2	37·4	8·0

TABLE XLVIII.—Fertility of Wives in the Twentieth Year of Married Life.

Ages at Child-bearing	35-39	40-44	45-49	Total.
Number of Wives.....	477	1171	649	2297
Number of Wives-Mothers.....	56	80	5	141
Number Child-bearing, 1 in	8·5	14·6	129·8	16·3

TABLE XLIX.—Fertility of Wives in the Twenty-fifth Year of Married Life.

Ages at Child-bearing	40-44	45-49	Total.
Number of Wives	408	961	1396
Number of Wives-Mothers	6	2	8
Number Child-bearing, 1 in	68·0	480·5	171·0

are no actual values to keep it close to the truth. Taking, then, Table XLIV. as giving actual values, we have the fertilities for 1855; or for 12 months. But as 20 months has been shown to be the average time-

I have now to add six tables, L. to LV. inclusive. These are constructed with a view to meet what might form a reasonable criticism on the six preceding, XLIV. to XLIX. inclusive. In these latter are included all the married—that is, the fertile and sterile. Now, the amount of sterility varies in marriages at different ages, and it may be considered desirable to eliminate this source of difference in order to have a view of the duration of fertility in those married women who are fertile. The tables of sterility hereafter given afford means of estimating the proportion sterile in marriages at different ages. By this means the tables L. to LV. are constructed. They give a view of the duration of the fertility of fertile women married at different ages.

TABLE L.—Showing the Duration of Fertility in Fertile Wives married at Various Ages (as shown within Twelve Months).

Age of Mother at Marriage.....	15-19	20-24	25-29	30-34	35-39	Total.
The number Child-bearing in the 5th year of Married Life is 1 in	2·4	2·7	3·0	3·1	4·9	2·8
The number Child-bearing in the 10th year of Married Life is 1 in	3·0	4·0	4·2	5·4	...	3·9
The number Child-bearing in the 15th Year of Married Life is 1 in	4·2	6·8	13·1	23·3	...	7·0
The number Child-bearing in the 20th year of Married Life is 1 in	7·9	14·6	95·8	...	...	14·7
The number Child-bearing in the 25th year of Married Life is 1 in	63·0	480·5	...	...	...	167·4

unit of fertility, the fertilities of 1855 must be increased in like proportion; for as 12 is to 20 so are the fertilities given in Table XLIV. to the real fertilities. All the fertile women cannot be presumed to have shown that quality in 12 months, but all may be presumed to have shown it in 20 months. In this way the following table (LVI.) may be held as an estimate of the

TABLE LI.—Fertility of Fertile Wives in Fifth Year of Married Life.

Ages at Child-bearing	20-24	25-29	30-34	35-39	40-44	Total.
Number of Wives.....	644	1686	1008	358	179	3875
Number of Wives-Mothers...	597	1686	728	223	84	3318
Number Child-bearing in 5th year of Married Life	247	611	244	72	17	1191
Or of Wives-Mothers, 1 in ...	2·4	2·7	3·0	3·1	4·9	2·8

TABLE LII.—Fertility of Fertile Wives in Tenth Year of Married Life.

Ages at Child-bearing	25-29	30-34	35-39	40-44	Total.
Number of Wives	594	1528	902	313	3337
Number of Wives-Mothers	551	1528	650	195	2924
Number Child-bearing in 10th year of Married Life	186	381	153	36	756
Or of Wives-Mothers, 1 in	3·0	4·0	4·2	5·4	3·9

TABLE LIII.—Fertility of Fertile Wives in Fifteenth Year of Married Life.

Ages at Child-bearing	30-34	35-39	40-44	45-49	Total.
Number of Wives	532	1360	782	262	2936
Number of Wives-Mothers	493	1360	565	163	2581
Number Child-bearing in 15th year of Married Life	116	200	43	7	366
Or of Wives-Mothers, 1 in	4·2	6·8	13·1	23·3	7·0

comparative amount of fertility in living children, shown by wives at different epochs of married life.

The table shows a gradually-diminishing amount of perseverance in fertility as age advances. In illustration of the mode of reading it, I may state that about a half of all wives are fertile at the fifth year of married life; more than a third are fertile at the tenth year of married life; and only a fifth part of the whole wives arrived at the fifteenth year of married life are fertile; and so on.

Another interesting result is got from this Table (LVI.), by comparing the different horizontal columns with one another. Reading the figures of adjacent columns obliquely from below upwards, we have a comparison of the fertility of a mass of wives of the

TABLE LIV.—Fertility of Fertile Wives in Twentieth Year of Married Life.

Ages at Child-bearing	35-39	40-44	45-49	Total.
Number of Wives	477	1171	649	2297
Number of Wives-Mothers	442	1171	469	2082
Number Child-bearing in 20th year of Married Life	56	80	5	141
Or of Wives-Mothers, 1 in	7·9	14·6	95·8	14·7

TABLE LV.—Fertility of Fertile Wives in Twenty-fifth Year of Married Life.

Ages at Child-bearing	40-44	45-49	Total.
Number of Wives	408	961	1396
Number of Wives-Mothers	378	961	1339
Number Child-bearing in 25th year of Married Life	6	2	8
Or of Wives-Mothers, 1 in	63·0	480·5	167·4

same age, but of quinquennial differences of duration of marriage. And it is very interesting to observe that the younger married closely approach in fertility those married five years later in life, both being arrived at the same year of life at the time of the comparison.

Short and Sussmilch maintain that early marriages are not favourable to the population. But, so far as I know, they adduce no satisfactory evidence whatever for their belief. Yet they have considerable authority on their side, including the redoubtable Sadler, who arrays in his support the venerable names of Aristotle, of Plato, of Virgil, and of Plutarch.

It is to be remarked that I here object to this statement of these authors only so far as the number of living births is concerned, and I do not consider the diminished chances of survival which children of very early marriages are believed to have. There can be, in my opinion, no doubt that early marriages are most favourable to the population;* and as I have already shown that wives under twenty are less fecund than those from twenty on to at least twenty-four years of age,† the fertility of the younger, as a mass, is the more striking. But although most highly fertile as a mass, the number of sterile among those married under twenty years of age is not inconsiderable, and it is probably this amount of sterility which, while satisfactory statistical evidence was deficient, has given

* See Chapter XIV. of this Part, farther on.

† *Transactions of the Royal Society*, 1864, or Table XIV. in this volume, p. 35.

rise to the error now commented upon. The authors referred to give no definition of what they mean by

TABLE LVI.—Showing the Probable Amount of Continuance in Fertility, at Different Epochs, of a Mass of Wives married at Various Ages.* (*See next page.*)

Age of Mother at Marriage . . .	15-19	20-24	25-29	30-34	35-39	Total.
The proportion Child-bearing about the 5th year of Married Life is 1 in . . .	1·56	1·62	2·46	2·94	6·30	1·92
Or a percentage of	64·1	61·7	40·6	34·0	15·9	52·1
The proportion Child-bearing about the 10th year of Married Life is 1 in . . .	1·92	2·40	3 54	5·22	...	2·64
Or a percentage of	52·1	41·7	28·2	19·2	...	37·9
The proportion Child-bearing about the 15th year of Married Life is 1 in . . .	2·76	4·08	10·92	22·44	...	4·80
Or a percentage of	36·2	24·5	9·1	4·5	...	20·8
The proportion Child-bearing about the 20th year of Married Life is about 1 in .	5·10	8·76	77·88	...	...	9·78
Or a percentage of	19·6	11·4	1·3	...	...	10·2
The proportion Child-bearing about the 25th year of Married Life is about 1 in .	40·80	288·3	...	...	...	102·6
Or a percentage of	2·4	·35	...	...	...	·97

early marriage. Whatever they may mean, they have no good evidence for their doctrine.

Quetelet† enunciates on this topic the following doctrine as a natural consequence from his data and reasonings. A marriage, says he, if it be not barren, produces the same number of births at whatever period it takes place, provided the age of the woman does not exceed twenty-six years. After this age the number of children, he adds, diminishes. Not only do I, of course, think Quetelet wrong in his conclusions, but I

* I here subjoin a table identical with the preceding (LVI.), except that it is corrected for sterility, just as Tables L. to LIV. inclusive have been.

TABLE LVII.—Showing the Probable Amount of Continuance in Fertility at Different Epochs of Fertile Wives married at Various Ages.

Age of Mother at Marriage .	15-19	20-24	25-29	30·34	35-39	Total.
The proportion Child-bearing about the 5th year of Married Life is 1 in	1·44	1·62	1·80	1·86	2·94	1·68
Or a percentage of	69·4	61·7	55·5	53·8	34·0	59·4
The proportion Child-bearing about the 10th year of Married Life is 1 in	1·80	2·40	2·52	3·24	...	2·34
Or a percentage of	55·5	41·7	39·7	30·8	...	42·7
The proportion Child-bearing about the 15th year of Married Life is 1 in	2·52	4·08	7·86	13·98	...	4·20
Or a percentage of	39·7	24·5	12·7	7·1	...	23·8
The proportion Child-bearing about the 20th year of Married Life is 1 in	4·74	8·76	57·48	...	...	8·82
Or a percentage of	21·1	11·4	1·7	...	...	11·3
The proportion Child-bearing about the 25th year of Married Life is 1 in	37·80	288·30	...	...	...	100·44
Or a percentage of	2·6	·38	...	...	...	·99

† *Treatise on Man*, p. 15.

cannot in his work discover any satisfactory grounds for them.

Before passing from the perseverance in fertility of the early married, I shall point out a difficulty of which it gives the solution. In Part I. Chapter V. I showed that fecundity in wives from fifteen to nineteen years of age is less than at from twenty to twenty-four; that is, of the young women fewer have children. At the same time (Chap. III.) I showed that the fecundity of the mass of wives in our population is greatest at the commencement of the child-bearing period of life, and after that epoch gradually diminishes; that is, those not the most fecund do, as a mass, produce most children. These two propositions are, at first sight, difficult to reconcile; and it is accordingly satisfactory to be able to show that the greater continuance in fertility of the mass of younger wives is the explanation of the apparent anomaly. To illustrate how the tables read in affording this explanation, I may state that while I formerly showed that the wives from fifteen to nineteen years of age are not so fecund as those from twenty to twenty-four years of age, the tables last adduced show that at the fifth year of marriage the youngest married—that is, at ages from fifteen to nineteen—already surpass all others in fertility, 1 in 1·44 bearing; that at the tenth year of marriage they still further surpass in fertility all others, 1 in 1·80 bearing; and that at the fifteenth year of marriage they in a still higher degree surpass all others, 1 in every 2·52 bearing children within a year. [Table LVI.]

Finally, under this head, I notice an important element of the inexactness that enters into the data here used—namely, the occurrence of second and third marriages. But the influence of this element is almost certainly inconsiderable, for the following reasons :—In cases of second and subsequent marriages the data used are exclusively those of the last marriage ; as far as is known, a woman's previous marriage does not interfere with her subsequent fertility ; it is shown in this paper that a woman's previous fertility tends to insure continuance in fertility ; it will be shown that a woman's previous fertility tends to diminish the intensity of her subsequent fertility, when that is compared with the fertility of women late in being married and having family ; and the admixture of second and subsequent marriages in the data which include only the last marriage, would tend to diminish the force of the results as bearing out these conclusions. They are therefore all the more secure, from the fact of the intermingling of some data which would diminish their apparent influence.

Another element of inexactness I shall only mention—the occurrence of twins, and both being counted in the figures.

CHAPTER XII.

FERTILITY OF PERSISTENTLY FERTILE WIVES OF DIFFERENT AGES.

I MAY here repeat, that by persistently fertile I mean fertile at, or up till, the time of the collection of the data; and I adduced a table which clearly shows, so far as the mass of figures can be relied on, that the fertility of the elder is greater than of the younger, while it lasts; or, in other words, the fertility of the elder is the more intense.

Table LVIII. is read in the following manner:—To take the second line—Fertile women five years married, and under ten, have, if they are now from 15 to 19 years of age, 2·5 children; if now from 20 to 24 years of age, 3·19 children; if now from 25 to 29 years of age, 3·75 children; and so on.

TABLE LVIII.—Showing the Intensity of Fertility in Wives-Mothers of Different Ages.

Duration of Marriage.	Mother's Age.						
	15-19.	20-24.	25-29.	30-34.	35-39.	40-44.	45-49.
Under 5 years	1·128	1·519	1·825	1·844	1·827	1·698	1·200
5 years and under 10 . . .	2·500	3·190	3·750	4·048	4·085	3·792	4·000
10 years and under 15 . . .	...	5·333	5·453	5·903	6·197	5·964	6·500
15 years and under 20 . . .	...	...	6·000	...	7·914	7·993	8·435
20 years and under 25 . .	...	...	...	7·000	9·396	9·718	10·528
25 years and under 30 . . .	...	...	...	...	...	12·368	13·600
30 years	...	...	...	...	...	...	13·000

The conclusion here arrived at is founded upon lengths of married life.* Were the figures such as to give, instead of lengths of married life, length of intervals between the births of first and last children, the results would be still more striking ; for I have already shown that, in the case of the elder, fertility is later in beginning to show itself than in the younger.

* The following tables give all the details and calculations from which Table LVIII. is constructed :—

TABLE LIX.—Of Women under 5 Years married.

	No. of Mothers.	No. of Children.	Average to each Mother.
	7183	11,880	1·654
Mother's Age—16 to 19 years .	374	422	1·128
,, 20 to 24 ,, . .	3180	4829	1·519
,, 25 to 29 ,, . .	2460	4489	1·825
,, 30 to 34 ,, . .	833	1536	1·844
,, 35 to 39 ,, . .	277	506	1·827
,, 40 to 44 ,, . .	53	90	1·698
,, 45 to 49 ,, . .	5	6	1·200

TABLE LX.—Of Women 5 Years married and less than 10.

	No. of Mothers.	No. of Children.	Average to each Mother.
	4637	17,690	3·815
Mother's Age—16 to 19 years .	2	5	2·500
,, 20 to 24 ,, . .	499	1592	3·190
,, 25 to 29 ,, . .	2155	8082	3·750
,, 30 to 34 ,, . .	1418	5740	4·048
,, 35 to 39 ,, . .	461	1883	4·085
,, 40 to 44 ,, . .	96	364	3·792
,, 45 to 49 ,, . .	5	20	4·000

If, as I have shown, the younger are more prolific than the elder, and if, as I also have shown, the elder are more intensely fertile while their fertility lasts, than the younger in the same time; then it necessarily follows as a corollary, that the fertile women married younger have a longer continued fertility than the fertile women married older. In no other way can the younger surpass the elder in their whole fertility; a conclusion which has already been otherwise demonstrated.

It may also be here pointed out that the figures of

TABLE LXI.—Of Women 10 Years married and less than 15.

	No. of Mothers.	No. of Children.	Average to each Mother.
	2739	16,233	5·930
Mother's Age—20 to 24 years .	9	48	5·333
,, 25 to 29 ,, . .	415	2263	5·453
,, 30 to 34 ,, . .	1345	7939	5·903
,, 35 to 39 ,, . .	814	5044	6·197
,, 40 to 44 ,, . .	140	835	5·964
,, 45 to 49 ,, . .	16	104	6·500

TABLE LXII.—Of Women 15 Years married and less than 20.

	No. of Mothers.	No. of Children.	Average to each Mother.
	1280	10,013	7·823
Mother's Age—25 to 29 years .	7	42	6·000
,, 30 to 34 ,, . .	253	1867	7·379
,, 35 to 39 ,, . .	721	5706	7·914
,, 40 to 44 ,, . .	273	2182	7·993
,, 45 to 49 ,, . .	23	194	8·435

Table XLI. make it probable that elderly women when fertile are more intensely so than younger, when their fertility has already resulted in a large family, for that table shows that the children in large families are born very quickly one after another.

In his work on *The Law of Population*, Mr. Sadler enters upon this subject of the varying fertility

TABLE LXIII.—Of Women 20 Years married and less than 25.

	No. of Mothers.	No. of Children.	Average to each Mother.
	432	4181	9·678
Mother's age—30 to 34 years. .	1	7	7·000
,, 35 to 39 ,, . .	134	1259	9·396
,, 40 to 44 ,, . .	259	2517	9·718
,, 45 to 49 ,, . .	36	379	10·528

TABLE LXIV.—Of Women 25 Years married and less than 30.

	No. of Mothers.	No. of Children.	Average to each Mother.
	29	371	12·793
Mother's age—40 to 44 years. .	19	235	12·368
,, 45 to 49 ,, . .	10	136	13·600

TABLE LXV.—Of Women 30 Years married.

	No. of Mothers.	No. of Children.	Average to each Mother.
	1	13	13·000
Mother's age—45 to 49 years. .	1	13	13·000

of women according to age. Seeking arguments wherewith to overturn the teaching of Malthus, whose principles he hated as well as opposed, he found data which at first sight appear to support his doctrine "that marriages are more prolific the longer they are deferred." Were this true doctrine, it would certainly go far to overturn the Malthusian system, and Mr. Sadler might be justly proud of the demonstration. The facts which he adduces may, without cavil, be allowed to be, as he says, indisputable. It is his illogical use of the facts which has to be pointed out. Without pretending to enter on the defence of Malthusian notions, we accept Mr. Sadler's challenge "to evade the demonstration" which the aforesaid facts afford. And it is of importance to do so, because, down to the latest authors, Sadler's facts and supposed demonstrations are quoted with unsuspicious approval.*

The first data afforded by Sadler are derived from the records of Dr. Granville's experience as physician to the Benevolent Lying-in Institution and the Westminster Dispensary, the calculations having been made by Mr. Finlayson.

* See Boudin, *Traité de Géographie et de Statistique Médicales*, etc., tome ii. p. 59.

TABLE LXVI.—Showing the Effect the Postponement of the Marriages of Females has upon their Annual Prolificness. (Sadler.)

Age when married.	Average number of Births for each year of Marriage.
From 13 to 16 . . .	·456706
„ 16 to 20 . . .	·503610
„ 21 to 24 . . .	·520227
„ 25 to 28 . . .	·545163
„ 29 to 32 . . .	·589811
„ 33 to 36 . . .	·776866
„ 37 to 39 . . .	1·125000

Now, this table is made from the data of lying-in charities. It is therefore not a table of fertile women, but of persistently fertile women; for every woman was entered in the records only when she came to have attendance in her confinement. All that the table offers is corroboration of the law enunciated in this chapter, that elderly women are more fertile than younger so long as their fertility endures.

It is almost incredible that so acute a reasoner as Mr. Sadler is, could be so deceived by appearances as to suppose his figures showed that marriages at thirty-nine years of age are as fruitful as marriages of any age down to thirteen. Yet, for aught he says, he appears so to believe.

Sadler did indeed get the length of seeing that the table just given was somewhat deficient. "It may,"

he says,* "perhaps be objected to the whole of the foregoing proofs, that they are derived from a register which cannot profess to give the whole number of children which the marriages it records shall produce from their commencement to their termination, but only those which have been born to each up to a period within these limits, all the facts which it can record being necessarily retrospective ones. I shall therefore," he continues, "proceed to another series of proofs of the same principle, which will at once silence every such exception, and afford a strong additional demonstration of its truth. These are derived from the registers of the peerage, which, as I have observed elsewhere, I have gone through in order to collect a body of authentic facts illustrative of many of the principles advanced in these volumes. As far as they relate to the subject before us, those facts are as follows:"—

TABLE LXVII.—SHOWING THE EFFECT OF THE POSTPONEMENT OF THE MARRIAGES OF THE PEERESSES ON THEIR PROLIFICNESS. (SADLER.)

Period of Marriage.	Number of Marriages.	Number of Children.	Births to each Marriage.
From 12 to 15 . . .	32	141	4·40
„ 16 to 19 . . .	172	797	4·63
„ 20 to 23 . . .	198	1033	5·21
„ 24 to 27 . . .	86	467	5·43

* *Law of Population*, vol. ii. p. 279.

To this table of Sadler's many objections may be made, such as the paucity and insecurity of the data, as also their deficiency, the highest age of marriage included in them being only twenty-seven, and all notice of the important element of the duration of marriage being omitted.

Sadler not only erred in supposing he had demonstrated that late marriages are more prolific than early; he was ignorant also that a larger proportion of the elder than of the younger wives bears no children at all, and that an older woman continues fertile a shorter time than a younger, counting in both cases only up to periods within the child-bearing portion of life.

It is a natural, and I believe a true notion, that twin-bearing is a sign of intense fertility in woman, as the number of a litter certainly is in bitches and other inferior animals. In confirmation of this notion, and of the law of intensity of fertility now demonstrated, we find that women are more likely to bear twins the older they are. This subject is capable of some interesting developments; but, as I have already elsewhere* entered upon them, I shall add no more in this place.

In like manner, it is natural to suppose that the length and weight of children should go with intensity of fertility. Yet my researches† seem to show that this is not the case, but that length and weight of

* *Edinburgh Medical Journal* for March and April 1865; and Part III. of this volume.

† *Edinburgh Medical Journal* for December 1864, and Part II. of this volume.

children go with the intensity of fecundity, or likelihood of bearing children, according to age. Professor Hecker, of Munich, has, however, elaborately shown that my conclusions on this head do not agree with those derived from his larger data.* Mine are based on 2087 observations only, and I am willing, in the meantime, to hold it as *sub judice* whether his or my conclusions are to be received. His do appear to me to be more probable because they bring the laws of length and weight of children, according to the mother's age, into agreement with the law of intensity of fertility here demonstrated.

* *Monatsschrift für Geburtskunde und Frauenkrankheiten*, November 1865.

For a table containing valuable data, bearing on the subject of this chapter, see Snow's *Twelfth Annual Report upon the Births, etc., in the City of Providence.* 1867, p. 8.

CHAPTER XIII.

THE FERTILITY OF THE OLDER WOMEN.

So ardently did Sadler desire the triumph of his attack on Malthus, that he adopted the dream of Mason Good, who says "that the usual term (of cessation of the menses) is between forty and fifty, except where women marry late in life, in which case, from the postponement of the generative orgasm, they will occasionally breed beyond their fiftieth year"!!* Mason Good refers to some extraordinary cases of pregnancy in old women, curiosities in physiology, but he adduces no good evidence in favour of the doctrine he here propounds. An opposite doctrine is taught by Burns, an author equally celebrated, and much more worthy of confidence in a question of the kind now before us. "It is well known," says the Glasgow professor,† "that women can only bear children until a certain age, after which the uterus is no longer capable of performing the action of gestation, or of performing it properly. Now it is observable that this incapability or imperfection takes place sooner in those who are advanced

* *The Study of Medicine*, 1822, vol. iv. p. 63.

† *Principles of Midwifery*, tenth edition, p. 309.

in life before they marry, than in those who have married and begun to bear children earlier. Thus we find that a woman who marries at forty shall be very apt to miscarry, whereas, had she married at thirty, she might have borne children when older than forty; from which it may be inferred that the organs of generation lose their power of acting properly sooner, if not employed, than in the connubial state. The same cause which tends to induce abortion at a certain age, in those who have remained until that time single, will also at a period somewhat later induce it in those who have been younger married; for in them we find that, after bearing several children, it is not uncommon to conclude with an abortion; or, sometimes, after this incomplete action, the uterus, in a considerable time, recruits, as it were, and the woman carries a child to the full time, after which she ceases to conceive." My own opinion has always coincided with that expressed by Burns;* and I may add, that the curious observation regarding abortion at the close of the fertile period of life has its analogue in the lower animals. Several times I have been told by men of experience that an old bitch often ends her career of breeding by a dead and premature pup. Whitehead also† regards those pregnancies which occur near the termination of the fruitful period in

* Burns' statement does not bear minute criticism, for he does not distinguish between sooner as implying earlier advent of relative sterility, and sooner as implying shorter duration of fertility.

† *On Abortion and Sterility*, p. 247.

women as being among the most commonly unsuccessful. More recently Dr. Arthur Mitchell has connected the occurrence of idiocy in a child with the circumstance of its being the last born of its mother.*

In Edinburgh and Glasgow, in 1855, 53 women above the age of 45 bore living children. Among those 53, only 1 was primiparous—her age was 49, and she had only been one year married; 2 bore second children—1 was aged 46 years, and had been four years married—the other was aged 52 years, and had been three years married; 4 bore fourth children; 4 bore fifth children; 3 bore sixth children; 3 bore seventh children; 6 bore eighth children; 8 bore ninth children; 7 bore tenth children; 4 bore eleventh children; 1 bore a twelfth child; 4 bore thirteenth children; 2 bore fourteenth children; 1 bore a fifteenth child; 2 bore sixteenth children; 1 bore a nineteenth child.

* *Edinburgh Medical Journal*, June 1863, p. 1142. "That in the mother (he remarks) which leads to the miscarriage may lead also to the idiocy, and the only connection may be one through a common cause." Again he says, "It frequently happens that between the birth of the idiot and that of the child which precedes or follows, an interval occurs which is much longer than usual, or that after the birth of the idiot permanent sterility appears. . . . . Again, when the idiot is born eighteen or twenty-four months after the preceding child, but when for six or seven years thereafter no impregnation occurs, he thought there was reason to suspect that the imperfection in reproductive power, which showed itself in the idiot, had merely another and fuller expression in the subsequent barrenness. And so also when permanent sterility follows. In many cases indications of barrenness preceded the birth of the idiot, and became permanent thereafter."

In short, the great majority of women child-bearing late in life are mothers of considerable families, not women for whom a postponement of the generative orgasm has to be imagined, a circumstance which destroys all shadow of ground for Mason Good's supposition.*

* For other corroborative evidence, see Roberton, *Physiology and Diseases of Women*, p. 183. "An examination (says he) of the table naturally suggests to the mind two questions: First, Have women, bearing children above the age of forty-five, generally been married late in life? . . . To the first question I can give only an imperfect, but perhaps a sufficient answer. Of eleven women, three of whom had a child each in her forty-ninth year, and the other eight had each a child above that age, I ascertained that the aggregate number of their children was 114—*i.e.* ten and a fraction for each woman; a fact indicating that they must have married rather early in life. Concerning the age of marriage in two out of the eleven, I possess some little information; the one married at eighteen, had two children before she was twenty-one, and brought forth her fourteenth child in her fiftieth year: the other was married from a boarding-school at a very early age; in her fifty-third year she was delivered of her twelfth child."

On the subject of this chapter see also some interesting remarks by Tilt, *On the Change of Life*, 3d Edition, p. 25, etc. On the legal age of "past child-bearing," see *Medical Times and Gazette*, July 22, 1871, p. 114.

CHAPTER XIV.

CONTRIBUTIONS TO THE ADULT POPULATION BY MARRIAGES AT DIFFERENT AGES.

THIS is the great question which political economists have aimed at discussing, however confused and irregular may have been their modes of proceeding. The attentive reader will have already seen how many subsidiary questions intervene between the mere calculation of the number of births by women of different ages and the question of fertility of marriages at different ages, in children that will survive to adult age. This last is the point which political philosophers chiefly wish to solve; yet several other calculations, to which we have made reference, have been taken and held as if they offered a solution of this great question of population.

Indeed, even now, I can offer nothing positive towards the solution of this important point. It is very desirable it should be settled by the accumulation and analysis of data; and considering the copiousness of the relative facts, I venture to express a hope that some of our statisticians, especially

those having use of public money and charge of the public records, will undertake the easy task.

The best solution I can offer appears to me to be very trustworthy, and I feel some confidence that actual data will be found to confirm it. It is arrived at by the following process of reasoning. The ascertained fertility of fertile marriages above twenty-five years of age (Tables XLII. and XLIII.) is so much less than that of those below, that no further consideration of the former requires to be made. Besides, the ascertained sterility of marriages above twenty-five is so much greater (Tables XIV. and LXX.) than that of marriages under twenty-five, as still further to put out of the competition all marriages above twenty-five.

The quinquenniads which may be regarded as mutual rivals in fertility are the two first—that is, from 15 to 19 inclusive, and from 20 to 24 inclusive. To aid us in deciding between the former and latter we have to inquire into:—1. Their fecundity (or sterility); 2. Their fertility; 3. Their survival of child-bearing; 4. The survival of their offspring; 5. The healthiness of their offspring.

In all except the second of these particulars, the first quinquenniad is surpassed by the second. Wives married at from 15 to 19 have seven per cent of sterility among them, while wives married at from 20 to 24 appear to have none (Table LXX.) This circumstance will make greatly against the fertility of a mass of wives from 15 to 19. Indeed, considering the small excess of the fertility of a mass of wives from

15 to 19 over those of the next quinquenniad (Table VII.), this drawback evidently has great effect. To this drawback has to be added their less chance of surviving a first confinement (Part VII.) Still further, the diminished amount of survival of their offspring (Table LXVIII.) has to be taken into account; and after all these, some weight against the early quinquenniad is to be ascribed to the general belief of the greater unhealthiness of their progeny as affecting their survival to adult age, or periods beyond those concerning which we have the numerical statements already referred to.

Although, then, fertile wives of the first quinquenniad have, in virtue of their great perseverance in fertility, a greater total fertility than wives of any other age, I do yet regard the wives of the second quinquenniad—that is, from 20 to 24—as most prolific in desirable offspring, as contributing most to the adult population; and this favourable view of the latter arises from the evils just enumerated as attending what we may now justly call premature marriages, or marriages of immature women.

As already shown, Sadler went far wrong in favouring the marriages of the elderly. I shall not here discuss his view again, contenting myself with merely quoting his words. "Thus, then, does it plainly appear," says he, "that among the wealthy as well as the poor the same law of nature prevails; and, consequently it is universal. As far as the preceding table goes, not only are the marriages more prolific the

longer they are deferred, but the deaths in their offspring are proportionally less numerous; causing, therefore, by the inverse rates of fecundity and mortality, the latter marriages to be far more conducive to permanent increase than the former ones."*

On this subject Major Graham has not, so far as I know, entered at any length. But I quote some remarks by him upon it, merely premising that while they are on the whole important and just, they yet appear to proceed on at least one insecure assumption—namely, that the number of births to each generation would necessarily grow less as the age of women at marriage increased—an assumption which is rendered doubtful by the demonstration of the variations, according to age at marriage, of fecundity, fertility, and other important circumstances already mentioned in this chapter. "The proportion of children to a marriage," says Graham, "and consequently the population, are regulated, not so much or so immediately by the numbers of the people who marry as by the age at which marriage is contracted. The mothers and fathers of nearly half of the children now born are under 30 years of age; and if all the women who attain the age of 30 should marry, and none should marry before that age is attained, the births would decline to about two-thirds, and if the marriage age were postponed to 35, the births would fall to one-third part of the present number: so the population would rapidly decline;

* *Law of Population*, vol. ii. p. 281.

firstly, because the number of births to each generation would grow less; and secondly, because, as the interval between the births of successive generations would increase, and the duration of life by hypothesis remain the same, the numbers living contemporaneously—in other words, the population—would be farther diminished." *

The most valuable contribution to this discussion of which I know, is afforded by the Committee of the Statistical Society of London, to whose labours I have repeatedly made reference. My own remarks at the beginning of this chapter are confirmatory of those of the committee, from whose report I now quote: "From this abstract (Table LXVIII.)† it is obvious, that of the three first periods, the children born of marriages formed in the quinquennial term of life 21-25, are subject to a less rate of mortality than those of the period immediately preceding or immediately following. The rate of mortality in the most advanced period, 31-35,

* Census, 1851. *Report*, vol. i. p. xlvi.

† TABLE LXVIII.

Years elapsed since Birth of First Child.	Mortality per cent of the Children born to Marriages formed at ages—			
	16-20	21-25	26-30	31-35
10	36·87	37·09	37·89	35·48
20	47·44	43·10	44·36	16·67
30	53·03	43·89	48·53	64·29
40	63·12	57·14	68·00	50·00

is very irregular, and no doubt arises from the small number of families included in that group. The two preceding series of facts furnish materials for the solution of a very interesting and highly important question —namely, what is the effect of the marriages formed at those different terms of life on the ultimate increase of population? By the first (Table XLIII.) of the two preceding abstracts it was found, that the earlier the period of life at which marriage was contracted, the greater the number of children born; but by the second abstract (Table LXVIII.) a difference is observable in the rate of mortality of the various periods, and this must disturb the results in the first class of facts.

Let a represent the results given in the first abstract, b represent those given in the second; then $a - \frac{a \times b}{100}$ = the actual increase resulting from each marriage to the population. The following is an abstract of the results thus arrived at:—

TABLE LXIX.

Years elapsed since Birth of first Child.	Children alive by each Marriage contracted at the following ages.			
	16-20	21-25	26-30	31-35
10	3·19	2·84	2·75	2·22
20	4·04	4·09	3·58	2·50
30	3·95	4·43	3·50	2·50
40	4·00	3·53	1·60	2·00

It hence follows that marriages formed under 25 years of age increase the population more than those formed above that age; and on a close examination it will be

found that there is very little difference in this respect between marriages contracted at ages 16-20 and 21-25, the rate of increase, however, being somewhat higher in the former period. With regard to the last two quinquennial terms at which marriage is formed, it will be seen that the rate of increase is not so great for ages 26-30 as in that immediately preceding, and in the period 31-35 the rate of increase is still less; in fact, the earlier the period of marriage the greater the increase resulting to the population, the difference between the first and second periods being very little, between the second and third very considerable, about 23 per cent, and between the third and fourth about 20 per cent.

"In the consideration of these facts and observations, although they relate to 1506 families, from which have resulted 8034 births, and of which 4616 children, or 57·46 per cent, are still alive, it must be borne in mind that they include only one class of the community, and may be subject to disturbing influences, such as to destroy their character as a type of the general population; however, there is reason to suppose that these results may be a more faithful representative of the condition of the whole population than if they were derived from a like number of facts from either the middling or higher classes of society. On reflection, it will also be found that the unfruitful marriages are not included in any of these 1506 families, all included being more or less productive. Likewise the marriages are all those in which

one or both the parents are still alive, and consequently the results of fruitful marriages, in which the parents have died before the lapse of the given period of years brought under review, are excluded. An influence, independent of the relative number of marriages at each age, will further affect the results arising from the varying rates of mortality at the different terms of life, even when equal numbers only at those periods are considered; and it will follow, that fewer marriages of limited fruitfulness will be excluded from the groups at the younger ages, the effect of which must be to show in the preceding figures a reduced ratio of children at each marriage formed at those periods of life, compared with that which would appear were all cases included. The relative bearing of all the results is therefore so far modified. Also, the children still alive, composing 57·46 per cent of all born, may, subsequent to the period now under observation, and when classified according to the ages at marriage of their parents, show a very different rate of mortality from that indicated in the respective classes by those who have hitherto died, and still more extended observations would be required to show, whether any and what difference exists in the fruitfulness of the marriages in the succeeding generation. Lastly, all these remarks have had reference to the age of the mother only, at birth of her first child."*

* *Journal of the Statistical Society of London*, vol. xi. 1848, p. 224.

CHAPTER XV.

THE COMPARISON OF THE FECUNDITY AND FERTILITY OF DIFFERENT PEOPLES.

I KNOW of no comparisons of the fecundity of different peoples—*i.e.* of the proportional number of married women who bear any children, who are not sterile. To give to such comparison any physiological value, it will be necessary to establish uniformity in the condition of age of the women at marriage, as it has been demonstrated that fecundity varies greatly according to the variations of this circumstance.

Some approach may, however, be made to a comparison, not of the fecundity simply, but of the actual fecundity of the whole marriages in England and Scotland. On this point, the reports of Major Graham and Dr. Stark may be quoted. "A great number of married people," says the former, "have no children living; and it was shown in the previous report, from a limited but perhaps a sufficient number of facts, that about 28 in 100 married pairs had no children residing with them on the census night. From other observations, it may be estimated, however, that not more than 20 in 100 families are childless, and consequently

that about 80 in 100 have children living."* This rough estimate of the fecundity of English women tallies very closely with that made for Scotland by Dr. Stark. "Taking," says he, "two of the largest registration districts of Glasgow, it was found that of 14,523 married persons living together, 11,718 had children living with them; while 2805 had no children with them. This would yield the proportion of 80·686 per cent with children, and 19·314 per cent without children; or, without the decimals, that in every 100 married couples, 81 had children, while 19 had none."†

I have already, in speaking of twins, shown how fallacious a test of the fertility of a people their frequency probably is. It has been used, however, as an index of such fertility.

Dr. Stark has tried another plan of ascertaining the comparative fertility of England and Scotland. After pointing out that more children are born to each marriage in Scotland than in England, he proceeds as follows:—"But the comparison may be carried further and closer, by ascertaining the exact number of the married women at the child-bearing ages, and comparing their number with that of the legitimate births. This ascertains to a nicety the fact we are in search of—viz. the comparative fruitfulness of the married women in England and in Scotland. In Scotland, in 1861, there were 305,524 married women between the ages of 15 and 45 years; and as, during that year, there

* Census of 1851, vol. i. p. xliii.

† Census of Scotland, 1861, vol. ii. p. xxxvi.

occurred 97,180 legitimate births, it is clear that every 314 married women at the child-bearing ages gave birth to 100 children during the year. In England, on the other hand, during the same year, there were 2,319,641 married women between the ages of 15 and 45 years; and as, during that year, there were born 652,249 legitimate children, it is apparent that every 355 married women at the child-bearing ages gave birth to 100 children. In other words, while Scotland required only 314 married women annually to produce 100 legitimate children, England required 355 married women (or 41 married women more) to produce the same number. These striking facts, therefore, establish the pre-eminent vitality of the Scottish population, and seem also to indicate that nature, in order to compensate for the smaller proportion of marriages, renders the married females more prolific."*

It appears to me that here Dr. Stark is satisfied with insufficient evidence. Before settling anything, it is necessary to inquire if the interesting figures quoted can be accounted for in any other way than by supposing a pre-eminent vitality or fertility of the Scottish people; and there appear to me to be several such ways. I agree with Major Graham, who suggests one out of several explanations of this difference between England and Scotland, in considering it to be not necessary to assume that there is any essential difference in the organisation, the fecundity, or the

* Census, 1861. *Report*, vol. ii. p. xxviii.

virtue of the women living north and south of the Tweed.

"The number (says Graham) of children to a marriage appears to be greater in Scotland than in England, and this is held to be a proof that married women are more prolific in Scotland than in England.

"Proceeding upon another basis, the annual number of legitimate children registered in England was 626,506 in the five years 1856-60; when the average number of wives of the age 15-55, determined directly from the census returns of 1851 and 1861, was 2,843,374; consequently 100 wives bore 22·0 children annually. In like manner it is found that 100 unmarried women bore on an average 1·7 illegitimate children; that is, 17 children to 1000 women. 100 women, including the married and unmarried, bear 12·3 children annually on an average.

"In Scotland, during the same years, the following proportions were found to exist:—100 wives bore 24·8 children annually, 100 spinsters or widows bore 1·9 illegitimate children; and 100 women bore 12·0 children, legitimate or illegitimate.

"The wives of Scotland, as well as the spinsters, are apparently more prolific than the corresponding classes in England; and yet, taken collectively, the women of England are more prolific than the women of Scotland. 1000 English women (age 15-55) bear 123 registered children annually; while 1000 Scotch women bear 120 children. The difference is slight, but it is in favour of the English women.

"This appears at first sight to be contradictory and paradoxical. It is explained by the circumstance that the proportion of recognised wives in the population is much lower in proportion in Scotland than it is in England, and as the fecundity of wives is to that of spinsters as 13 to 1, a slight difference in the proportions alters the birth-rates of the two populations. The difference in this respect between England and Scotland is great; in England 52 in 100 women of the age 15-55 are wives, 48 only are spinsters and widows; in Scotland the proportions are reversedly 44 recognised wives to 56 spinsters and widows.

"By altering the proportions in Scotland, for instance, by transferring 57,608 women from the ranks of the unmarried to the married women, and by transferring 2130 children from the ranks of the illegitimate to the legitimate children, the fecundity of women—of the wives and of the spinsters—of Scotland, becomes the same as the fecundity of the corresponding classes in England—namely, wives having children 22·034 per cent, spinsters and widows 1·676; instead of 24·790 and 1·916 per cent; and when the transfer is made, the proportions remaining still show a less excess of women living in the state of marriage in Scotland than in England." *

These passages illustrate forcibly the difficulty of establishing a comparison between the fertilities of two countries, even when they are in so many respects

* *Twenty-seventh Annual Report of the Registrar-General*, 1866, p. xxi.

alike as is England to Scotland. There are two ways of making this comparison justly. I, of course, imply not a mere juxtaposition of the actual fertilities; that is already made; but an estimation of the comparative fertilities; and this word, when applied to a people, cannot be construed so as to exclude the element of fecundity (or sterility).

The first is the direct method. A number of women, married at the same age, and under other respectively similar circumstances, are compared with one another, as to their families, after having lived in wedlock, all, the same number of years.

The second or indirect method is difficult and complicated. Of it, Dr. Stark may be said to have made the first step. The further progress to a true result by this method consists in correcting Dr. Stark's result by eliminating from it all errors whose source is suspected; in other words, by correcting it for all known causes of possible error. Of these the chief probably are the fertility of different ages at marriage, and the fecundity of different ages; the mortality of primiparæ and multiparæ at different ages; and the survival of labour at different ages.

For further elaboration of this topic I refer to the Sixth Part of this volume by Professor Tait.

This completes my remarks on the fertility of married women. But the subject is susceptible of further interesting developments, by an inverted method of proceeding, which I proceed to carry out.

It is evident that the conclusions arrived at in this part, or others still more definite, can alone form a sure basis for speculation in the great questions in political economy regarding population, and the various means of increasing it, or of retarding its excessive growth. And it is to be hoped that the promoters of that science will avail themselves of information which Malthus, Sadler, and their followers, evidently desired ardently to possess.

But it is not to the political economist alone that such information is valuable. It will form an element in the guidance of social life, and will certainly greatly contribute to the wisdom in council of the well-informed medical practitioner.*

* The French Census—Fertility of Races.—*Scotsman*, Feb. 26, 1867. The census of the population of France for 1866 has now been made public. It is clear, in the first place, that this population is neither diminishing nor stationary, as has been so often carelessly asserted. It has increased (without including Savoy and Nice) by about 1,300,000 in the last ten years; amounting now to upwards of 37,000,000. This is not quite half the English rate of increase; it about equals that of Scotland. But then from Scotland, as well as from England, there is a continual and considerable emigration; from France scarcely any emigration at all. This augmentation, therefore, must be taken as comprising the whole natural growth.

On the other hand, the length of life in France is continually increasing, or (which is the same thing) the death-rate diminishing. The slowness of increase is therefore wholly owing to diminution in the number of births, relatively to population. In 1782, with scarcely 25,000,000 inhabitants, there were 975,000 births; in 1865, only 995,000. Fifty years ago there were 3·70 births to a marriage; now, about 3·10. In other words, ten marriages

produced half-a-century ago thirty-seven children; now, only about thirty-one.

This remarkable falling-off in births is, of course, wholly attributable to what Malthus termed the "preventive check." People who are determined to maintain a certain rate of comfort and social dignity, and who cannot dispose of surplus numbers by emigration, will not multiply beyond their means. The chief reason of the small number of births in France appears to be merely the remarkable, and increasing, lateness of marriages. If we can trust the *Annuaire du Bureau des Longitudes*, the average age at which men marry in France is now thirty and a half; women at twenty-six.

This, then, seems to be the law under which advanced societies, which have not the habit of emigration, live—increasing longevity, diminishing fertility. In Malthus's time this state of things was considered a subject of high commendation. Some communes in the Pays de Vaud were pointed out as models, in which the death-rate had continually diminished, and yet the population remained stationary for a long series of years. France possesses extensive and thriving rural districts, in which the same social economy has prevailed for generations. Normandy has not increased above a fourth or a fifth for the last two hundred years.

In such a country, however, the only net result appears to be that which we have pointed out—diminished numbers, increased well-being. It is otherwise where an old community, jealous of its comforts and trained in self-restraint, is planted amidst strangers of a ruder type, content with less comfort, and multiplying accordingly. Thus, in old times, the pure-blooded Spartans, and similar oligarchical races, died gradually out. In Hungary, the more advanced tribe of Magyars is stationary in numbers; the Slav and the Rouman swarm in their beggary. In Transylvania there is one small and remarkable people, the "Saxons" or descendants of German settlers—well-to-do, industrious, and peaceful—who have nevertheless diminished one-third in fifty years; they will not have children, as their ministers complain, in order not to diminish their substance; while their neighbours and quondam drudges, the Wallachs or Roumans, marry young and multiply exceedingly, and will soon occupy the quaint old Saxon settlements

with their healthy ragged colonies. And, strange to say, the same law of advancing civilisation seems to be working itself out in America. We are already told that in Massachusetts, not only is the birth-rate, taken on the whole, nearly stationary, but that the increase, such as it is, is due solely to the immigrants ; that "the foreign population have from two to three times as many births as the American, and it follows that the American deaths actually exceed the births." The Americans are so prone to give exaggerated pictures of the unfavourable as well as the favourable circumstances in their condition, that we receive such statements under considerable reserve ; but they are probably not without foundation. And thus it would seem the first ranks in the advancing army of civilisation are continually decimated ; but the masses from behind press in to fill up the gap.—*Pall Mall Gazette.*

For a note on fertility of races, see Villermé, *Annales d'Hygiène Publique*, tome v. p. 96 : 1831.

PART V.

ON SOME LAWS OF THE STERILITY OF WOMEN.

BEFORE commencing a discussion on the subject, it is necessary to make some definitions, with a view to avoiding the confusion which extensively prevails, from the neglect of the all-important definition of terms. I might be even more exact than I shall be, and excuse myself from adopting such a seeming improvement, on the ground that further refinement of definition would itself cause confusion in the present stage of advancement of our knowledge.

Absolute sterility I shall hold to mean the condition of a woman who, under ordinary favourable circumstances for breeding, produces no living or dead child, nor any kind of abortion.

Sterility I shall hold to mean the condition of a woman who, under ordinary favourable circumstances for breeding, adds not even one to the population, or produces no living or viable child.

Relative sterility I shall hold to mean the condition of a woman who, while she may or may not be sterile, is, under ordinary favourable circumstances for breeding, sterile in relation to the circumstances of time; or, in other words, in relation to her age, and the duration of her married life.

CHAPTER I.

STERILITY OF MARRIAGES IN THE POPULATION.

UNDER this head, the age at marriage, and the duration of it, are not regarded. We simply compare the number of people living in the married state, without and with living children. The only information I have on this point is derived from the writings of Major Graham* and Dr. Stark.† "It is a pity," says the latter, "that when the census was taken up, a query had not been put to every married woman whether she had borne children. We have at present no means of ascertaining what proportion of the marriages proves unfruitful; and it is no criterion to ascertain the number of married persons who had children living with them on the night of the census. Married persons who had a numerous family may have none with them, because they are grown up, or are absent at schools or trades. We know, however, from other sources, that a considerable proportion of

* Census of England, 1851, vol. i. p. xliii., of Reports by Messrs. Graham, Farr, and Mann.

† Census of Scotland, 1861. Population Tables and Report, vol. ii. p. xxxvi.

marriages proves unfruitful; and as it was shown that the married women of Scotland produce more children in proportion to their number than the married women of England, it would have been extremely interesting to have ascertained whether that depended on more of the Scottish married women being fruitful.

"As it may," continues Dr. Stark, "however, give a distant approximation, it may be stated that, taking two of the largest registration districts of Glasgow, it was found that of 14,523 married persons living together, 11,718 had children living with them; while 2805 had no children with them. This would yield the proportion of 80·686 per cent with children, and 19·314 per cent without children; or, without the decimals, that in every 100 married couples, 81 had children, while 19 had none. These numbers may be safely taken as the proportion in the town populations, seeing that for each district the proportions came out within a very small decimal fraction of one another; also from the circumstance, that in other tables which have been published in the Registrar-General's Second Detailed Annual Report, relative to the proportions of children borne by mothers at different ages in Edinburgh and in Glasgow, the results of the one town almost exactly corresponded with those of the other."

I now quote from the report of the English Registrar-General:—"A great number of married people have no children living; and it was shown in the previous Report, from a limited but perhaps a sufficient number of facts, that about 28 in 100 married pairs

had no children residing with them on the census night. From other observations, it may be estimated, however, that not more than 20 in 100 families are childless, and consequently that about 80 in 100 have children living."

CHAPTER II.

STERILITY OF WIVES.

THE wives who do not increase the population may be called sterile. But a wife who has one or several abortions, or who bears one or several dead children, or to whom both of these events happen, adds not a unit to the population; and such a wife cannot be said to be absolutely sterile. In order to discover the amount of sterility of married women, I proceed on the following plan. I take the registers of Edinburgh and Glasgow for 1855, and find what is the number of first children produced in that year. With this I compare the number of marriages in that year. It is evident that the number of first children only should be counted, for they indicate all the wives who are not sterile. If one living child is born to a marriage, that marriage is not sterile. Further, it is evident that, although the first births in 1855 will not all pertain to the women married in that year, it may be assumed that, if the marriages be nearly the same in number for a few contiguous years, the first births in one year will give the fertility very accurately of any

of the contiguous years. From this fertility the sterility can be easily computed.

Now, in 1855 there were, in Edinburgh and Glasgow, 4447 marriages, and 3722 first deliveries of living children, leaving 725 marriages sterile, or 1 in 6·1. But in these figures are included 75 marriages which did not take place till after the women had passed forty-four years of age, and these will damage the physiological value of the statement, as these 75 women could not be expected to be prolific.

Of women between the ages of fifteen and forty-four inclusive, there were married 4372; among wives of the same ages, 3710 had first children, leaving 662 marriages sterile, or 1 in 6·6. In other words, 15 per cent of all the marriages between fifteen and forty-four years of age, as they occur in our population, are sterile.

The statement of the amount of sterility just given appears to me, from the largeness of the figures used, to be far more valuable than any other I know of. But on account of their great interest, I shall quote the statements of two authors:*—"In the *Dictionnaire des Sciences Médicales* (vol. vi. p. 245; see also *Neue Abhandlungen der Schwedischen Akademie der Wissenschaften*, vol. xi. p. 70), it is stated," says Sir James

* Lever's statement I here submit, but I cannot ascribe much value to it, because no evidence is adduced, and because there is an evident numerical error in some part of the passage. He says, "It is found that $\frac{1}{20}$th, or 5 per cent, of married women are wholly unprolific."—*Organic Diseases of Uterus*, p. 5.

Y. Simpson,* "that Hedin, a Swedish minister, had noticed that in his parish, composed of 800 souls, one barren woman is not met with for ten fertile. It is further stated that Frank asserted, but from what data is not mentioned, that it would be found, on investigation, that in most communities containing 300 to 400 couples, at least six or seven would be sterile, without anything in their physical condition to explain the fact. It seems to have been from this assertion of Frank's that Burdach, who is almost the only author who even alludes to the matter, has given the general statement that one marriage only in fifty is unproductive (Dr. Allen Thomson's excellent essay on Generation, in *Todd's Cyclopædia*, vol. ii. p. 478, footnote).

"For the purpose of ascertaining the point by numerical data, I had a census taken of two villages of considerable size—viz. Grangemouth in Stirlingshire, and Bathgate in West Lothian—the one consisting principally of a seafaring population, and the other of persons engaged in agriculture and manufacture.

"The following form the results in these two places:—Of 210 marriages in Grangemouth, 182 had offspring; 27 had none; or about 1 marriage in 10 was without issue. Of the 27 unproductive marriages, all the subjects had lived in wedlock upwards of 5 years, and in all, the female had been married that period before she reached the age of 45. Again, of 402

* *Obstetric Works*, vol. i. p. 323.

marriages in Bathgate, 365 had offspring; 37 had none; or about 1 marriage in 11 was unproductive. There were at the same time living in the village 122 relicts of marriages, and of these 102 were mothers; 20 were not mothers; or about 1 in 6 had no family. In all, of 467 wives and widows, 410 had offspring; 57 had none; or about 1 marriage in 8 was unproductive. Of these last 57, 6 had not been 5 years married, and there were other 6 above the age of 45 when married. If we subtract these 12, we have of 455 marriages, 410 productive; 45 unproductive; or 1 in $10\frac{1}{9}$th without issue.

"Returns such as I have just now adduced are exceedingly difficult to obtain, in consequence of no registers being anywhere kept, so far as I know, that could be brought to bear upon the question. If it had been otherwise, I would here, if possible, have gladly appealed to a larger body of statistical facts, in order to arrive at a more certain and determinate average of the proportion of unproductive marriages in the general community. For the purpose, however, of extending this basis of data, I have analysed, with some care and trouble, the history of 503 marriages, detailed by Sharpe, in his work on the *British Peerage* for 1833. Among British peers there were 401 marriages with issue; 102 without issue; or of 503 existing marriages among British peers in 1833, 74 were without issue after a period of 5 years. Of those who had not yet lived in the married state for 5 years, 28 were still without family; and in Burke's *Peerage* for 1842

there still remained among these 28 marriages, 7 without issue, making 81 as the total number of unproductive marriages among the original 503; or the proportion of the unproductive to the productive marriages among this number is, as nearly as possible, 1 in $6\frac{3}{7}$. In the above calculation I have excluded 8 unproductive marriages, in which the age of the husband at the date of marriage exceeded 56. These 8, however, ought to be deducted from the original sum of total marriages that were included; or, in other words, the 503 should be reduced to 495, and then the whole result would stand thus:—Among 495 marriages in the British peerage, 81 were unproductive, or 1 in $6\frac{1}{9}$ were without any family." The proportion of unproductive marriages in Grangemouth, Bathgate, and the British peerage, all taken together, was found by Simpson to be 1 in $8\frac{4}{7}$.

Dr. West* states that he found the general average of sterile marriages among his patients at St. Bartholomew's Hospital to be 1 sterile marriage in every 8·5.†

* *Diseases of Women*, 3d edition, p. 366. See also p. 44 of his work entitled, *An Enquiry into the Pathological importance of Ulceration of the Os Uteri.* See also Mr. Spencer Wells "On some remediable causes of Sterility," in *Medical Times and Gazette*, Dec. 14, 1861, p. 601.

† A statement of the sterility of Esquimaux women is given by Roberton, *Essays and Notes on the Physiology and Diseases of Women*, p. 53.

CHAPTER III.

ABSOLUTE STERILITY OF WIVES.

In order to arrive at the absolute fertility, or, conversely, at the absolute sterility, of the wives in Edinburgh and Glasgow, it is necessary to add to the number of wives bearing first living children the number of those who bear only dead children or abortions.

The number of abortions has been variously estimated by Graunt, Short, Whitehead, and others. The number of children born dead has been the subject of much investigation, among others by Jacquemier, Boudin, and Legoyt. But were our information on these points very exact, it would not help us in this inquiry. For our purpose, the desideratum is not the number of abortions in a number of pregnancies, nor the number of children born dead in a number of births, but the proportional number of married women who produce nought else than abortions or dead children, who, while not absolutely sterile, yet add none to the population. Of this class of wives I know of no estimate.* I believe they are few, and I leave the

* The following extract from the work of Dr. West on *Diseases of Women* (3d edit. p. 367) may be of some value. It refers to

statement of the sterile as a near approximation to a correct statement of the absolutely sterile.

the histories of a set of poor women labouring under uterine cancer. "There were but two out of the whole 150 women whose pregnancy had issued merely in abortion."

See Hegar, *Monatsschrift für Geb.*, xxi. Band, Supplement-Heft, 1863, S. 34. See also a paper by M. Loua, referred to in *Lancet* for January 8, 1867, p. 21.

CHAPTER IV.

STERILITY ACCORDING TO THE AGES OF WIVES.

To illustrate the variations of sterility according to age, I bring forward the accompanying Table (LXX.)

With the numbers of marriages taking place in Edinburgh and Glasgow in 1855, at different ages of the wives, are compared the numbers of first children born in the same year to wives married at the same ages in that year or previously. The number of sterile wives is got by subtracting the latter figures from the former, and the percentage of sterile marriages is given in the penultimate horizontal line.

So far as the numbers are to be relied upon, we have from this table the interesting results, that about 7 per cent of all the marriages between 15 and 19 years of age inclusive, and as they occur in our population, are without offspring; that those married at ages from 20 to 24 inclusive are almost all fertile; and that after that age sterility gradually increases according to the greater age at the time of marriage.

TABLE LXX.—Showing the Variations of Sterility according to the Ages of the Wives.*

Ages of Wives at Marriage .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50, etc.	Total.
Number of Wives	700	1835	1120	402	205	110	46	29	4447
First Children	649	1905	809	251	96	10	2	...	3722
Sterile Wives	51	...	311	151	109	100	44	29	725
Percentage Sterile	7·3	...	27·7	37·5	53·2	90·9	95·6	100	16·3
Proportion Sterile, 1 in . .	13·72	...	3·60	2·66	1·88	1·10	1·05	1·00	6·13

* In like manner I have made from Dr. Snow's Tables of the marriages in Providence in 1866, a Table showing the sterility of

wives there. It is to be noted that there is a proportion of sterility at the age 20 to 25, and this is a difference from that obtained from the Edinburgh and Glasgow statistics. Of course, no one believes that there is no sterility among women aged from 20 to 25; but the quantity shown in Snow's data is not of much value, from the paucity of the data and from the circumstance that "the proportion of persons, male and female, between the ages of 20 and 25, who were married in 1866, was considerably greater than in the previous year."

TABLE LXXI.—Showing the Variations of Sterility according to the Ages of the Wives in Providence.

Ages of Brides.	Under 20.	20-25.	25-30.	30-40.	40-50.	Total.
Number of Brides . .	144	366	161	108	22	801
First Children . . .	49	215	113	45	2	424
Sterile Wives . . .	95	151	48	63	20	377
Percentage Sterile . .	65·97	41·25	29·8	58·3	90·9	47·06
Proportion Sterile 1 in .	1·51	2·42	3·35	1·71	1·10	2·12

CHAPTER V.

EXPECTATION OF STERILITY.

THE main element in the expectation of sterility is the age of the woman at marriage. This has just been described. But, besides this, our statistics suggest to us other laws as to the expectation of sterility. Of these the first is :—

That the question of a woman's being probably sterile is decided in three years of married life. For while a large number are fertile for a first time in each of the first three years of married life, only 7 per cent of the fertile bear first children after three years of marriage, or about 1 in 13. (Table LXXII.)

This same Table affords us a second law of expectation of sterility :—

That when the expectation of fertility is greatest, the question of probable sterility is soonest decided, and vice versa. For our Tables show that of the wives married from 20 to 24, who are all fertile, only 6·2 per cent begin to breed after three years of marriage ; while at the other ages, with less fecundity, a greater percentage commences after the completion of the third year of marriage.

TABLE LXXII.—SHOWING THE FERTILITY OF MOTHERS, OF DIFFERENT AGES AT MARRIAGE, COMMENCING AFTER THREE YEARS OF MARRIED LIFE.

Mother's Age at Marriage . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	Total.
Number of Fertile	649	1905	809	251	96	10	2	3722
Number commencing Fertility after being 3 years married . . .	63	119	62	27	15	1	...	287
Percentage commencing Fertility after being 3 years married . . .	9·7	6·2	7·7	10·7	15·6	10·0	...	7·7
Or 1 in	10·3	16·0	13·0	9·3	6·4	10·0	...	13·0

CHAPTER VI.

RELATIVE STERILITY.*

HERE I take into consideration only those who have borne children—only those who are not sterile. Of course all these wives, if they survive in wedlock, will sooner or later become relatively sterile. Now, I have already shown that the prolongation or length of endurance of fertility was greater according as the age at marriage was less. From this conclusion it is easy to derive one in regard to relative sterility, to the effect that—

Relative sterility will arrive after a shorter time (*not earlier*) *according as the age at marriage is greater.* The demonstration of these propositions is arrived at by showing the proportional numbers bearing at different years of married life, according to age at marriage. This is an indirect way of proceeding, but it is the only one I can find available, while I have no documents giving the ages of mothers at marriage, and their ages at birth of last children, the mothers continuing to live in wedlock.

* On some points in the relative sterility of some of the lower animals, see Villermé : *Annales d'hygiéne publique,* tome v. p. 85, 1831.

TABLE LXXIII.—SHOWING THE RELATIVE STERILITY OF A MASS OF WIVES MARRIED AT DIFFERENT AGES AT SUCCEEDING EPOCHS IN MARRIED LIFE.

Age of Mother at Marriage	15–19	20–24	25–29	30–34	35–39	Total.
Proportion Sterile about the 5th Year of Married Life is about 1 in	2·78	2·61	1·68	1·51	1·19	2·09
Or a percentage of	35·9	38·3	59·4	66·0	84·1	47·9
Proportion Sterile about the 10th Year of Married Life is about 1 in	2·09	1·71	1·39	1·24	...	1·61
Or a percentage of	47·9	58·3	71·8	80·8	...	62·1
Proportion Sterile about the 15th Year of Married Life is about 1 in	1·57	1·32	1·10	1·05	...	1·26
Or a percentage of	63·8	75·5	90·9	95·5	...	79·2
Proportion Sterile about the 20th Year of Married Life is about 1 in	1·24	1·13	1·01	...	...	1·11
Or a percentage of	80·4	88·6	98·7	...	...	89·8
Proportion Sterile about the 25th Year of Married Life is about 1 in	1·02	1·00	...	...	...	1·01
Or a percentage of	97·6	99·65	...	...	...	99·03

Table LXXIII. gives the calculated amounts of sterility at different periods of married life in women married at different ages. It is needless to enter on the method of construction of this Table. It is merely the complement of Table LVI., given already, where full details are stated. I shall only add, that this Table is all calculated for 20 months, with a view to giving the nearest accurate estimate, 20 months being what I have called the time-unit of fertility, the shortest time within which all women may be expected to show fertility if they possess it.

In my remarks on the fertility of elderly women (p. 167) I gave reason for believing that there was no prolongation of the reproductive powers beyond ordinary ages in the case of women married late in life. In the same part (p. 145) I showed that the greater a woman's age at marriage, the shorter is her era of child-bearing; or, in other words, the less is her perseverance in fertility. But in neither of the chapters referred to have I pointed out what Professor Tait has shown (p. 216) regarding the advent of relative sterility. His Table (No. LXXXVII.) shows that—

The older a fertile woman is at marriage, the older is she before her fertility is exhausted; that is, before the advent of relative sterility.

Similar results are deducible from Table LXXIV., to which I refer the reader for actual numbers, not of fertility, as in Professor Tait's, but of sterility. This law, then, of the advent of sterility does not modify the other closely-placed laws already alluded to. It does not touch the question of the fertility of elderly women,

nor the varying perseverance in fertility of women married at different ages; it merely shows that, although it is true that the older a fertile woman, the shorter is her era of fertility, or the sooner does she arrive at sterility; yet this era of fertility, shortened in proportion to age at marriage, carries the subject of it into greater actual ages of fertility than are reached by the earlier married.*

* I here subjoin a Table identical with the preceding (LXXIII.), except that it is corrected for sterility, as the similar Tables in Part IV. have been amended.

TABLE LXXIV.—SHOWING THE RELATIVE STERILITY OF A MASS OF FERTILE WIVES MARRIED AT DIFFERENT AGES, AT SUCCESSIVE PERIODS IN MARRIED LIFE.

Age of Mother at Marriage .	15-19	20-24	25-29	30-34	35-39	Total.
Proportion Sterile about the 5th year of Married Life is about 1 in . . .	3·27	2·61	2·25	2·16	1·51	2·46
Or a percentage of . .	30·6	38·3	44·5	46·2	66·0	40·6
Proportion Sterile about the 10th year of Married Life is about 1 in . . .	2·25	1·71	1·66	1·44	...	1·74
Or a percentage of . .	44·5	58·3	60·3	69·2	...	57·3
Proportion Sterile about the 15th year of Married Life is about 1 in . . .	1·66	1·32	1·14	1·07	...	1·31
Or a percentage of . .	60·3	75·5	87·3	92·9	...	76·2
Proportion Sterile about the 20th year of Married Life is about 1 in . . .	1·27	1·13	1·02	...	...	1·13
Or a percentage of . .	78·9	88·6	98·3	...	...	88·7
Proportion Sterile about the 25th year of Married Life is about 1 in . . .	1·03	1·00	...	...	...	1·01
Or a percentage of . .	97·4	99·62	...	...	...	99·01

CHAPTER VII.

EXPECTATION OF RELATIVE STERILITY.

As a sort of appendix, I produce five Tables, giving all the details of the expectation of continued fertility, and, conversely, of relative sterility. These tables not only give data for calculating the chances of relative sterility, but also for calculating the probable number of the family produced in women at different ages becoming relatively sterile. To enter further upon these considerations would be merely to give in writing what is more succinctly stated in the tables themselves.

Lastly, I state a law of relative sterility, for which I do not here adduce the numerical proofs, these having already been given in the former part. This law is, that—

A wife who, having had children, has ceased for three years to exhibit fertility, has probably become relatively sterile; that is, will probably bear no more children; and the probability increases as time elapses. For the probability of sterility only commences after three years of sterile marriage. Further, the data given in Table XXXVI. show that fertile

TABLE LXXV.—FIFTH YEAR OF MARRIED LIFE.

Number of Child	1st	2d	3d	4th	5th	6th	7th	8th	9th	Total.
Wives-Mothers of Ages 20-24 . . .	13	39	160	31	4	...	...	...	...	247
Proportion of above to 644 surviving	49·5	16·5	4·0	20·8	161·0	...	...	...	...	2·6
Wives Married at from 15-19 is 1 in										
Wives-Mothers, of Ages 25-29 . .	10	82	398	106	13	2	...	...	...	611
Proportion of above to 1686 surviving	168·6	20·5	4·2	15·9	13·0	843·0	...	...	...	2·7
Wives Married at from 20-24 is 1 in										
Wives-Mothers, of Ages 30-34 . .	3	31	147	52	8	2	1	...	...	241
Proportion of above to 1008 surviving	336·0	32·5	6·8	19·4	126·0	504·0	1008	...	...	4·1
Wives Married at from 25-29 is 1 in										
Wives-Mothers, of Ages 35-39 . .	3	12	37	14	1	1	2	1	1	72
Proportion of above to 358 surviving	119·3	29·8	9·7	25·6	358·	358·	179·	358·	358·	4·9
Wives Married at from 30-34 is 1 in										
Wives-Mothers, of Ages 40-44 . .	3	2	11	1	...	...	...	...	...	17
Proportion of above to 179 surviving	59·6	89·5	16·3	179·	...	...	...	...	...	10·5
Wives Married at from 35-39 is 1 in										
Total Wives-Mothers, of Ages 20-44	32	166	753	204	26	5	3	1	1	1191
Proportion of above to 3875 surviving	121·1	23·3	5·1	19·0	149·	775·	1291·6	3875·	3875·	3·2
Wives Married at from 15-39 is 1 in										

women bear a child, on an average, about every two years, so long as they remain fecund. The data given in Table XL. show that successive children in a family succeed one another with an average interval of about 20 months. To these propositions I have to add the general consent, shown in the same place, that fertile wives breed generally every two years; consequently, that no class breeds, though individuals do, at shorter intervals; and no class breeds at longer intervals, though individuals do so. Considering these different statements, it is apparent to the student that there is no room left for any but a very inconsiderable number of women to breed at longer intervals than two years. For were there any considerable number of wives breeding at longer intervals, the averages just given would be far overpassed. And some of these averages are, as already shown, considerably less than were believed to be the true averages by writers who were not thinking of the law now demonstrated, but of the ordinary rate of time-fertility of married women.

Besides being of evident intrinsic value, the conclusions here arrived at will afford to medical men means of estimating the utility of the many vaunted methods of curing sterility which are now much in vogue, and which, considering the nature of the condition to be cured, justly excite anxiety for the honour of the profession in the minds of its best friends.

TABLE LXXVI.—Tenth Year of Married Life.

Number of Child . . .	1st	2d	3d	4th	5th	6th	7th	8th	9th	10th	11th	Total.
Wives-Mothers, of ages 25-29 .	...	1	14	30	78	51	8	4	...	...	...	186
Proportion of above to 594 surviving Wives Married at from 15-19 is 1 in . .	...	·594	42·4	19·8	7·6	11·6	74·2	148·5	...	...	...	3·2
Wives-Mothers, of ages 30-34 .	1	4	17	55	148	105	34	11	3	1	2	381
Proportion of above to 1528 surviving Wives Married at from 20-24 is 1 in . . .	1528·	382·	89·9	27·8	10·3	14·5	44·9	138·9	509·3	1528·	764·	4·0
Wives-Mothers, of ages 35-39 .	...	2	4	19	60	48	13	5	2	...	...	153
Proportion of above to 902 surviving Wives Married at from 25-29 is 1 in . . .	...	451·	225·5	47·5	15·	18·8	69·4	180·4	451·	...	...	5·9
Wives-Mothers, of ages 40-44 .	...	...	5	11	10	6	2	1	...	1	...	36
Proportion of above to 313 surviving Wives Married at from 30-34 is 1 in	...	...	62·6	28·5	31·3	52·2	156·5	313·	...	313·	...	8·7
Total Wives-Mothers, of ages 25-44	1	7	40	115	296	210	57	21	5	2	2	756
Proportion of above to 3337 surviving Wives Married at from 15-34 is 1 in . . .	3337·	476·7	83·4	29·0	11·3	15·9	58·5	158·9	667·4	1668·5	1668·5	4·4

TABLE LXXVII.—Fifteenth Year of Married Life.

Number of Child . .	1st	2d	3d	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	Total.
Wives-Mothers, of ages 30-34	1	3	2	6	11	18	24	28	18	2	1	1	1	116
Proportion of above to 532 surviving Wives Married at from 15-19 is 1 in .	532·	177·3	266·	88·7	48·4	29·6	22·2	19·0	29·6	266·	532·	532·	53·2	4·6
Wives-Mothers, of ages 35-39	...	...	5	4	18	32	53	41	29	14	2	1	1	200
Proportion of above to 1360 surviving Wives Married at from 20-24 is 1 in .	...	...	272·	340·	75·5	42·5	25·6	33·2	46·9	97·1	680·	1360·	1360·	6·8
Wives-Mothers, of ages 40-44	...	...	1	2	4	7	12	14	2	1	...	...	...	43
Proportion of above to 782 surviving Wives Married at from 25-29 as 1 in .	...	...	782·	391·	195·5	111·7	65·2	55·9	391·	782·	...	...	...	18·2
Wives-Mothers, of ages 45-49	...	...	...	1	1	1	...	1	2	...	1	...	...	7
Proportion of above to 262 surviving Wives Married at from 30-34 is 1 in .	...	...	...	262·	262·	262·	...	262·	131·	...	262·	...	...	37·4
Total Wives-Mothers, of ages 30-49 . . .	1	3	8	13	34	58	89	84	51	17	4	2	2	366
Proportion of above to 2936 surviving Wives Married at from 15-34 is 1 in .	2936·	978·6	367·	225·8	86·3	50·6	33·0	34·9	57·5	172·7	734·	1468·	1468·	8·0

TABLE LXXVIII.—Twentieth Year of Married Life.

Number of Child . . .	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	Total.
Wives-Mothers, of ages 35-39 .	...	...	2	5	5	17	15	9	3	...	56
Proportion of above to 477 surviving Wives Married at from 15-19 is 1 in . .	...	...	238·5	95·4	95·4	28·0	31·8	53·	159·	...	8·5
Wives-Mothers, of ages 40-44 .	1	1	2	9	14	28	8	13	2	2	80
Proportion of above to 1171 surviving Wives Married at from 20-24 is 1 in . . .	1171·	1171·	585·5	130·	83·6	41·8	146·4	90·1	585·5	585·5	14·6
Wives-Mothers, of ages 45-49 .	...	...	...	...	...	3	...	1	1	...	5
Proportion of above to 649 surviving Wives Married at from 25-29 is 1 in . . .	...	...	...	...	...	216·3	...	649	649	...	129·8
Total Wives-Mothers, of ages 35-49	1	1	4	14	19	48	23	23	6	2	141
Proportion of above to 2297 surviving Wives Married at from 15-29 is 1 in . . .	2297·	2297·	574·2	164·	121·	47·8	99·9	99·9	383·	1148·	16·3

TABLE LXXIX.—Twenty-Fifth Year of Married Life.

No. of Child	10th	11th	12th	13th	14th	15th	16th	17th	Total.
Wives-Mothers of Ages 40-44 .	2	2	1	1	...	...	...	...	6
Proportion of above to 408 surviving Wives Married at from 15-19 is 1 in	204·	204·	408·	408·	...	...	...	...	68·
Wives-Mothers of Ages 45-49 .	...	1	...	...	...	...	...	1	2
Proportion of above to 961 surviving Wives Married at from 20-24 is 1 in	...	961·	...	...	...	...	...	961·	480·5
Total Wives-Mothers of Ages 40-49	2	3	1	1	...	...	...	1	8
Proportion of above to 1369 surviving Wives Married at from 15-24 is 1 in	684·	456·	1369·	1369·	...	...	...	1369	171·

PART VI.

NOTE ON FORMULÆ REPRESENTING THE FECUNDITY AND FERTILITY OF WOMEN.*

By Professor Tait.

1. Dr. Matthews Duncan having requested me to point out to him some simple method of comparing the fertility of different races, I endeavoured, as a preliminary step, to represent by formulæ some of the chief results which he has obtained in his very lucid and elaborate papers recently read to this Society, and printed in their Transactions for 1863-4, and for the present session. Some of the formulæ which I have obtained are so simple, and accord so well with the Tables, that I have thought them worth bringing before the Society. Of course it must be understood that I advocate no theory, and pretend to no physiological knowledge of the question. I merely try to represent, in a simple analytical form, the contents of some of Dr. Duncan's Tables.

* These chapters are here republished, with a few alterations and additions, from the *Transactions of the Royal Society of Edinburgh*, 1865-6, by the permission of the Council of the Society, and with the kind acquiescence of Professor Tait. Their interest and importance are such as to make me very glad to be able to present them, in this place, to my readers.—J. M. D.

2. To prevent misconception, let us begin by defining the terms *fecundity* and *fertility*, as they will be used in this note, unless qualified in some manner.

By *fecundity* at a given age, we mean the probability that during the lapse of one year of married life, at that age, pregnancy, producing a viable child, will ensue. This is, in all likelihood, modified in each individual woman by the previous duration of marriage (see § 10 below). But at present, in dealing with the mass of wives, we omit this consideration. We do not require, in our calculations, to consider any questions connected with the duration of life of husband and wife, of the length of time the child may live, etc., as the numbers in the Tables are already influenced by such causes. The numbers in the Tables do not usually denote the fecundity as above defined, but are quantities proportional to its values.

By *fertility*, at any age, we mean the number of children which a married woman of that age is likely to have during the rest of her life, or some numerical multiple of it.

The subject divides itself into three heads—(I.) The fertility and fecundity of the mass of wives; (II.) Their value for the average individual; (III.) The *relative* fertility and fecundity of different races.

These we proceed to consider in order.

CHAPTER I.

FERTILITY AND FECUNDITY OF THE MASS OF WIVES.

3. If f_t represent the fecundity, and F_t the fertility at the age of t years, the ordinary laws of probability, if applicable to this question, give us the expression—

$$F_t = f_t + f_{t+1} + \ldots + f_{49} = \Sigma_t^{50} f_t$$

assuming that sterility arrives about the age of fifty.

[It is to be observed that the values of f_t, etc., dealt with here, are those of actual life. We do not mean by them something theoretical—as, for instance, what their values would be were a woman not prevented by nursing, etc., from exhibiting her full fertility. While it is to be feared that legitimate abstinence produces but little diminution, as regards the mass of the people, it is to be hoped that none is produced by the abominable methods sanctioned by certain advanced thinkers.]

Before going further, it may be well to verify this formula by comparison with the Tables, so that we may be assured of the validity of our reasoning.

Dr. Duncan gives (*Trans. R.S.E.*, 1863-4, p. 358)*

* Also p. 19 of this volume.

the following numbers for the wives in Edinburgh and Glasgow, *taken as a whole*:—

TABLE LXXX.—Showing Fecundity of Wives taken as a whole at different Ages.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Fecundity	50	41·8	34·6	26·6	20·4	8	1·3

The two last numbers are probably not so accurate as the others—one from vague statements as to "forty years of age;" the other on account of some omissions noticed in a footnote to the Table. As, unfortunately, we cannot get data for each year separately, we can only test the above formula for intervals of five years. The numbers just given may therefore be taken as *proportional* to f_{17}, f_{22}, f_{27}, f_{32}, f_{37}, f_{42}, and f_{47}, respectively.

4. We may now construct the second line of the following Table, according to the formula above, by adding to the number for any quinquennial period all those which follow it:—

TABLE LXXXI.—Showing Fertility calculated from Table LXXX.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Fertility	182·7	132·7	90·9	56·3	29·7	9·3	1·3
F_t	12	8·9	6	3·7	2	0·6	0·1

The numbers in the last line are proportional to those in the second, on the assumption that a woman of 15-19 will have a family of twelve.

Dr. Duncan quotes (*Trans. R.S.E.*, 1865-6, p. 302)* from the journal of the Statistical Society the following table of values of F_t for the mass of married women, in the district of St. George's-in-the-East. This is, unfortunately, not quite comparable with the last, as the quinquenniads differ by one year of age; and, besides, the ages at marriage differ in the different columns. But there seems to be no attainable Table so nearly approaching what we require for comparison.

TABLE LXXXII.—Showing Fertility at different Ages.

Age	16-20	21-25	26-30	31-35
F_t	10·85	8·24	5·00	4·00

Neglecting the difference of the quinquenniads in the two tables, and taking 11 instead of 12, for the sake of direct comparison, as the value of F at 15-19 in the first, we have—

TABLE LXXXIII.—Showing a Comparison of Tables LXXXI. and LXXXII.

Calculated	11	8·16	5·5	3·4	1·9	0·55
Observed	10·85	8·24	5·0	4·0		

* Also p. 144 of this volume.

These numbers agree as well as could possibly be expected.

5. If we project the numbers above given for $f_{17} \ldots f_{47}$, and try to represent the values of f for all ages by the ordinates of a curve, whose abscissæ denote the corresponding ages, we have the continuous curve of the following diagram.

The straight line, which almost coincides with the continuous curve—at least from the age of 17 to that of 40—and whose departure from it above that age must depend to some extent on the defects of the Table pointed out in § 3, intersects the axis at 50. We may obviously assume it as very nearly representing the Tables. And we can therefore express the value of f_t by a number proportional to $50 - t$. Thus—

$$f_t = k\,(50 - t)$$

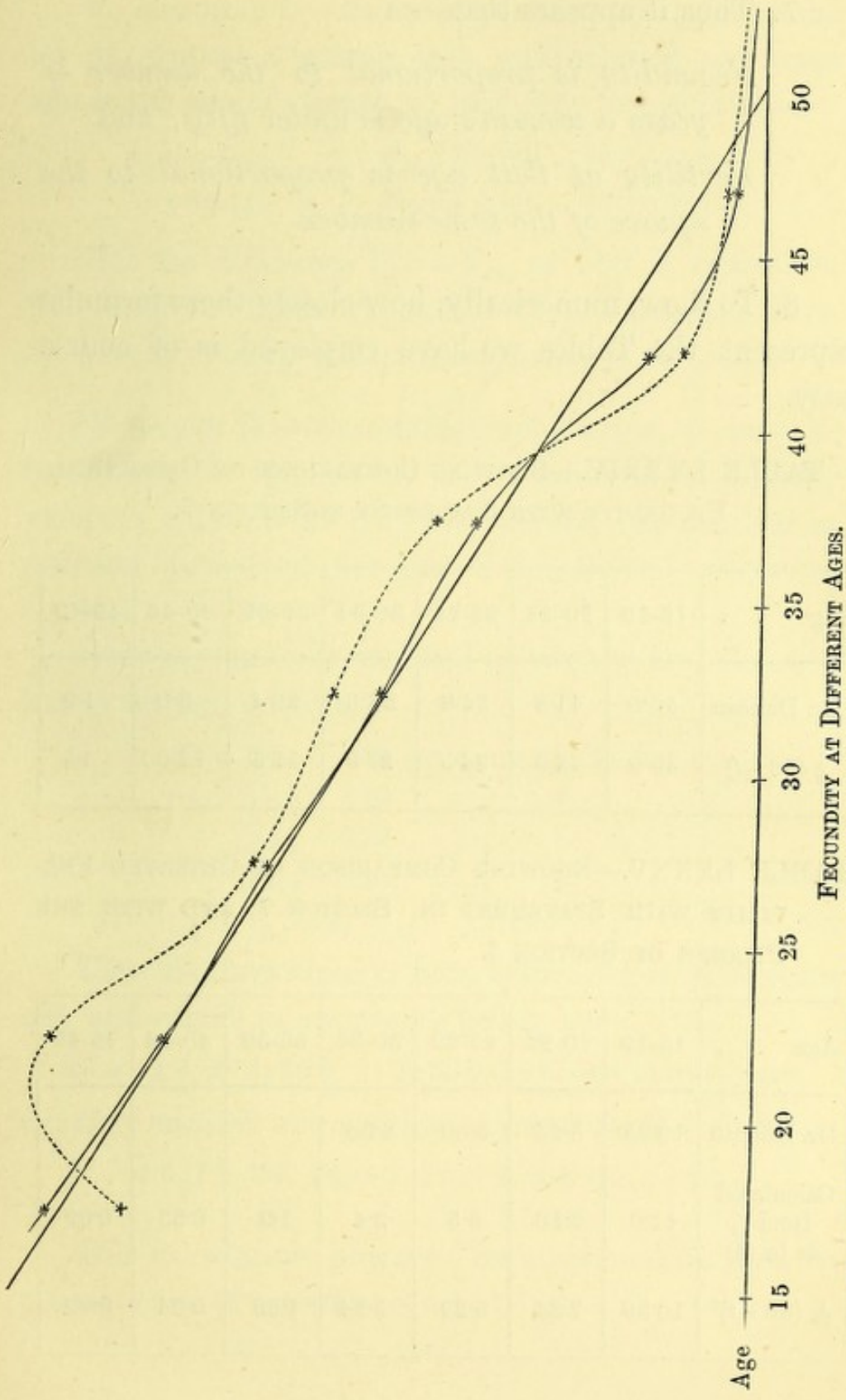
(where k is a *number*, whose value we can easily find) is a simple formula very closely representing the tabulated results.

6. But F_t can now be represented in a form almost as simple. For—

$$F_t = f_t + f_{t+1} + \ldots\ldots + f_{49}$$

$$= k \left\{ (50 - t) + (49 - t) + \ldots\ldots + 1 \right\} = \tfrac{1}{2}\, k\,(50 - t)\,(51 - t)$$

$$= \tfrac{1}{2}\, k\,(50 - t)^2, \text{ nearly enough for our purpose.}$$



FECUNDITY AT DIFFERENT AGES.

The continuous curve is founded on Edinburgh and Glasgow Tables. The dotted curve on Tables of Sterility.
The straight line from the formula $k\,(50-t)$

7. Thus it appears that—

Fecundity is proportional to the number of years a woman's age is under fifty; and

Fertility at that age is proportional to the square of the same number.

8. To show, numerically, how closely these formulæ represent the Tables we have employed is of course easy.

TABLE LXXXIV.—Showing Comparison of Observed Fecundity with Statement in Section 7.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Dr. Duncan	50·0	41·8	34·6	26·6	20·4	8·0	1·3
$\frac{3}{2}(50-t)$	49·5	42·0	34·5	27·0	19·5	12·0	4·5

TABLE LXXXV.—Showing Comparison of Observed Fertility with Statement in Section 7, and with the Process of Section 3.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Dr. Duncan	10·85	8·24	5·00	4·00			
Calculated from f_t as in (4)	11·0	8·16	5·5	3·4	1·9	0·55	0·09
$\frac{1}{10}(50-t)^2$	10·89	7·84	5·29	3·24	1·69	0·64	0·09

9. *Example* I.—As an application of the formula, let us suppose a woman, who was married ten years ago at the age of twenty, to have now five children :—

At marriage . . $F_{20} = \frac{1}{2} k (50 - 20)^2 = 450 k$
At present . . $F_{30} = \frac{1}{2} k (50 - 30)^2 = 200 k$

But the difference $F_{20} - F_{30}$, or 250 k, represents five children. Hence F_{30}, or 200 k, represents four more. So that her family will probably amount to nine.

[*Example* II.—As another application, it may be interesting to inquire *at what intervals* a healthy woman's children are likely to succeed one another at different periods of her life, if the above formulæ be correct.

Suppose her to be married at twenty, and that she has in all ten children—

Then F_{20} = 900 represents 10 children.
$F_{21\cdot6}$ = 810 „ 9 „
$F_{23\cdot2}$ = 720 „ 8 „
$F_{24\cdot9}$ = 630 „ 7 „
etc. etc.

Thus the first child is born before she is 21·6 years old, and others in succession before her

23·2, 24·9, 26·8, 28·8, 31·1, 33·6, 36·6, 40·6th, etc., years,

i.e. they succeed one another at intervals of (roughly)

1·6, 1·7, 1·9, 2·0, 2·3, 2·5, 3·0, 4·0, etc., years.

This calculation, however, requires modification, in consequence of a result soon to be given.]

10. As illustrating the subject further, I append portions of another of Dr. Duncan's Tables (*Trans. R.S.E.*, 1865-6, p. 306),* with formulæ for comparison, roughly founded on the type $f_t = k\ (C - t)$.

TABLE LXXXVI.—Showing the Modification produced by Age at Marriage upon the Formula representing Fecundity.

Age at Marriage		Age 20-24	Age 25-29	Age 30-34	Age 35-39	Age 40-44	Age 45-49
15-19							
	Table	64·1	52·1	36·2	19·6	2·4	
	3 (43—age) . .	63·0	48·0	33·0	18·0	3·	
20-24							
	Table	...	61·7	41·7	24·5	11·4	0·3
	3 (46—age) . .	...	57·0	42·0	27·0	12·0	0·0
25-29							
	Table	...	...	40·6	28·2	9·1	1·3
	2·66 (47·5—age).	...	...	41·4	28·0	14·7	1·4
30-34							
	Table	...	...	...	34·0	19·2	4·5
	3 (48·5—age) .	...	...	...	34·5	19·5	4·5

These formulæ seem to represent the tables pretty closely—with the exception of a solitary number for those married at 25-29—and, if they may be trusted, indicate a very curious result. Of course, when the fecundity is given by an expression of the form $k\ (C - t)$, C is the age at which sterility arrives.

* Also p. 153 of this volume.

Now, it appears from the last given Table, that we have for wives married at

	Fecundity.
15–19	k (43—age)
20–24	k (46—age)
25–29	k (47·5—age)
30–34	k (48·5—age)

In words, *the advent of sterility* is hastened by early marriage.*

Thus sterility occurs according to the following Table :—

TABLE LXXXVII.—SHOWING THE AGE AT MARRIAGE, AND THAT OF THE ADVENT OF STERILITY.

Age at Marriage.	Age of Sterility.
15–19	43
20–24	46
25–29	47·5
30–34	48·5

This is singular enough, and seems to be well borne out by the Tables, since the age of sterility is *uniformly* later as the age at marriage is greater. But, of course, far more extensive observations must be made and discussed before such a point as this can be settled.

Accepting it, however, for the present, we may calculate from the last Table, and the Table of fecundity already given, the whole fertility as depending on the age at marriage. For, if t be the age at marriage, C_t the corresponding age of sterility—

$$\text{Whole fertility} = \tfrac{1}{2}\, k\, (C_t - t)^2.$$

In this formula k is to be found. But we have

$$\text{Fecundity at marriage} = f_t = k\, (C_t - t).$$
$$\text{Hence, whole fertility} = \tfrac{1}{2}\, f_t\, (C_t - t).$$

* This is the *relative* sterility of other parts of this work.—J. M. D.

If we accept 10 children as representing the whole fertility at 15-19, which seems a reasonable assumption, we have

$$10 = \tfrac{1}{2} f_{17} (43 - 17) = 13 f_{17}.$$

Hence $f_{17} = \frac{10}{13}$, from which (by proportion) the other values of f are easily found. Hence—

TABLE LXXXVIII.—SHOWING THE INFLUENCE OF THE ADVENT OF STERILITY UPON THE WHOLE FERTILITY OF MARRIAGE.

Age at Marriage.	Whole Fertility.	F.
15–19	10·	10·
20–24	7·7	7·4
25–29	5·5	5·0
30–34	3·4	3·1

The last column has been added for comparison, so as to show how the later advent of sterility in the more advanced marriages increases the fertility.

It may be well to notice that the interpretation of the expression $f_{17} = \frac{10}{13}$ is, that *a wife of* 15-19 *will, on the average, become pregnant at* 1·3 *years after marriage*—that is, she will have a child within about two years of marriage. This limit of time depends, however, on our assumption of 10 children as the measure of the fertility at 15-19, and childless marriages are included in the data. Dr. Duncan gives (*Trans. R.S.E.*, 1865-6, p. 297)* 1·52 as the average

* Also p. 132 of this volume.

interval between marriage and the birth of the first child. The reason of the discrepancy is of course this, that in our calculation the mass of wives is considered, and in Dr. Duncan's only fruitful marriages are taken account of.

[Take again the first example of § 9, with this improvement in our formulæ. As the average age of sterility for age of marriage 15-19 is 43, and, for 20-24, 46, we may take that at 20 as 45.

$$\text{Hence } F_{20} = \tfrac{1}{2} k (25)^2 = \frac{625}{2} k,$$

$$\text{and } F_{30} = \tfrac{1}{2} k (15)^2 = \frac{225}{2} k.$$

The difference is 200 k, which represents 5 children. Hence F_{30} represents $\frac{225}{80} = 2{\cdot}8$ children more. The family will therefore probably amount to 8, a considerable improvement on the former result. It would be easy to carry these speculations farther; to show, for instance, that (so far as our present data are concerned) we have very nearly

$$50 - C_t \propto F_t.$$

i.e. that *the advance of the date of sterility due to early marriage is great in proportion to the fertility at marriage.* But our data are as yet far too slender to allow us safely to build upon them conclusions (like this) of a much higher order of uncertainty than those already given.]

CHAPTER II.

FECUNDITY AND FERTILITY OF THE AVERAGE INDIVIDUAL.

11. If we endeavour to derive formulæ of a similar character from the Tables of absolute sterility,* the results are not quite so simple. Thus we find (*Trans. R.S.E.*, 1865-6, p. 319)—†

TABLE LXXXIX.—Showing the Percentage of Sterility in Women married at different Ages.

Age at Marriage	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Percentage Sterile	7·3	0	27·7	37·5	53·2	90·9	95·6
Percentage not Sterile	92·7	100	72·3	62·5	46·8	9·1	4·4

From the manner in which this Table was formed, it would appear that we are to a certain extent justified in taking the numbers in the last line as proportional to the average fecundity at the respective ages. But the curve representing these numbers differs considerably more from a straight line than that derived from the other Tables. It is the dotted curve in the figure.

* This is the sterility (not *absolute*) of other parts of this volume.—J. M. D.

† Also p. 200 of this volume.

It is true that if we consider the loose way in which women from 30 to 40 call themselves 30, and those from 40 to 50 call themselves 40, we might expect the smaller ordinates belonging to higher ages to be pushed back as it were towards 30 and 40, thereby apparently accounting for the two depressions which appear in the curve about those ages. That this is no fancied explanation may be gathered from the following scandalous facts recorded in the Census Report of 1851, p. 24 :—

In 1841 the number of girls, of ages 10-15 was	1,003,119
But in 1851 these had become young women aged 20-25, and numbered	1,030,456

This number, when corrected from the tables of mortality, obviously includes about 140,000 women whose ages had increased by less than 10 in ten years.

Again, in 1841 the number of women aged 20-25 was	973,696
But in 1851 those who had reached 30-35 were only	768,711

indicating at first sight a fearful death-rate, but really showing how strong is the desire, among women at least, to be considered as remaining under the magic limit of thirty years of age.

To complete the examination, however, let us see how far these data from sterility agree with the formula deduced from the other considerations.

TABLE XC.—Showing Comparison of Fecundity as derived from Sterility, with the Statement in Section 7.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
Percentage not Sterile	92·7	100	72·3	62·5	46·8	9·1	4·4
$3\frac{3}{7}$ (50–age)	113·1	96	78·9	61·8	44·5	27·4	10·4

It is easy, of course, to construct a formula to represent any series of numbers, but unless it be simple it is of little use; and the approximation we have got seems close enough, if we remember the almost certain deficiencies in the numbers for the two highest ages, and the immaturity, etc., which may easily be supposed to account for that at 15-19.

But there is another cause which may serve to account for part of the discrepancy, as in fact Dr. Duncan's Table shows. This is, that *plural births are not eliminated.* In fact, at age 20-24 there are a good many more children per annum than mothers in his Table, which thus virtually assumes that no woman of 20-24 is sterile. This accounts for the great rise in the (dotted) curve of the figure at the age of 22.

Taking it as we have it, however, let us try how far it accords with our former results. By the process of § 3 we form the first line of the following Table. The second is formed on the type $F_t = \frac{1}{2}\, k\, (50 - t)^2$.

TABLE XCI.—Showing Comparisons of Results respecting Fertility derived by applying the Process of Section 3 to Table LXXXIX., with the Statement in Section 7.

Age . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49
F_t from f_t } observed }	387·8	295·1	195·1	120·8	60·3	13·5	4·4
$\frac{19}{50}(50-t)^2$	414	298	202	123	64	24	4

This coincidence also is close enough, and would be still closer if we had the numbers for f_{50} and upwards; as the smaller numbers in the Table, where the deficiency lies, would thus be increased proportionally much more than the larger ones. The number for 15-19 is, of course, from what has been already explained, considerably in defect of the theoretical number.

CHAPTER III.

RELATIVE FERTILITY OF DIFFERENT RACES.

12. We may apply the above results to compare the fertility of different races—a problem of considerable interest. We shall not attempt a rigorous solution, for the application of which, indeed, we have no sufficient data ; but, to utilise as far as possible such data as we possess, we shall make one or two postulates, which will probably be easily admitted, and which will enable us to avoid complication.

13. Suppose that for ten or fifteen years we may consider the number of marriages at each given age to remain practically unaltered, we may then *consider the births in any one year as represented by the total fertility of those married in that year.* That is, the children born in that year of mothers married at 30-34, for instance, are due to those married last year, the year before last, and so on for fifteen years back, at the age of 30-34; and as the number is supposed nearly constant for some years, we have the fertility of *all* for one year (very nearly) by calculating the total fertility for the rest of their lives of those married in that year. As population, and with it the number of marriages, is generally increasing, this process will slightly exag-

gerate the numbers sought; but, in *comparing* two growing countries, such as England and Scotland, no perceptible error will be introduced.

14. We next *assume that the law of fertility as depending on age is the same in the two countries compared.* That is, we assume that

$$\frac{F'_t}{F_t} = \frac{F'_{t+1}}{F_{t+1}} = \frac{F'_{t+2}}{F_{t+2}} = \text{etc.} = e,$$

where e is some definite number; and F_t, F'_t represent the fertility in the two races at age t.

This will evidently be the case if the fertility be really expressible, as above, in the form

$$F_t = \tfrac{1}{2}\, k\, (50 - t)^2,$$

for two such expressions can only differ through the number k. But it will not hold if the age at which sterility arrives in the mass of wives, here represented by 50, should happen to be greatly different for different races. On this point we have no information.

15. Let, then, μ_t be the number of marriages of women at t years of age in any one year, β the number of legitimate births in a year, we have, by the above postulates

$$\beta = \Sigma\, \mu\, F = \mu_{15}\, F_{15} + \mu_{16}\, F_{16} + \ldots + \mu_{49}\, F_{49}.$$

For another country

$$\beta' = \Sigma\, \mu'\, F' = e\, \Sigma\, \mu'\, F,$$

where e is, as above, the ratio of the fertility of the second race to that of the first. These equations give

$$e = \frac{\beta'}{\beta} \cdot \frac{\Sigma\, \mu\, F}{\Sigma\, \mu'\, F}$$

where the *absolute* fertility of either country is no longer involved, so that we may employ for the values of F the expressions in § 4, or those in § 8.

16. *Example.*—From the Registrar-Generals' reports for England and Scotland, we extract the following data :—

TABLE XCII.—Showing Number of Marriages and Number of Legitimate Children in England and Scotland in one Year.

	England, 1864.	Scotland, 1862.
No. of Marriages . . .	180,387	20,597
Legitimate Births . . .	692,827	96,693

TABLE XCIII.—Showing Percentage for each Quinquenniad of Women married in one Year in England and Scotland, and the Corresponding Fertility from Table LXXXI.

	15-20	20-25	25-30	30-35	35-40	40-45	45-50
England . .	13·60	49·74	19·55	7·28	3·89	2·67	1·57
Scotland . .	13·07	46·28	24·13	8·54	4·36	2·03	0·95
And we assume, in accordance with §§ 8 and 9—							
F proportional to	12·0	8·9	6·0	3·7	2·0	0·6	0 1

This gives $\Sigma \mu F = \frac{20,597}{100}[12 \times 13{\cdot}07 + 8{\cdot}9 \times 46{\cdot}28 + \ldots]$

$= 20,597 \times 7{\cdot}55$ for Scotland.

Also $\Sigma \mu' F = 180,387 \times 7{\cdot}595$ for England.

Hence $e = \frac{692,827}{96,693} \cdot \frac{20,597}{180,387} \cdot \frac{7{\cdot}55}{7{\cdot}595} = 0{\cdot}812$ nearly.

A more accurate comparison would be obtained by employing the average number of marriages at various ages for five or ten consecutive years, instead of those in any one year, as above, which are liable to considerable fluctuations. But we have not data enough. It would appear then, that *the absolute fertility of the mass of married women in England is about* 80 *per cent only of that in Scotland.*

That the fertility is less in England than in Scotland has been shown by the Registrar-General for Scotland (*Report* 1866). But he makes the ratio considerably greater than the preceding estimate.

It is to be observed that if the insinuations we sometimes hear about Scottish marriages have any foundation in fact, their consideration would tend to make the difference in fertility between the two countries even greater than that just given; for legitimation *per subsequens matrimonium* does not shift the position of a child's name in the Registrar's books.

17. The fact that in England and Scotland the quantities $\Sigma \mu F$ and $\Sigma \mu' F$ are almost exactly proportional to the numbers of marriages in the two countries, shows that, although Scottish women, as a rule, marry later in life than English women, the long period (25-40) during which their marriage-rate exceeds that in England, as compared with the shorter period (20-25), during which it falls behind, almost makes up for the diminished fertility at the more advanced age.

18. It only remains to construct the values of the

quantities F_t for each country, taken, of course, from the mass of the wives.

As before (§ 15), we should have had

$$\beta = \Sigma \mu F$$

if we had used proper absolute values of F. But we used the numbers 12, 8·9, 6, etc., which are obviously too large. Reducing them all in the ratio ε to 1, and substituting for β, etc., their values, we get

$$96{,}693 = 20{,}597 \times 7{\cdot}55\ \varepsilon$$

This gives $\varepsilon = 0{\cdot}622$;

from which we construct the following Table :—

TABLE XCIV.—Comparing Actual Fertility of a Mass of Wives in England and in Scotland, for Different Ages at Marriage.

	15-19	29-24	25-29	30-34	35-39	40-44	45-49
Scotland . . .	7·44	5·54	3·77	2·30	1·24	0·37	0·06
England . . .	6·04	4·49	3·02	1·87	1·01	0·30	0·05

19. In conclusion, it may merely be repeated that we have attempted no elaborate or exact inquiry into this question; indeed the utter insufficiency of data would have rendered such a proceeding absurd; and we have, for the same reason, abstained from employing some of our own results, such as those of § 10, in modifying the earlier ones, by the help of which they have been arrived at. Thus, for instance, we should be led by the results of § 10 to use in the formulæ of §§ 5, 6, a number somewhat less than 50, as corre-

sponding to the average age of sterility. As in all questions of average, the value of our deductions in this matter is mainly dependent on the extent and accuracy of our data, and it is sad to think that the enormous blue-books which load our shelves contain so much painfully-elaborated information which is of no use, and so little of those precious statistics which would at once be easy of acquirement and invaluable to physiologists.

[By the kindness of M. Berg of the Statistical Bureau, Stockholm, I have recently been put in possession of the necessary data for Sweden, and Messrs. A. and C. Black have given me access to a number of the Reports of the Registrars-General for the three kingdoms. The results I have obtained from these data are given in the following Table.

The first column gives the whole number of marriages in each year.

The second the number of legitimate births.

The third contains numbers depending on the proportional numbers of marriages at each age, and may be considered as showing to what extent the customs of each country allow the married women *to take advantage of their fertility* by comparatively early marriage. The larger are the numbers in this column, the greater should we expect to be the proportion of births to marriages in each year. Or, from another point of view, if these numbers were the same for all the races, the relative fertility would be directly as the number of births and inversely as the number of mar-

riages. The numbers in fact represent how much of the original fertility (here assumed as twelve for age 15-19) is left at the average time of marriage.

The fourth column combines the data contained in the first three, and its terms are directly proportional to the fertility of each race.

The numbers in the fifth column represent for the mass of wives the real value of the fertility at 15-19.

It is curious to see how nearly the ratio given above between the fertility in Scotland and England is maintained; and to see that Ireland is almost exactly on a par with Scotland; while both are very inferior (so far as the mode of calculation may be trusted) to Sweden.

The second table, founded entirely on the averages of the others, gives the most probable values of F_t for women of 15-19 in each country.

These new results are certainly more trustworthy than those given above, inasmuch as they are founded on a much greater number of data; but it is certain that these data are not very trustworthy, and I have been obliged to assume in all my calculations Dr. Duncan's little Table from the parish of St. George's-in-the-East, as the *only* one obtainable for a verification (however rough) of the dependence of fertility upon fecundity.

In the above paper, however, I believe I have given all that is necessary for attacking those good and comprehensive data, which (it is to be hoped) the proverbial wisdom of our legislature will ere long furnish to the rapidly increasing thirst for knowledge among its all too patient flock.

TABLE XCV.—Showing Numbers of Marriages, and of Legitimate Births; also of the Amount to which Women by age at Marriage avail themselves of their Fertility; and the relative and absolute Fertility: in England, Scotland, Ireland, and Sweden.

Year.	Marriages.	Leg. Births.			Fertility.
ENGLAND.					
1863	173,510	680,276	7·51	·524	6·3
1864	180,387	692,827	7·61	·507	6·04
1865	185,474	701,484	7·60	·5	6·0
1866	187,776	708,369	7·606	·5	6·0
1867	179,154	723,163	7·59	·532	6·4
1868	176,962	740,520	7·706	·543	6·5
SCOTLAND.					
1861	20,896	97,080	7·494	·621	7·45
1862	20,597	96,693	7·55	·625	7·5
1863	22,234	98,393	7·52	·592	7·1
1864	22,725	101,136	7·50	·595	7·14
1865	23,621	101,808	7·48	·579	6·95
1866	23,629	101,994	7·406	·585	7·0
1867	22,618	102,896	7·36	·621	7·45
IRELAND.					
1864	27,406	131,234	8·006	·602	7·2
1865	30,802	139,600	7·79	·581	6·97
1866	30,121	141,221	7·76	·604	7·25
SWEDEN.					
1861	28,272	114,819	6·04	·672	8·06
1862	27,825	119,984	6·15	·701	8·40
1863	29,013	122,076	6·10	·690	8·28
1864	28,248	123,085	6·169	·706	8·47
1865	28,944	121,841	5·801	·726	8·71
1866	27,797	123,919	6·15	·725	8·70
1867	25,440	116,005	6·26	·728	8·73
1868	22,833	103,551	6·65	·682	8·18
1869	23,503	105,712	6·15	·731	8·77

TABLE XCVI.—SHOWING THE AVERAGE ACTUAL FERTILITY OF MARRIED WOMEN OF 15-19, IN ENGLAND, SCOTLAND, IRELAND, AND SWEDEN, AS DEDUCED FROM TABLE XCV.

England	6·21
Scotland	7·23
Ireland	7·14
Sweden	8·48

This would appear to show that while the fertility in England is only 86 per cent of that in Scotland, in Ireland it is 98 per cent, and in Sweden 117 per cent. It is possible, however, that in the latter country the fertility may be longer protracted than in the others, so that the mode of calculation adopted may have given it an undue advantage.

P. G. T.]

CHAPTER IV.

FECUNDITY, FERTILITY, AND STERILITY.*

THE statistician, economist, physiologist, and doctor, are allowed to feel an interest in the fertility and fecundity of the human race; but may not simple men and women consider what family they may probably have, and what risk of death awaits the woman at each successive childbirth? Few subjects can more affect their welfare, but after perusal of Dr. Duncan's book we perceive that few subjects have received less attention, and he will be the first to admit that the information he has gleaned is incomplete, though he has spared no pains in analysing the limited number of facts observed and recorded.

The main data used by him are obtained from the Register of Births in Edinburgh and Glasgow in the year 1855. He seems to know all about 16,301 wives whose children were registered in that year, and very properly regrets that an alteration in the schedule to be filled up by mothers prevented his and our acquiring equally complete information about those same and other wives in subsequent years. It is poor consolation to think that the alteration must have been agreeable

* Reprinted, with the author's sanction, almost entire, from the ninety-fourth number of the *North British Review*, for December 1867; being an article on the first edition of this work.

to sixteen thousand and odd wives, for, seeing how very much has been extracted from the registration in one year, and how very much remains unknown, we do wish the 1855 form had been retained, troublesome though it was. If women, as child-bearers, suffer remediable hardships, they must furnish the data by which the grievances may be proved. The suffering attending pregnancy and childbirth is so great, and affects so many persons, that great value must be set on statistics showing the circumstances under which least suffering is entailed and least risk run, and women may fairly be compelled to give the information which is required for their own good. Much folly has been talked about the rights of women, but those who most oppose the assumption by women of the parts now played by men should in consistency grant wives the right to bear and rear children with the least possible risk and labour. Who will dare to say that this condition obtains?

Dr. Duncan's book contains much information as to the risk entailed by marriage; but this information is still incomplete. The rough comparisons usually made between the deaths among single and married women of the same age tell us nothing, for married women belong to what an insurance company calls a class of selected lives. A presumption exists that men will on the whole choose healthy, well-to-do women, rejecting the sickly, deformed, poverty-stricken, and vicious, whose deaths cannot fairly be set off against those of the bride in childbirth.* Even from the full

* See Chapter IV. of Part VII.—J. M. D.

registration of 1855, Dr. Duncan can extract nothing as to the additional risk supposed to be entailed by rapid child-bearing. It is probable that some interval between successive children entails less risk than any other, and, if so, this interval should be known.

We may, indeed, be told that if we had the knowledge we crave we could not use it, but must let nature take its course. Let us know the facts before giving a decision on this point. We do not let nature take its course even now, but throw impediments in the way of excessive production by the civil obligations imposed by marriage laws, and these obligations are sanctioned by the highest morality. Let us first learn the facts accurately, and we may then consider how far they are or may be under our control.

Dr. Duncan gives some of the facts on which our reasoning must be based; for instance, his tables conclusively show the great rapidity with which young married women will probably bear children. Let us defer further consideration of the moral aspect of the question, and examine critically the facts he lays before us. The meaning of the terms "fecundity" and "fertility" must be first explained.

The fertility of a woman, or of a mass of women, is measured by the number of children born to that woman, or mass of women. We may speak of the past fertility, the future fertility, or the fertility during a given period of a mass of women; these several fertilities will be measured by the number of children born to the women during the periods named. We

may speak of the fertility of all the women in a given population, of the wives only, or of the mothers only; the same number may measure the fertility in the three cases, but the mean fertility of women, wives, and mothers will differ, inasmuch as the number by which the total fertility must be divided will differ in the three cases. The quality is in every case mensurable, and may, therefore, be the subject of exact knowledge. When a woman is called fertile, we mean that she has children; a very fertile woman has many children. Dr. Duncan further uses the term "persistently fertile," to express the fact that the women in particular tables have had children during the year in which the statistics as to their families have been collected; he also uses the words "intensely fertile" occasionally, to express the fact that a given mass of women have a great many children per annum, or in a given time.

The fecundity of women is measured by the same number as would measure the intensity of their fertility, or by the number of children they bear per annum; and it would perhaps be better to avoid the expression of intense fertility altogether, even when applied to a mass of women, some of whom may be sterile, or not subject to the conditions necessary for child-bearing. In one sense women who are capable of bearing children might be termed fecund, but Dr. Duncan's measurements of fecundity are necessarily drawn from those women only who are subject to the conditions required for child-bearing. The woman of unit fertility is the woman who has or will have one child. The woman

of unit fecundity might be defined as the woman who, subject to the necessary conditions, has or will have one child per annum. The above definitions are not quite the same as those given by Dr. Duncan, but they approach very closely to those given by Professor Tait, who has contributed a very valuable section to Dr. Duncan's work. Professor Tait says, "By fecundity at a given age, we mean the probability that, during the lapse of one year of married life at that age, pregnancy producing a living child will ensue." This definition will correspond with that given above, if in one average year of married life be included the average number of months of pregnancy; but there would be a difference of nine months between the ages at which fecundity as defined by Professor Tait and by us would be identical. We think the new definition preferable, because Dr. Duncan's tables give the ages at childbirth, not those at pregnancy. Of course, our definition would frequently give a fraction, such as 0·56 of a child per annum, as a measure of fecundity of each one of a given group of women. Those who find this idea difficult to grasp, may think of fecundity as inversely proportional to the interval of time between successive children;—the woman who has a child once in two years is twice as fecund as she who has a child once in four years; the fecundity of the first is 0·5; of the second, 0·25.

"By fertility at any age," says Professor Tait, "we mean the number of children which a married woman of that age is likely to have during the rest of her life,

or some numerical multiple of it." This is what we should call the probable future fertility of the woman at that age.

Having now cleared the way of the obstructions which an imperfect comprehension of the words would throw in our path, we will state a general law of great importance which Professor Tait has derived from Dr. Duncan's tables :—

"*Fecundity at various ages is proportional to the number of years a woman's age is under* 50."

This implies, for instance, that if at 20 a woman has a child once a year, at 40 the interval separating successive children would probably be three years, because the difference between 20 and 50 is three times as great as the difference between 40 and 50.

Strictly speaking, the law has been proved for a mass of women only, and would be more correctly stated as follows :—The average number of children per annum born to a mass of women of any one age is proportional to the difference between that age and 50. The accordance between observed figures and figures calculated by this law is wonderfully close. In order to prove its perfect applicability to individual cases, it would be necessary to verify it for large groups of women, each group including only women married at the same age; but we think there is every probability that the law does apply to individual women as well as to the mass. That some law exists for individuals is clear, or so very simple a law for the mass would be unintelligible; and it is

certainly most probable, that the simple result should depend on the addition of equally simple parts. We often find a complex result depending on the co-existence of a few simple elements. It is very rare to find a very simple result derived from complex elements; now the law of decrease of fecundity discovered by Professor Tait is the very simplest possible in form, and he therefore has stated it as applicable to individuals, though proved for a mass only.

According to this view, women are not likely to have children at constant intervals of time, but these intervals will probably increase with increasing age. Calculating the probable fecundity at 17 of an average woman from the data before us, we find that she will, if fertile, probably have her first child at about $18\frac{1}{4}$ years of age, and successive children at the ages of $19\frac{3}{4}$, $21\frac{1}{4}$, 23, $24\frac{3}{4}$, $26\frac{1}{2}$, $28\frac{1}{2}$, 31, 34, and 38; so that the interval between successive children will gradually increase from about eighteen months to four years. In making this calculation we have modified the law, as above stated, by making the fecundity proportional to the difference between the age at each child, and 43 instead of 50; for Tait further shows, by Dr. Duncan's tables, the curious result, that the advent of sterility is hastened by early marriage: thus a woman married at 17 will probably be sterile at 43, and, if married at 30, sterility will be delayed till $48\frac{1}{2}$.

This fact modifies the application of the general law to particular cases, but does not alter the simple form of the law.

The fecundity of various individuals varies of course within very wide limits, but Tait's formulæ allow us easily to calculate the number of children a woman may expect when we know how many she has had, and her age at marriage. The example is given of a woman who, married at 20, has already five children at the age of 30. She will probably have four more. The proof is as follows :—If fecundity is proportional to the number of years a woman's age is under 50, fertility at that age is proportional to the square of the same number. This is Tait's second law, which follows as a direct consequence from his first law, and from the definitions of fertility and fecundity. Now $(50—20)^2 = 900$, proportional to the fertility at 20 ; $(50—30)^2 = 400$, proportional to the future fertility at 30 ; the past fertility at 30 must have been proportional to 500, the difference between 900 and 400 ; so that as 500 represents five children, the remaining fertility of 400 must represent four children—*Q. E. D.* (Strictly, 46 should have been the limiting age in this case.)

It is very curious to observe, that while the proportional fecundity and fertility are thus known with considerable exactitude, the average fertility and fecundity of women is most imperfectly determined. Positively, some statistics about the poor in St. George's-in-the-East seem the only data by which any estimate of those most important numbers can be guessed at, and these statistics refer to only about eighty mothers whose cases are applicable to our object. We want to

know how many children a woman married at 15-19 is likely to have, if she and her husband both live together till the woman is past 50. It seems marvellous, that with the vast machinery of registration now at work, Dr. Duncan and Professor Tait should be driven to use data obtained from a single parish by a committee of the Statistical Society. But so it is. Once we know the average number of children a woman married at 17 may expect if she remain married till 50, Tait's law will allow us to distribute those children, and tell her at what intervals the children will probably follow one another. Conversely, if we knew the intervals at which children do succeed one another at given ages, and the age of the mother when married, we could calculate the total probable fertility of the woman; but none of these data are to be had for any considerable mass of women. Even the average interval between marriage and the birth of a first child is not known with any accuracy. In the Table XL. given by Dr. Duncan, he fails to show a true average for this interval. He has there treated all children born within two years of marriage as born at an average of twelve months after marriage, and, in calculating his average, has lengthened this interval only in proportion to the number of children born in subsequent years; but this calculation gives no true average, for, as the bulk is born within two years, the whole average will be far more affected by the average number of months elapsing between marriage and childbirth during these two years than by any other figures. In saying this we

do not blame Dr. Duncan. He had no data on which to ground a true calculation.

Tait's law shows that the youngest women capable of bearing children are the most fecund; but it does not necessarily follow that older women, newly married, should not be more fecund than their friends of equal age who have been longer married. The initial fecundity of newly married women may always be higher than that of women of equal age who have already borne children; and statistics lend some support to this view, but do not show the law of the decrease of initial fecundity.

The reader has now as complete a view of the consequences of Tait's formulæ as space will allow us to give. Let us examine Dr. Duncan's conclusions from the same facts. He has honestly given his own conclusions, and seems even to have refrained from comparing them with the deductions from Professor Tait's theory. We have never read a book in which less effort was made to twist facts to suit views. Our only criticism is, that the consonance between the facts and the theory has occasionally been lost sight of.

Dr. Duncan first shows, by several tables, that the comparative fertility per annum of the whole population increases gradually from the commencement of the child-bearing period of life until about the age of 30 years, and that then it still more gradually declines, being greater in the decade of years following the climax than in the preceding decade. Next comes the table showing comparative fecundity of the whole mass

of wives in our population at different ages, on which table the mathematical law is based. The comparison in the table, which we here extract, is made between wives at a given age and mothers at a given age :—

TABLE XCVII.—SHOWING THE COMPARATIVE FECUNDITY AT DIFFERENT AGES OF THE WHOLE WIVES IN EDINBURGH AND GLASGOW IN 1855.

AGES . . .	15-19.	20-24.	25-29.	30-34.	35-39.	40-44.	45-49.
Wives . . .	756	8,874	14,622	14,579	11,871	10,506	7,537
Wives-Mothers	378	3,709	5,065	3,872	2,421	845	96
Proportion of latter to former is 1 in. . .	2·0	2·4	2·9	3·7	4·9	12·4	78·5
Or percentage.	50·00	41·79	34·64	26·56	20·39	8·04	1·27

As one year would include too small a number to give a fair average, the wives and mothers are grouped in lustres of five years—15-19, 20-24, 26-30, etc.

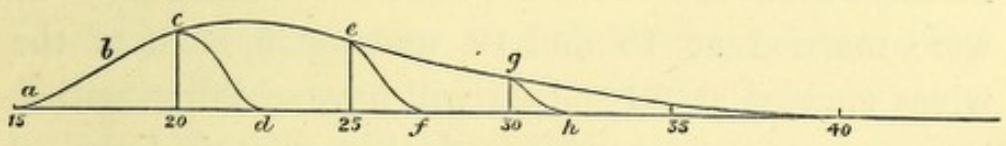
Some of the wives in each lustre must have been married for so short a time that their children can only appear in the next lustre. We do not, therefore, get from the table a measure of the probability that a wife at each age will have a child within one year. Moreover, the fecundity of the 15-19 lustre will, more than any other, be affected by this circumstance, for whereas the 20-24 period includes many mothers married at 19, the 15-19 period includes no mothers married at 14, the beginning of the 15-19 period

must be a mere blank, not because no women at that age are fecund, but because they have had no time to prove their fecundity. In the 20-24 period, on the contrary, children are counted who may be born the day after a woman is 20, she having been married in the previous lustre. Notwithstanding this disadvantage, half the wives between 15-19 were mothers, whereas only 42 per cent of the wives between 20-24 were mothers. A better measure of the fecundity of different ages would be obtained by comparing the wives of 15-19 with the mothers at 16-20; the only perfect measure would be obtained by observing the average interval between marriage and a first child, and between successive children at each age. Dr. Duncan could not obtain all these facts, but his conclusion is well established, "that the fecundity of the mass of wives in our population is greatest at the commencement of the child-bearing period of life, and after that period gradually diminishes."

The tables also show "that the fecundity of the whole wives in our population, included within the child-bearing period of life, is, before 30 years of age is reached, more than twice as great as it is after that period." So far Dr. Duncan and Professor Tait agree, but Dr. Duncan next says, "A different law governs individuals—their fecundity is greatest from twenty to twenty-five." He explains this by saying, that though less fecund, they are more fertile as a mass. This last is an intelligible and apparently true proposition if understood to mean that fewer young wives bear

children, but those who do bear, bear more rapidly; but we think Dr. Duncan fails to establish the proposition, that fecundity as we have defined it is for individuals greatest between 20 and 25—a conclusion which is entirely at variance with Tait's first law. Dr. Duncan has been led to his conclusion by tables purporting to show the initial fecundity of women at different ages. In these tables for a given year, the number of wives of given ages are compared with the number of mothers of the same ages, who have been only one and two years married; thus, in 1855, 700 wives were married between the ages of fifteen and nineteen, and in the same year 306 women of the same ages became mothers within two years of their marriage.

It will at once be evident that this table does not give the number of women married at 15-19 who have children within two years of their marriage; but it is this information we require to compare the fecundity of the 15-19 lustre with the fecundity of the 20-24 lustre. The overflow which slightly falsified the Comparative Fertility table, wholly falsifies the Initial Fecundity table. This can be made more clear by the use of a simple diagram :—



Let the ages of mothers be represented by even divisions on a horizontal line; let vertical heights be

drawn corresponding to the number of children born to mothers at each age within two years of marriage; then, if the curve *a b c e g* 40 bounds all these vertical heights, the area between the curve and the horizontal line will represent the total fertility of women at all ages in first-born children produced within two years of marriage. The curve will rise from nothing to a maximum between 20 and 25, because more women are married between 20 and 25 than at any other period of five years. The area between the horizontal line and the curve *a b c d*, will represent the number of children born to wives married at 15-19 within two years of marriage, some of these being produced when the wives are nearly 22; the area *c e f d* will represent the number born to women married at 20-24; the area *e g h f* will correspond to women married at 25-30. These areas will afford a fair comparison of the relative initial fecundity at each age, when the whole fertility has been divided by the number of wives married at each age; they will then show the percentage of children born within two years to wives married at each age. The areas bounded by straight vertical lines, as 20 *c e* 25, do not give this information; they do indeed tell us how many children were born to mothers between 20 and 25, but some of these mothers were married at 18 and 19, and, again, some of the wives married at 23 and 24 will have children within two years of their marriage, which are not included in the area 20 *c e* 25. This area does give the total fertility of wives between 20-25 in children born within

two years of marriage, but it does not give the fecundity, because the number by which the fertility should be divided is undetermined. It will be seen that the 20-25 area, thus bounded, begins high and ends high ; losing one triangular portion at the end which belongs to it by rights, but gaining more than an equivalent at the beginning; that the 15-19 period loses the large triangular part at the end, but gains no compensation at the beginning, not through any fault of the 15-19 wives, who are very fecund, but because girls at 13-14 are not fertile at all.

The error due to this cause increases the longer we make the period after marriage within which the children are counted. Thus Table X., which has misled Dr. Duncan, seems to show, with a two-year limit, that the initial fecundity of the younger women is less than half that of the elder, whereas Table IX., with a one-year limit, seems to show that the younger are only 25 per cent less fecund. Both conclusions are clearly erroneous. Dr. Duncan's explanation of the apparent discrepancy as to the fecundity of the mass and of individuals, is, therefore, not required. If it had been true that within two years of marriage women at 15-19 were far more sterile than those at 20-24, in anything like the proportion indicated by the Table X. of initial fecundity, those who did bear children would have had to bear them about twice as rapidly at 15-19 as at 20-24. We need not discuss this hypothesis, which is unsupported, and indeed not suggested by Dr. Duncan. Tables drawn up to give the areas *a b c d*, *d c e f*,

etc., prove conclusively that the women married at 15-19 have more children within two years of marriage than women married at 20-24.

Dr. Duncan, who has at heart the establishment of facts, irrespective of any theory, most kindly supplied these tables, which we print with his permission. They seem most curiously and perfectly to confirm Tait's law of the increased fecundity with the diminution of age even to the low limit of 16. We should certainly have expected, from various analogies given by Dr. Duncan, that fecundity would not begin as a maximum; but the tables seem to prove that it does. Whether we take the number of children written under the heading of wives at 19 as indicating the fertility at 19, or at 20, or even 21, when a two-years' limit is given, depends on the definition of fecundity; but whatever rule be adopted in this respect simply shifts the whole row of figures, leaving undisturbed the fact that the younger women have far more children within a given time after marriage than the older ones, and this law holds good within a period extending from the earliest to the latest age of child-bearing.

Dr. Duncan must in no way be considered responsible for the deductions drawn from these new tables, which he kindly sent to a complete stranger. His views may be wholly at variance with those in this article, though, as we assume his facts to be correct, we imagine that any difference in the conclusions drawn can only arise from different meanings attached to the word "fecundity."

TABLE XCVIII. Showing Number of Children born within One Year and Two Years of Marriage to Wives married at various Ages, grouped in periods of Five Years.

1. Age at Marriage . . .	15-19.	20-24.	25-29.	30-34.	35-39.	40-44.	45-49.	49-50.	Total.
2. Number of wives married per annum at ages in row 1	700	1835	1120	402	205	110	46	29	4447
3. Number of children born within one year of marriage to wives married at above ages	163	313	113	33	18	3	...	...	643
4. Ratio of row 2 to row 3 .	4·3	5·9	9·9	12·2	11·4	36·7	...	...	6·9
5. Corresponding percentage of children to mothers	23·28	17·06	10·09	8·21	8·78	2·73	...	...	14·46
6. Number of children born within two years of marriage to wives married at above ages	905	1528	523	164	47	4	1	...	3172
7. Ratio of row 2 to row 6 .	0·773	1·2	2·1	2·4	4·4	27	46	...	14
8. Corresponding percentage of children to wives .	129·28	83·27	46·70	40·80	22·93	3·63	2·17	...	71·23

TABLE XCIX.—Showing Number of Children born within One Year and Two Years of Marriage to Wives married at 15-19, grouped in periods of Twelve Months.

1. Age at Marriage . . .	15.	16.	17.	18.	19.
2. Number of wives married per annum at ages in row 1 .	...	43	108	225	314
3. Number of children born under one year of marriage to wives married at above ages .	2	7	31	56	67
4. Ratio of row 2 to row 3 .	...	*6·14	3·48	4·00	4·68
5. Corresponding percentage of children to mothers . .	...	*16·3	28·7	24·9	21·3
6. Number of children born within two years of marriage to wives married at above ages . .	27	98	177	276	323
7. Ratio of row 2 to row 6 .	...	0·439	0·61	0·817	0·972
8. Corresponding percentage of children to wives . .	...	228	164	122	103

* These figures, which seem to show small fecundity in women married at 16, are largely affected by immaturity.

Dr. Duncan shows, that when women are married at 15-19, they are more likely to be wholly sterile than when married at 20-24, unless indeed his conclusion is falsified by the absence of correction for twins; but allowing, as is probable, that his conclusion as to absolute sterility is correct, this fact affects a very small percentage of the younger women. A true measure of initial and subsequent fecundity, as defined in this article, can only be obtained from data showing the average interval at various ages between marriage and the birth of a first child, and between successive children. Dr. Duncan, in Tables XL. and XLI., gives probably the best information ever yet collected on this subject; but his average of seventeen months between marriage and the birth of a first child, as already explained, is to be received with caution. The true average could only be obtained from data giving information month by month, and these data are wanting. Table XLI. gives twenty months as the average interval between successive births (or more truly, as the author says, the average interval between marriage and the birth of the child divided by the number of children born).

This average, though far more carefully obtained than any preceding estimate, is not quite satisfactory. The table is formed by taking a mass of women who, up to 1855, have borne a given number, say six children, and ascertaining the average length of time which has passed between the marriage of the mothers and the birth of the last child. For mothers of six

children this was 137 months. This time divided by 6, gives 22·8, which may be roughly called the average interval between successive births. The table at first sight seems to show that the first, second, and third children follow one another very rapidly, as Tait's law would indicate, that for subsequent children a very constant period of about twenty-two months is observed; but that after the fourteenth child, births succeed one another with an alarming rapidity, at intervals falling at last to about eleven months, in apparent direct contradiction with Tait's law.

This, as pointed out by Dr. Duncan, is not the true meaning of the table, which simply shows that women who have 16 children or more have them very fast, which we might have guessed. These women of high fecundity, who also bear their 6th, 10th, 14th children very rapidly, bring down the average periods as above calculated for all these ages, and affect the average more and more, as the size of the families increases, for which the average is calculated. We should like to know the average time separating births for women who in all have two, three, ten, sixteen children; and again for each class the average time, separating the first from the second, the second from the third, and so forth. This Dr. Duncan cannot give us—by no fault of his. But though he is fully aware of the limited deduction to be drawn from his table, we think he should qualify slightly his conclusion drawn from it, that a wife who having had children has ceased for three years to exhibit fertility, will probably have no

more children. If Tait's law be right, the average interval varies at each age, and varies so considerably, that whereas the probability of relative sterility may at 18-19 be decided within eighteen months, at 34-39 it may not be decided even within three years. Dr. Duncan himself indicates this conclusion, but does not, we think, insist upon it sufficiently. Again, the probability of the relative sterility of a woman of high fecundity would be much sooner determined than that of a woman of low fecundity. Dr. Duncan divides women in his tables into fertile and persistently fertile, the definition of the latter being simply women who have borne a child during the year in which the particular information in the given table was collected. The division is perfectly rational, but the reader must not conceive that women really are of two kinds, one of which has a reasonable number of children, while the other is an awful being, specially liable to produce sixteen or nineteen children. The only true division appears to be that indicated by Tait's law—women of high, low, and intermediate degrees of fecundity; the woman of low fecundity breeds at greater intervals, and after a few children the probable interval becomes so great as practically to amount to probable sterility.* Thus, in the instance above given, our average woman married at 17, though not sterile till 43, would probably not have a child after 38. The woman of high fecundity breeds at short intervals, and though these increase they do not pass the limiting age till a very numerous

* The *relative* sterility of other parts of this book.—J. M. D.

progeny has been born. This suggests an explanation of two curious results, which seem indicated by the tables. Women married late, who do have children, seem to have a higher fecundity than the average of married fertile women at the same age. May not this be explained by a supposition that at the later ages only women of high fecundity have children? and may not the same circumstance explain the curious fact, that women married late, who are fertile, continue to bear children later in life than the average of women married younger? The highly fecund will in all probability bear later. This would explain apparently all the observed results, without any hypothesis involving a different law of decrease for initial and subsequent fecundities, or for the advent of sterility in women married at different ages.

The laws of fecundity and fertility are interesting in many ways. A true comparison of the fertility of different races can only be made when those laws are known. This is well shown by Professor Tait, who, as one result of the application of his formulæ, tells us that the absolute fertility of the mass of married women in England is only about eighty per cent of that of Scotland,—a conclusion arrived at after the influence of the varying age at marriage has been eliminated in accordance with the law of decrease of fecundity. We hope that similar reasoning may be applied to some inferior races, who seem to be endowed with very high fecundity. We may then speculate on the number of years which would be required for the extinction of the

superior race if the lower race were not happily deprived of some other qualities useful in the struggle for life, such as the power of producing food, attending to sanitary arrangements, and keeping the peace. Those who do not advocate the extinction of savages, as Mr. Roebuck was supposed to have done, should really reflect, that if we were ever to succeed in imparting but a small fraction more of some of those useful qualities to our savage competitors we should infallibly be extinguished ourselves, owing to the high value of their F_t as Tait calls it. We cannot help believing that, if Professor Tait's figures are right, England must at no very distant time be peopled with the Scots race only. We are certain that, once born, a Scotsman is quite as able to take care of himself as an Englishman. If, in addition to this, the Scots wives are twenty-five per cent more fertile, their children will necessarily form an increasing fraction of the whole population, and unless this whole population itself increases fast enough, the English element will vanish. A Darwinian might say that this is a providential arrangement for improving mankind, but we do not like to see this high fertility quality counting so heavily in the scale of merit.

The believer in Malthus may now calculate the age at which marriage may be allowed, after determining the number of children per marriage which he desires that the population should produce. If three children were allowed, women might marry at 30, without further restriction as to production; whereas, on the rough calculation adopted, according to Dr. Duncan

by Malthus himself, that women might produce once in two years till the age of 50, the Malthusian three-child marriage would have been necessarily delayed till the age of 44. Dr. Duncan's criticisms on the assumptions both of Malthus and his opponent Sadler are excellent, and show how roughly this great problem was treated by them. The contrast drawn by Malthus between the increase of corn and the increase of mankind is fallacious. Plants and men are limited in number by similar checks. Each perpetually wages a Darwinian struggle for existence, and the analogy between the struggles is perfect but for the one fact, that man by the exercise of his will can impose a preventive check on his increase, whereas beasts and vegetables cannot. Nevertheless, the main doctrine, that if mankind bred as fast as possible, they would produce, in fully populated countries, more children than could be supported by the produce, remains quite unassailed. Hunger and want do impose a preventive check; but if these be the only efficient motives of abstinence, we may be certain that, as the average of human life is prolonged by sanitary and other improvements, so the living population will increase more and more rapidly, with hordes of wretched beings, barely able to maintain existence, and yet only restrained from further procreation by their misery. Can any prospect be worse than this? and are not Malthus and Mill right when they call upon us to exercise our privilege of free-will after more noble fashion, and to renounce our animal instincts in order to attain a higher ideal of

life? Few dare to write and say it were well that married men and women should voluntarily limit their families; yet if it were established that by so doing poverty could even in some degree be banished, and the minds of men even a little raised, what duty could be more sacred than the restraint of self-indulgence with such an object? Unfortunately, the case is by no means clear. In the first place, it is hard to determine what increase of population in a given country is admissible. Sardinia of old, with a large population, could feed other countries; now it is stricken with great poverty when its population is very small. The island of Singapore, a few years since, supported a few savages; it now bears a large and rich town. These facts do not contradict Malthus, but they do show that, *à priori*, it is impossible to fix on the proper ratio between so many square miles of earth and the desirable population. In the second place, even if we were to grant that in a given country, say England, the increase of population outstripped the increase of production so as necessarily to entail pauperism, this consideration could only influence the very class which it is desirable to multiply—the moral, the prudent, and the well-to-do. The bad men would put themselves to no restraint for the sake of children, nor yet the thoughtless. As for the pauper, if sheer want does not restrain him, we see that prudence will not. He has no pride, and cares not that his children should be paupers too. It is only the man with a position who fears that his children should lose caste.

Well, then, how can Mr. Mill ask the rich, the well-educated, the moral classes, to abstain from producing children, when they know that by so doing they will simply make way for the children of the ignorant and brutal? Less cogent arguments are urged by some, who fear that without the sharp spur of want mankind would not work, and that, as the population dwindled, rank after rank of the great army of mankind would fall out as stragglers, and so production too would dwindle, and poverty be, as now, master of the world.

Another argument is this:—Now the strong and able are selected, and thus, year by year perhaps, the race improves or does not fall off. If the conditions of life are so altered that the weak and foolish too can live and produce, the race will be gradually enfeebled. Some say this action is observed in France, and that, while the population remains constant, the conscripts are feebler year by year. Good tending of children might, however, more than make amends for the diminished range of selection. Comfort may, perhaps, rival death as the improver of the human race; but while all these more or less plausible and possibly true arguments can be urged against Malthus, no large class will on public grounds abstain from producing children, especially while public opinion is adverse rather than favourable to restraint. In some other countries, indeed, marriage does not in public estimation imply a duty to bear the utmost possible number of children, but our author evidently feels himself on tender ground

when he suggests that perhaps more than ten children may constitute an excessive family. This family of ten is a probable family when the bride is young, and both husband and wife live till she is 50. The average of families is not half ten, but this average includes late marriages, and all cases in which husband or wife dies. When a girl of 17 is married, she may expect nine or ten children. If she and her husband hope to escape with less, they are hoping for sterility or death.

Sickness is counted in computing the average; if man and woman retain their health, their children will in all probability exceed ten. It is worth while to consider the arguments as to whether our young couple are in duty bound to produce all these children. Those who answer yes must argue as follows:—Abstinence on the part of the husband will tempt him to immorality, and on the part of the wife it implies a slothful reluctance to bear the burden of life; by refusing to bear children she is shirking a duty and hurrying her husband into temptation. Further, if husbands and wives come to think the limitation of children a desirable object, they will not be nice as to the means; and thus foul practices will be engendered, and infanticide increase. These arguments are, we think, every one of them sound, and in part true. The answers might be stated thus:—If men must be prepared for large families, they cannot afford to marry young. If not married, they are tempted while young, and the large unmarried population of young men nourishes the vast prostitution too well proved. Choose,

then, between young marriages with subsequent abstinence, or late marriages with early abstinence. Will one system cause more temptation than the other? Next, our protagonist would deny that the reluctance of a woman to bear many children implied a reluctance to perform her duties. Is it not the duty of a mother to tend and educate her children, and can she perform this duty if her life alternates between the sickness of gestation and the occupation of nursing the last-born infant? Can women of the lower class thus occupied perform their household duties? Can women of the upper class preserve their mental culture? Next, he would argue that to acknowledge an object as desirable does not imply approval of all means by which to attain it. Paupers and lunatics are evils, but we do not sanction their murder; infanticide would decrease if the misery of large families decreased.

Several of these rival arguments turn on matters of fact which can be observed, such as the prevalence of infant mortality in countries in which public opinion approves or disapproves of a limitation to the size of families. We can hardly hope to determine by observation the relative mental and moral culture in large and small families, but statistics might be collected showing whether children born in rapid succession are as healthy as others; we suspect that their death-rate would be found sensibly above the average. But though statistics cannot do more than this, each father and mother may, in their own case, consider whether, while their children follow one another in rapid succes-

sion, they are well cared for and duly educated; a nurse is, at best, a poor substitute for a mother, and among the poor, the nurse is a little ignorant child. Women who believe that in bearing children year by year they are fulfilling a sacred though painful duty, may ask themselves whether they are performing their duty to the children already born.

It is probable that no general rule can be established, but that each case must be decided on its own merits. Neither the arguments of Malthus, nor any others, apparently justify us in calling on a healthy couple to limit the number of their children, when these will receive a fair education and such an outfit as will enable them to produce so much wealth by their labours as will probably insure them against want. It may well be doubted whether, for the sake of self-indulgence, a little more rest, a little more wealth, such a couple would be justified in placing a limit to the number of their children. But think of another and too frequent case. Think of a man and woman struggling with poverty, absolute or relative, with more children already than they know how to educate, to clothe, even to feed. Think of the woman, bowed with ill-health, peevish from petty trials; think of the children, each on its arrival regarded as a misfortune if not a curse, growing up unhealthy, ill-cared-for, dirty, ignorant, with no better prospect than to repeat the life of its wretched parents. Would these parents do wrong in refusing to be instrumental in multiplying a race of paupers? Between these two extremes may not each

man and woman ask themselves the question, whether any duty obliges them to procreate children whose advent they will deplore.

Some may be shocked even at the question, regarding the births of children as the result of a special intervention of Providence. We shall not quarrel with these persons, remembering what are the faculties and possible destiny of each child born; but we cannot refuse to see that Providence will not send us children without some action on our part. There is no obligation binding on men and women to begin the begetting of children; having begun, must they go on per force? The argument as to interfering with Providence is quite disregarded now as to epidemics, and it is a little difficult to see the distinction between interference to prevent excessive deaths and excessive births. Indeed, if we do disturb the old balance by preventing a high death-rate, it seems almost incumbent on us to restore equilibrium by diminishing the birth-rate. It seems a strange doctrine that we, with our privilege of free-will, with reason, with religion, for our guides, shall be debarred all choice in this matter, and reduced to a level with brute beasts, each species of which is limited by death and suffering alone; we wholly disagree with those who indulge their senses and expect Providence to protect them from the consequences of their incontinence.

When parents observe that they produce diseased children, idiots for instance, their duty clearly is to produce no more such wretched beings, burdens to the

world and to themselves; we may pity and pardon the infatuation leading unhappy parents to hope that perhaps the next child may bloom a fit object of pride as well as love; but medical science in many cases can sanction no hope, and at best can only promise a chance that misery may be deferred; disease may spare the child, but only to strike the full-grown man. General opinion already condemns marriages likely to result in such offspring as these; but even after marriage, so soon as the eyes of the parents have been opened to the probable fate of their children, their duty is clear.

Dr. Duncan does not enter into these questions, but his tables and Professor Tait's law seem to prove that, until now, the mass of the married population has exercised no restraint whatever on its procreative power.

It would be well if the honest opinion of women on these points could be obtained; unfortunately, this opinion cannot well be obtained. It is the opinion of pure and able mothers that would have weight, but these women do not court publicity. We may suspect that few women bear more than three or four children except from necessity, or from a sense of duty, and of very painful duty; but we get no public sign of such a feeling, except now and then from a wild cry of some poor woman, who mostly does her cause harm, as, when last year, *apropôs* to women's votes for Members of Parliament, a complaint was made that the law did not recognise a rape by a husband on his wife. The idea was simply ludicrous; not indeed that intolerable

hardship, misery, nay murder itself, may not result from the full exercise of a husband's rights, but because of the preposterous inadequacy of any conceivable law to meet such cases. The only remedy lies in the education of public opinion, which, we imagine, is far from willing to allow a woman any exemption from total subjection to her husband in this matter. We do not feel certain that public opinion now wholly condemns even the man who, knowing that should his wife conceive again she will certainly die, nevertheless subjects her to the risk of conception. When she conceives and dies he has committed no legal murder, yet he has killed the woman he was most bound to cherish, and killed her to gratify his senses.

Much mischief is done by the veil thrown over the subjects we are treating. Young women of the better classes know really nothing of the suffering they may expect in marriage, beyond the fact that some of their friends die in childbirth. Novels do not, and ought not, to tell them of the weary months of pregnancy, with infinite petty, almost degrading ills, nor of the weary years of ill health passed by thousands who escape the risk of immediate death. Married women keep their counsel only too well. Young men are almost as ignorant as their young wives, who, at a moment when their imaginations are fired by all that romance and youth can promise, when their daily lives shine with a light darkened by no cloud of evil and mistrust, from this great happiness often pass suddenly to a state of bodily and mental degradation,

of fretfulness, worry, and vexation. What wonder if the young woman fancies she has awakened from a happy dream to face a harsh reality? what wonder if she resents as a lie the romance of chivalry so lately told and heard in perfect faith? what wonder if the husband becomes repulsive, and the word of love a mockery?

The husband, in his turn, bewildered, ignorant, fears that a veil has fallen from his eyes, and that he now sees in the peevish, discontented woman before him the true form of the goddess he had worshipped. Would it not have been better for that young woman and man to have known what child-bearing meant? Possibly they would never have married. Better so, then. Better still, if, knowing the trials before them, they met them bravely—she prepared to suffer, and he prepared, at the expense of self-denial, to spare her suffering. Some may fear that poetry and purity would vanish before knowledge; but love is no lie. Knowledge of vice may render love impossible, knowledge of pain will not render love less pure.

To return to our author: Dr. Duncan shows us that the risk run even by healthy women is enough to give us pause. He is chiefly occupied in comparing the mortality at various ages, and at the birth of successive children, and he proves "that youth has less influence in producing mortality from parturition than age, or rather 'elderliness,'" as he calls it; "that the age of least mortality is near 25 years; and that on either side of this age mortality gradually increases

with the diminution or increase of age." He also shows that the percentage of mortality in first labours is about twice as great as that of the average of all subsequent labours. In four series of observations quoted by him, the mortality in first labours is 1 in 60, 74, 54, and 62 respectively; the mortality in subsequent labours 1 in 150, 123, 115, and 155; from data by Dr. Farr it appears that in England the average mortality in childbirth is 1 in 188. As the number of a woman's labours increases above nine, the risk of death following labour increases with the number. We can refer but very briefly to this part of the volume, in which special care has been taken to analyse the effects of puerperal fever, and of the duration of labour. The author does not make too high a claim in saying, that "from the data thus given an actuary may calculate the answers to the most important questions in this topic. He can determine the fecundity of the female, or her chance of having offspring; her fertility in the number she is likely to have; the time when she will probably become relatively sterile; the risk of death in bearing her first child; the risk of death in the subsequent confinements."

Some explanation is, however, desirable of the very discrepant results obtained when various classes are selected for observation. Thus Sir James Simpson states, in his address to the Public Health Section of the Social Science Congress at Belfast, as the result of Dr. Leon le Fort's semi-official investigations, that out

of 934,781 parturient women delivered at their own homes, and often very wretched homes, 4405 died, or 1 in every 212; while out of 888,512 delivered in maternity hospitals, where every kind of professional care and comfort was bestowed upon them, 30,394 died, or 1 in every 29. Sir J. Simpson also states, from statistics published by Dr. Barnes, that out of 4000 women confined in the four chief maternity hospitals of London, 142 died, or 1 in every 28; while out of 18,383 confined at their own homes, as dispensary or out-patients in connection with the hospitals of St. Thomas and Guy, 53 died, or 1 in 346. Dr. Duncan gives even worse death-rates than 1 in 28 from the register of a St. Petersburg hospital, but does not discuss the causes of the low death-rates occasionally observed, nor of the terrible hospital mortality. The difference is not due to puerperal fever alone.*

It seems to be clearly established that between 15 and 20 the life of a married woman is much more precarious than that of an unmarried woman. To use Dr. Stark's words, quoted by Dr. Duncan—

"Supposing married and unmarried were in equal numbers between 15 and 20 years of age, ten married would die for every seven unmarried; between 20 and 25, nine married women would die for every eight

* On this subject, into which Le Fort and Simpson have introduced much error, see Chapter I of Part VII. of this work, and my book entitled *The Mortality of Childbed and Maternity Hospitals.*—J. M. D.

unmarried women; above 30 the chances appear to be in favour of married life. The high mortality attending the birth of first children explains the risk run by the younger women. Even after this the risk expressed by say 1 in 150 is no trifle, especially if it has to be frequently encountered. What would men say if 1 in 150 of the gentlemen travelling to business in the City were killed every two years by railway accidents, with wounded or maimed in proportion? We think they would shirk season-tickets, and feel very uncomfortable if forced by their wives to travel daily; they might possibly resent the argument that they were only doing their duty in earning daily bread for their wives and children in the country."

We hope Dr. Duncan may be induced to collect statistics as to the mortality when labours are separated by various intervals. It is just possible, that if a husband knew that the risk to a woman he loved could be seriously diminished, he might practise a little self-denial.

Dr. Duncan gives valuable information as to the probable duration of pregnancy, for which we must refer readers to his work; it appears that Montgomery's term, which is much relied on, is too long by nearly a week; the older and more popular modes of calculation being the more correct. If so, Dr. Montgomery must have much misery of a minor kind to answer for; indeed, serious risk must often result from miscalculation. Dr. Duncan adheres to the doctrine that conception in the vast majority of cases can only take place

during a period of about a week in each month, but he does not mention the researches which have established this fact. Much curious information is given as to the size and weight of new-born children, the frequency of twins, and the effect of age and previous child-bearing on fertility in twins. On the question of sterility, our author says :—"Of women between the ages of 15 and 41 inclusive there were married 4372; among wives of the same ages 3710 had first children, leaving 662 marriages sterile, or 1 in 6·6; in other words, 15 per cent of all marriages between 15 and 44 years of age, as they occur in our population, are sterile." "Dr. West states that he found the general average of sterile marriages among the patients at St. Bartholomew's Hospital to be 1 sterile marriage in every 8·5," but it appears that almost every woman married at 20-25 proves fertile. Our author's data did not apparently allow twin children to be distinguished from others, or we should have had, in Table LXX., an almost perfect measure of the sterility at each age. In this table we find that 7·3 per cent, 0, and 28 per cent of all women, married respectively at 15-19, 20-25, 25-29, are absolutely sterile; of women married at 30-34, 35-39, 40-44, 45-49, the sterile are 37, 53, 91, and 96 per cent respectively.

A mere list of the headings of chapters will show the very large number of problems which are connected with fertility, each problem having its own special importance. Thus we find chapters on "The Fertility of the whole Marriages in a Population;" "The Fer-

tility of the whole Fertile Marriages in a Population at a given Time;" "On the Annual Fertility of the Married Women of Child-bearing Age in a Population;" "The Size of Families in a given Population at a given Time;" "The Fertility of the whole Marriages in a Population that are Fertile at a given Time;" "The Fertility of Fertile Marriages lasting during the whole Child-bearing Period of Life;" "The Fertility of Persistently Fertile Marriages lasting during the whole Child-bearing Period of Life;" "The Fertility of Persistently Fertile Wives at different Years of Married Life;" "The Fertility of Fertile Wives at different Periods of Married Life;" "The Degrees of Fertility of Wives-Mothers of Families of different Numbers;" "The Fertility of Wives-Mothers married at different Ages;" "The Fertility of Persistently Fertile Wives of different Ages;" "The Fertility of the Older Women;" "The Contributions to the Adult Population by Marriages at different Ages;" and, finally, "The Comparison of the Fertility and Fecundity of different Peoples."

As might be expected, Dr. Duncan, having looked at the question from all these points of view, finds his predecessors at fault in all directions. The book is not at all prolix or dogmatic, for Dr. Duncan belongs to the very valuable class of authors who collect and digest facts, but refrain from the reflections which those facts suggest. We have been tempted to indulge in some speculations, and feel certain that all readers who can think will find new matter for consideration in the book. They will find nothing garbled, no con-

cealment, no prejudice; but a large collection of interesting materials intelligently arranged. Professor Tait has ventured farther than Dr. Duncan on the sea of speculation, and has suggested extremely simple laws, grouping vast numbers of apparently disconnected facts into two short sentences. We think the laws are proved for a mass of women, but further statistics are wanted before we can judge how far they can be applied to individual cases. Their author knows this well, and expresses some indignation at being deprived of the data by which to check and extend his curious formulæ, and we fully agree with the opinion expressed in the following passage:—"As in all questions of average, the value of our deductions in this matter is mainly dependent on the extent and accuracy of our data; and it is sad to think that the enormous Blue-books which load our shelves contain so much painfully elaborated information which is of no use, and so little of those precious statistics which would at once be easy of acquirement and invaluable to physiologists."

PART VII.

THE MORTALITY OF CHILDBED: ITS AMOUNT, AS AFFECTED BY THE NUMBER OF THE LABOUR, THE AGE OF THE MOTHER, ETC., ETC.

CHAPTER I.

THE MORTALITY OF CHILDBED.

THERE are some terms frequently used in this book which require some definition preliminarily. Childbirth implies parturition. The accidents of childbirth, or deaths from childbirth, are accidents or deaths arising from parturition. From these accidents and deaths, those of puerperal fever or metria are arbitrarily excluded. Childbed is a more general expression, implying the special conditions in a period of time, generally understood as of four weeks, extending from parturition, which it also includes, onwards for the puerperal or childbed month of lying-in. Childbed deaths include those from childbirth and metria. Mortality or deaths *of* childbed are those belonging to that state—*i.e.* childbirth and metria deaths. Mortality or deaths *in*, not *of*, childbed, include all deaths in the four weeks of childbed. Deaths *in*, not *of*, childbed, are all deaths, deaths from whatever cause, occurring within the four childbed weeks, including the period of labour.

To illustrate the use of these terms, I may take

some examples. A death from unavoidable hæmorrhage is a childbirth death, and so is a death from rupture of the uterus, though it may be delayed for a week after parturition. A death from septicæmia or pyæmia, after delivery, is not a childbirth death, but a death from metria. All of these cases are childbed deaths. They are deaths *of* childbed. They are also deaths *in* childbed. But if a woman is poisoned by laudanum within four weeks after delivery, that is death *in*, not *of*, childbed.

These various definitions are not strictly natural or scientific, but they are here made for reasons which the reader will easily recognise as he goes on. They can scarcely be called new terms or new definitions of terms, for their meaning is such as is either already generally recognised, or is easily apprehended.

The mortality *of* childbed is a quantity not only not ascertained, but, so far as I can see, not at present ascertainable in a perfectly or even a nearly satisfactory manner. Yet it appears to me very desirable to make a definite, single-eyed attempt to approach as nearly as possible to a correct statement of this quantity. The result, if even moderately well established, cannot fail to be of immense value in contributing to the settlement of disputes as to the injurious or beneficent character of practices or of hospitals. I say only *contribute*, for much more is required for the purpose than a standard to measure by. But without some approach to a fixed standard, no progress can be made in discussions such as those alluded to.

In the recent animated debate on the value of Maternity Hospitals in the Dublin Obstetrical Society, more than one speaker set himself to answer the question proposed in this paper. I shall use much valuable information derived from that source;* but I think I have added considerably to it; and I have an advantage over the speakers there in this respect, that I am considering at present only this single point, "What is the mortality of childbed?" separate from the other questions raised in the famous debate.

In the present question there are two great statistical difficulties. The first is, to decide upon the facts or circumstances to be compared. The second is, to get the facts or circumstances, after settling the first difficulty, as to what facts are to be got or are worth getting. Unless a thoroughly good understanding is arrived at on these points, the argument cannot advance a step; the quantity desired, the mortality of childbed, must remain unknown, and not even approximately fixed. We shall now inquire how they can be best settled.

First, What facts or circumstances settling or contributing to settle the quantity, the mortality *of* childbed, can be agreed upon as being worth obtaining and studying? Now, the Registrar-General gives us deaths of childbed, and he places them in two categories: first, childbirth deaths, or deaths of childbed not arising from what is called metria; second, deaths from puerperal fever or metria. In the first category are placed

* *Dublin Quarterly Med. Journal*, August 1869.

deaths from rupture of the uterus, from puerperal eclampsia, from phlegmasia dolens, from puerperal mania, from placenta prævia, etc. In the second are placed deaths from metria or puerperal fever. This mode of arranging the deaths of childbed is very generally adopted, and at present I do not wish to make any theoretical objection to it; but a single statement is sufficient to show that the use of these two categories does not ensure the production of facts which the profession can agree upon and unite to accept: for the profession are not agreed upon the questions, What are childbirth deaths? and, What are metria deaths? Not only may obstetricians, well informed and strictly honest, differ as to which is the right category for a particular case: but there is also, and this is the great point, room for their differing as to a particular case being a childbed death (*i.e.* from childbirth or metria) or not. One class of practitioners may deliberately and honestly say of a case, This is not a childbed death (*i.e.* from childbirth or metria), while another class of practitioners may equally deliberately and honestly say of the same case, This is a childbed death (*i.e.* from childbirth or metria); and there is no means of always settling the question between them either scientifically or by authority. What, then, is to be done? The fact is, that all attempts at ascertaining scientifically or exactly the desiderated quantity, the mortality *of* childbed, must meantime be given up. There is no method of even getting facts upon the nature of which the profession are agreed. I could prove this by tedious

references to writings of obstetricians of high authority, and by other arguments, but I believe it is quite unnecessary.

There are many valuable results obtainable, which, though not exactly what is desired, are very nearly so, and extremely useful, because the best obtainable, with a view to guidance in great practical questions which demand an immediate answer of some kind—the best that can be got. Now, in the present instance, we can get the deaths *in* childbed indisputably, though not those *of* childbed; and there will be, in my opinion, no very great difference between the two quantities. The quantity wanted is the mortality *of* childbed (*i.e.* of childbirth and metria): it is unattainable. The quantity attainable is the mortality *in* childbed (*i.e.* of childbirth and metria, and every other influence producing a fatal result in the interval between the commencement of parturition and the end of the lying-in or childbed—that is, a period of four or six weeks, or any other time that may be agreed upon). It must be evident to all that this result can be got—namely, the number of women dying in the interval between the beginning of labour and, say, four weeks thereafter. As matters stand there is a difficulty, for we have no security as to the length of time included in the term lying-in or childbed. A month is the term generally adopted, but it is not settled whether this means twenty-eight, thirty, or thirty-one days. While we recognise this difficulty, yet we cannot doubt that this quantity is the best that can be fixed upon for observation, just

because there can be no cavil about what it is—namely, the mortality *in* childbed from whatever cause, not the mortality *of* childbed (*i.e.* childbirth and metria).*

It will be observed that I have said that I do not believe there is any great difference between the mortality *of*, and the mortality *in*, childbed; and the statement is indeed capable of demonstration. For the total four weeks' mortality of wives, or of women generally, at any child-bearing age, is a very small amount compared with the four weeks' mortality of wives in childbed, or of women in childbed generally; and this very small amount is of course more than the amount of the deaths *in*, and yet not *of*, childbed, or, in other words, of the deaths not connected with childbed except by occurring during it. This embodies, I believe, a nearly correct scientific statement of the matter. But the subject requires to be otherwise looked at: namely, as a practical matter of calculation; and here we find that there is an immense difference made by authors or statisticians between the deaths *in*, and the deaths *of*, childbed. It is to Dr. M'Clintock that we are indebted for the best elucidation of this practical side of the question, and I give his own account of this matter from his speech before the Dublin Obstetrical Society:†

"A reliable estimate of the mortality among lying-in women confined at their own homes is a very great

* On this subject see the *Lancet* for 1859, vol. ii. p. 213.

† *Dublin Quarterly Journal of Medical Science*, p. 266. See also p. 269.

desideratum. I must honestly declare my conviction that up to the present time, notwithstanding our multiplied and elaborate Registration Reports, there is no reliable return of such deaths, and therefore it is no better than 'arithmetical idleness,' to be constructing out of these reports any standard of comparison between hospital and home midwifery practice: and this opinion is shared in by every medical man of experience who has bestowed any consideration on this matter. Nor have we to go far to discover the reason of this. The death of a woman in childbed, as every one here well knows, always attracts a great deal of attention, and is a fertile subject for popular comment and animadversion; but, if the cause of death is known to be puerperal fever—or anything pertaining thereto—then, indeed, quite a panic is created in the neighbourhood, and both doctor and nurse come in for more than their full share of blame. Hence, for their own reputation's sake, as well with the charitable motive of not alarming all the pregnant women in the community, the death is imputed to any other possible cause rather than to the dreaded puerperal.

"It is not necessary, however, that any motive be assigned for this. The defect lies in our system of registration—not in those who supply the returns. Practitioners make a very proper distinction between dying *in* childbirth, and dying *of* childbirth. When a woman happens to die in childbed of some intercurrent disease—as phthisis, pneumonia, dysentery, apoplexy, albuminuria, bronchitis, morbus cordis, etc.—this alone is returned, and rightly so, to the registrar as the cause

of death. *Consequently all these deaths have no place in the registration reports of deaths in childbed. But a lying-in hospital is debited with every death occurring among its patients*—whether the death arise directly from parturition, from puerperal disease, or from any accidental intercurrent disease.

"In this way, I think, we can account for much of the discrepancy between the death-rate of lying-in hospitals, and that deduced from the returns of the Registrar-General. To illustrate my meaning, and to show how statistics must be influenced by this source of inaccuracy, I have compiled the following Table :—

TABLE C.—SHOWING DEATHS IN CHILDBED, CLASSIFIED UNDER THREE HEADS. (*From Home Practice exclusively.*)

AUTHORITY.	Number of Cases.	Deaths from Accidents of Labour.	Deaths from Puerperal Diseases.	Deaths from non-Puerperal Diseases.	Totals.
Joseph Clarke . .	3,847	7	6	9	22
J. G. Crosse . .	1,377	1	8	5	14
John Beatty . .	5,616	2	9	2	13
Thomas E. Beatty .	2,064	4	8	5	17
Churchill . . .	2,548	6	8	2	16
Browne * . . .	...	22	6	7	35
M'Clintock . . .	652	0	4	2	6
Sir P. Dun's Maternity	336	...	...	2	2
Brunker	334	3	3	0	6
Totals . .	16,774	45	52	34	131

* Dr. John Browne of Dundalk. The above results are from twenty years' practice, private and dispensary.

"In compiling this Table,* the results of private practice chiefly are taken, as being *thoroughly reliable*, and the patients having been all attended at home. Hospital statistics are excluded, because the great excess of metria cases in hospitals would necessarily disqualify them from showing the proportion that deaths from non-puerperal disease bear to all other deaths in childbed among patients out of hospital.

"From this Table it is apparent that the deaths occurring in childbed from non-puerperal disease form considerably over one-fourth of all the fatalities. You can at once perceive the great effect which the omission of these deaths must have in lowering the death-rate of childbirth out of hospital. For example, when the deaths are returned as 1 in 200, they should, on the

* Commenting on my use of the Table just quoted, a reviewer says (*Glasgow Medical Journal*, May 1871, p. 403), "that Dr. Duncan is so far illogical, that having announced the impossibility of ascertaining the mortality of childbed, he yet employs a method of arriving at the deaths in childbed, which involves an estimate of the very quantity which he had just declared unattainable." In answer to this I have, I think, a perfectly satisfactory and rebutting statement—namely, that the object I sought was to ascertain the deaths *in* childbed, not the deaths *of* childbed; that it was the ascertainment of the amount of the deaths *of* childbed that I declared to be very difficult; that M'Clintock's method, the method objected to, or some similar one, was necessary, not to arrive at the deaths *of* childbed, but in order to make available in my inquiry the Registrar-General's reports. Besides, as I say in my paper, the whole inquiry is unsatisfactorily conducted; and a slight deviation from strict logical proceeding may be excused, if without it the inquiry must stop. It did, and does, appear to me that some solution of this question was urgently wanted, and I have honestly done my best, confessedly somewhat illogically, yet my best.

principle just laid down, be 1 in 150. Or, one other example: 11,722 English women died in childbirth in the years 1838, 1839, 1840, and 1841—the rate of mortality being one death to 171 births, or 168 women (after making the allowance for twins).* But if what I have stated be correct—viz. that only three-fourths of the gross number of deaths are registered—the true rate of mortality is found to be *one* in 126." †

Having thus settled what can be agreed upon as obtainable with a view to this question, let us *secondly* inquire what sources we can look to for the desiderated data. Derived from any source, they will be imperfect in various ways. We must choose the best, the most trustworthy.

There can be no doubt that hospitals, with well-kept records, offer us data far more trustworthy than any other; and I believe this circumstance, while it ought to excite our admiration of them, has, on the contrary, been the prolific cause of much injurious slander. Whose character can endure or survive the divulging of the whole truth about it? One of the great difficulties in adopting hospital statistics arises from the early dismissal of the patients generally, the

* *Fifth Annual Report* of the Registrar-General for England and Wales.

† In justice to the returns of the Registrar-General, it must here be stated that they do not profess to include *all* the women dying in childbed, but only those dying from the effects or accidents of labour and from puerperal diseases, which are included under the comprehensive term *metria*.

dismissal of some of the sick with a view to admission to other hospitals, and uncertainty thus introduced as to the number dying within a period longer than that during which all are indiscriminately retained in the institution. Like difficulties damage all other sources of data, and this similarity abates much of the consequent evil. The security of hospital statistics arises from their being compiled at the time of the facts emerging, from their being recorded by uninterested parties or without a view to any discussion, and from their being of undoubted truthfulness.

After hospitals we turn, secondly, to the reports of the various Registrars-General. These have a certain and a very high value, arising chiefly from the largeness of the figures. This largeness, while it covers many errors, does not cover others. We have already said that we have no security that the deaths included under the designations "childbirth" and "metria" include all the deaths of either category; nor have we any security that both taken together include all deaths of childbed. They are intended to include all deaths *of* childbed, but not to give us any clue to the number of deaths *in* childbed, the quantity we hope to find out, or approximate to. Probably few omissions from these categories take place from a desire of the recording practitioner to conceal or delude; but no one knows how many may be omitted because the recording practitioner does not deem it right to record his case as one justly belonging to either category, or *vice versa;* and the practitioner cannot be found

fault with, for he has no unexceptionable rule of guidance.

To show what sources of error in the registrars' reports are hidden from observation, I shall quote from Dr. Barnes a passage bringing one to light from a single locality:—"It is stated," says he, "in the Registrar-General's Report for 1856, that the mortality in childbirth in England and Wales in 1847 was 1 in 167, and that it had fallen to 1 in 227 in 1856. Now, having applied to Dr. Elkington for the puerperal statistics of Birmingham, I learn that the registrar of that town says, that 'no one ever specifies the deaths in childbed or from puerperal fever!!!'"*

Trusting too implicitly to tables of mortality and registrars' reports, a student might be led to adopt such an absurd notion as that the mortality of childbed had been reduced from 1 in 167 in 1847 to 1 in 227 in 1856 in England and Wales. Like unfounded and improbable views as to the progress of midwifery have been so often repeated as to appear now to be generally believed. The paradox referred to, like many others, is based on statistics, and I shall not quote them, nor stop to show their worthlessness, for it must be apparent to all on a very little reflection. It is, on such deceitful grounds, asserted that between 1660 and 1820, the mortality of childbed in London fell from 1 in 44 to 1 in 107!!!† In 1870, with our

* See *Dublin Quarterly Journal of Medical Science*, vol. xxviii. 1859, p. 103.

* Simpson's *Obstetric Works*, vol. ii. p. 545. Merriman, who

great registering machinery all at work, we cannot find out what is the mortality of childbed in London.* If we could, all the labour of obstetricians on the subject now under discussion might be spared. For my part, I think obstetrical common sense will be very contented if the true childbed mortality of modern London is at all less than in the London of 1660. It is very doubtful whether it is even now as low as 1 in 107.

I have already given Dr. M'Clintock's demonstration of the difference actually existing between registered deaths *of* childbed and those that occur *in* childbed.

The third source of data is a private search of the public registers, and the discovery, by this means, of all the women dying within a certain time, say four weeks, after childbirth. No doubt, in the statistics thus framed there may be errors, but they will certainly all be errors of omission of deaths, from want of success in tracing them. The errors will all tend to make the mortality too small.

publishes the statistics referred to, points out their untrustworthiness. The births are got, says Merriman, by counting the christenings and the dead-born. The diminishing percentage of mortality is naturally accounted for by an increase of the number of children brought to the parish churches and registered, instead of being unchristened, or christened by dissenting ministers and not registered; while the dead mothers are supposed to be all registered, irrespective of the registering of the children. But the whole data are really so insecure as to be quite worthless.

* See Barnes, *Dublin Quarterly Journal of Medical Science*, vol. xxviii. 1859, p. 100.

The fourth source is the reports of out-of-door or home practice of dispensaries or hospitals. To them I attach no value whatever. I am well acquainted with the management of these institutions, and I know that the best of them present no data that I can dare to use in an inquiry like the present. The mortality which they record is often incredibly small. There is no security whatever that the deaths are recorded, whatever may be their cause. There is no security that theoretical views as to the nature of the cause of death may not completely destroy the value of the records. I could, from extensive experience of my own in dispensary work, adduce data which would indicate a figure of mortality that would be very delighting, if I could only believe it. Le Fort, in his well-known work, has fallen into this, which I must call, gross error, taking the data of the home practice of charitable institutions as reliable and fit for comparison with the data derived from hospital records. Doing so, he has, of course, arrived at extraordinary results.

Le Fort* says the mortality in hospitals or maternities is 1 in 29. What does this mean? It is that, taking the data of all maternity hospitals together, he finds that 1 of every 29 delivered has died. I do not doubt it. But what is the value of this result, with a view to the question now before us, and I may add also (among others) before Le Fort? If the maternity department of the Hôpital de la Charité is so badly

* *Des Maternités*, Paris, 1866, p. 31.

managed as to have a mortality of 1 in 7, what does that show with a view to the question of the mortality of childbed generally, or in hospitals as compared with that in homes? It is plain that it shows absolutely nothing with this view. It should, for mine and for Le Fort's purposes, be simply thrown aside out of view. I daresay an hospital could be so constructed and managed as to kill all its inmates. What of that, in the questions before Le Fort and myself? Will the addition of such data as are furnished by La Charité to such data as are got from all other hospitals, bad and good, such as that of Troyes (1 in 230), lead to any desirable result? In my opinion, to no result but confusion and darkness. Such statements as that of Le Fort, regarding the mortality in hospitals, only show how disgracefully mismanaged many hospitals are, how much need there is of the exertions of the philanthropist. Superabundant evidence can be adduced to show that it is easy to have far better results in maternity hospitals than 1 in 29; and it is well known that the best maternities are susceptible of vast improvements. Le Fort's labours show how bad they may be, and little more. They do not bring out what he and I want.

Having made out the mortality of all hospitals to be, in fact, 1 in 29, Le Fort proceeds to investigate the mortality in home or dispensary practice, and he pursues with this the same method. But there are great differences in the two sets of data. In the case of hospitals, it is probable that the data are nearly

what they pretend to be, nearly true. But in the case of the data of home practice, there is, in my opinion, not the least probability that they are, what they pretend to be, nearly true. The method of collecting the data of home practice renders them valueless. They are not got as the data of hospital practice are got.* But further, the data of M. Le Fort are, without doubt, themselves mutually destructive. We know that the mortality of different hospitals is extremely different. A bad one may have a mortality of 1 in 7; a good one a mortality of 1 in 100. But we have no reason to believe that such an extreme difference occurs in mortalities of home practice. Le Fort cites home practice (Stettin) having no mortality at all! Is this a valuable and instructive result? He cites home practice having a mortality of 1 in 595. Is this a valuable and instructive result? Is any one so foolish as to believe it? Will the jumbling of such figures together produce a result (1 in 212†) of any value?

* On this subject, see some remarks by Dr. Churchill, *Dublin Quarterly Journal of Medical Science*, Aug. 1869, p. 249. See also remarks by Dr. Kidd, *ibid.* p. 242; also by Dr. Denham, p. 273.

† *Des Maternités*, p. 33. I am astonished to find Depaul quoting Berard, approvingly, as the reporter of 1258 deliveries without a single death following, and this among the poorest of Paris. This fault is probably the result of mere thoughtlessness. If the statement of Berard is true, it surpasses anything known in any kind of practice; and the enemies of hospitals who adduce it are logically bound to commend, as favourable to recovery after delivery, the attendance of a student, and the immersion in all the loathsome peculiarities of the most wretched abodes of Paris. See *Fièvre Puerpérale*; Paris, 1858, p. 371.

Errors heaped on errors produce only a more egregious error.

In the case of hospitals, we may get near the truth by studying one that is large enough and long enough established, and that has laws and conditions that are well known. In the case of the home practice of maternities, I know of nothing reliable as to mortality.

Lastly, there is another source of data—namely, private practice. But I regard it as a very questionable source. The reception of evidence derived from it is encumbered with difficulties. And there are some conditions of such data which I regard as to be always required before they are received as quite satisfactory. The first is, that the items or facts be written down at the time of their occurrence. The memory is a frail and treacherous source of statistics. The second is, that the data be not asked for by a second party known to have any object in view in their use; for such asking will inevitably lead, through the amiable qualities of the petitioned parties, to the production of data favourable to the petitioner's views, and the non-production of unfavourable data. In depreciation of the value of data derived from private practice, it is to be remembered that medical men are mortal, and have an indisputable tendency, and an inalienable right, to say nothing of what looks like want of success. Suspicion naturally attaches to data remarkable for apparent success. This does not arise from any doubt as to veracity, but from the probability that practice, having apparently remarkable success, is published on that

account. If the data of private practice could be got, they would be the best. But, as yet, no satisfactory data of this kind are procurable on a large scale.

We now proceed to the results afforded by the various sources above enumerated and considered.

The important result, let me repeat, which is sought is the mortality, not *of* childbed (*i.e.* childbirth and metria), but *in* childbed (*i.e.* from all causes resulting in death within the period called that of childbed).

I.—Hospitals.

Some hospitals show an appalling, and I may add, a disgraceful mortality. I could adduce a mortality of 1 in 3 in a certain period of the history of a great hospital.* From this climax, I could rise through successive stages of badness to a mortality that, so far as I can see, is nearly the present ordinary mortality in childbed. Let us take a well-known and well-managed hospital, and see what mortality it presents. It would be misleading to take all hospitals, for the bad would destroy the evidence of the comparatively good; and most are bad, many very bad. In order to get at the mortality in childbed, in such a rough practical way as we are now pursuing, it appears to me only to be necessary to take an hospital large enough and long enough established to give its statistical

* *De la Fièvre Puerpérale*, etc. Communications à l'Académie Impériale de Médecine. Paris, 1858, p. 27.

figures security against accidental interferences. I shall take the great Dublin Hospital.

During the seven years of Collins, 16,414 women were delivered, and 164 died; or in the proportion of 1 in 100.

During the three years of M'Clintock and Hardy, 6634 women were delivered, and 65 died; or 1 in 102.

During the seven years of Sinclair and Johnston, 13,748 were delivered, and 163 died; or 1 in 84.

II.—Reports of Registrars-General.

These are a great quarry for statistical data. They give the deaths *of* childbed. But it is only by some ingenuity that the deaths *in* childbed can be even approximately reached through them.

According to Faye and Schönberg, the mortality of childbed in Norway is 1 in 135.*

The mortality of childbed in Paris was 1 in 169 in 1861; 1 in 160 in 1862.†

The mortality of childbed in St. Petersburg is given from data supplied by Hugenberger as 1 in 149.‡

The mortality of childbed in Dublin is given by Dr. Evory Kennedy as 1 in 114.§

The mortality of childbed in England and Wales is given by Farr as 1 in 189.‖

* See *Dublin Quarterly Journal of Medical Science*, August 1869, p. 270. † Le Fort, *Des Maternités*, p. 33. ‡ *Ibid.*

§ *Dublin Quarterly Journal of Medical Science*, vol. xlvii., 1869, p. 289.

‖ *Seventeenth Annual Report of the Registrar-General*, etc., *England*, 1856, p. 73.

The mortality in Edinburgh for the six years, 1860 to 1865, is 1 in 162.

The mortality of childbed in Prussia is said by Hoffman, calculating from 7,654,021 deliveries, to be 1 in 108.*

I need not give more of these statements. They show what deaths are returned to the public registers as childbed deaths. They give us the registrars' statements of the deaths of childbed. We have already given reasons for distrusting these statements, and we have also shown why the quantity sought should be the deaths *in* childbed, not *of* childbed.

Dr. M'Clintock has taken pains to find out how far the deaths in childbed exceed the deaths of childbed. He finds this quantity to be equal to at least one-third of the deaths of childbed; in other words, deaths in childbed, and not given as being of childbed, are at least one-fourth of the deaths in childbed. In his own words, "One-fourth is not at all too much to allow for the deaths omitted in the registration returns of deaths in childbed." In another place he says, "The deaths occurring in childbed from non-puerperal disease form considerably over one-fourth of all the fatalities."†

Correcting by this plan the data of childbed mortality just given, we have—

* *Fifth Report of the Registrar-General for England*, 1843, p. 396.

† *Dublin Quarterly Journal of Medical Science*, August 1869, p. 267. I adopt M'Clintock's method, without strictly inquiring into the justice of his plan of handling the figures.

The mortality in childbed in Norway, 1 in 101.
" " Paris in 1862, 1 in 120.
" " St. Petersburg, 1 in 112.
" " Dublin, 1 in 86.
" " England and Wales, 1 in 142.
" " Edinburgh, 1 in 122.
" " Prussia, 1 in 81.

I cannot pretend to say what value I attach to these calculations. There is certainly a great want of precision about them. But that, for our present purpose, the registrars' reports require much correction, I am quite certain, and I shall here give an illustration of their faultiness. The Scottish Registrar's Report for 1855 gives a total of 118 deaths of childbed in Edinburgh and Glasgow. A private search made for me by experienced census clerks, discovered, among the married women alone, 153 deaths within six weeks after delivery, in 1855.

III.—Private Search of the Public Records.

The only private searches of which I know are those by Tarnier and myself.

Tarnier examined the registers of the poor twelfth arrondissement of Paris, and found the mortality in childbed to be 1 in 322.* I have already laid down enough of well-considered figures to render this statement of the mortality in childbed highly improbable. No such smallness of mortality is pretended in any place well known; and Dubois, after pointing out the special difficulties of the search made by Tarnier, does

* *Fièvre Puerpérale*, etc., par le Dr. S. Tarnier; Paris, 1858, p. 75.

not hesitate to throw discredit on it, and adds that, among the comfortable and well-to-do classes, private practice yields nothing like such a happily small mortality as Tarnier represents in the poverty of Paris.* The same statistic of M. Tarnier is unfavourably commented on by Danyau.† He had the death-rate of the same district investigated, and found in it a mortality of 1 in 60 from puerperal fever alone!

I had a careful search made in the Scottish registers for deaths in Edinburgh and Glasgow in the six weeks following the deliveries of the married women there, and I found 153 deaths in 16,393 deliveries, or 1 in 107. It may be supposed that the addition of a fortnight to the usual puerperal four weeks may account for much of this mortality. But this is not the case. The omission of the fortnight, or the confining of the search for deaths to a period of four weeks after delivery, would probably have made little difference in the result, for the fortnightly percentage of deaths among women of child-bearing age, and apart from the immediate influence of child-bearing, must be very small.

IV.—Private Practice.

In the years of my practice of which I have preserved records, I find 8 deaths in 736 cases, or 1 in 92. One of the fatal cases was not attended by me during labour, and may be omitted from the statistic. The mortality will then be 1 in 105. This is the mortality from all causes.

* *Fièvre Puerpérale*, etc. ; Paris, 1858, p. 260. † *Ibid.* p. 402.

In a report of two years of his practice, Sir James Simpson* says he lost 4 cases in at most 180 deliveries; a mortality of 1 in 45. It may be supposed that this is the total mortality, but it is not expressly stated whether it is so or not. And the same is the case in some of the other examples from private practice which I shall give.

Dr. J. Clarke,† in 3847 deliveries had 22 deaths, or 1 in 174.

Dr. Crosse,† in 1377 cases had 14 deaths, or 1 in 98.

Dr. Labatt,† in 4368 cases had 26 deaths, or 1 in 168.

A London accoucheur,† in 2982 cases had 30 deaths, or 1 in 99.

Dr. M'Clintock,† in 652 cases had 6 deaths, or 1 in 108.

Dr. Brunker,† in 334 cases had 6 deaths, or 1 in 56.

Dr. Churchill,† in 2548 patients had 16 deaths, or 1 in 159.

Among 10,190 cases, a physician‡ had 107 deaths, or 1 in 95.

Among 2064 cases, Dr. T. E. Beatty had 17 deaths, or 1 in 121.§

Among 2000 cases Mr. Jones had 16 deaths, or 1 in 125.‖

* *Obstetric Works*, vol. ii. p. 642.

† Speech by Dr. M'Clintock. *Dublin Quarterly Journal* for August 1869, p. 268.

‡ Merriman, *Difficult Parturition*, p. 320; where will also be found the reference for the practice of the London accoucheur.

‖ *British Medical Journal*, July 8, 1871, p. 32.

§ *Dublin Quarterly Journal of Medical Science*, August 1869, p. 299. Hegar (*Archiv f. Gynæk.*, Bd. I. S. 192) states that among 34,553

General Result.

Having led all the evidence I propose to adduce, I now attempt to draw a conclusion from it. I do so with much diffidence, yet with the full conviction that it is the best approximation to the truth that I can make.

NOT FEWER THAN 1 IN EVERY 120 WOMEN DELIVERED AT OR NEAR THE FULL TIME DIE WITHIN THE FOUR WEEKS OF CHILDBED.

At this result there need be no astonishment. How many women are delivered in circumstances unfavourable for recovery? Some mothers are immature. Many are diseased. Some begin child-bearing when old. All have to pass through the great risks involved in a first confinement. Some have excessive families. Some are confined under the murderously depressing influence of shame.* The accidents of childbirth are

cases attended by physicians and midwives in a district of Baden, were 253 deaths, or 1 in 137. This collection is valuable; but it is of a kind which I can scarcely class with any other. I therefore put it in a footnote.

* "I have already alluded (says Dr. M'Clintock, *Dublin Quarterly Journal of Medical Science*, p. 272) to the number of seduced or deserted women who seek the refuge and concealment of a large hospital. At the time of their lying-in, every one knows how peculiarly obnoxious these patients are to the fatal influences of labour and childbed. During my seven years' mastership of the Rotunda (1854 to 1861), it came to my knowledge, without making any special inquiries regarding it, that 127 patients were unmarried women, and had come from every part of the country, and of these 31, or very nearly one-fourth, died in childbed, and chiefly from some form of metria."

numerous—malpresentations, ruptures, eclampsia, floodings; obstetric operations are frequent. Puerperal fever is common.*

Before concluding I must observe that the mortality of any hospital or practice is not, of itself, a measure of success or of failure. It is quite possible that an hospital or a practice with a high mortality may be especially successful. For it may number among its items an extraordinary number of cases of danger and difficulty, and the figures may be so small that a very little addition to the deaths will have a very remarkable influence in increasing the average mortality in it.

* Le Fort (*Des Maternités*, p. 63) says that the statistics of private practice of several English accoucheurs have been published, and that their total mortality does not exceed 2 or 3 in 1000. This is vague enough, but I feel confident it is also quite incorrect. Le Fort gives no authority for his statement. I could adduce many more statistics of private practice, but as they do not change the view I have given, I do not encumber my pages with them.

Simpson has said that the mortality of childbed is now 1 in 150 or 200 (*Obstetric Works*, vol. ii. p. 482).

It is quite common to hear practitioners orally report one or several thousands of cases without a death. Such reports are, of course, of no value. There is no Utopia for lying-in women—no place where a woman can avoid dangers such as that of a first confinement. Hervieux, in the following statement, well exemplifies this kind of foolish talk, which he unfortunately embodies in his valuable book (*Maladies Puerpérales*, p. 26).

"There are (says he) localities, and, thank God! very numerous, where lying-in, whatever may be the traumatism which accompanies it, never or almost never brings in its train serious pathological consequences. I could cite countries where the lethality of women in childbed has not risen, during a long series of years, above 1 in 1000. In these localities, what becomes of the pathogenic influence of the puerperal state?"

CHAPTER II.

THE RELATION OF THE NUMBER OF THE LABOUR TO THE MORTALITY FROM PUERPERAL FEVER.

THERE are two important questions regarding the mortality of lying-in women which certainly have not received the amount of attention which they deserve. They are interesting and important, not only in themselves, but also, in a high degree, on account of their bearing upon topics which are constantly discussed without taking into account the great light and influence which the answers to them might bring upon such topics.

The questions I allude to are :—Does the number of a woman's pregnancy indicate in any degree the mortality to be expected from lying-in? Does the age of the childbearing woman indicate in any degree the mortality accompanying this function?

Analogous questions in regard to some surgical operations and to some diseases have been discussed, and not without good results. This circumstance renders it the more extraordinary that the questions I have proposed regarding parturition have stimulated so little inquiry. The topics of childbearing and of its mortality, and particularly the indefinite disease

puerperal fever, are among the most interesting and carefully studied in the whole range of medicine, and the neglect of the two questions named I can attribute only to the want of materials for their settlement. Yet I do not hesitate to say, that had the profession set these questions before them in their simplicity and importance, materials would ere now have been found or accumulated, and their most desiderated solution satisfactorily effected ere this time.

I regret that at present I know of no data sufficient satisfactorily to decide the questions raised. Yet I shall lay before the profession such as I have collected. They are deficient in point of number and of precision. Had the numbers been much larger, the results would have had value in spite of the want of precision in the data as arranged for comparison. The element of want of precision consists mainly in the comparison of different pregnancies not being confined to women of the same age, or *vice versâ*. This condition is of course necessary to ensure against a probable source of error, the amount of which is unknown, consisting in the disturbing influence of age, or of the number of the labour, upon mortality.

It is well known that a large amount of puerperal mortality is produced by that indefinite class of diseases unphilosophically and injuriously combined under the name of puerperal fever. So important is this class of diseases, that it appears to me worth while to discuss separately the influence of age on the mortality from them. The same should be done for all causes of death

in or after labour as soon as data are collected. In the case of puerperal fever some data are at hand. It is not my object at present to enter on the vexed questions in reference to this erroneously so-called fever. No doubt many great and cardinal errors prevail regarding this class of diseases, and statistics may contribute aid to demolish some of them. The invasion of this disease is well known to be described by a class of obstetricians as an "accident." To remove it from this category is a just object of ambition. To some extent this has already been effected by Sir J. Y. Simpson, who has shown that it is subject to the law of the duration of labour.* The object will be further promoted if it can be shown to be under a law of the number of the pregnancy, or of the age of the mother, or of both.

It may at first sight appear unnatural to enter upon a special kind of mortality in childbed, before describing the whole mortality in childbed. And it is worth while to consider this point briefly, to show that, at least with the means at my disposal, the influence of age is better and more securely observed in this special kind of mortality than in the whole mortality of childbed. For, in proportion as puerperal fever has the quality of an accident, as many obstetricians believe, so will the unalloyed influence of the number of the pregnancy upon its occurrence be observable. Were it evidently not an accident, but due to this cause or the

* *Obstetric Works*, vol. i. p. 530. See also *Edinburgh Medical Journal* for July 1857.

other cause, it would be more and more difficult to eliminate the influence of such causes upon the mortality, with a view to arriving at the results produced by the number of the pregnancy. There are, specially in many first labours, such evident and direct causes of death in many cases, that the influence of the number of the pregnancy can make no alteration in the fate of the mother. Such cases, in however great numbers, can throw no light on the influence we are studying. In proportion as such cases are intermingled with others fitted to throw light on the subject, so will they obscure that light. Deaths in childbed from puerperal fever are, to some small extent, truly described as accidental: no cause for the supervention of the disease may be detected; just as this is the case, so will be the value of the testimony of such accidents to the influence of the number of the pregnancy.

Before discussing generally the influence of the number of the different successive pregnancies, I shall compare, first of all, the influence of primiparity as compared with that of births after all subsequent pregnancies. It is well known that first pregnancies are, as a whole, attended by a much greater mortality than subsequent pregnancies, and this is a circumstance which scarcely demands explanation, for the primiparous woman has a longer and more difficult labour than others; many primiparæ are delivered under the influence of depressing mental emotions; in primiparous women all the arrangements, mechanical and other, for delivery are tested, and subsequent deliveries occur

only in those who have so far successfully endured the trial as to survive. But it is particularly illustrative of the topic of this chapter to inquire what effect primiparity has in labours that are natural, in women against whose chances of recovery nothing is known, who have easily passed through their trials.*

Drs. Johnston and Sinclair, in their valuable work on *Practical Midwifery*, described 11,874 cases in which the labours were "purely natural." Of these 3699 were examples of primiparity. There were, therefore, 8175 births after the first. Of the 3699 primiparæ, whose labours were purely natural, 20 died of puerperal fever. Of the 8175 natural deliveries in women who had already passed safely through the dangers of parturition, 21 were followed by puerperal fever and death. To compare these proportionally, among purely natural deliveries in primiparæ, every 185th woman died of puerperal fever, or ·57 per cent; while among similar deliveries in multiparæ only every 389th woman died, or ·25 per cent. Puerperal fever deaths are described by Messrs. Johnston and Sinclair as "considered accidental." Their interesting data show that primiparæ are very greatly more liable to this awful accident than others.

In order to illustrate this particular point, I have

* Kiwisch (*Klinische Vorträge*, I. Bd., iv. Aufl., S. 622) makes the following important statement regarding Dugés, but unfortunately does not subjoin a reference :—"Dugés (says he) maintains that primiparæ, and especially those who have passed the thirtieth year of life, are more liable to puerperal fever."

no other collection of cases of natural labour to refer to, and therefore nothing so valuable and directly applicable. But I shall adduce evidence derived from more general collections of cases, including all kinds of labour.

Professor Hugenberger of St. Petersburg has published some observations on this point made in hospital practice.* Of 2253 primiparæ, 97 died of puerperal fever. Of 5783 multiparæ, 141 died of the same kind of disease. Among the primiparæ puerperal fever death seized every 23d woman, or 4·35 per cent; while among the multiparæ death in the same form seized only every 40th woman, or 2·44 per cent.

Dr. Collins in his *Practical Treatise* describes 56 deaths from puerperal fever. Of these, 30 occurred among 4969 primiparæ, and 26 occurred among 11,445 multiparæ. Among the primiparæ, every 165th woman died of this disease, or ·6 per cent. Among the multiparæ, every 440th woman died of it, or ·23 per cent.

Among the married women whose deliveries were registered in Edinburgh and Glasgow in 1855 there were 58 puerperal fever deaths. Of these, 26 occurred in 3722 primiparæ, and 32 in 12,671 multiparæ. Of the primiparæ puerperal fever carried off every 143d, or ·7 per cent; of the multiparæ every 396th, or ·25 per cent.

* "Das Puerperalfieber im St. Petersburger Hebammen Institute von 1845–1859." S. 24. Separat-Abdruck aus der *St. Petersburger Medicinischen Zeitschrift.* For some further statistics pertinent to the question, see the *Klinik der Geburtskunde.* Von Dr. C. Hecker und Dr. L. Buhl. S. 226.

Having shown, by the statistics already brought forward, that deaths from puerperal fever are among primiparæ at least twice as numerous as among multiparæ, I proceed now to inquire into the comparative mortality from this cause in labours following succeeding pregnancies.

Hugenberger devotes a short paragraph to this topic, and gives interesting data, which I here produce in a tabular form :—

TABLE CI.—SHOWING THE MORTALITY FROM PUERPERAL FEVER IN DIFFERENT PREGNANCIES. (*From Hugenberger.*)

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage of Deaths.	Or one in
1st . . .	2253	97	4·30	23
2d to 4th .	4031	85	2·11	47
5th to 9th .	1563	47	3·01	33
10th to 19th	189	9	4·76	21

This table of Hugenberger's data justifies his remarks. He says that the greater or less frequency of previous pregnancies appears to be not without influence upon the lying-in ; for while those pregnant from the second to the fourth time show the most favourable results, the first increment of mortality begins with those in the fifth to the ninth pregnancies ; and a greater mortality still is observed in women in the tenth to the nineteenth pregnancies. If we could dare to adopt as demonstrated what Hugenberger's data seem to show—and as yet I have adduced nothing

calculated to shake their evidence—we should have an extremely interesting addition to our knowledge of the influence of the number of the pregnancy upon the danger of confinement. It would appear that from the very great danger of a first confinement, the woman passed into a period of comparative safety in the next succeeding confinements, till she came to about the fifth lying-in, when danger began to increase; and as pregnancy succeeded pregnancy, danger still further increased, until it reached a degree as great as that of a first confinement.

An interesting contrast of these results with what is known of the fecundity of women at different ages may be made. The average age of wives in Edinburgh and Glasgow bearing first children is 24 years. The average age of wives bearing fifth children is 31 years. From the 25th to the 30th year women are more fertile than at any other time. It is within the ages of 25 to 30 that are included the average ages of women bearing second, third, and fourth children, those produced with least danger to life. Hence, if the data are good and sufficient, there is a coincidence between the time of the greatest amount of safety and that of the greatest fecundity; and diminished fecundity, or likelihood of having children, occurs when danger is great; that is, in first pregnancies and in fifth and subsequent pregnancies, or in pregnancies of women below 25 years of age and above 30. But this point will be better and more directly demonstrated when the influence of age is itself discussed.

I shall now bring forward other data similar to Hugenberger's, with a view to observing whether they confirm his results or not.

In Edinburgh and Glasgow in 1855 there were 58 puerperal fever deaths of wives, all occurring before the ninth pregnancy. There were in that year delivered 15,384 wives pregnant for the eighth time or less. Arranging these according to the number of the pregnancy, we have the following :—

TABLE CII.—Showing the Puerperal Fever Deaths of Wives delivered in Edinburgh and Glasgow in 1855.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage of Deaths.	Or one in
1	3722	26	·698	143
2	2893	8	·276	361
3	2534	11	·434	230
4	1982	6	·303	330
5	1543	2	·129	721
6	1221	2	·164	610
7	848	1	·118	848
8	641	2	·312	320

This table is scarcely a fit object of comparison with Hugenberger's, for it will be observed that while his table has cases of death in women even in the nineteenth pregnancy, no wife died after delivery in Edinburgh and Glasgow in 1855 whose pregnancy was above the eighth. So far as this imperfect table goes, however, it is somewhat in opposition to the general tenor of the results published by Hugenberger.

In the work on practical midwifery by Johnston and Sinclair I find a table of 75 puerperal fever deaths, in 74 of which the number of the pregnancy is given. Unfortunately, I can discover in the work no data regarding the number of the pregnancies of the whole women delivered. Unwilling, however, to lose any advantage that may be gained from the table of pregnancies of 74 puerperal fever deaths, I have in the following table arranged them for comparison with the whole women delivered in Edinburgh and

TABLE CIII.—SHOWING A COMPARISON OF PUERPERAL FEVER DEATHS IN THE DUBLIN HOSPITAL, WITH THE NUMBER OF PARTURIENT WIVES IN EDINBURGH AND GLASGOW IN 1855.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage of Deaths.	Or one in
1	3722	40	1·07	93
2	2893	6	·27	482
3	2534	11	·43	230
4	1982	3	·15	661
5	1543	3	·19	514
6	1221	4	·32	307
7	848	2	·23	424
8	641	0		
9	425	3	·70	141
10	222	1	·45	222
11	152	1	·60	152

Glasgow in 1855. Of course the percentages derived from this comparison are not figures of actual value, but only of value for comparison with one another; and it is interesting to observe that they roughly

confirm the results of Hugenberger. After a great mortality in first pregnancies, there is a great improvement in second, third, and fourth pregnancies; and then, again, as the fifth pregnancy is passed, the mortality rises as the number of the pregnancy increases. It must be admitted that this accordance is not very exact, the regularity of the results being disturbed by the great mortality in third pregnancies, and the absence of mortality in eighth pregnancies. There can be no doubt that the value of the table is not very great; yet it evidently points towards confirmation of Hugenberger. Larger and better data are required to produce a satisfactory assurance.

CHAPTER III.

THE RELATION OF THE NUMBER OF THE LABOUR TO THE MORTALITY ACCOMPANYING PARTURITION.

In pursuing this topic I shall follow the same course as I observed in describing the mortality from puerperal fever, beginning by a comparison of the mortality of first labours with that of all subsequent labours.*

The first data which I adduce are Johnston and Sinclair's 11,874 cases of purely natural labour. These are specially valuable for the purpose, for nearly the very reasons which enhanced their value when puerperal fever was the only cause of mortality under consideration—reasons which, therefore, need not be repeated here. Of 11,874 purely natural cases, 3699 were first labours, and 8175 subsequent labours. Of

* On this subject see some valuable remarks by Dr. Barnes, in the *London Obstetrical Society's Transactions*, vol. i. p. 311; and some data, with remarks, by Hegar, *Archiv f. Gynæk.* Bd. I. S. 192. Hervieux (*Maladies Puerpérales*, p. 53) has the following statement:—

"Primiparité.—Sur les 190 malades mortes d'affections puerpérales dont j'ai recueilli les observations, 119 etaient primipares, 71 multipares. La primiparité prédisposerait donc aux accidents puerpéraux graves. Des résultats statistiques analogues et plus accentués encore avaient été déjà publiés.

"Sur 1025 primipares, Lasserre comptait 89 malades et 66 décès; sur 1314 multipares, 43 malades et 21 décès (Lasserre, *Théses:*

the primiparæ 33 died, and of the multiparæ 34, or 1 in 112 of the former, or ·89 per cent, and only 1 in 240 of the latter, or ·41 per cent.

These purely natural cases form part of a total of 13,748 labours described by Drs. Johnston and Sinclair. Of these, 4535 were primiparæ and 9213 multiparæ. Among the former 83 died, or 1 in 54, or 1·8 per cent; and among the latter 80 died, or 1 in 115, or ·86 per cent—a mortality, it is to be remarked, relatively almost the same as among the purely natural labours.

We may now take those labours alone which were not purely natural. Of these, 836 were in primiparæ, and of these 50 died, being 1 in 17, or 5·98 per cent; while 1037 were among multiparæ, and of these 46 died, being 1 in 22, or 4·43 per cent. Here it is at once observed that the relative mortalities are nearly alike, forming a striking contrast to the relative mortalities under any other circumstances. It is unfortunate that this striking observation is founded on so few data. It cannot fail to excite reflections in the prac-

Paris, 1842). D' une autre part, Botrel affirme que les $\frac{9}{11}$ des malades observées par lui étaient des primipares (Botrel, *Arch. de Med.*, 1845, 4 sér., t. viii. p. 10). Sur 313 cas de mort, Charrier a trouvé 155 décès fournis par des primipares (Charrier, *Théses:* Paris, 1855). Enfin Tarnier dit que, sur 71 décès pris au hasard, il a compté 51 femmes primipares (Tarnier, *De la fièvre puerpérale observée à la Maternité*, Paris, 1858, p. 62). La concordance de tous ces chiffres ne pouvant laisser aucun doute sur l'influence prédisposante de la primiparité, je n' insisterai pas davantage sur un point qui parait etre définitivement eclairci."

tical obstetrician. Such would be out of place in this book, and I shall only diverge so far as to remark that here the primiparæ evidently hold such a greatly improved position, that while in natural labour puerperal fever carries off proportionally twice as many primiparæ as multiparæ, and that, while in labours generally twice as many of primiparæ die as of multiparæ; yet in unnatural labours the balance is restored, the primiparæ escaping the special danger conjoined to all the evils connected with primiparity, nearly as often as multiparæ escape the special danger alone, without the evils which all the statistics hitherto adduced show to attend primiparity.

In Dr. Collins' *Practical Treatise* the deliveries of 16,414 women are described. Of these, 4969 were primiparæ, and 11,445 were multiparæ. Among the whole there occurred 164 deaths, but the number of the pregnancy is given only in 160 cases. Of these 160 deaths, 80 occurred among the primiparæ, being 1 in 62, or 1·61 per cent; and 74 occurred among the multiparæ, being 1 in 155, or ·64 per cent.

In the work of Messrs. Hardy and M'Clintock on Midwifery and Puerperal Diseases, 6635 cases of delivery are described. Of these 5852 are described as natural deliveries. Among them were 1752 first labours, and 4100 subsequent labours. In the former the deaths were 7, being 1 in 250, or ·4 per cent; in the latter 9, being 1 in 455, or ·22 per cent.

The whole cases in the work of M'Clintock and Hardy are, as already said, 6635. Of these 2125 were

in primiparous women, and 35 died, being 1 in 60, or 1·65 per cent. Among multiparæ were 4510 deliveries and 30 deaths, being 1 in 150, or ·66 per cent.

In Edinburgh and Glasgow in 1855 there were 16,393 deliveries of married women. Of these, 3722 were in first labours, and 50 died within six weeks after delivery, being 1 in 74, or 1·34 per cent. The multiparæ numbered 12,671, and of these 103 died, being 1 in 123, or ·81 per cent.

Having thus compared the mortality of primiparæ with that of all other parturient women, I proceed to inquire into the mortality of each successive pregnancy.

The accompanying table is made from the Edinburgh and Glasgow registers for 1855. It exhibits the number of wives delivered in each successive pregnancy, their mortality, and the percentage of mortality to deliveries. Casting the eye along the percentage column of this table, one does not discover any marked indication of a regular variation after the great mortality of primiparæ is passed.

I have no other similar exact data to add to what is given in the opposite table (CIV.) The authors from whom I derive the following data as to the pregnancies of women dying after delivery do not give the numbers of the pregnancies of all their cases, with which to compare the pregnancies of those that died. But I here make the data regarding deaths available by comparing them with the pregnancies of the whole wives delivered in Edinburgh and Glasgow in 1855. This

composite table will thus not give results or percentages representing actual values, but only results for mutual comparison, and I venture to think they are valuable. The table is prepared as follows :—The first column states the number of the pregnancy ; the second gives the number of wives delivered in Edin-

TABLE CIV.— SHOWING THE MORTALITY AMONG WIVES DELIVERED IN EDINBURGH AND GLASGOW IN 1855, IN EACH SUCCESSIVE PREGNANCY.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage.	Or one in
1	3722	50	1·343	74
2	2893	24	·829	120
3	2534	25	·986	101
4	1982	13	·655	152
5	1543	13	·842	119
6	1221	7	·573	174
7	848	7	·825	121
8	641	8	1·248	80
9	425	3	·706	142
10	222	1	·450	222
11	152	1	·658	152
12	61			
13	34			
14	11			
15	6	1	16·666	6

burgh and Glasgow in 1855, in each successive pregnancy; the third column gives the number in each successive pregnancy of a collected mass of cases of childbirth deaths; the fourth column gives the percentages of these deaths in the deliveries in each successive pregnancy. In the third column are given

540 deaths gathered with care from the following sources:—153 from the Edinburgh and Glasgow registers above referred to; 160 from a table in page 364 of the *Practical Treatise* of Dr. Collins; 162 from the *Practical Midwifery* of Drs. Johnston and Sinclair; and 65 from the work on *Midwifery and Puerperal Diseases* of Drs. M'Clintock and Hardy.

TABLE CV.—Showing a Comparative Percentage of Deaths in successive Pregnancies.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage.	Or 1 in
1	3722	254	6·82	15
2	2893	60	2·07	48
3	2534	64	2·52	39
4	1982	39	1·97	51
5	1543	31	2·01	49
6	1221	28	2·29	43
7	848	16	1·88	53
8	641	15	2·34	42
9	425	13	3·06	32
10	222	9	4·05	24
11	152	5	3·28	30
12	61	1	1·64	61
13	34	4	11·77	8
14	11			
15	6	1	16·66	6

This last table appears to me to show, with considerable force, that after a woman has passed her ninth pregnancy or thereabout, she comes gradually into more perilous child-bearing, danger increasing with every unit added to the number of her children.

To collate with this, it is interesting to the obstetrician to note, what has been already shown, that after the ninth, pregnancies recur with greater rapidity than before it.

Having now led all the evidence I intend to adduce, I shall, in conclusion, add a few general and recapitulatory remarks.

First of all, it must be noted that I have, hitherto at least, said nothing regarding the nature of the relation between the number of the delivery and the mortality attending it. It is true the data recorded demonstrate more or less completely certain coincidences, which may be called laws. But they establish nothing further. These laws are as follows :—

1. The mortality of first labours is about twice the mortality of all subsequent labours taken together.

2. The mortality from puerperal fever following first labours is about twice the mortality from puerperal fever following all subsequent labours taken together.

3. As the number of a woman's labour increases above nine, the risk of death following labour increases with the number.

4. As the number of a woman's labour increases above nine, the risk of death from puerperal fever following labour increases with the number.

5. If a woman have a large family, she escapes extraordinary risk in surviving her first labour, to come again into extraordinary and increasing risk as she bears her ninth and subsequent children.

These laws, although they merely state coincidences, have very important practical bearings, which are too self-evident to require description. They have also important pathological bearings. The most important, perhaps, of these relate to puerperal fever. These also I shall not enter upon further than to say that the occurrence of puerperal fever specially among primiparæ, and women who have borne large families, —its pretty close correspondence in relative amount to the general mortality of parturition after different pregnancies—its subjection also to the law of the duration of labour,—do not appear to me to lend support to the views hitherto generally entertained regarding it, and expressed in the words accidental, fever, infectious, epidemic. Another point under this head, I shall merely mention. Authors, comparing the mortalities of lying-in institutions, whether from puerperal fever or from other causes, are frequently found neglecting to begin by ascertaining whether or not they are fit objects of comparison, and under this head, *inter alia*, neglecting to ascertain the comparative amount of primiparity in each institution. It is plain that, unless there be nearly the same comparative amount of primiparity in the institutions, their respective gross mortalities cannot be justly contrasted with one another.

The well-known protraction of labour in primiparæ may to some appear a sufficient cause of the increased mortality of first child-bearing. But mere prolongation of labour for a few hours cannot, in my opinion,

be regarded as any satisfactory explanation of the causation of this increased mortality. In one set of Johnston and Sinclair's cases, the labours of primiparæ are called purely natural, and they are compared with similar purely natural cases in multiparæ; and the mere addition of a few hours to the length of labour in such primiparæ is not a sufficient cause of their mortality being twice as great as that of similar multiparæ. Denman alludes to "a vulgar and pernicious error which makes no distinction between the slowness and the danger of a labour." It would be to fall into this error to explain the increase of mortality merely by increased length of labour.

It must be held as proved, that according as labour increases in length, so the mortality accompanying it increases; and that this is true not only of the whole mortality, but also of the special mortality from puerperal fever. This law, although it must have weighty bearings on the mortality of primiparæ with their long labours, cannot be regarded as to any great degree throwing light on it; for we find new increments of mortality after the ninth labour, when we have no reason to believe that labour is more prolonged than in labours preceding the ninth, in which the mortality is less. In other words, we have the number of the labour denoting increase of mortality where there is no evidence of accompanying increase of its duration. The law of duration, then, does not enable us to explain the variations of mortality in different labours.

To completely exclude the influence of the law of

duration would be very desirable; but we see no present prospect of doing this, except by processes of reasoning. Without such, it could only be done by comparing a series of labours of different number, but in all which the duration was the same.

It must be remarked that the law of duration certainly has important bearings on the data and arguments herein adduced to show the influence of the number of the labour, and that the extent of these bearings is undecided. At the same time, it is equally sure that the law of the number of the pregnancy has important bearings on the data and arguments adduced to show the influence of the duration of labour, and the extent of these bearings is undecided. The mutual influence of the data and arguments in these demonstrations must be great, and it remains for future observers to accumulate materials for either showing the amount of these influences, or for a separate demonstration of the laws by data which do not intermingle them in their conditions.

It is worth while to remark that, restricting for a moment our regard to the great mortality of primiparæ (exceeding as it does that of multiparæ, taken together), we have a set of cases—those of Johnston and Sinclair—where the deaths were from puerperal fever, and in which the average excess of duration of labour in primiparæ above that observed in multiparæ was 4 hours. In multiparæ the average duration was said to be 8 hours; in primiparæ 12 hours. Looking at this increased duration, and the correspondingly in-

creased mortality in primiparæ, with the light thrown on it by tables published by various authors to demonstrate the law of duration, it appears to me that the increase of mortality in primiparæ is above that which these tables appear to give as the increase corresponding to a rise in duration from an 8-hours' labour to a 12-hours' labour.

These various remarks I have made with a view to keeping the demonstration of the influence of the number of labour on childbed mortality in its proper light, to keep it separate from other laws or supposed laws with which it may be confounded. I have alluded, with this view, to the causation of the variations of mortality according to the number of the pregnancy. It is no main part of this chapter to enter on this subject, but a few words may not be out of place. It would be foolish to imagine that any injurious influence or the reverse could spring from the mere number of the pregnancy. A woman in a first may, and often does, have as fortunate a delivery as in any other. To ascribe to the number of pregnancy any potency would be to fall into the error of those students of the duration of labour who ascribe great potency to the mere addition of length to a labour. In the case of the law demonstrated in this part, and in the case also of the law of the duration of labour, it appears to me that the source of the variations of mortality is to be looked for in the introduction of complications. I here use the word complications in a much wider sense than is generally ascribed to it, wishing it to imply injuries or

injurious tendencies far slighter than those ordinarily classed as complications of labour. I have no doubt that all of these, however minute or slight, have their weight in giving proclivity to a fatal termination of the childbed. Puerperal fever may have its root in an otherwise insignificant perineal laceration, as well as in a phlebitis or endometritis.*

In primiparæ, as labour goes on, complications occur which are not nearly so liable to attack a woman in her next subsequent labours. These have their origin in various sources, chiefly in mechanical difficulties, and these often so slight as not to take the case from the category of purely natural, in an arrangement where the labour is alone taken into consideration, to the exclusion of the childbed.

Multiparæ are specially and increasingly liable to complications of a different kind connected with constitutional diseases, and with local infirmities of the uterus.

This introduction of complications forms also the main explanation of the law of the duration of labour.† Indeed, in a rough way, it may be held that the statement of duration is a statement of the increase of complications; for it is known that as labour lengthens out, so complications increase in frequency. Without these complications duration would be of small import-

* On this subject see remarks by Schroeder. *Schwangerschaft, Geburt.* etc., S. 197.

† See a mass of important and pertinent facts tabulated and discussed by Simpson. *Obstetric Works*, vol. i. p. 430.

ance, as the profession has generally held. Their introduction is present evil and the seed of future disasters. Tables have been framed to show the increasing introduction of complications as labour is prolonged, but I only refer to them. They are utterly useless, so far as I know them, because they are founded only on an enumeration of those of the graver sort. Further, the introduction of complications is not ruled exclusively by the duration of labour. Many are rather connected with precipitate parturition. The complications which probably contribute largely to produce the increased fatality of labours after the ninth are not all included, or capable of inclusion, in any statement of duration, being present before and after the process.

CHAPTER IV.

THE INFLUENCE OF CHILDBED MORTALITY, AND SPECIALLY OF THE MORTALITY CONSEQUENT ON PRIMIPARITY, ON THE WHOLE MORTALITY OF WOMEN AT THE CHILD-BEARING AGES.

BEFORE leaving the subject of the great danger of primiparity, I have pleasure in referring to and quoting Dr. Stark's remarks on the reciprocal influence of the mortality of primiparæ on the whole mortality of married women, as compared with unmarried women. In the part quoted it will also be noticed that Dr. Stark arrives, by a method of his own, at the conclusion that primiparity is specially a cause of death in lying-in women. He also devotes some remarks to the influence of immaturity of the female (15 to 20 years of age) in increasing the mortality of childbed.

"When," says the author, "the proportional death-rates at the quinquennial periods of life are compared, it is seen that the high death-rate of the married female appears to be confined to the ages under 30 years; but that from 30 to 35, and again from 35 to 40 years, the death-rate of the married female falls below that of the unmarried.

"But the striking fact is, that the mortality of the married females between the ages of 15 and 20 years is higher than that of the married females even during the next three quinquennial ages—viz. it is higher than the married females at the ages of 20 to 25, 25 to 30, and 30 to 35 years. This is very remarkable, seeing that the mortality, as a general rule, increases with age; and the fact brought to light by the table, that the mortality of the married women between 15 and 20 years of age is much greater than at the three immediately higher quinquennial ages, seems to indicate that marrying before the frame has acquired its full development causes the woman to run greater risk of her life than if marriage had been delayed till the full growth was completed.

"But it is between 15 and 20 years of age that the greatest difference occurs between the mortality of the married and that of the unmarried female. Thus, according to the table, it would appear that in every 100,000 married women at that age, 983 deaths occurred; while in an equal number of unmarried women at the same ages only 691 deaths occurred. In other words, supposing married and unmarried were in equal numbers between 15 and 20 years of age, 10 married would die for every 7 unmarried.

"At the next quinquennial period of life—viz. between the ages of 20 and 25 years—it appears that in every 100,000 married women 910 deaths occurred; while in an equal number of unmarried women only 783 deaths took place. In other words, in equal num-

bers of each class from 20 to 25 years of age, 9 married women would die for every 8 unmarried women; so that the difference was much less between the death-rates of the married and the unmarried at that age than at the previous or junior age.

"From the 25th to the 30th year of life the difference in the death-rates of the married and the unmarried is slight, inasmuch as 928 deaths occurred in every 100,000 married women, while 903 deaths occurred in a like number of unmarried.

"From the 30th year of life the chances appear to be in favour of married life, inasmuch as the table shows that between 30 and 35 years of age only 927 deaths occurred in every 100,000 married women; whereas 941 deaths occurred in a like number of unmarried women at the same ages.

"The same is seen at the next age—viz. from 35 to 40 years of age; for while only 1116 deaths occurred among every 100,000 married women, there were 1181 deaths in a like number of unmarried women at the same ages.

"It seems to be unnecessary to compare the relative death-rates of the married and unmarried females above this age. Speaking generally, it may be said that at every age above 30 years, the mortality of the married female is lower than that of the unmarried, so that the higher mortality of the married female is confined to the ages under 30 years.

"The important question for solution, therefore, is, What is the cause of the mortality of the married

female being so much higher than that of the unmarried at the three quinquennial ages, 15-20, 20-25, and 25-30? To aid us in such an inquiry some additional facts are required; and they are so far supplied by tables which were published for Edinburgh and Glasgow in our *Second Detailed Annual Report.* From these tables it appears that in Edinburgh and Glasgow in 1855, of 16,573 mothers 9274 were under 30 years of age, and 7299 above that age. That is to say, that, speaking in a very general way, nearly as many women above 30 years as under that age bore children in these towns during 1855. Now, it may be assumed that the relative proportions of women under and above 30 years of age who annually bear children in Scotland are much the same; let us therefore apply this knowledge to the solution of the above problem.

"If nearly an equal number of women above and under 30 years of age bear children, and if it be found, as Table XXV. proves, that the mortality of the married female is only greater than that of the unmarried female below 30 years, while above that age fewer married females die than unmarried, the conclusion seems irresistible that the greater mortality of the married female under 30 years of age cannot be owing simply to child-bearing, because it does not increase the mortality of the married female above 30 years of age. But that table just as distinctly proves that it is some danger connected with married life, because the unmarried females under 30 years of age are not subject to it; and that it is a danger which is

greatest during the earlier years of marriage, and which disappears as life advances. Every medical practitioner seeing these facts would at once suggest as the explanation, that this additional danger during the earlier years of marriage is the birth of the first child. It is a well-known fact that the risk to the mother is far greater at the birth of her first child than at any subsequent delivery; and it is extremely probable that the whole extra mortality of the married female under 30 years of age, over that of the unmarried at the same ages, may be caused by the greater dangers which attend the delivery of the first child. This can almost be demonstrated to be the true explanation.

"If we throw the tables above referred to, having reference to the ages of the mothers in Edinburgh and Glasgow in 1855, into another form, as here given, they will show us the number of mothers in these towns at each quinquennial period of life, the numbers who at each age bore their first child, and the proportion of mothers bearing their first child to every 100 mothers at each age. We have then the following interesting result:—

TABLE CVI.

Ages of Mothers.	Total Number of Mothers.	Number of Mothers bearing their First Child[n].	Proportion of Mothers bearing their First Child[n]. to every 100 Mothers.
15–20 years	403	354	87·8
20–25 „	3814	1921	50·3
25–30 „	5057	1019	20·1
30–35 „	3943	331	8·3
35–40 „	2395	124	5·1
40 and above	961	32	3·3

"Between the ages of 15 and 20 years, 87 per cent of the mothers were confined with their first child. Between the ages of 20 and 25 years, 50 per cent of the mothers were confined with their first child. Between 25 and 30 years, only 20 per cent of the mothers were confined with their first child; and the proportion diminished to 8, 5, and 3 per cent at the three succeeding quinquennial periods of life.

"Assuming, therefore, that in all Scotland the proportion of mothers bearing their first child is at each age the same as in these towns, and comparing these proportions with those of the married women who died at each quinquennial period of life, we are almost driven to the conclusion that the excessive mortality in the married female, as compared with that of the unmarried at the same ages, is almost solely due to the superadded dangers which attend the birth of the first child. That, in fact, after the birth of her first child, the married female, even during the rest of her

child-bearing life, has an equal chance of life with the unmarried, and has a better life than the unmarried after she has passed her 30th year."*

I have abstained from discussing in this chapter the subject of the influence of marriage upon mortality. Farr and Stark have both entered upon it at length, the former using French and the latter using Scotch data as the foundation of their arguments. Both arrive at similar conclusions, which are very strongly in favour of the influence of marriage. But I cannot assent to the justice of the arguments of either, or to their conclusions. They do not exclude an influence which is enough, alone, to account for all the statistical differences in favour of the married state, which they show. This influence is the constant selection or picking of the married. At every age the healthy and beautiful go over from the unmarried side to the married, leaving the unmarried columns crowded with the sickly and unfortunate. I have too much faith in the common sense and observation of people generally to allow me to believe that the immense disadvantage of the unmarried state, which Farr and Stark think they demonstrate, could have remained unknown or concealed from the public till they showed it. The fact is, that they are wrong in supposing they have shown it.

* *Seventh Detailed Annual Report*, p. xxxi. Consult also Boudin, *Geographie et Statistique Médicales*, tom. ii. p. 77.

CHAPTER V.

THE RELATION OF AGE TO THE MORTALITY FROM PUERPERAL FEVER.

On this subject important information is to be found in a letter addressed by Dr. Farr to the English Registrar-General, and published in the Appendix to that officer's Seventeenth Annual Report :—

"What (says Dr. Farr) is the danger of death by childbirth among women of different ages who bear children during the year? This is a different question, which is of practical importance both in medical science and in the business of life-insurance. The defect in the English schedule, which, as yet, contains no column for the ages of the parents of the children registered, renders it impossible to answer this question with precision. It will, however, be useful to obtain an approximate answer; and this we have been able to give by determining the probable proportion of women who bear children at each age, from the Swedish returns, and by applying the fraction expressive of this proportion to the English women living in 1851 at the corresponding age, the probable number of them who became mothers every year is determined. The total

number thus determined for the year 1851 is 609,845 ; while the actual average number of the births in the seven years by the returns was 603,045. It is thus evident that the estimate differs to no great extent from the facts, and it may be assumed that the births, corrected for twins, triplets, and still-born children, in England, would represent nearly 609,845 child-bearings." *

The following table, extracted from the data supplied by Dr. Farr, shows the mortality from puerperal fever in four decenniads :—

TABLE CVII.—SHOWING THE MORTALITY OF CHILD-BEARING WOMEN FROM PUERPERAL FEVER, IN ENGLAND, AT FOUR DIFFERENT AGES. (FARR.)

Ages	15-24	25-34	35-44	45-54
Child-bearing women .	107,440	328,720	166,140	7545
Deaths from puerperal fever	298	486	256	12
Percentage . . .	·277	·148	·154	·163
Or 1 in every . .	360	676	649	628

The large figures in this table give great value to the result, that while childbearing women aged from 15 to 25 do die of puerperal fever in a proportion far

* I have been repeatedly consulted by the medical officers of insurance offices as to the proper conduct of cases of application of women for insurance who were child-bearing or had the prospect of child-bearing in future. Any advice I have hitherto given has been in very general terms. In this volume, however, there is now given a basis from which the actuary may calculate the answers to the

exceeding that of women at any other age, the child-bearing women aged from 25 to 35 are carried off by the same disease in the lowest proportion compared with all others. Puerperal fever mortality at its lowest among the lying-in aged from 25 to 35, rises on either side of this age, but it rises far more quickly and highly as age decreases than as age advances.

It would be unphilosophical to draw from this table even a presumption as to the influence of age on puerperal mortality, until careful consideration has been made of all the influences besides age which may have a bearing on it. Now, as far as I know, the paramount influence interfering with deductions from this table as to the influence of age is that of the number of the labour. Of the influence of primiparity, Dr. Farr, Dr. Tyler Smith (*Manual of Obstetrics*, chap. xlviii.), Dr. Barnes, and Dr. Stark, have had some degree of appreciation. But Dr. Hugenberger has, in some data he has published, actually separated the primiparous from the multiparous, with the view of eliminating this great influence. I here produce the tables of Hugenberger, re-arranged for uniformity's sake (CVIII. and CIX.)

These tables are interesting, and seem to show that Hugenberger felt the necessity, in the study of the bearing of age on puerperal fever mortality, of separat-

most important questions in this topic. He can determine the fecundity of the female, or her chance of having offspring; the fertility, or the number she is likely to have; the time when she will probably become relatively sterile; the risk of death in bearing her first child; and, if she survives the birth of her first child, the risk of death in her subsequent confinements.

ing primiparæ from multiparæ. Any special results which might be drawn from them I think little worthy of consideration, in deference to the much larger and more valuable data which I adduce, and on account of the extraordinary mortality which the tables reveal.

TABLE CVIII.—Showing the Mortality of Primiparæ, of different Ages, from Puerperal Fever, in the Midwives' Institute of St. Petersburg. (Hugenberger.)

Ages	15-18	19-22	23-26	27-35	36-45
Childbearing women . .	147	859	711	495	41
Deaths from puerperal fever	7	25	22	39	4
Percentage	4·76	2·91	3·09	7·88	9·75
Or 1 in every	21	34	32	13	10

TABLE CIX.—Showing the Mortality of Multiparæ of different Ages, from Puerperal Fever, in the Midwives' Institute of St. Petersburg. (Hugenberger.)

Ages	18-22	23-26	27-35	36-53
Childbearing women . . .	503	1410	2967	903
Deaths from puerperal fever	11	29	74	27
Percentage	2·18	2·05	2·49	2·99
Or 1 in every	46	48	40	33

It may with truth be said that to make a perfectly satisfactory comparison of the mortalities of women of different ages, it is necessary to compare with one another masses of women of different ages in each successive pregnancy. I know of no data for this

purpose. Hugenberger's data of primiparæ are a poor instalment, and my own Edinburgh and Glasgow data are equally insufficient, and I do not think it necessary to encumber these pages with them.

I have, however, ventured to increase the value of Farr's data, with a view to the question of the influence of age, by the following method. In Table CX. the result is given.

The correction for primiparity is made because the puerperal fever mortality after first labours is at least double the puerperal fever mortality of all other

TABLE CX.—Showing the Mortality of Childbearing Women from Puerperal Fever in England, at Four Different Ages, corrected for Primiparity.*

Ages	15-24	25-34	35-44	45-54
Childbearing women	107,440	328,720	166,140	7545
Deaths from puerperal fever corrected for primiparity . . .	194½	339½	256	12
Percentage . . .	·181	·121	·154	·159
Or 1 in every . .	552	823	649	629

* It is to be remarked that this and the following tables corrected from Farr's data give results for different decenniads that may be compared only with one another. The table would not give actual values even were Farr's data actual values, which they are not. Correction has been made only in the line of deaths by

labours taken together. In order to remove entirely, or almost entirely, the disturbing influence of primiparity, then, it is necessary to turn out of the data one-half of the deaths of primiparæ. The number of puerperal fever deaths of primiparæ at different ages is got by determining their probable proportion from the Edinburgh and Glasgow mortality of 1855.* The preponderance of primiparæ at the earlier ages renders this correction necessary, and I only regret that the smallness of the data prevents us from ascribing to the correction a high value.

It may with truth and with some cogency be said that Farr's table should be further corrected for the increased mortality accompanying ninth and subsequent pregnancies which fall into the more advanced ages. I do not attempt this correction, because it

taking away one-half of the deaths of primiparæ. This makes the table read as if a table of multiparæ. This proceeding, being simpler, has been preferred to another, which might have been followed—namely, to extract from the mothers the whole primiparous by estimate, and to extract from the deaths those of primiparæ, and compare the remaining multiparæ and deaths of multiparæ.

* TABLE CXI.—Showing the Mortality from Puerperal Fever of Primiparæ in Edinburgh and Glasgow in 1855.

Ages	15-19	20-24	25-29	30-34	35-39	40-44
No. of primiparæ . .	331	1859	1007	354	134	33
Deaths by puerperal fever .	2	14	7	3		
Percentage . . .	·604	·753	·695	·847		
Or 1 in	165	133	144	118		

cannot, with the means at my disposal, be done satisfactorily. But the omission of this correction will, comparatively, cause little inaccuracy in the results drawn from the table; for births in ninth and subsequent pregnancies are proportionally few, and the average age of women in ninth and subsequent pregnancies is above thirty-seven years, an age before which the injurious influence of elderliness appears to have already shown itself.*

* TABLE CXII.—Showing the Number of Children born in First and Subsequent Pregnancies in Edinburgh and Glasgow in 1855, and the Average Ages of the Mothers in each successive Pregnancy.

No. of Pregnancy.	No. of Children.	Average Age of Mother.
1st	3722	24·6
2d	2893	26·2
3d	2534	27·6
4th	1982	29·9
5th	1543	31·5
6th	1221	32·9
7th	848	34·9
8th	641	36·1
9th	425	37·5
10th	222	38·8
11th	152	39·2
12th	61	40·0
13th	34	41·7
14th	11	42·4
15th	6	42·7
16th	2	48·5
17th	2	41·5
18th	1	40·0
19th	1	48·0

It has, lastly, only to be remarked that reference to Table CX., corrected as it is for primiparity, shows results still closely resembling in general features those derived from the uncorrected table. Though similar in general features, there is evidently great difference in the numerical variations in the two Tables (CVII. and CX.), and I think there can be no doubt that the last table (CX.) gives an approximation to a view of the influence of age far more faithful than the first (CVII.)

CHAPTER VI.

THE RELATION OF THE AGE OF THE MOTHER TO THE MORTALITY ACCOMPANYING PARTURITION.

THE first table which I shall adduce under this head is extracted from the data of Dr. Farr, already referred to. The calculations, as made by Dr. Farr, give the mortality according to age, but, since primiparous females are included in the lists, they are of little value as indicating the influence of age. I have, as in the table of puerperal fever deaths, corrected Dr. Farr's data for primiparity, and in the penultimate line given the percentages; which may be held as showing, when compared one with another, an approximate estimate of the influence of age on the mortality of parturition.*

* See some valuable and corroborative statements by Hegar, *Archiv f. Gynæk.* Bd. I. S. 192.

TABLE CXIII.—Showing the Mortality of Childbearing Women in England at Four Different Ages, and the same corrected for Primiparity.

Ages	15–24	25–34	35–44	45–54
Childbearing women	107,440	328,720	166,140	7545
Deaths	718	1397	1051	66
Percentage . . .	·668	·425	·633	·883
Deaths corrected for primiparity .	473	1216	1033	66
Percentage . . .	·440	·369	·621	·875
Or 1 in every . .	227	270	160	114

Here the large figures give a proportionate value to the results. The women aged from 25 to 34 have the fewest deaths among them—namely, ·369 per cent. They are more fortunate than the very young women aged from 15 to 24 by ·07 per cent. But the seeming influence of this youthfulness in aggravating the danger of women is slight when compared with that of advancing years, the decenniads following that from 25 to 34 showing a mortality increasing in a far higher ratio.

The next table which I adduce is made from data furnished by Dr. Collins' *Practical Treatise*.

TABLE CXIV.—Showing the Mortality of Childbearing Women in the Dublin Hospital during Dr. Collins' Mastership.

Ages	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	Total.
No. of Mothers	762	4862	5309	3817	1210	397	22	6	16,385
Deaths	10	38	41	41	26	4	...	...	160
Percentage	1·312	·781	·772	1·074	2·148	1·007	...	...	·976
Or 1 in every	76	128	129	93	46	99	...	...	102

It is not corrected for primiparity, and I bring it forward for its own value, and because it gives the data for periods of five years. It will be observed that its results agree in the main with those derived from the data of Dr. Farr. But a further step is attained by the five-year divisions, showing that youthfulness is influential chiefly below 20 years, and increasing age not until 30 years are passed. These results are, in my opinion, however, somewhat modified by the data which follow.

The next table (CXVI.) contains only multiparæ. Primiparæ are excluded; there is therefore no correction to be made. Further, it is not estimated; the figures all show actual values.*

It is worthy of remark that this table of actual values shows a minimum of mortality at the age of from 25 to 29 years.

The last table (CXVII.) to be given is a composite one, but appears to me to be of value with reference to the present question. It is made up as follows :—

* To complete the view of the mortality of childbearing women in Edinburgh and Glasgow in 1855, I here give the mortality of the primiparæ. It is not placed in the text because of the smallness of the figures compared with those of multiparæ, and because the table of multiparæ is more like the others in the text.

TABLE CXV.—Showing the Mortality of Primiparæ at different Ages, in Edinburgh and Glasgow in 1855.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45-49	Total.
No. of Primiparæ	331	1859	1007	354	134	33	4	3722
Deaths of ditto .	4	24	13	8	1	...	...	50
Percentage . .	1·208	1·291	1·291	2·260	·746	...	...	1·343
Or 1 in every . .	83	77	77	44	134	...	...	74

TABLE CXVI.—Showing the Mortality of Multiparæ at different Ages in Edinburgh and Glasgow in 1855.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45-49	Total.
Multiparæ	45	1829	4030	3496	2273	807	92	12,572
Deaths in ditto . . .	...	13	21	39	20	8	2	103
Percentage	...	·711	·521	1·115	·879	·991	2·174	·819
Or 1 in every	...	140	192	89	113	100	46	122

TABLE CXVII.—SHOWING THE COMPARATIVE MORTALITY OF CHILDBEARING WOMEN AT DIFFERENT AGES.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54
Mothers (Collins) . . .	762	4862	5309	3817	1210	397	22	6
Deaths, corrected for primiparity	$10\frac{1}{2}$	59	89	$91\frac{1}{2}$	$52\frac{3}{4}$	15	1	
Percentage	1·3	1·2	1·6	2·3	4·3	3·7	4·5	
Or 1 in every . . .	72	82	60	42	23	26	22	

With the number of women confined at different ages in the Dublin Hospital under Dr. Collins are compared the deaths at different ages recorded by Drs. Collins, M'Clintock and Hardy, and Johnston and Sinclair. These deaths have been collected with considerable care to ensure a close approach to their true number. All the deaths were among women delivered in the same hospital, and these are compared with another set of cases also delivered in it. The deaths are corrected, as in former tables, for primiparity. The resulting percentages are of course of value only when compared with one another, and in this respect they appear to me to be very valuable. The smallest mortality is seen to be in the age 20-24 ; and the increased mortality from greater youthfulness is in the next quinquennial period less than the corresponding increase on the other side from greater age.

Looking over these tables, one cannot doubt that the result of Farr's data, showing 25 to 35 as the age of smallest mortality, may be considered as justly supplanted by the results of the tables showing quinquennial periods. In all of these the smallest mortality is found to be under 30 years of age. Of the tables showing quinquennial periods, given in the text, No. CXVII. gives 20-24 as the safest age for parturition, while the CXVI. gives it as 25 to 29 ; and we may guess with considerable assurance that the age of minimum mortality from parturition is at or near 25 years.

The following are the chief conclusions deducible from the whole exposition :—

1*st*, Youthfulness has less influence in producing mortality from parturition than elderliness.

2*d*, From the earliest age of child-bearing there is a climax of diminishing puerperal mortality,* succeeded by an anti-climax of puerperal mortality increasing till the end of childbearing life.

3*d*, The age of least mortality is near 25 years, and on either side of this age mortality gradually increases with the diminution or increase of age.

4*th*, Above 25 years puerperal mortality increases at a much higher rate than it increases at corresponding periods below 25 years, a circumstance which decidedly throws the greater safety to the side of the quinquenniad 20-25.

5*th*, Though it is not deducible from anything in this part, it is too interesting to escape notice that the age of greatest safety in parturition coincides with the age of greatest fecundity, and that during the whole of childbearing life safety in parturition appears to be directly as fecundity, and *vice versa*. "To the female sex," says Aristotle, "premature wedlock is peculiarly dangerous, since, in consequence of anticipating the demands of nature, many of them suffer greatly in childbirth, and many of them die."†

* See Horwitz, *Monatsschrift für Geb.*, etc., 1868, Bd. xxxii. S. 296.

† Sadler, *Law of Population*, vol. ii. p. 272.

On the other hand, the practical obstetrician can feel no astonishment at the influence of age and multiparity, for he has only to reflect on the history of after-pains, phlebitis, phlegmasia dolens, rupture of uterus, and twin-bearing, in order to find sources of its explanation.

CHAPTER VII.

OF PUERPERAL FEVER IN HOSPITAL AND IN PRIVATE PRACTICE.

To attempt to settle any disputed point, such as the salubrity of hospitals, by using the mortality from metria or puerperal fever as the criterion, is a most unsatisfactory kind of proceeding, and certain to lead to no exact or convincing results. This arises simply from the disease being one regarding which very little is exactly known or even agreed upon, and the consequent uncertainty of the diagnosis or opinion of any physician regarding it, and the consequent still greater uncertainty that any two or more physicians alluding to it are giving figures or stating opinions regarding the same things. Dr. Evory Kennedy defines metria as including "puerperal fever, metritis, peritonitis, pleuritis, phlebitis, arthritis, pyæmia, purpuric or cerebro-spinal metria, traumatic metria, erysipelas, and hospital gangrene."* Although this appears to me to be the opposite of defining or settling, I shall not here enter upon any detailed objection to it, because it would involve a discussion not essential to my present purpose.

I have elsewhere† stated my reasons for adopting

* *Hospitalism*, p. 24.

† *Edin. Med. Jour.* Nov. 1869, and page 282 of this book.

the total, and not merely the metria or puerperal fever mortality, as the best criterion for judging of the salubrity of hospitals, yet as affording results that require to be corrected according to the varying conditions of the patients other than such as affect the salubrity of residence. But though this is the case, some good may come of discussing the metria mortality, taking up the subject as vulgarly treated in all modern writings, even though the ground is insecure and unworthy of confidence.

Puerperal fever, or metria, is to me a hotbed of insufficient and false hypotheses. I do not believe there is any such single disease. The term includes a variety of diseases, and a variety of modifications or terminations of diseases. It is familiarly described as a zymotic disease. It is described universally as occurring in epidemics; not merely as an endemic. I feel certain, and believe I can prove, that an epidemic of puerperal fever never occurred—that no accepted definition of an epidemic can be made to include metria. When authors speak of epidemics of metria, of puerperal fever sweeping over a country, as they do speak, they are merely giving the reins to a misleading hypothesis which suits their present purposes or fancies. This disease is described as being contagious, or infectious, or both; but the authors who do this forget, with an almost ridiculous carelessness, to define what the disease is of which they are speaking so confidently, to show whether there is any such fever or not, and still more to define contagion

and infection. I cannot refrain from referring to a common occurrence in medical and in general circles, as illustrating the present unsatisfactory state of the theory of this great disease, so called. When, in any town or country-side, even a single woman of position dies of puerperal fever, one immediately hears of childbed fever raging—of an epidemic.* If two or three cases occur simultaneously in the same exalted class, the outcry is dreadful about the pestilence. The doctors and the public are all talking. A superstitious terror, the natural child of ignorance, prevails. But though many die in the hovels of the poor or the cottages of a better sort, there is little talk among the doctors and less among the people. Both doctors and people seem ignorant of the fact that there is a regular and practically constant mortality from puerperal fever all around them.

In order to make arguments from the prevalence of metria tell against hospitals, it has been called preventable. The whole of Dr. Kennedy's essay on Hospitalism is based on this allegation. It may be true, but there is not a particle of evidence of its truth. The disease may have its ravages diminished, but it has yet to be shown that it can be altogether prevented. The term "preventable," so attached to metria, is a sufficient proof of the thoroughly unpractical or sensational character of the speculations of any writer who uses it as implying that we have means of preventing its appearance. It is, in truth, as

* See remarks at page 285.

little preventable as any disease in the nosology, or any crime in the statute-book. It is, possibly, preventable; but it has certainly never been prevented. It is preventable, and physicians are at hand; but it is common everywhere! It is preventable; but it occurs in spite of the combination of every circumstance that is known or supposed to prevent it!

Metria and pyæmia (certainly not very far from being identical) are the chief causes or indices of variations of mortality in hospital and in private practice. They are more frequent the worse the general health, the more depraved the constitutions, of the patients. They are more frequent the worse or the more serious the kind of cases treated. They are also more frequent the worse the arrangements affecting the salubrity of the patient's bed and house. Positive statements like these respecting puerperal fever and pyæmia are not of a nature to be disputed by any one, and I believe they may be as nearly proved as is admitted by the nature of the case.

It has, during recent discussions regarding hospitals, been repeatedly asserted that puerperal fever is a constantly-existing or constantly re-appearing disease in them, or at least in the larger examples of them; and that it is only an occasionally and rarely-occurring disease in private practice. These statements are so important as to appear to me to demand very careful consideration and investigation. If they are true, they at least suggest a very powerful argument against even the best hospitals as at present con-

structed and managed. This is one source of the importance of the statements; but there are several others.

Speaking of this disease, Dr. Kennedy says—"Its local cause approaches more nearly to a *constant* quantity in the wards of a crowded lying-in hospital; whereas it is only an *occasional* quantity in the houses of the affluent." * Again, he says—"That in lying-in hospitals, where large numbers of patients are delivered under the same roof, the disease finds its habitat, appearing and reappearing at uncertain intervals." † Again, he adds—"It is therefore not a disease found to prevail in small lying-in hospitals, or cottages where only one or two patients cohabit in their lying-in." ‡

The first question to which I would direct special attention regards the alleged only occasional occurrence of this so-called puerperal fever outside of large hospitals. It is well known to be a common disease in large hospitals, and to have regularly ascribed to it a certain proportion of the hospital yearly mortality. Dr. Kennedy and others assert that this kind of statement is true only of large hospitals. Is it so?

With a view to getting the solution of this question, I might look to my own private practice, but there I find the numbers too small for comparison with large hospital yearly statistics. If with my private practice I include my consultation practice, then I find a regularly reappearing yearly quantum of puerperal fever, just as in hospitals. But I prefer,

* *Hospitalism*, etc., p. 10. † *Ibid.* p. 25. ‡ *Ibid.* p. 26.

with a view to the greatest attainable accuracy, to take the public documents of the Registrar-General of Scotland. From these I have framed the following table, showing the reported mortality from metria in the towns of this part of the United Kingdom.

TABLE CXVIII.

Year.	No. of Births.	No. of Deaths from Metria.	Proportion of Deaths from Metria to Births.
1860	42,158	116	1 in 363
1861	44,319	94	1 in 471
1862	44,054	59	1 in 746
1863	45,783	78	1 in 587
1864	47,321	114	1 in 415

This table affords a sufficient answer to the question raised. It shows that, out of hospitals, there is a regularly and constantly recurring quantum of mortality from puerperal fever or metria. Nothing more is required to show that, in this respect, hospital and private practice are alike, and that the assertions of Dr. Kennedy are erroneous. The table might have easily been extended, and with the same result, to the mainland-rural and to the insular districts of Scotland. No doubt the result is a truth comprehending the whole world.*

I shall now proceed a step farther, and try to

* I say nothing here as to the value of this table as an index of the actual amount of metria occurring in different years in the large towns of Scotland. To aid in forming a judgment on this point, I refer the reader to remarks already made at p. 290. To contrast with this town mortality from metria (*quantum valeat*), I give the following data from the *Dublin Quarterly*, p. 250 :—"The

show whether or not hospitals and private practice are alike in the fraction of their gross mortality, or mortality from all causes, after parturition attributable to metria alone. The deaths in connection with parturition have been by many divided into three classes:—*First,* deaths from childbirth; *second,* deaths from puerperal fever or metria—that is, from causes connected with parturition other than are included under the head of childbirth deaths; *third,* deaths during or after parturition from causes quite unconnected, or supposed to be quite unconnected, with childbirth. The question now raised concerns the second of these categories. It is—Are hospitals, in contrast with private practice, remarkable for the high proportional share of metria in producing the gross mortality after parturition?

This is a very important question; for the answer given to it in different cases will form a very good criterion of the value of hospital or of private practice. Were puerperal fever well defined, it might form the best criterion; for an hospital cannot, of course, be blamed for its childbirth deaths, or mortality in the first category—that is, they are not the result of hospital arrangements, whether salubrious or not. As Messrs. Bristowe and Holmes say of deaths generally, and with truth, so far as a fairly-arranged hospital is concerned, it may be said with certainty of childbirth

death-rate from puerperal fever given by Dr. Collins is 1 in 297; by Dr. M'Clintock, 1 in 214; by Dr. Johnston, 1 in 289; and by Dr. E. Kennedy, 1 in 112." I do not enter on the consideration of these actual values, as I wish only to consider comparative values.

deaths, that the more of them the more is the efficiency and utility of the hospital demonstrated. But it is otherwise with metria alone, regarded as a cause of death. It will increase with the gravity of the child-birth cases; but at the same time its amount will no doubt rise and fall with the salubrity of the same or of different hospitals.

To show the proportion of metria deaths in hospital and in private practice comparatively to the total deaths, I have constructed the following table :—

TABLE CXIX.

Nature of Data.*	Total Mortality.	Metria Mortality.	Proportion of Metria to total Mortality.
Towns of Scotland, 1860 to 1864 inclusive	1254	461	1 in 2·7†
Private practice (M'Clintock)	131	52	1 in 2·5
Private practice (Beatty) .	30	17	1 in 1·8
Coombe Hospital, Dublin .	89	55	1 in 1·6
Rotunda, Dublin (Collins) .	164	56	1 in 3
Ditto (M'Clintock and Hardy)	65	31	1 in 2
Ditto (Johnston and Sinclair)	67	41	1 in 1·6
Ditto (E. Kennedy) . . .	224	171	1 in 1·3
Hospital practice (Beatty) .	17	11	1 in 1·5
Hospital practice (Churchill)	4	2	1 in 2
Montreal Hospital . . .	17	5	1 in 3·4
Waterford Hospital . . .	15	5	1 in 3

* With the exception of those in the first line, all the data in this table are to be found in the report of the discussion in the Obstetrical Society of Dublin. See *Dublin Quarterly Medical Journal*, August 1869. For more data, see p. 740 of the *Sixth Report of the Medical Officer of the Privy Council*, 1864.

† Corrected to 1 in 3·6.

In the foregoing table the first line requires alteration to make it justly comparable with the other data. This is effected by adding to the total mortality there given (which is, in that line only, the mortality *of* childbed) the mortality from causes unconnected with childbirth (so as to make it then the mortality *in*, not *of*, childbed, as it is in the subsequent lines). The correction consists in adding to the mortality a third of the sums stated, the omitted deaths *in* (not *of*) childbed being, according to Dr. M'Clintock, a fourth of the total deaths *in* childbed, or a third of the deaths *of* childbed.

Now the table shows no very great amount of difference, in the examples cited, as to the proportion of the total deaths caused by metria in hospital and in private practice respectively.

In private practice, metria destroys 1 in 3·6, 1 in 2·5, 1 in 1·8 of those that die in childbed.

In hospital practice, metria destroys from 1 in 3·4 to 1 in 1·3 of those that die in childbed.

It is thus apparent, so far as the examples cited go, that metria or puerperal fever causes nearly as much havoc in private practice as in hospitals—at least in those that are pretty well arranged and managed.

There can be no doubt that in bad lying-in hospitals the proportion of the mortality due to metria will be far higher than any of the figures I have cited indicate. Bad arrangements will increase the proportionate quantity of metria mortality, whether in hospital or in private practice. The hospitals I have adduced will be

universally admitted to be fair examples of their class, and will not be credited with the possession of a degree of perfection that may not be successfully imitated.

I have thus shown that puerperal mortality is a constant quantity in every kind of obstetric practice, and that it is only to a small degree more prevalent in certain hospitals than in private practice. This greater prevalence may be easily accounted for by reference to the general degradation, to the comparatively great number of the seduced, and to other unfavourable conditions, among those delivered in hospitals. There is no need to look for any cause of the greater mortality other than is active in rural or cottage practice.

In a previous chapter, on the Mortality of Childbed, I have shown that there is not demonstrated so great a difference between the mortalities of childbed in fairly managed hospitals and in private practice as has been often, indeed generally, alleged; and I have expressed my opinion that the difference is easily accounted for, without attaching to hospitals and to their benevolent supporters those fearful charges of wholesale murder which recent writings imply. To this one might retort that I have here proved that metria—which, I admit, is fostered by bad arrangements—is a cause of more deaths in hospital than in private practice. While I admit that this is an apparent result of my demonstration, I must, in conclusion, show that is not a real or justly reached result.

The difference between hospital and private practice in respect of the proportion of deaths attributable to

metria is shown to be not great. The difference in favour of private practice is observed only in the towns of Scotland collectively. To show now the untenable character of the apparent result referred to, I shall merely point out that the towns of Scotland include a vast mass of happy, healthy, and comfortable women; while the hospital population consists of a vast mass of degraded, wretched, and often unhealthy women. To make a just comparison between hospitals and the towns of Scotland, we ought to place in contrast with hospitals the data regarding that class of the poor who are like those that enter hospitals. Looking at the figures in the table, and also considering the results of the private practice of the justly eminent individuals there cited, one can scarcely doubt that, were they properly tried, hospitals would be at least not far from equal to private practice so far as regards lowness of death-rate.

I can find no ground for the awful suspicion that well-managed hospitals have caused a large, unnecessary, or avoidable mortality, or developed diseases previously unheard of.

CHAPTER VIII.

ON AGGREGATION AS A SOURCE OF DANGER TO LYING-IN WOMEN.

It has been of late frequently asserted that the mere congregation of human beings in towns is the cause of much shortening or loss of life, and that this loss of life or danger to life is greater as the density of the population is increased. It has also been affirmed, and if the foregoing assertion be true, it follows as a matter of course, that the mere aggregation of human beings in one building is a source of danger to their lives. This latter danger, again, will of course be increased if the human beings are sick, and also be in proportion to the degree of their crowding, or to the smallness of space occupied.

These allegations have, one or other, the support of such authorities as Stark* and Simpson,† of whom the latter leans upon Farr, Duncan, and Gairdner, but I do not inquire whether these latter gentlemen are justly used as props or not.

In regard to all of these assertions, I unhesitatingly express my assurance that they are without any sufficient foundation. They form a climax, rising from rural districts to towns, from open to crowded parts of

* *Edinburgh Medical Journal*, 1869, pp. 481, etc.

† *Lancet*, Nov. 1869, p. 700.

towns, from towns to buildings, from buildings full of healthy to buildings full of sick human beings. If any of these assertions be true, or capable of proof, it should be that regarding the mortality of hospitals or buildings full of sick. This forms the head and front, the climax, of the alleged offence against good sanitation. In this chapter I consider this point, and I leave the reader who peruses it to judge whether or not the assertion is proved in regard to the kind of building and the individual building best adapted to test it.

There can be no doubt that, as you leave the rural districts and pass through different degrees of aggregation on to overcrowding in an hospital, you have increasing and new sources of insalubrity. But these causes of insalubrity may be counteracted. They are not inevitable nor invincible. The highest authorities maintain, that a barrack may be made as salubrious as any residence. So may an hospital, for aught I can see. Authors have yet to prove that residence in well-arranged towns and hospitals is insalubrious. They have yet to prove that mere aggregation is, *per se*, pernicious in its influence. If such be the case, then no measures can avert the evils of town or of hospital residence. For my part, I believe, that even now we know how to make a town or an hospital as healthy as any other place, if the condition of overcrowding is prevented.

Authors who rashly make assertions such as those just given from Stark and Simpson, appear to me to rely on very insufficient evidence, to have a great

power of closing the eye against most pertinent facts, or to have grievously neglected to show their readers how they evade the evidence of such facts. For instance, when they assert that towns are more unhealthy than rural districts, they are satisfied with giving us the mortality in the two kinds of places as evidence of their comparative healthiness. But it is plain, that such facts are mere statements of mortality, not evidence as to salubrity or insalubrity. Are the shambles insalubrious because every bullock there dies? Were the rural districts around Metz insalubrious in 1870 because there was a great mortality there? Is no distinction to be made between the insalubrity of Sheffield and the insalubrity of steel-grinding? Is the ploughman or field-labourer in the country more or less healthy than the artisan or labourer in town employed in an equally healthy occupation? I shall go no further in rebutting the assertions of Stark and Simpson: first, because there is probably some little truth in their views; second, because the subject is foreign to my purpose; and third, because I have said enough to show where their arguments are utterly insecure.

Again it is asserted that the greater the aggregation the greater the insalubrity. Curiously enough, those who make this statement forget the gigantic fact of London—the largest aggregation of human beings that perhaps ever existed; yet a town often said to be the healthiest in the kingdom, and which certainly has a smaller annual mortality than most large towns, and than many villages.

I have no doubt that, even now, towns are built in which the mortality is as small as in rural districts; barracks in which the mortality is as small as in towns; hospitals in which the mortality is as small as in the homes of the poor. I wish I could also say, that such is generally, or more than exceptionally, the case.

I propose now to inquire what information can be derived from the statistics of the Lying-in Hospital of Dublin, as to the influence of aggregation of lying-in women on their mortality. In the records of that institution there have been accumulating, for above 100 years, facts bearing upon this point. These facts are embodied in a table, showing the number annually delivered, and the corresponding yearly mortality. This table may be got in various places. I shall use that which is to be found at p. 30 of Dr. Evory Kennedy's little book, entitled *Hospitalism and Zymotic Diseases*, etc.

I suppose no one doubts that overcrowding the wards of a maternity, or of any other hospital, is one of the most certain causes of danger and death to the inmates. Every one has heard of the Black Hole of Calcutta, or of the Hôtel Dieu as a maternity in the last century. On this great subject I do not now propose to say a single word. I mean to show what information can be derived, with certainty, from the records of the Dublin Hospital, as to the influence on mortality resulting from the bringing together of different numbers of lying-in women. In other words, does the experience of the Dublin Hospital show that

there has been, in bringing together for lying-in a small number, greater mortality than in bringing together a large number for the same purpose ?

This inquiry will be valuable with a view to throwing light on important discussions regarding hospitals. The information derived will, for reasons already given elsewhere, be more reliable for this purpose than that obtained by studying corresponding amputation statistics.* Further, the information derived will be, in my opinion, valuable, so far as it goes, to a degree which no other statistical information can approach, because of the long time and the large numbers involved in this great hospital's experience—above 190,000 cases in 113 years.

These data have, as might be expected, been already used by students of the influence of aggregation. Especially, Dr. Evory Kennedy has availed himself of them; and, trusting to them almost alone, he tries to establish the proposition, "that the generation and absorption of this contagion (of puerperal metria) is in a direct proportion to the number of parturient females cohabiting in a given number of feet of atmospheric space at their parturient period, or who breathe the same atmosphere when lying-in." † So important does Dr. Kennedy consider this proposition, that he calls it his Redan proposition; and it truly is so. If it is secure, his cause is gained, and *vice versâ*.

Dr. Kennedy has made no proper analysis of the

* See page viii. of preface to my work entitled *Mortality of Childbed*, etc.

† *Hospitalism*, etc., p. 25.

table on which he founds his proposition, but trusts to what he calls a bird's-eye view of it, and to a running commentary on its figures.* Both of these processes of taking the evidence derivable from the table are, to me at least, quite novel. To them Dr. Grimshaw adds another "upon the system of curvilinear test."† All three methods prove nothing whatever—show nothing towards proof of the proposition. They are not calculated, as they stand, to show anything. There is only one way of extracting the evidence from the table, and Dr. Kennedy has not tried it. Dr. Kennedy presents nothing in the way of proof of his proposition. He makes many statements bearing upon it, which different physicians will value differently, but he gives no positive proof.

Criticising Dr. Kennedy's grand proposition, Dr. Denham, Dr. Atthill, and Dr. M'Clintock‡ show, that so far from the data lending it any support, they at least appear to be hostile to it. While Dr. Kennedy gives no statistical evidence in proof of his proposition, they adduce abundant evidence to show that it is in the highest degree improbable. For instance, Dr. Atthill produces the following

TABLE CXX.

Years.	Deliveries.	Deaths.	Rates.
1800 to 1804 inclusive	8,990	134	1 in 67
1805 to 1809 „	12,691	81	1 in 157
1864 to 1868 „	5,758	175	1 in 31

* *Hospitalism*, p. 118. † *Ibid.* p. 123.

‡ *Dublin Quarterly Journal of Medical Science*, August 1869, pp. 276, 262, and 232.

The mortality in these periods of the hospital's history is in a sort of inverse proportion to the deliveries. This table does not disprove Dr. Kennedy's proposition. It renders it highly probable that Kennedy's proposition is opposed to truth. In favour of his own proposition, Dr. Kennedy adduces no evidence nearly so strong as this of Dr. Atthill against it.

But the Dublin Hospital data can, without difficulty or strain, be made to yield direct and exact evidence on the influence of aggregation upon its inmates; evidence, as already said, of the highest value, from the unity and other characters of delivery, from the length of the hospital's existence, and the consequent greatness of its figures. It is easy to avoid a misleading bird's-eye view, or a dangerous selection of special strongly-marked years that may favour a preconceived notion. The whole data can easily be interrogated, and the answer taken for every degree of aggregation. That answer will settle the question, so far as these invaluable hospital statistics can settle it, and the result will be certain and irrefragable. To obtain the solution of this question, then, I have arranged and tabulated the data as under :—

TABLE CXXI.—Showing Rate of Mortality per Thousand amongst the Inmates of the Dublin Lying-in Hospital for the Years from 1757 to 1868 inclusive, arranged according to the Number of Hundreds of Inmates.

Women delivered Annually.	Rate of Mortality per Thousand.			Number of Years or Observations.	Mean Age of Hospital.
	Lowest.	Average.	Highest.		
Less than 100 . .	18·1	18·1	18·1	1	1
400 and under 500	12·3	16·1	18·4	3	4
500 „ 600	7·2	13·5	20·4	5	6
600 „ 700	4·4	15·8	30·8	8	14
700 „ 800	5·7	6·3	6·9	2	17
800 „ 900	8·4	29·8	72·5	3	49
900 „ 1000	5·4	7·4	10·8	3	24
1000 „ 1100	5·8	25·0	38·2	6	78
1100 „ 1200	12·8	30·4	51·9	4	88
1200 „ 1300	6·2	13·7	26·1	3	55
1300 „ 1400	5·9	12·9	22·5	5	63
1400 „ 1500	15·7	19·1	24·8	4	64
1500 „ 1600	4·7	11·8	21·9	6	56
1600 „ 1700	4·9	9·7	15·6	5	51
1700 „ 1800	7·6	15·8	27·6	4	53
1800 „ 1900	13·1	17·4	19·9	3	84
1900 „ 2000	5·6	11·6	19·0	8	80
2000 „ 2100	6·8	13·9	21·7	6	81
2100 „ 2200	5·5	10·6	21·2	7	81
2200 „ 2300	5·2	5·3	5·4	3	66
2300 „ 2400	—	—	—	—	—
2400 „ 2500	8·2	20·9	33·2	5	62
2500 „ 2600	4·8	20·0	22·8	5	60
2600 „ 2700	4·1	4·5	4·9	2	59
2700 „ 2800	9·5	12·5	15·6	2	62
2800 „ 2900	7·3	11·0	15·1	4	61
2900 „ 3000	—	—	—	—	—
3000 „ 3100	5·5	5·5	5·5	1	59
3100 „ 3200	29·4	29·4	29·4	1	63
3200 „ 3300	5·5	5·5	5·5	1	60
3300 „ 3400	—	—	—	—	—
3400 „ 3500	9·2	9·2	9·2	1	61
3500 „ 3600	15·8	15·8	15·8	1	62

Any one can read off for himself the results of this table. It is well worthy of study; and to facilitate the observation of its general conclusions, I give another condensed view of its facts.

TABLE CXXII.—SHOWING THE RATE OF MORTALITY PER THOUSAND AMONGST THE INMATES OF THE DUBLIN LYING-IN HOSPITAL, ARRANGED ACCORDING TO THE NUMBERS OF INMATES.

Women delivered Annually.	Rate of Mortality per Thousand.			Number of Years or Observations.	Mean Age of Hospital.
	Lowest.	Average.	Highest.		
Less than 800 .	9·5	14·0	18·9	19	8
800 and under 1300	7·7	21·3	39·9	19	59
1300 „ 1800	7·8	13·9	22·5	24	57
1800 „ 2300	7·2	11·8	17·4	27	78
2300 „ 2800	6·6	14·5	19·1	14	61
2800 „ 3300	11·9	12·8	13·9	7	61
3300 and upwards .	12·5	12·5	12·5	2	61

Taking, then, the best data which, so far as I know, the world affords, as our sole, and as, in the meantime, a sufficient guide in this matter, we find that *the mortality of the Dublin Lying-in Hospital does not increase with the increased number of the inmates—does not rise with the aggregation. The mortality of the Dublin Lying-in Hospital is neither in the direct nor in the inverse ratio of the aggregation.*

The figures, indeed, seem to favour the view that the hospital is a better and safer institution the greater the aggregation. Certainly, a smaller proportional

number die when there are many in it than when there are fewer. It is plain that we cannot look to aggregation as an important cause of mortality in the Dublin Hospital. This is a great practical result; for it sets inquiry into other directions to find out the hidden sources of increased mortality. Dr. Kennedy's proposition, above quoted, is not only not proved by the data he refers to—it is proved to be false. The opposite of his proposition is shown to be nearer the truth. With the fall of his Redan proposition, fall all his conclusions regarding puerperal fever and the advantages of small hospitals.

Although the supposed paramount evil influence of aggregation in this great hospital is now disproved, much more requires to be done with a view to discover and avert the causes of so-called metria, which is the chief source of variations in its death-rate.

Holding in view the splendid results secured by the antiseptic system of Professor Lister, the prospects of still further benefits which the study of the system opens up before the eyes of the surgical and obstetrical philosopher, and the corroborative evidence of Saxtorph in favour of its efficiency in preventing pyæmia, we cannot but be sanguine in our expectations, that, as Syme predicted, a ready way of preventing insalubrity of hospital wards may be very soon established.

It is a natural and just idea that this question of the influence of aggregation should not be discussed exclusively on the field of the Dublin Lying-in Hospital, but that the statistics of other great institu-

tions of a like kind should be scrutinised in the same view. To this step there can on my part be only one objection—namely, that I am not well informed as to the conditions of other great maternities, and that ignorance as to the possibly interfering conditions makes an argument from their figures of diminished value. The Dublin Hospital is well known to British accoucheurs, and the great Dublin obstetricians, full of knowledge as they are, have, tacitly at least, agreed to accept its figures as forming a suitable basis for the decision of the question before us.

But, with this preliminary word of caution, I proceed very briefly to state what two other great hospitals show as to the influence of aggregation. The two largest continental maternities are those of Vienna and Paris. I now give, in a tabular form, a view of their rates of mortality, according to degrees of aggregation.

I take Vienna first, and quite arbitrarily I limit myself to the period 1834 to 1863, during which the lying-in women were divided into two cliniques; but I may add that a view of the entire statistics of the hospital, from 1784 onwards,* confirms the conclusion derivable from the more limited view which I here give.

* Spaeth, *Zeitschrift d. k. k. Gesellsch. der Aerzte in Wien*, 1863.

TABLE CXXIII.—Showing the Rate of Mortality per cent among the inmates of the Vienna Lying-in Hospital, arranged according to the Numbers of Inmates. I. Klinik.

Women delivered annually.	Number of Births.	Number of Deaths.	Mortality per cent.
Under 2500	11,309	853	7·5
2500 and under 3000	14,181	1190	8·4
3000 „ 3500	13,059	1397	10·7
3500 „ 4000	22,652	667	2·9
4000 „ 4500	33,925	1199	3·5
4500 „ 5000	9,366	254	2·7

TABLE CXXIV. II. Klinik.

Women delivered annually.	Number of Births.	Number of Deaths.	Mortality per cent.
Under 2000 . . .	10,666	668	6·3
2000 and under 2500	6,784	343	5·0
2500 „ 3000	8,303	404	4·8
3000 „ 3500	39,236	1189	3·0
3500 „ 4000	14,673	352	2·4
4000 „ 4500	8,421	108	1·2

I now extract from the valuable work of Le Fort * the necessary figures regarding the Maternity of Paris.

* *Des Maternités*, p. 24.

TABLE CXXV.—Showing the Rate of Mortality per cent among the Inmates of the Paris Maternity, according to their comparative Numbers at different times.

Time.	Deliveries.	Deaths.	Mortality per cent.
1802 to 1809	15,307	610	3·9
1810 „ 1819	23,484	1114	4·7
1820 „ 1829	25,895	1293	4·9
1830 „ 1839	26,538	1125	4·2
1840 „ 1849	34,776	1458	4·1
1850 „ 1859	25,094	1298	5·1
1860 „ 1864	9,886	1226	12·4

The evidence afforded by these last three tables, CXXIII. CXXIV. CXXV., is abundantly confirmatory of that afforded by the Dublin data, and must convince the most obdurate that hospital statistics, the best resource for evidence, lend no support to the doctrine that aggregation is a great (or, according to some, the greatest) source of danger to lying-in women.

The opinion of Le Fort is well given in the following extract:*—"L'encombrement agit de deux façons: en développant primitivement la fièvre puerpérale, en facilitant outre mesure la contagion. Mais, je ne saurais trop le répéter, la contagion de la fièvre puerpérale est la cause principale de la mortalité excessive des femmes en couches, et si les appels incessants de la science ont pu faire diminuer l'encombrement et ses funestes effets, rien de sérieux n'a été fait à Paris

* *Des Maternités*, p. 80.

contre la contagion. L'encombrement a presque disparu, la contagion persiste, à laquelle de ces deux causes faut-il attribuer la plus large part dans la persistance d'une effroyable mortalité ?"

Hervieux, a recent estimable author, substantially admitting that he has no evidence for his belief, or rather that the evidence is against him, yet clings to the doctrine of the necessarily baneful influence of mere aggregation.* His statements are a repetition of the erroneous arguments of Kennedy on the Dublin statistics. He fancies that by ingeniously manipulating the figures he can make them support his doctrine, but, indeed, such attempts show only that he wishes to compel evidence, not to elicit truth.

The influence of aggregation, if it can be discovered and demonstrated, must be so on some other plan than that adopted by Kennedy and Hervieux. My efforts only go the length of showing that it has not yet been discovered.

* *Maladies Puerpérales*, p. 64.

PART VIII.

ON THE AGE OF NUBILITY.

In this discussion I have nothing to say that is applicable to individual women. Such may, by peculiarities of constitution, be to a greater or less extent removed from subjection to the laws which govern the sex generally. Individuals may, with propriety, be advised to marry earlier than general laws would sanction, or later. My object is to point out the ages within which women generally should enter the married state, if they are to be guided by physiological laws. I shall hold it to be the object of marriage, as it is its natural result, to "multiply and replenish the earth." And I shall omit entirely from consideration at present those moral considerations bearing on this topic, which may, in an important sense, be justly included under the designation of physiological laws.

It is, I believe, a common notion that the occurrence of menstruation indicates the arrival of the nubile age. Authors occasionally use such expressions as advent of nubility and commencement of menstruation as synonymous. The age of puberty may be contemporaneous with the age of nubility; but it cannot be assumed to be so without proof, for very little reflection will suggest to the physiologist many

reasons for supposing that the marriageable age is generally delayed for several years after the arrival of the age of puberty. And it is my object now to show at what age it is wisest for women to enter the married state.

Speaking of nubility, MM. Dubois and Pajot* make the following remarks:—"When this aptitude is developed, and when the genital functions may be exercised in woman, without probable prejudice to herself or her offspring, she is considered as nubile.

"Thus understood, nubility not only implies the abstract faculty of procreating, but the possibility of a procreation not injurious to the mother or to the infant.

"Nubility results from the necessary accomplishment of two orders of modifications: the one is local and is produced in the organs of generation, the other is general and affects the whole economy.

"The manifestation of the first, which is also the more important, has for its consequence puberty, which it is necessary to take care not to confound, as several authors have done, with the nubility of which it is only one of the essential conditions.

"I wish thus to give their true meaning to two expressions, too often employed as if they had the same signification.† The word nubility implies the idea of

* *Traité Complet des Accouch.*, tome premier, p. 269.

† "Nubility. This word is synonymous with puberty in the female sex." Nysten, *Dictionnaire de Médecine*, 9e edition, 1845.

"As in boys, nubility or puberty is announced in girls by numerous changes." Velpeau, *Traité d'Accouchement*, 2e edition, p. 115.

an aptitude, puberty implies that of a particular condition which favours or renders possible the exercise of this aptitude. A girl to be nubile must first have puberty. But when she has puberty, it does not follow that she is nubile, because puberty is not the only condition necessary for nubility. The ancients made no confusion in this matter. *Puberty*, from *pubes*, hair, indicated the age when certain parts began to be covered with hair. *Nubility*, from *nubes*, a cloud, veil, and from its derivative *nubilis*, indicated the age when the young girl was in a state to wear the nuptial veil, that is to say, to be married."

M. Joulin* makes some remarks which appear to me so just as to demand quotation here: "Nubility," says he, "is the complement of puberty. These two states should not be confounded; it is rarely that they are developed simultaneously, and their appearance is ordinarily separated by an interval of several years. Puberty is the age when the young girl becomes a woman; nubility is the epoch when she may fulfil all the duties of maternity. It is frequent in our latitude to see the menses appear at eleven or twelve years; parturition will be, strictly speaking, possible, but after the young mother has escaped the dangers of a labour very painful to her, will she be able to nurse her child, to lavish on it the necessary attentions? will she be able to comprehend the whole extent of her task, and to fulfil its laborious duties? No. For nubility is not arrived, and it is only then that the complete develop-

* *Traité Complet d'Accouch.*, i. partie, p. 105.

ment of the organs, the powers, and the intelligence, will permit her to be sufficient for the undertaking. The civil code authorises matrimonial union when the woman is fifteen years and the man eighteen; but civil law is not in accordance with physiological law, and the race which would spring from unions so premature would soon proceed to degenerate. No general limit should be fixed, as is done for nubility; were this term 18, 20, or 22 years, numerous deviations from the common level would occur.

"When I am consulted," he adds, "as to the opportuneness of a marriage for subjects who are too young, I am accustomed to respond to the parents, that they should not marry their daughter—that is to say, expose her to the chance of becoming a mother—until for a year at least her stature has ceased to increase. This is the epoch that I fix for nubility; embonpoint may add to the volume of the organs, but nature will add nothing to their development."

It is generally supposed, especially by cattle-breeders, and I believe justly, that incomplete development of the body indicates a certain unfitness for bearing young; the too early performance of the function having an injurious influence upon the young mother, and resulting in offspring that is not generally excellent. In women the question of development is susceptible of more intimate examination than in any animal. For we have, recorded, careful inquiries into the growth of her physical frame that can be turned to account. I have already indicated that I do not

here enter upon the important considerations of moral development in the female; and in physical development I shall only condescend on the stature, the ossification of the pelvis, its development in shape, and the development of the genital organs.

There is a deficiency of minute observations on the development of the essential or internal genital organs of the female before and after the age of puberty, so that we cannot well judge as to their incompleteness when puberty arrives. But the measurements of Arnold show so great a difference in one of the internal organs, namely, the uterus, before and after 20 years of age, as to leave no doubt that it is not fully grown when menstruation commences. The following table is extracted from his work on anatomy.*

TABLE CXXVI.—SHOWING THE DIMENSIONS OF THE WOMB BEFORE AND AFTER THE TWENTIETH YEAR OF AGE.

PARTS.	GIRLS.	
	Before 20th Year.	After 20th Year.
Length of whole uterus . .	2″	2½″–3″
„ neck	1″ 3‴	1″ 6
Breadth of fundus	1″ 3‴	1″ 6‴–9‴
„ at beginning of neck .	8‴	10‴
„ vaginal portion . .	8‴	10‴
„ of cavity at fundus .	9‴	1″ 2‴
„ at internal os uteri .	3‴	3‴
„ at external „ . .	½‴–1‴	1½‴–2‴
Thickness at fundus	5‴–6‴	6‴–10‴
„ at upper part of neck	4‴–5‴	6‴–8‴
„ at vaginal portion .	4‴	6‴

* *Handbuch der Anatomie des Menschen,* II. Band. 1 Abth. s. 295.

On this subject Kussmaul* has the following remarks :—

"Wenn die Geschlechtsreife eintritt, so wächst die Gebärmutter rasch und bedeutend. Doch geschieht die volle Ausbildung nicht mit einem Male, oder in Jahresfrist, sondern allmälig, und selbst wenn die Menstruation eingetreten ist, ja noch nach dem zwanzigsten Lebensjahre, wächst der Uterus sehr ansehnlich, wie namentlich Arnold durch Messungen nachgewiesen hat. Immerhin aber erfolgen die wesentlichsten Umwandlungen und die grösste Zunahme in Umfang und Wachsthum vor dem zwanzigsten Jahre."

It has also attracted the attention of Klob,† who says :—

"During the development of puberty, the uterus increases considerably in size. Arnold has shown that this increase continues even beyond the twentieth year; and of such importance is this circumstance, that it should be taken into consideration by those about to marry, more than it usually is."

"The full growth of man," says Quetelet, "does not appear to be attained at his twenty-fifth year."‡ Elsewhere the same author remarks that "the limits of growth in the two sexes are unequal: first, because woman is born smaller than man; second, because

* *Von dem Mangel, etc., der Gebärmutter*, s. 15.

† *Pathological Anatomy of the Female Sexual Organs.* Transl. by Kammerer and Dawson, p. 5. See also Farre, *Cyclopædia of Anatomy and Physiology.* Art. Uterus, vol. v., suppl. p. 624.

‡ *A Treatise on Man*, p. 61.

she sooner finishes her complete development; third, because the annual increase which she receives is smaller than that of man." These conclusions of Quetelet regarding the stature of woman are founded on a large collection of data of different kinds, and may be accepted as proof that women generally are increasing in stature till at least about their twenty-fifth year of age, that till this age they are immature inasmuch as they are not full-grown in height.

The tardy ossification of the bones of the pelvis naturally attracts attention from their locality and inseparable connection with the function of reproduction. With the full details of the osseous growth and perfectioning of the pelvic bones I shall not encumber this discussion. I shall merely refer the inquirer to anatomical works on the subject, and here make two quotations* from the article "Pelvis," by John Wood, published in the *Cyclopædia of Anatomy and Physiology*:—"About the time of puberty, as first pointed out by M. Serres, a distinct complementary point of ossification appears in the cartilage dividing the bones in the cotyloid cavity. . . . . According to Meckel, the pubes and ischium join first with each other, and the ilium becomes united to them afterwards. At the same time appear the four remaining complementary points as epiphyses. . . . All these are soldered to the bone about the twenty-fourth or twenty-fifth year, the epiphysis of the iliac crest being the last

* Vol. v. p. 120.

to join." Speaking of the sacrum, the same author remarks:—"At the age of sixteen years, the epiphysial or complementary ossific points begin to form—viz. on each articulating surface of the bodies of the sacral vertebræ is developed, as in the true vertebræ, a horizontal plate of bone, which, after coalescing with the bodies to which they respectively belong, finally (except the first and last) become soldered to each other from below upwards, commencing with the two last vertebræ, at from the sixteenth to the eighteenth years, and completing the formation of the sacral bone by the union of the two first vertebræ, at from the twenty-fifth to the thirtieth years. Between the eighteenth and the twentieth years begins the formation, by scattered granules, of four lateral plates of bone—one on each side, forming the iliac articular surfaces, opposite to the three first vertebræ—and one on each side, opposite the two last. These unite with the sacral bone about the same time that its upper vertebræ coalesce." *

Having thus shown the lateness of the completion of the structural development of the pelvis as a bony skeleton, I advance to the still more important topic of the time of the complete construction in shape or form of the same part. It would be tedious and out of place here to show the obstetrical advantage and use of the special shape of the fully-grown female pelvis. I assume this.

"According to Dupuytren," says Mr. Wood, in the

* See also Litzmann, *Die Formen des Beckens*, s. 16.

article from which I have just been quoting, "the female pelvis differs very little from that of the male till puberty, at which period it has a general triangular form in both sexes, but after that period it becomes rapidly developed, and soon assumes its distinctive sexual character. The transverse diameters begin to exceed the conjugate, and, in the female, attain a great preponderance, constituting one of the great characteristics of the fully-formed human pelvis, as distinguished from that of the lower animals."

Burns offers us statements which are more apposite, and in distinct words gives them a bearing upon the question of nubility. The grand feature of his remarks, for our present purpose, is his proving that the female pelvis just before puberty, and perhaps so late as eighteen, is far from having assumed the form best suited for the difficulties implied in commencing maternity. "The shape," he says,* "is different in the child and the adult. The dimensions of the brim are reversed in these two states; the long diameter of the fœtal pelvis extending from the pubis to the sacrum. By slow degrees the shape changes. These changes, however, must be affected by the general growth of the body and the term of puberty. At nine years the conjugate diameter is two inches and seven-eighths, the lateral an eighth less; at ten years of age the antero-posterior diameter is three inches and a quarter, the lateral is an eighth more; at thirteen the former is still the same, but the latter has increased to three inches and three-

* *Principles of Midwifery*, tenth edition, p. 23.

quarters; at fourteen the former is three and three-quarters, the latter four inches. Just before puberty, perhaps so late as eighteen, the antero-posterior diameter is three inches and seven-eighths, the lateral four and a half. These measurements I give, however, from individual pelvises. If a girl should very early become a mother, the shape of the pelvis may occasion a painful and tedious labour." "To the female sex," says Aristotle, "premature wedlock is peculiarly dangerous, since, in consequence of anticipating the demands of nature, many of them suffer greatly in childbirth, and many of them die."* Litzmann† has gone into this question with greater fulness than Burns, but I shall only extract from his work the observation that he has given the dimensions of two pelves of young women of nineteen years of age, and that in both the measurements distinctly indicate that they have not yet arrived at the average size and shape. They therefore confirm the statements of Burns on this point in every respect.

It is known that a first confinement is much more dangerous than any of those which follow, at least until the confinement reaches a number above that ordinarily attained to by fertile women. I have elsewhere shown that this extraordinary mortality accompanying first labours is about twice that accompanying all subsequent labours taken together.‡ It evidently, then, becomes of extreme importance for the young woman

* *De Repub.* l. vii. c. 16. See Sadler, *Law of Population*, vol. ii. p. 272. † *Die Formen des Beckens.* ‡ P. 304.

entering on the risk of a first confinement to do so at the most favourable age. The age of smallest mortality after a first confinement should be chosen for encountering its risks. It has been long known that age has considerable influence on this mortality. But I know of no satisfactory data for deciding at what age a woman most safely bears a first child. In another place,* I have entered upon the subject, and shown that the quinquenniad 20 to 24 years inclusive is the safest for parturition generally, and I think it a natural inference that that age is the safest for a first parturition; an inference, too, which appears to me to have the support of the general tenor of the argument to which I have made reference. If a woman is to multiply and replenish the earth, as married women ordinarily do, she must survive her first confinement. To have the best chance of this survival she should marry between 20 and 25 years of age.

There is scarcely any condition of a married woman which more surely causes unhappiness than sterility; its avoidance is therefore a great object. If a married woman is sterile, she fails to secure the great end of the union. It is evident that the age of nubility should be fixed with a view to the securing of fecundity. I have elsewhere† shown that age at marriage has considerable influence upon the occurrence of sterility; and the age at marriage found to be most secure of

* Table CXVII. p. 348.

† *Transactions of Royal Society of Edinburgh for* 1866. "On some Laws of the Sterility of Wives," chap. iv. See also p. 199.

fecundity is the quinqenniad 20 to 24 years inclusive. So far, then, as the avoidance of sterility has any bearing upon the age of nubility, the quinquenniad 20 to 24 years inclusive is to be selected.

"Premature conjunctions," says Aristotle, "produce imperfect offspring, females rather than males, and these feeble in make and short in stature. That this happens in the human race as well as in other animals, is visible in the puny inhabitants of countries where early marriages prevail."* These opinions of Aristotle are confirmed, so far as stature is concerned, by my own researches and those of Professor Hecker, regarding the length and weight of children born of mothers of different ages.† And the statistics of Dr. A. Mitchell seem to show that immature mothers and old mothers are specially liable to bear idiot children.‡

If, in the foregoing paragraph, it is established or rendered probable that the children of very early marriages are less strong and healthy than other children, it may be considered a work of supererogation to show that such children die in a higher proportion in early life than others. But the demonstration of both of these points is not perfect, and the proof of the one goes far to confirm the other, and is therefore demanded.

* *De Repub.* c. iv. p. 246, Gillies' translation. See Sadler, *l. c.* p. 273.

† *Edinburgh Medical Journal*, December 1864. See also p. 64.

‡ *Ibid.* January 1866. See also p. 392. See also some remarks by Druitt, *Medical Times and Gazette*, October 24, 1868, p. 474.

Sadler, in his work on the *Law of Population*, enters upon the mortality of children as influenced by the age of the mother at marriage. His enthusiasm for a preconceived theory diminishes the value of his remarks on the point; and the circumstance, that his data do not give the results of marriages after 32 years of age of the woman, renders the whole of less value than a more extended series of data and calculations would possess.* So far as they go, however, they show a diminishing mortality among the children in proportion as the age at marriage increases.

A more valuable collection of data is to be found in the report of an investigation into the state of the poorer classes of St. George's-in-the-East.† The following table, extracted from that document, gives the foundation of the conclusions arrived at, which I now

TABLE CXXVII.

Years elapsed since Birth of first Child.	Mortality per cent of the Children born to Marriages formed at Ages			
	16–20	21–25	25–30	31–35
10	36·87	37·09	37·89	35·48
20	47·44	43·10	44·36	16·67
30	53·03	43·89	48·53	64·29
40	63·12	57·14	68·00	50·00

give in the words of the report. "From this abstract it is obvious that, of the three first periods, the children

* *Law of Population*, vol. ii. Tables xliv. and xlvi.

† *Journal of Statistical Society*, xi. p. 223.

born of marriages formed in the quinquennial term of life 21-25, are subject to a less rate of mortality than those of the period immediately preceding or immediately following; the rate of mortality in the most advanced period, 31-35, is very irregular, and no doubt arises from the very small number of families included in that group."

This interesting table, then, of the report cited, shows a greater survival of children born of women married at from 20 to 25 years of age than at any other; and as the rearing of children is assumed to be one of the chief objects of marriage, the age to be selected for marriage with a view to this object is 20 to 25 years.

Before leaving this point I must add the evidence of two gentlemen skilled in the breeding of lambs and of calves. They say that the mortality of the young of these animals, when the mothers are immature, is much greater than when they are well-grown. One of them says—"Taking the first lamb from ewes at one year old has in almost every case failed to be remunerative, owing to the frequent deaths of the lambs. The same may be said of young heifers, though the mortality of the offspring may not be so marked as in that of sheep."

Considering the argument drawn in this chapter from the avoidance of sterility, it may appear to some to be unfair to found any argument upon the avoidance of an excessive family. And I admit that what I have to adduce on this subject may partake in some degree of

the nature of an arbitrary assumption. Having, however, shown some grounds for believing that ten is the ordinary limit of fertility in women living in wedlock during the whole child-bearing period,* and having shown the very great mortality attendant upon confinements numbering the tenth, or higher,† I venture to express my belief that a family rising above ten begins to be excessive. Now it appears to me that all the knowledge we possess of the laws of fertility refers the excessively numerous families in a population to fertile women who have been prematurely married. Such women certainly go on longer bearing children than any others, counting up to the end of the child-bearing period in the women compared (not up to mere age).

Another class of women is liable to have children with dangerous rapidity, and often a family that is excessive, at least when the duration of married life is taken into account—namely, those who are fertile when married comparatively late in life.‡

* *Transactions of the Royal Society*, 1866, p. 292. See also p. 119.

† *Edinburgh Medical Journal*, September 1865. See also p. 321.

‡ I might have introduced into the text what I here subjoin in a note, had I regarded the opinions suggested in connection with twinning as very well ascertained. If the connection of twin-bearing with primiparity be, for the time, left out of view, there would remain little doubt upon the conclusions now to be suggested: Primiparity, involving, as it does, special danger to mother and to child, as well as to the latter liability to idiocy and other evils, demands study as a peculiar parturition distinguished from those which follow it. I have shown (see page 92) that twins

Child-bearing by an immature mother is popularly held to be dangerous to the continued general health of the mother, and to prevent her complete develop-

increase in frequency as the number of the pregnancy increases: twin-bearing then is, in some sense, an indication of an excessive family. They specially often come to overburden a mother already overwhelmed with progeny. The children of advanced life and of pregnancies of high number Dr. A. Mitchell has shown to be more frequently idiots than other children. Twins (see page 68) are also specially liable to idiocy. The accumulated evils of an excessive family are thus apparent.

As the conclusions of Dr. A. Mitchell have a weighty bearing on this point, I here quote the relative part of his paper from the *Edinburgh Medical Journal*, January 1866:—

"*Comparative frequency of Births of Idiots in First and Subsequent Pregnancies.*

"Among 443 idiots and imbeciles consecutively examined, I found 138 first-born, or 31·1 per cent; and 89 last-born, or 20·1 per cent.

"When it was known, however, that almost every sixth idiot in Scotland was illegitimate (663 idiots and imbeciles giving 108 illegitimate, or 17·1 per cent), it was thought that an element of disturbance was probably thus introduced into the foregoing figures, which might affect their value. The great majority of illegitimate children are known to be *first-born* and *only* children; while not a few of them are *last-born*, though the last of a small number of pregnancies—say of two or three. It was therefore thought desirable that a fresh series of observations should be made, excluding the illegitimate, and dealing only with those born in marriage. It was also thought well to confine these observations to those cases in which not more than one idiot occurred in a family, and in which the idiocy was noticed very soon after birth—that is, in which it was probably congenital. Further, no cases were accepted but those in which the mothers at the time of the inquiry had passed the age of child-bearing, though some of them I think were

ment in size and beauty. I have no positive evidence to adduce in favour of this generally-entertained notion, which my own experience appears to me to

widows before that age was reached. All these restrictions made it difficult to obtain a large series of observations, and account for their number not exceeding 85—44 males and 41 females.

"I sent my results in detail to Dr. Matthews Duncan, who kindly drew up for me the two tables (CXXVIII. and CXXIX.) embodying the facts in a way which makes their teaching apparent.

"TABLE CXXVIII.—SHOWING THE COMPARATIVE FREQUENCY OF BIRTHS OF IDIOTS, AND OF ALL BIRTHS IN FIRST AND SUBSEQUENT PREGNANCIES.

No. of Pregnancy.	Percentage of *all* Births.	Percentage of Idiot Births.
1st	22·8	33·0
2d	17·7	18·8
3d	15·5	17·6
4th	12·1	2·4
5th	9·4	2·4
6th	7·4	2·4
7th	5·2	7·0
8th	3·9	3·5
9th	2·6	2·4
10th	1·3	7·0
11th	·9	3·5

"This table is read in this way:—Of *all* the children born in Edinburgh and Glasgow in 1855, 22·8 per cent were first pregnancies, while of the 85 idiots 33 per cent were first pregnancies, and so on.

"What the table appears to teach is briefly this:—That idiocy is more likely to occur among first and latest (7th to 11th) pregnancies than among others. This is substantially the same thing as was taught by the first inquiry, which included 443 cases, and in which all that was asked was whether the patient was first-born or last-born.

confirm.* In its corroboration, however, I can adduce the ample experience of eminent breeders of the lower animals. I have had this opinion expressed to me, especially in regard to mares, cows, ewes, and bitches. "Experience," says Sussmilch, "shows this in animals; as, for example, among great cattle, the cow which has a calf too young never comes to the size and strength which she otherwise would have done. To this Sadler adds—Of this principle Virgil was fully aware; hence he says,—

'Sed non ulla magis vires industria firmat,
Quam venerem et cœci stimulos avertere amoris,
Sive boüm, sive est cui gratior usus equorum.'

Age of Mother of Idiot at time of Birth of Idiot.

"The same 85 cases are used in the following table which were used in Table CXXVIII. :—

"TABLE CXXIX.—Showing a Comparative Percentage of the Children born at Different Ages of Mothers to all Children born, and of the Idiots born at Different Ages of Mothers to all Idiots born.

Age	20-24	25-29	30-34	35-39	40-44	45-49
P. C. of all Children .	22·62	39·99	23·61	14·76	5·15	0·58
P. C. of Idiots . . .	25·88	25·88	10·58	10·58	23·53	3·53

"This table is read thus:—Of all children born in Edinburgh and Glasgow in 1855, 22·6 per cent were born of mothers whose ages were from 20 to 24 years; while of the 85 idiots 25·8 per cent were born of mothers of corresponding ages; and so on.

"What we learn from the table is this:—That mothers under 24 years of age and above 35 are those more specially liable to have idiocy in their children."

* I wished at this point to make use of a valuable paper by Dr. Tuke of the Fife County Asylum, on Puerperal Insanity; but I find it is not easily adapted to my purpose, though it contains

I might again appeal to the very same principle in the vegetable kingdom; for instance, there is not a horticulturist who is not fully aware that premature fruition is injurious to the growth and future prolificness of all the fructiferous tribes in existence."*

In conclusion, it is almost useless to add that I consider the age of about from 20 to 25 the nubile age of woman. The numerous facts and arguments I have adduced appear to me to bear out distinctly this conclusion. Below 20 years of age woman is immature; she runs considerable risk of proving sterile; and if she does bear a child, she runs a comparatively high risk of dying in childbed; besides her early marriage brings other disadvantages which need not be again enumerated. The woman above 25 years of age is mature, but to counterbalance this she encounters some greater risks than the very young wife, though of a similar nature.†

many details which, especially if further elaborated, may be turned to account on questions like that discussed in this part.

* *Law of Population*, vol. ii. p. 275.

† "Without the sanction (says Major Graham) of the laws of physiology, or of common sense, a girl may—but in the present day rarely does—marry at the age of 12, a boy at the age of 14, under the existing laws of England; but the consent of parents and guardians is required in certain cases when either party has not attained the age of 21; and the proportional number of either boys or girls who marry under the age of 20 is happily small."

"The 26th year (he adds in another place) is the mean age at which men marry, and the 25th year the mean age at which women marry, in England and Wales. About this period of life the growth of man is completed. Half of the husbands and of the wives are married at the years of age 21 and under 25; the higher average is the result of later marriages, which occur in great numbers at the age of 25-30.

"Plato laid it down in his *Republic* that the men should be united about the age 30-55, the women at the age of 20-40. Aristotle, who possessed a greater knowledge of natural history than any author of antiquity, remarks that the young of very old or very young animals are imperfect, and that the children also of very young or very old people are imperfect in mind and body. He asserts, too, that people should, for reasons that he alleges, marry at such ages that when the wife is in her 50th the husband should be near his 70th year, or that men should marry about the age of 37, women about the age of 18. In particular cases, as has been seen, to meet the infinite variety of social circumstances, greater disparities of age than these occur in Great Britain; but the rule of Aristotle, if acted on universally, would work mischievously in various ways. Thirty-three women attain the age of 18 to every twenty-eight men who attain the age of 37; and the women of the age of 18 and upwards are to the men of 37 and upwards as 1402 to 804; so that a disproportionate number of the women would be unmarried. The proportion of widows would be increased, and fathers would less frequently live to see their children attain maturity. The object which Aristotle had in view is obtained by the re-marriage of widowers.

"The age of marriage cannot be directly fixed by laws; but legislation, by prescribing the minimum age of marriage, and the age of majority, does exercise a considerable influence on good numbers of the people directly, and on all indirectly. It becomes the custom or the fashion not to marry below the age of majority. Thus in England about 9000 young persons of the age of 20 and under 21 married in the year 1851; while about 139,000 married in the four years after they were of age, as it is called, or in the years of age 21-25. The age of majority is 25 years in France; and the age of 25 divided the minores from the majores in Roman law. This advanced age of majority, or of what becomes practically the lowest age of marriage, retards marriage indefinitely in many cases, and will probably be found, on investigation, to account, at least partially, for the comparatively small number of children to a marriage in France.

"By raising or depressing the age of majority, the legislature then has the power to exercise considerable control over the population."—*Census of Great Britain*, 1851, vol. i. pp. xxxi. and xlvi.

PART IX.

THE DOCTRINE OF THE DURATION OF LABOUR.

THE progress of obstetrics is not characterised, as is that of some of the more exact sciences, by a secure and gradual advance with unassailable step, always conquering some part of the region of the unknown. Our science, seeking to enlarge the boundaries of what is certain and fixed, makes its conquests from the unknown in a field, wide indeed, and surrounding it on every side, composed, in its nearer parts, of doctrines more or less nearly approaching in stability to those admitted within the true boundaries of the science, but, in its more distant regions, of mere shadowy hypotheses that have not yet acquired any roots, and of ephemeral conjectures, often the offspring of shallowness, of special pleadings, and of vanity.

It is my object to claim a place in the science of Midwifery for the doctrine of the Duration of Labour —a doctrine exceeded in importance by none within the limits of obstetrics, and having the most extensive bearings upon that invaluable art or practice of the accoucheur of which the science is the chief expositor. It and similar doctrines have been deprived of their real features and importance, and hid from general

appreciation, by the violent and not always seemly struggles which have taken place upon them, and which have uniformly ended, like many battles with more sanguinary weapons, either in absence of real result or in the more or less complete discomfiture of all the contending parties. But perhaps the medical philosophers of another age will have wisdom to regard, without pity or shame, these squabbles of our day as necessary episodes in the story of the progress of imperfect beings towards perfect truth—in the progress of human intellects towards real science.

The doctrine of the duration of labour has been the real centre of many discussions which have been invested with other names, derived from some therapeutical principle which has been supposed to receive confirmation or confutation from its bearings upon it. In these discussions the obstetric schools of Edinburgh and of Dublin have more than once been found on opposite sides, as if truth were indicated by different symbols in the two countries. A dispassionate inquirer, perusing these interesting discussions, will not fail to discover that, while each party had much truth as well as error in its arguments, each, with a blind zeal, attacked indiscriminately both truth and error in its opponents.

The chief practical questions which have been investigated in connection with the doctrine of the duration of labour are the artificial dilatation of the os uteri, certain other points in the management of protracted labour, turning as a substitute for cranio-

tomy, and the use of anæsthesia in midwifery. With these questions I shall not at present interfere; only I may cite them occasionally to illustrate and facilitate the development of the great doctrine of the duration of labour, which is now my object. The names of Harvey, Denman, Osborn, Breen, Hamilton, Burns, Murphy, Collins, Beatty, Simpson, Veit, and Busch, will always be honourably associated with the history of this doctrine. If, in the sequel, I do not frequently refer to all these writers, it is not because I lightly appreciate their labours, but because the subject appears to me to have now arrived at a stage at which it may with advantage be as far as possible dissociated from those various questions which have been its parents, but would at present only injuriously encumber it. It is necessary to add that Collins and Simpson are the two obstetricians involved in the latest dispute on this subject. In its various stages much talent was shown, and much truth elicited on both sides. With this last discussion I am best acquainted, and will naturally, therefore, refer to it more than to the views of the other authors distinguished in connection with the subject.

Into the questions we shall have to discuss the use of statistics has been introduced; and it would be difficult to decide whether their application has tended more to elucidate or to confuse. It is evident that accurate statistics can never yield false results; but false results are easily made to appear as if yielded by them. In other words, if a disputant resorts to

statistics, without the most careful use of logic, he easily flatters himself that they really supply the results he wishes from them. Against this fatal seduction into error many beacons have been erected, but they have not produced the safeguard desired by their sanguine authors. The present discussion, like many others in obstetrics, will afford clear examples of this abuse of a means of research which is among the most valuable on points where it is really available. If, in the preceding portions of this book, errors in statistical reasoning are to be found, they are not explained by the temptations of disputation, or by the allurements of a preconceived theory to be supported.

CHAPTER I.

THE DURATION OF LABOUR IN RELATION TO THE MORTALITY OF THE MOTHER IN PARTURITION AND CHILDBED.

In this chapter we have to propound and prove two propositions.

1*st Proposition.*—The mortality of women in parturition and childbed increases with the increasing duration of labour (in an undetermined ratio).

2*d Proposition.*—The duration of labour is only an inconsiderable item among the many causes (single or combined) of the mortality of women in parturition and childbed.

These two propositions have hitherto been either confounded together, or made to conflict with one another. They really stand side by side, declaring separate truths, between which no collision can justly be made to arise.

1*st Proposition—The mortality of women in parturition and childbed increases with the increasing duration of labour* (*in an undetermined ratio*).

This proposition is one which easily gains credence,

when the obstetrician reflects on the abstract nature of it. It is one whose practical bearings are most remote and indefinite. But although this is the case, it enunciates a solid truth, and can never be with justice either neglected or depreciated. The proposition does not affirm anything whatever in regard to the influence of prolongation of labour upon the consequent maternal mortality; nor does it affirm anything whatever as to the dangerousness of the pains of labour. It affirms nothing in regard to any individual case. It merely asserts the general law, that as labours increase in duration, or become protracted, they are also accompanied or followed by a greater maternal mortality.

A proposition such as this scarcely requires proof. As labour becomes protracted, so does life; and we know that every hour of life added in adult age increases the mortality of mankind. But in the human female many dangers accompany the function of childbearing, and combine to raise, for the childbed month at least, the mortality of females very far above what can be accounted for by the mere general law applicable to all mankind. The dangers of childbearing are, for the most part, concentrated into the period of labour, or derive from it their origin. The longer the labour, there will be the more opportunities for such dangers to intervene; and hence it naturally follows, that the mortality of women in parturition and childbed increases with the increasing duration of labour.

But this proposition has been confirmed by nume-

rical investigations. I shall avail myself of Simpson's careful calculations,* made from the data contained in Dr. Collins' admirable report of the Dublin Lying-in Hospital for a like purpose. Dr. Collins has in his report stated the duration of labour in 15,850 cases, of which 138 proved fatal. Table CXXX. exhibits these cases, arranged so as to show that the maternal mortality increases as the duration of the process of labour is augmented. It requires no explanation or commentary.

TABLE CXXX.

Duration of Labour.	Number of Deliveries.	Number of Deaths.	Proportion of Deaths.
Within 1 hour	3537	11	1 in 322
From 2 to 3 hours	6000	26	1 in 231
From 4 to 6 hours	3875	29	1 in 134
From 7 to 12 hours	1672	21	1 in 80
From 13 to 24 hours	502	19	1 in 26
From 25 to 36 hours	134	8	1 in 17
Above 36 hours	130	24	1 in 6

Such, then, is the statement and demonstration of this proposition.

It will be observed that the table of Dr. Collins' data gives us no information as to the special mortality of labours of extremely short duration, finished at various periods less than one hour. It is a very general opinion, and I believe a very correct one, that very rapid labours are, comparatively speaking, in-

* *Provincial Med. and Surg. Journal*, 1848, p. 602.

jurious and dangerous. And more minute investigation as to the relations of very brief labours to maternal mortality, will probably show that there is a limit, at some point within an hour, beneath which, if labours go on diminishing in brevity, they increase in mortality.*

It must also be kept in mind that the peculiar case of primiparous women is included in the data. These have such peculiar conditions and dangers as must manifestly render their admixture with others prejudicial to the value of the data as demonstrating the proposition under consideration. It is desirable to have tables like that cited, composed of labours following pregnancies all of the same number.

It is not my purpose here to trace farther than in a single author the history of this proposition. It has been stated, in terms almost identical with those I have used, by Professor J. Y. Simpson, and confirmed by the table which I have adduced. To, him, therefore, belongs the merit of formally enunciating it.† This we admit, although it would be scarcely a stretch of literary justice to refuse him any credit whatever in connection with it; for it will afterwards appear that he has so misunderstood and so used the principle, and the table

* Hippocrates is quoted by Tarnier (*De la Fièvre Puerpérale*, p. 63) as saying that a sudden and easy labour should be looked on with suspicion, especially if the woman be already sick or languishing. Such labours (he adds) have often the most fatal consequences.

† *Provincial Med. and Surg. Journal*, loc. cit.; *Obstetric Works*, vol. i. p. 527.

on which alone he founds it, that his merit in the matter can be established only by separating the two or three sentences containing the bare principle and table from the mass of writing and argument in which he has enveloped them.

We find this author first using the statistics of Table CXXX. to show that "the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles. In the Dublin Lying-in Hospital (he says), when under Dr. Collins' able care, out of all the women, 7050 in number, who were delivered within a period of two hours from the commencement of labour, twenty-two died, or one in every 320. In 452 of his cases, the labour was prolonged above twenty hours; and of these 452, forty-two died, or one in every eleven—a difference enormous in its amount, and one surely calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution."* Now, it is evident that these statistics afford no ground whatever for such reflections. No doubt, sufferings and struggles are important elements in the history of any labour or set of labours; but nothing in regard to the influence of sufferings and struggles upon the mortality of parturition can be wrested from the statistics adduced. These statistics support only the general proposition (the first) as to the relation of duration to mortality of

* *Monthly Journal of Medical Science*, October 1848; and *Obstetric Works*, vol. ii. p. 689.

labour. This relation is determined by a thousand circumstances, known and unknown, besides sufferings and struggles, in regard to the special baneful influence of which last it affords scarcely the slightest presumption.

When thus using Dr. Collins' data, Sir J. Y. Simpson was simultaneously engaged in his defence of anæsthesia in midwifery. In this cause, searching everywhere for arguments to convince Professor Meigs, he may be to a great extent excused, even when again falling into his former error in the use of these statistics. Addressing his transatlantic friend, and speaking of the pain of labour, he says, "It is safe in proportion to its shortness, and dangerous in proportion to its length. In the Dublin Hospital, the tables of which afford the only data on this point that I know to refer to, when the women were four hours in labour, more subsequently died than when their pain did not exceed two hours; of those that were eight hours in labour, more subsequently died than of those that were four hours ill; of those that were twelve hours in suffering, more died than of those that were eight: and so on, in a regular progression. The longer this supposed salutary and conservative manifestation of life-force (as Dr. Meigs terms it), the greater became the mortality. . . . . etc."* It is not to be wondered at that this argument did not convince Dr. Meigs, since it is as illogical in its use as it is wrong in its essence. What

* *Association Medical Journal*, July 1853, p. 582. *Obstetric Works*, vol. ii. p. 710.

accoucheur could for a moment resist the argument, if true? It is not our object here to discuss the influence of painfulness, or sufferings and struggles, or, in short, of whatever anæsthesia could annul, upon the maternal mortality of labours; we shall only say, that all accoucheurs must recognise it as a great exaggeration, to imply that pain, etc., has any such immense influence as Dr. Meigs is asked to believe. Were it so, then anæsthesia should deprive parturition of its most formidable sources of mortality.

In defending his views with regard to turning in cases of deformed pelvis, we find the same author reverting to the same statistics of Dr. Collins for assistance. Here he supplies evidence against his own former use of these data, or *vice versâ*. For he now interprets them as affording "ample evidence that, contrary to the general opinion of the obstetric profession, the mere length of the labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process."* But again, it will be evident that these statistics afford no ground for attributing the maternal mortality to length or duration of labour as a cause, just as they afforded no ground for attributing the same mortality to the pain, etc., of the process. The proposition, that the increasing length of labour is accompanied by an increasing mortality, is a proposition at once true and proved by the statistics in question; while the pro-

* *Provincial Med. and Surg. Journal*, Feb. 9, 1848, p. 58; and *Obstetric Works*, vol. i. p. 527.

position, that the "mere length of the labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process," is one, to say the least, very questionable, and one to which the statistics afford no countenance. It is not necessary further to point out that, if the statistics so often referred to show that pain, etc., is the cause of the mortality, the same statistics cannot show that the mere duration is the cause of it; and if they prove either of these two points, they cannot be fairly extended so as to demonstrate our first proposition.

Dr. Collins justly objected to Dr. Simpson's uses of his data. The truth that was in them Collins rejected along with the error. A man of practical sagacity and immense experience, he at once repelled Dr. Simpson's erroneous conclusions, from the data in his *Practical Treatise*, in regard to the influence of pain and of length of labour upon maternal mortality. The inward testimony of his experience was so strong as to lead him instantly, and without analysing the statistical reasoning, to denounce these conclusions as visionary and extravagant. The truth of our first proposition he never grappled with. It had no apparent practical bearings; and therefore he refused to consider it.

Dr. Collins might have gone a little farther. It would have been quite a legitimate use of Sir J. Y. Simpson's argument, as to the influence of length of labour upon the maternal mortality, to turn it against the whole practice of anæsthesia in midwifery. For

it is a very general belief that anæsthetics, by diminishing the force of the uterine contractions, increase the duration of labour, at least in many cases. Hence it follows, if Dr. Simpson is right in regard to the baneful influence of mere length of labour, that anæsthesia must tend to increase the maternal mortality. But, as we have shown that the statistics do not demonstrate this baneful influence of mere length of labour, the opponents of anæsthesia are deprived of this otherwise strong argument provided for them by the greatest promoter of the practice.

Before advancing to the second proposition, I shall illustrate the errors fallen into with regard to the first by a reference to a subject long within the recognised domain of statistics.

TABLE CXXXI.

Period of Life.	Proportion of Deaths.
At the age of 20 years	1 in every 141
" " 30 "	1 " 99
" " 40 "	1 " 77
" " 50 "	1 " 74

This one hundred and thirty-first table may be assumed to be a correct statement of the mortality of mankind at different periods of life. An intelligent actuary will at once say, that it proves that the mortality of mankind increases with the increasing duration of life, just as he would recognise our former table as

bearing direct testimony to the truth of our first proposition. But such an actuary will never say or admit that the adjoining table proves anything with regard to the sufferings and struggles, or pain, endured by mankind, or in regard to the effects of advancing life. It cannot be proved by our former table that the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles (nor is it true); so it cannot be proved by this table that the mortality of mankind is regulated principally by the previous length and degree of the individual's sufferings and struggles (nor is it true). It cannot be proved by our former table that the sufferings of labour are safe in proportion to their shortness, and dangerous in proportion to their length (nor is it true); so it cannot be proved by this table that the pains occurring during life are safe in proportion to their shortness, and dangerous in proportion to their length (nor is it true). It cannot be proved by our former table that, contrary to the general opinion of the obstetric profession, the mere length of labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process (nor is it true); so it cannot be proved by this table that, contrary to the general opinion of mankind, and of the medical profession, the mere length of life is a most serious and important element in reference to the degree of danger and fatality accompanying life (nor is it true).

2d Proposition.—The duration of labour is only an inconsiderable part of the many causes (single or combined) of the mortality of women in parturition and the subsequent childbed.

As we have, under our first proposition, cleared away many of the incumbrances of the whole subject, the treatment of this second will be much more brief.

There is no obstetrical doctrine more deeply impressed on all the valuable literature of our profession than this, that the mere duration of labour, considered in itself and apart from other causes of danger likely to spring up as the process becomes protracted, is of little importance, so far, at least, as recovery of the mother is concerned. This doctrine is embodied in the ever-recurring inculcation of patience, as the highest virtue of both mother and attendant, in many and various circumstances of distress during labour. Sometimes it is expressed in an apophthegm, "Meddlesome midwifery is bad;" at all times it is diligently instilled into the minds of young midwives and accoucheurs. Unlike our first proposition, a comparatively barren theorem, this is one of the best recognised and most valuable doctrines in obstetrics. It is, therefore, of the utmost consequence to defend and confirm it.*

The proposition does not affirm that the mere duration of labour is of no importance,—quite the reverse. Far less does it affirm that the duration of

* See Harvey's Works, Sydenham edition, p. 534, for his opinion of the influence of duration of labour.

labour, with the accompanying pain and struggles, is not a very considerable element in the history of every case. It says nothing in regard to the very important effects of the duration of labour after bad symptoms or dangerous complications have supervened. It asserts that the duration of labour is in itself (*per se*) only an inconsiderable part (probably a very inconsiderable part) of the many causes of the mortality of women from parturition and its consequences.

Perhaps the strongest evidence in favour of this proposition is the fact, that it is the ancient and generally received opinion of the profession.* It rests upon what may be called the instincts of all experienced accoucheurs. In a science like medicine, where so little is capable of absolute demonstration, ancient traditions, especially if supported by the opinions of the great and wise, are among the most valuable and trustworthy guides of practice.

* In attempting the defence of the opposite view, Sir J. Y. Simpson says—"I am fully aware that when I state my conviction that the mere degree of duration and continuance of a labour is *per se* dangerous both to the mother and child, and very often fatal even in its influence, I venture to broach a doctrine which stands up alike against the opinion and the practice of some of the highest authorities in the obstetric profession.

"About half-a-century ago, when treating of the influence of the duration of labour in difficult and instrumental deliveries, Dr. Osborn observed—'I believe it is confirmed by general observation, that women recover at least as well after long, lingering, and laborious labours, the duration of which may have been extended to several days, as after the easiest, quickest, and most natural delivery.' In making this remark, Dr. Osborn stated, not his own

But the proposition may be supported most satisfactorily, both by direct and indirect evidence. Were it true that, "contrary to the general opinion of the obstetric profession, the mere length of the labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process," then a well-established rule of philosophising must be declared to be at fault. It was a maxim of Newton's, that no more causes are to be admitted than are true and sufficient to explain the effects. Few indeed will ever be found to assert that any obstetric patient dies without a very evident, true, and sufficient cause. The causes of such deaths are very various no doubt; but the mere length of labour is, by Newton's maxim, excluded from the number, as the truth of its influence is in question, and it is not required to explain the phenomena.

Moreover, it is always true in nature that uniformity of cause insures uniformity of effect. This axiom also is at variance with the belief that mere duration of labour is an important cause of fatality in the pro-

opinion only, but, I believe, the general opinion of the accoucheurs of his time; and the same doctrine, little, or not at all modified, still continues to be taught and acted upon, down to the present day, in the great English and Irish schools of midwifery, as the able and excellent writings of (for example) Professors Davis and Murphy, in London, and Drs. Collins and Beatty, in Dublin, etc., fully testify."—*Provincial Medical and Surgical Journal*, Feb. 9, 1848, p. 57.

In contrast with the above quotation, the student may do well to peruse some remarks by Dr. Beatty in his *Contributions to Medicine and Midwifery*, p. 43.

cess. For it is a common observation, that after long labours, even after the longest uncomplicated labours, there is often unusually rapid recovery. In the great mass of very long cases there is generally present some distinct and dangerous complication, which obscures the influence of the mere length of the labour, and destroys their value as arguments with regard to the effects of mere protraction. Again, in short and easy labours, where duration as a cause of fatality, supposed by some to be supremely important, is absent, there is still a considerable mortality.

Dr. Collins has distinguished himself by his zealous defence of the doctrine embodied in our second proposition, maintaining, as he does, that the mortality from protraction of labour, apart from other causes, is comparatively small. His elaborate *Practical Treatise* contains no record of any patient dying from the mere length of the labour; and his experience, founded on his wide field of observation, leads him to consider mere protraction of labour an inconsiderable cause of maternal mortality. It would be difficult to adduce statistics, at least from Dr. Collins' work, to prove our second proposition. We have already shown how erroneously statistics framed from the data in his work have been used, and pushed forward as if proving that our second proposition is false. But some of Dr. Collins' data are almost as valuable as if they were positive proofs, from the light which they throw on the real causes of death in protracted cases.

To take one aspect of Dr. Collins' cases, as he has

himself given it.* Of 16,414 parturient women under his care in the Dublin Lying-in Hospital, forty-two died whose labours were longer than twenty hours. "Of the forty-two, three died of typhus fever; nine of puerperal fever; one of stricture of the intestine, with effusion into the thorax; three where the placenta was retained; two of convulsions; one of abdominal inflammation previous to labour; nine of rupture of the uterus; one of inflammation of the intestines, with pus in the uterine sinuses; three of anomalous disease; one of diffuse cellular inflammation; six of inflammation, etc., subsequent to difficult labour; one of ulceration and sloughing of the vagina; one of disease of the lungs and hemorrhage; and one of abdominal abscess." Here it is evident that we have a list of causes of death, apart from mere duration of labour, in all the cases where the length of the process exceeded twenty hours. No doubt the mere length of the labour may have been an aggravation in all these cases, but of this there is no evidence whatever in Dr. Collins' data, however arranged; and we must accept the opinion of Dr. Collins, who took care of all the cases—an opinion sanctioned by previous general acceptation for ages, that protraction of labour was an inconsiderable part of the many causes of this maternal mortality in childbed.

The true bearing upon the great question before us, of the statement just quoted from Dr. Collins, has been altogether misconceived in some quarters. Dr. Collins'

* *Provincial Medical and Surgical Journal*, Oct. 18, 1848, p. 573.

statement has been represented as "a list merely of such injuries and diseases as tedious labour does produce;" and it is added, as if it were an apt illustration, that "long ago surgeons always used to argue, in regard to their lithotomy and other cases, that the deaths were from inflammation of the bladder, or inflammation of the intestines, or disease of the kidneys, or of the liver, or—anything, in fact, but the operation itself. Modern surgery (it is said) does not admit of such pathological casuistry. Nor does modern midwifery."* It is scarcely worth while to stop to contradict the indiscreet reproach so easily cast upon old surgery and surgeons. Let us submit for a moment, and for argument's sake, to consider it true—and only for a moment, as its irrelevancy will be easily made apparent. These old surgeons argued that their patients did not die of lithotomy, or of its consequences. Dr. Collins does not argue that his patients did not die of labour and its consequences; on the contrary he admits it. Dr. Collins argues, in opposition to Dr. Simpson, that the "mere length of labour was not a cause of death." To make a just use of the analogy above given, Dr. Simpson should have condemned the old surgeons for not considering the mere duration of the operation of lithotomy as a chief cause of the mortality of the operation. Dr. Simpson wishes us to condemn the old surgeons for not admitting inflammation of the bladder and intestines, etc., as causes of

* *Provincial Medical and Surgical Journal*, Nov. 1, 1848, p. 506.

death in connection with lithotomy. In his zeal to prove the importance of mere duration of labour in reference to the fatality of the process, he censures Dr. Collins for admitting exactly analogous diseases as causes of death in connection with labour. Moreover, when Dr. Simpson speaks of "tedious" labour, he uses a well-known term, implying a great deal more than mere length of labour. When he says that tedious labour produces such diseases as Dr. Collins enumerates, then he and Dr. Collins are at one, and he had no right to address him as if committing a very great error. When he says that tedious labour produces these effects, he is not differing from, but agreeing with, the whole profession; only, he is deserting the position which Dr. Collins attacked, and which he would fain appear still to hold. For his statement is, not that tedious labour leads to these causes of death —a true one; but "that the mere degree of duration and continuance of labour is, *per se*, dangerous both to the mother and child, and very often fatal even in its influence;"—a doctrine without foundation.

The element of mere duration of labour is, in fatal cases, so mixed up with other circumstances, that I despair of medical philosophers being ever able so to handle obstetric statistics as to make them yield anything like an approximation to a proper estimate of its baneful influence. In protracted cases, with no other evident dangerous complication, it is a common remark that the patients appear to make unusually rapid recoveries.

In tedious cases it is not the protraction which causes the complications and danger, but the complications which cause the protraction and danger, leaving the mere protraction as a negation, destitute of any presiding influence.

Such is the statement of, and evidence for, our second proposition.

In the discussion between Dr. Collins and Dr. Simpson as to the influence of mere duration of labour upon maternal mortality, we have seen that the latter, by his use of Table CXXX., tried to prove that Dr. Collins was wrong in asserting that the mortality of mothers from protracted labour was strikingly small. Although Dr. Collins was not very happy in his statement of his views, and sometimes not to be justified in his arguments, yet there can be no doubt that the essence of the truth of our second proposition, as bearing on practice, was contained in his defence of his views.

Dr. Collins was personally engaged in watching and managing the great mass of cases reported in his valuable *Practical Treatise*. This circumstance will always give his views a peculiar force and value, even were his reputation as an author and observer not so high as it deservedly is. It was at least rash in any author, addressing Dr. Collins, to say—"Against the truth of your own recorded opinions I appeal to the truth of your own recorded facts. Against your own doctrines I appeal merely to your own data." Such are

indeed very tame expressions compared with others that appeared in this controversy. And yet we think we have made it evident that Collins, in common with the general mass of the profession, was right in regard to the main question, and his opponent wrong. Any one who reads the controversy will find in it an admirable illustration of the fable of the two knights looking at opposite sides of the same shield. But although to a careful perusal this becomes evident, it is only just to add that with Dr. Collins rested the practical truth, fairly founded on experience, while some theoretical truth was fitfully maintained by his opponent, yet so as almost to be concealed by error.

Let us consider for a moment what such reasoning as Sir J. Y. Simpson adopts in this controversy would lead to. It appears to us that, if he had looked whither his arguments might lead, he would have himself been probably deterred from urging them. If mere length of labour be an important element in the causation of deaths from labour, then certainly patience is no virtue in an accoucheur. If mere length of labour be as he describes it, then meddlesome midwifery must, I fear, be declared good instead of bad. If mere length of labour be as important as he represents it, then any treatment which will accelerate delivery may be easily defended. If it be right to disregard all the real causes of danger and death in labours, as this author does, in order to make prominent the danger of protraction, with the ulterior view of supporting an artificial interference which accelerates the

process, then a like reasoning may be used to support the most absurd and unjustifiable measures, and the art of midwifery will be at the mercy of any specious reasoner, however ill founded his arguments may be.

It would be a waste of words to enter farther on this discussion of the influence of the doctrine of the duration of labour. The doctrine has important relations to the mortality of children, in parturition,* and to other matters. It is enough here to point out that, in connection with these questions, the same errors in reasoning have been committed as have been made in tracing the bearing of the doctrine on the mortality of mothers.

* I beg to refer the student who wishes to pursue this subject to a paper by Gassner, in the *Monatssch. für Geb.*, 1862.

PART X.

ON THE DURATION OF PREGNANCY.

In many of the elaborate essays which have been written on the subject of the duration of pregnancy in women and in the inferior animals, it has appeared to me that an important source of error lies concealed.* The exposition of it will, I trust, throw some light on this interesting subject; and I am sure that, when it

* In the following passage Montgomery (*Signs of Pregnancy*, p. 503) evidently confuses insemination and conception. "It is, I think (says he), universally admitted that a woman may conceive on any day of the interval between one menstruation and another." Writing after the original publication of my remarks in the text, and probably referring to them, Montgomery admits the source of error pointed out, but illogically refuses it any place in his calculations. He says (p. 509)—"It has been suggested that pregnancy should be dated, not from the single fruitful intercourse, or insemination, which has produced it, but from the time when the ovum comes into contact with the semen masculinum, which union should be considered as constituting conception. Now, this may be true; but supposing it so, how are we to make it available in practice?" (*Dr. Montgomery refuses to make it available even in reasoning.*) "Is not our attempting to do so more likely to engender confusion than to lead to satisfactory results?" . . . "If this view were adopted," he most erroneously believes and says, "we should have no means of calculating the period of gestation with anything like an approximation to accuracy in any case."

comes to be completely investigated, our notions as to the duration of pregnancy will be much more definite and satisfactory than they now are. I proceed to make a few remarks on this particular point, and then briefly to discuss the general question.

CHAPTER I.

THE INTERVAL BETWEEN INSEMINATION AND CONCEPTION.

In commencing, it will be useful to define the meaning to be attached to some important terms frequently recurring in this discussion—viz. insemination, conception, and impregnation. By the word insemination is to be understood simply the injection of semen into the genital passages, the result of sexual conjunction. By conception is to be understood the more hidden and mysterious union of the semen and ovum; while the word impregnation implies both of these processes.

The confusion of the two former of these different processes is so general among obstetric writers that it is needless to quote authorities for the assertion. That they should always be kept distinct in studying this subject will, I hope, be made apparent. For, in fixing the commencement of pregnancy, it is necessary to date from the period of conception.* Authors, in discussing this subject, have delighted to quote as

* Joulin (*Traité Complet d'Accouch.* 1866, p. 449) points out this source of error. My remarks were first published in the *Edinburgh Medical Journal* for March 1854.

crucial examples those cases where the data of an only connection, or of connections within a short and limited time, could be satisfactorily decided. But it is evident that such a date only fixes the time of insemination, and not the time of the commencement of pregnancy; for a woman cannot be said to be pregnant whose body merely *contains* seminal matter. Pregnancy is a state of fertility, of breeding, which, as Leeuwenhoek long ago pointed out,* cannot be said to commence until such time has elapsed as may intervene between insemination and the union of the ovum or ova and semen. This period of time, whatever may be its possible length, must be subtracted from all these supposed crucial cases of the duration of pregnancy. The interval described as the duration of pregnancy—that is, between successful insemination and parturition—must be considered as, in strict language, a false period; and it is so because it contains the period between insemination and conception, during which a woman is not pregnant. Of this interval, then, all such cases must be curtailed.

Very little has as yet been ascertained as to the

* Hinc, hæc animalcula diutius in tuba sive matrice posse vivere, animo præsumebam meo, ac quoque nostræ mulieres non prœcise eo die sive tempore, quo cum viro rem habuerunt, fecondas sive gravidas fieri; sed easdem post octo, aut decem, imo plures quidem dies, postquam coiverunt, gravidas posse fieri, quia post aliquot coitus dies ex multis saltem animalculis, unum animalculum eousque pervenire potest, ut punctum sive punctulum istud, animalculum fovendo aptum, attingat.—*Arcana Naturæ, etc.*, tom. ii. p. 150, edit. in 4to. Lugd. 1708.

possible length of this interval. It was my intention to have attempted to make it out in regard to some of the lower animals; but my inexperience in such investigations, and the pressure of other avocations, have hitherto deterred me from the pursuit of this object. There is, then, at present no resource in this question but to facts already known. Now, it has been ascertained by physiologists that for impregnation it is not necessary that the semen should be newly expelled by the male.* Animals have been frequently impregnated, by Spallanzani and others, with semen, which has not only been kept for some time, but has even been variously altered, in certain properties at least, in experiments. And there seems to be no limit to the time during which the semen may be kept without losing its virtues, except the term of the life of the spermatozoa.

That this period is not insignificant, and cannot be passed over without risk of important error—in fact, that it may extend to many days or weeks—will appear from the following observations. We omit the facts in

* "On opening the body of a female mammal, one or more days after it has received the male, semen may be found not only in the body and horns of the uterus, but also in the oviducts, and on the surface of the ovary. The spermatozoa are in vigorous movement. These may retain their activity for a week or more in the female organs. And in many insects this period of time is much greater. Here the ova are only expelled long after copulation. The females, therefore, possess a special receptacle in which the moving spermatozoa are preserved until the ova finally reach them. In this receptacle their activity remains uninjured for many months."—Valentin, *Text-Book of Physiol.* Eng. tr. p. 641.

regard to animals so low in the scale as insects, in some of the females of which the semen is laid up in cavities where it retains its power for months. In regard to the dog, Leeuwenhoek * pointed out that the spermatozoa might live for more than seven days preserved in a glass tube; and if such be the case in a rude experiment, it may be expected that they would retain vitality considerably longer in the passages of the bitch, where they have heat and moisture supplied under favourable circumstances. That they do live for some days in the genital passages has been proved by abundant observations, although the possible length of this period is not certain. The decision, indeed, of this point by microscopic observations would be a very difficult matter, as it would involve the almost impossible search for spermatozoa over every part of a long tract of mucous membrane. And this search would be necessary, with a view to deciding the question of the interval between insemination and conception, for we know by the experiments of Spallanzani that semen highly diluted, or, in other words, the smallest quantity of semen, is sufficient for impregnation.†

* "Si enim animalcula plures quam septem integros dies in tuba vitrea vivere possint, quantum temporis illa in matrice, his animalculis recipiendis ac fovendis unice constituta, vivere quidem possent."—*Arcana Naturæ, etc.*, tom. ii. p. 150.

† These observations of Spallanzani have been considerably modified and corrected by the researches of Mr. Newport upon the quantity or number of spermatozoa required to fecundate an ovum in the frog, etc. See his paper in the *London Phil. Trans.* for 1853, part ii.

The elaborate experiments of Haighton,* long ago performed, show that in the rabbit conception generally does not take place till about fifty hours, or more than two days, after insemination. He found that division of the fallopian tube earlier than this time prevented conception, and that, after waiting longer the conception was not prevented by the mutilation. It thus appeared that the conjunction of the ova and semen in the rabbit generally did not take place till more than two days after insemination. In the rabbit, then, there was found, in Haighton's experiments, this long interval between insemination and conception; and in some cases it is possibly much longer. In the rabbit the interval between insemination and parturition is ordinarily thirty days. The observations of Tessier upon 161 rabbits give five days as the extreme limit of the protraction of this term, a period of time which may be accounted for without any stretch of the space during which the semen may retain its fructifying power. And in this way it may have happened that the real period of gestation—that is, from conception to parturition—may not have been at all protracted in these cases. The cases also in which the period was less than thirty days may be explained by supposing the ova to have been further matured or even advanced into the uterine horns before impregnation took place, so that conception may have happened very soon after insemination. And in Tessier's observations it is remarkable that in none of the rabbits did labour

* *Philosophical Transactions*, 1797.

anticipate the usual time more than two days, the period which Haighton's experiments seem to show to be the usual interval between insemination and conception in that animal. In the present state of our knowledge, however, these explanations cannot be considered as absolutely established.

Experiments of Cruikshank upon the rabbit and doe, experiments of Wharton Jones, Martin Barry, and others, might be adduced as throwing light on this point.

For reasons which do not require to be stated, there is great deficiency of evidence in regard to the analogous subject in the human female. But there is every reason to believe that the circumstances of conception in her closely resemble those in the higher animals. It has of late years been shown that, in woman, at every menstrual period, an ovum is matured and expelled from its graafian vesicle, and that she is liable to conceive during its progress along the fallopian tube. How long after its maturation the ovum can retain its vitality and susceptibility to the seminal influence is not known, but probably the time is short. Nevertheless, cases might be easily adduced from the works of eminent obstetricians to prove that a single insemination at any period of the interval between two menstrual periods may result in the fertilisation of the female. Of such cases those only are important from our present point of view where conception has resulted from insemination shortly before the return of a period. They admit of explanation in three different

ways.* Either the ovum has remained up till this time entire and susceptible of being influenced by the semen; a supposition which is very improbable as regards the ovum,† and is at variance with what we know of the history of the decidua or nidus prepared for the egg's further development. Or the excitement of connection may have hastened the maturation and rupture of a graafian vesicle; a view which is in itself improbable and inconsistent with what we know to result from similar circumstances in the lower animals. But it may also happen, and I believe it does not unfrequently happen, that the seminal animalcules remain in the passages till the ovum is prepared and discharged from its vesicle. An objection at once appears to this explanation—namely, that these spermatozoa would be removed by the menstruation contemporaneous with the discharge of the ovum. When

* As a good example we may refer to a case of Dr. Montgomery's (*Signs, etc., of Pregnancy*, p. 258). The last menstruation was on the 18th October. Insemination took place on the 10th November; parturition on the 17th August. The interval between insemination and parturition was thus 280 days; between last menstruation and parturition it was about three weeks more.

† "The passage of the ovum from the ovary to the uterus, occupies, M. Bischoff says, three days in the rabbit, and four or five days in ruminants, and therefore probably eight or ten days in the human female. M. Bischoff believes that the ovum escapes from the graafian follicle at the time when the menstrual discharge is about to cease, and he is of opinion that in order to be fecundated it must be acted on by the semen while it is in the fallopian tube."—Baly and Kirkes' *Suppl. to the 2d vol. of Müller's Physiol.* p. 58.

menstruation does supervene on a single recent coitus, this will probably happen, unless the semen have permeated the fallopian tubes, and thus advanced beyond the scope of the menstrual flux.* But the study of such

* Several authors have stated their belief that the mucous membrane of the tubes yields a contribution to the bloody menstrual flow. I have seen the mucus of the tubes tinged with blood in the autopsy of a woman dying during menstruation, and in some other cases. Cases of occlusion at the uterine extremities of the tubes, in which they have been found distended with bloody fluid (see Bernutz et Goupil, *Maladies des Femmes*, vol. i.), are upon record. Numerous other instances of repletion of the tubes with blood or bloody fluid are to be found, but in these last there is generally no good evidence that the blood was derived from the tubes themselves. It is, however, sufficiently well demonstrated that in some cases blood is excreted from the mucous membrane of the tubes in small quantity. It may be regarded, I think, as nearly certain that, in natural menstruation and in menorrhagia, blood is not excreted from the tubes in considerable quantity. The statement by Tuckwell (see his thesis *On Effusions of Blood in the neighbourhood of the Uterus*, p. 7) of his opinion to an opposite effect is unsupported by any adduced evidence. Were the uterine ends of the tubes as they are generally described, and did the mucous membrane of the tubes supply, as Dr. Tuckwell believes, no inconsiderable part of the blood that escapes in what is called an attack of menorrhagia, then great hæmatoceles would be frequent to a very much greater degree than any one at present supposes them to be. Further, the anatomical demonstrations of Rouget are hostile to the tubal source of menstrual blood. I have already said that I have seen the mucus of the tubes tinged with blood : I have also seen the mucous membrane of the uterine extremities of the tubes detached—hornlike—with the proper decidua uterina in abortion ; the hornlike tubal projections from the uterine decidua measuring three lines in length. This detachment from the tubes appears to be analogous to that occasionally observed from the cervix uteri : in both situations some little blood may, no doubt, be excreted in natural menstruation,

cases as recorded by various authors* reveals this interesting fact, that under such circumstances menstruation often does not take place at all, or only very scantily; the uterine system, as it were, anticipating the conception, and preventing the failure which might result from a free discharge of blood. It is evident that such cases occurring in married women would be very liable to be considered cases of gestation protracted a month.

Some actual observations on women, bearing on these points, have recently been published by Dr. Marion Sims.† They demand quotation. "It would be important (says he) to determine how long spermatozoa can live in the matrix. On this point we need more extended experiments, for I do not think that their duration of life has yet been fully established. Dr. S. R. Percy (*American Medical Times*, March 9, 1861), of New York, reports a case in which he found 'living spermatozoa, and many dead ones,' issuing from the os uteri eight and a half days after the last sexual connection. During this time the husband of the patient had been from home.

"I have examined the semen many times with the

but neither does afford the hæmorrhage in menstruation or menorrhagia.

* Mauriceau (*Maladies des Femmes Grosses, obs.* 676) mentions a case interesting in this point of view, in which a woman was impregnated during the flow of menses. See also Schwegel, referred to by Ahlfeld, *Monatsschr. f. Geb.* Bd. xxxiv. S. 182.

† *Clinical Notes on Uterine Surgery*, p. 384. For a similar statement see Joulin, *Traité Complet d'Accouch.* p. 449.

view of determining this point, and think I can safely say that spermatozoa never live more than twelve hours in the vaginal mucus. But in the mucus of the cervix they live much longer. At the end of twelve hours, while all are dead in the vagina, there are but few dead ones to be found in the cervix. When the cervical mucus is examined from thirty-seven to forty hours after coition, we shall ordinarily find as many spermatozoa dead as alive. But my observations on this point could not, under the nature of things, be accepted as the rule, for they were all made upon those who were, or had been, the subjects of uterine disease in some form or other.

"Here is the report of an observation made upon a patient who is perfectly reliable: 'Sexual intercourse at eleven P.M. on Saturday. A microscopic examination of the secretions was made on Monday at three P.M., just forty hours afterwards. The vaginal mucus contained a few dead spermatozoa—none alive; the cervical mucus contained great numbers very active—a few dead.'

"The above is copied from notes made at the time. I saw no reason why many of these active spermatozoa should not have lived for a still longer time. Many of them lived six hours after their removal. This was in July."

I refer my readers to the same author* for some interesting cases of impregnation shortly before an expected menstruation which did not make its appearance.

* *Clinical Notes on Uterine Surgery*, p. 381.

CHAPTER II.

THE INTERVAL BETWEEN INSEMINATION AND PARTURITION.

THIS is a period of the greatest importance in a medico-legal point of view. It is discussed by obstetric authors as the period of gestation, or as the term of the duration of pregnancy. We have already shown that the present state of our knowledge requires us to make a distinction between the date of insemination and that of conception, and it strongly appears to us that the full comprehension of the bearings of this distinction will go far to equalise the discordant views as to the term of pregnancy in the human female, and to account for many of the so-called cases of prolonged gestation. But with our present ignorance of the possible interval between insemination and conception the exact attainment of this result is impracticable.

In attempting to settle this point, authors have resorted to numerous sources of evidence, the fallacy of which they themselves well knew. For instance, we find Dr. Montgomery, in his classical essay on the period of human gestation, and many other authors, quoting examples based upon the evidence of peculiar

sensations felt at the moment of conception,* on the last appearance of the menses, and on the time of quickening—phenomena which, however important in aiding the accoucheur to make a good guess of the day of confinement in single cases, can never be for a moment relied upon in deciding such an exact question as that before us. An excellent story, illustrating the fallaciousness of such evidence, is related by Dr. Reid, of an expert midwife, who, when examined in the celebrated Gardner peerage case, "deposed that she had once gone ten months with child, that she was always right in her calculations, that she always fainted away at quickening, etc., so that she could not be deceived."† Some time after the trial she applied to Dr. Reid, convinced on such grounds that she was seven months pregnant. But on examination there was found no pregnancy at all.

No reliance can be placed but upon accurately-ascertained dates of parturition and of fruitful connection. In regard to the latter of these dates no confidence can be placed in the statements of women living habitually with males, however truthful they may be, or whatever additional evidences they adduce. We are therefore reduced to a limited class of observations—namely, those where the pregnancy resulted from a single coitus, including those where this never took place but on a single day, and those where it was

* See an example recorded by me. *Edinburgh Medical Journal*, April 1868, p. 919.

† *Lancet*, vol. ii. p. 78, 1850.

removed on both sides from other similar occasions by months, or such other period as would render it absurd to refer the parturition of a fully-developed fœtus to them. With those dating from a single day we have included some dating from one of two days; but in such cases our calculations commence from the coitus of the first day only. These statistics (for the details of which we refer to the note)* contain 46 cases, which

* Raciborski (*De la Puberté*, etc., p. 460, etc.) relates five cases which come within this category. The intervals were 275, 270, 268, 273, and 274 days respectively. Montgomery, in his work on the *Signs, etc., of Pregnancy*, quotes or relates seven cases. The intervals were 281, 280, 287, 289, 288, 284, and 291 days respectively. These cases differ manifestly from those of Raciborski, but this is accounted for by observing that, like some of those yet to be quoted, they are selected by Montgomery as proofs of the prolongation of pregnancy in some cases. Rigby, in his *System of Midwifery* (p. 84), mentions three cases. The intervals were 260, 264, and 276 days respectively. Reid, in his elaborate essay on the "Duration of Pregnancy" (*Lancet*, vol. ii. 1850), notices twenty-five cases. The intervals were 276, 274, 274, 275, 273, 271, 274, 274, 278, 263, 280, 264, 274, 276, 274, 276, 280, 266, 265, 266, 272, 275, 271, 287, and 293 days respectively. Besides many of those already mentioned, he adds five cases from the *American Journal of Medical Sciences*, which were 270, 272, 276, 284, 272 days respectively, and Mr. Skey's case of 293 days. All the above are carefully-selected cases, where the date of coitus taking place only during a single day, and the date of parturition, were accurately ascertained. They are in all 46 cases. The average interval is 275 days. More than two-thirds of the cases have an interval of 276 days or less. Other collections of cases have been made with great care. I may especially refer to those of Veit (*Verhandlungen der Gesellschaft für Geburtshülfe in Berlin*, hft. 7, 1853, S. 122) and Hecker (*Klinik der Geburtskunde*, S. 35). These make this interval about 273 and 277 days.

yield the period of 275 days as the average interval between insemination and parturition.* While 275 days was the average interval, it may be remarked that the largest number of cases at any particular day was 7 at the 274th day.

* In France 270 days is the ordinarily-accepted duration of pregnancy. See the works of Jacquemier, Velpeau, etc.

CHAPTER III.

THE INTERVAL BETWEEN THE LAST MENSTRUATION AND PARTURITION.

THIS is a period which, for obvious reasons, can be much more easily and frequently ascertained than that last under discussion. It is one, the knowledge of which is of the greatest practical importance in the every-day life of the married female and of the obstetric practitioner, seeing that, by aid of it, he attempts to predict the date of the expected confinement. In the vast majority of cases it is the only fixed point from which the calculation can be made, and hence the necessity of accurately ascertaining it, if possible.

Authors have frequently neglected the discussion of this important period, the only one available in most cases of pregnancy. They generally decide the term of pregnancy theoretically, and upon insufficient grounds, and direct that, in calculating for the day of confinement, this term should be told off from some day after the last menses, which day they conceive to be that on which conception most frequently or most probably takes place. For instance, Montgomery

states, upon the evidence of a very few cases only, that the natural period of human gestation is 280 days, and in calculating the date of parturition, recommends this to be added to any day within a week after the last menstruation. He thus includes between the last menses and the date of parturition a period varying from 281 to 287 days—a period which, we shall show, considerably overpasses the mark. Other authors and teachers, considering that a woman is equally liable to conceive on any day between two menstrual periods, direct that the middle day of that interval be taken, and the supposed period of gestation, 280 days, added thereto—thus including the exaggerated space of 290 to 295 days between the last menstruation and parturition.

The exact determination of this interval, as of that last under discussion, can be obtained only by a reference to actual observations. Modern researches have shown that it is at the menstrual period that the ovum quits its graafian vesicle, and traverses the fallopian tube on its way to the uterus. It is in the course of this passage that it encounters the semen, and conception results. This passage occupies about three days in the rabbit, and in M. Bischoff's opinion it occupies eight or ten days in woman. During all this time, then, the woman will be liable to conceive. It will, therefore, be expected that the interval of which we are at present speaking will be some days, at least, longer than the last.

The statistical calculations on this subject (for

details see foot-note)* give on an average 278 days as the interval between the last menstruation and parturition—a period less even than the 280 days which we have generally been taught in this country to be the interval between impregnation and parturition, or the duration of pregnancy.†

The largest numbers of cases on particular days conglomerate about the 280th. Among Dr. Reid's 500 instances, 283 were within the 280 days, and 217 beyond it. So far is it, then, from 280 days being the ordinary duration of pregnancy, that a woman generally does not go more than 278 days after the last menstruation is over. This period exceeds the average interval between insemination and parturition by three days; and we may argue from this, with

* The valuable statistics from which these results have been derived, by a tedious calculation, are published by Drs. Merriman and Reid. The observations of the former were originally published in the 13th volume of the *Medico-Chirurgical Transactions*, and subsequently extended in the edition in 1838 of his work on *Difficult Parturition*. The observations of Dr. Reid are to be found in the 2d volume of the *Lancet* for 1850. In Simpson's paper on the "Duration of Human Pregnancy," these and other allied statistics will be found carefully elaborated. See *Monthly Journal* for July 1853. In a statistic which I have made of the cases having sufficient details, recorded in the books of the Royal Maternity Hospital, a result comes out similar to that derived from the far more extensive records above mentioned.

† "The common term of pregnancy (says Smellie) is limited to nine solar months, reckoning from the last discharge of the catamenia." — *Treatise on Midwifery*, fifth edition, vol. i. p. 127.

some little probability, that conception takes place generally a few days after menstruation is finished—a view which is confirmed by numerous other physiological observations. Sexual connection in the days immediately following menstruation is generally believed to be especially likely to produce impregnation.

CHAPTER IV.

THE PREDICTION OF THE DAY OF CONFINEMENT.

This is one of the functions ascribed to the accoucheur; and apart from the comfort and convenience which the mother experiences from the foreknowledge of this date, she often makes its failure or success a test of the more subtle acquirements of the physician. The foregoing statistics, however, will always justify the physician in never giving a decided prognosis of the day of confinement; and if he has been guarded and careful, will afford him an asylum, showing as they do, that with the most certain knowledge of the termination of the last menstruation, or even of the date of a single coitus, no safe prediction can be made unless within limits so extended as to deprive it of much of its value. At the same time there is no doubt it will always be desirable to know the most probable day of confinement, and this can generally be settled with some exactness.

If the date of a single connection is ascertained, which is, of course, very rarely the case, then the process of deciding the probable day of confinement simply consists in telling off 275 days (the average interval

between insemination and parturition) from that date. Now, any nine consecutive calendar months include 275 days, if February is not in the number. If February is in the number, the nine calendar months include only 273 days (leap-years excepted), and the correction necessary is apparent. The whole process of calculation, then, consists in attaching the number of the day of connection to the name of the ninth succeeding month, and adding two additional days if February is included in the interval.*

In the vast majority of cases the day of confinement is predicted from the date of the termination of the last menstrual period. In many cases the calculation can be aided and corrected by comparison with former pregnancies in the same female. But when this source of information is wanting, the nearest approach to truth will be made by adding to the day of the disappearance of the menses 278 days (the average interval between the end of menstruation and parturition). The prediction will, of course, prove erroneous in a great number, nay, in the majority of cases, but it forms the nearest approximation which the mother can obtain to guide her. If a woman, then, knows the last day of her last period, she has only to tell the same day for the ninth month following (most mothers do so on their fingers, which thus form an admirable periodoscope), and add

* Nine months do not always contain 275 or 273 days. Dating from December and July, nine months contain 274 days, and from May 276. The statements in the text, although sufficiently correct for general use, require this correction to be exact.

three days, or, if February is in the interval, five days. She thus has the most likely day of her confinement; or, perhaps better, she has the middle day of the fortnight in which she will probably be laid up.

I have already casually shown how this varies from the calculations ordinarily recommended by most British authors and teachers. It would be tedious to enter further on this subject. I may merely remark that a more correct plan prevails on the Continent; and, from some inquiries and observations I have made in Scotland and England, I find that, popularly, a more correct calculation is extensively in use than that recommended in the schools. For instance, in Edinburgh, and some parts of Scotland, it is common to find women calculate in this way. They find the last day of being menstruated, and they hold that the same day nine months after will be the day of confinement. The celebrated Harvey's opinion on this subject was also very correct. His remarks tally with Dr. Tyler Smith's ingenious views on the question, and are deserving of quotation:—"Unquestionably," says he, "the ordinary term of utero-gestation is that which we believe was kept in the womb of his mother by our Saviour Christ, of men the most perfect; counting —viz. from the festival of the Annunciation, in the month of March, to the day of the blessed Nativity, which we celebrate in December. Prudent matrons, calculating after this rule, as long as they note the day of the month in which the catamenia usually appear, are rarely out of their reckoning; but after

ten lunar months have elapsed, fall in labour, and reap the fruit of their womb the very day on which the catamenia would have appeared had impregnation not taken place."*

* *Harvey's Works.* Willis's trans. p. 529.

CHAPTER V.

PRACTICE IN THE PREDICTION OF THE DAY OF CONFINEMENT.

In this chapter I propose to pursue a purely synthetical line of argument, in order to show the accuracy of the conclusions regarding the best mode of predicting the day of confinement, which I have just given.

Since these conclusions were first published in 1854, there has been considerable discussion of the subject both at home and abroad; and there can be no doubt that there is a general tendency among scientific inquirers to advance in the direction which I followed,—that is, tending to show that the average duration of pregnancy is shorter than older authors generally supposed.

Many still very erroneously write, and to a great extent reason, as if the date of conception could be made out, and as if the date of a fertilising coitus were the date of conception. It is surely unnecessary for me to go over this ground again; for not a single argument is adduced in support of these views, and they are known to be not only not demonstrated, but to be not in accordance with our positive knowledge.

The most elaborate recent paper on this calculation

which I know is by Dr. Ahlfeld, and is published in the *Monatsschrift für Geburtskunde* for 1869. His theoretic conclusions differ from mine chiefly in reducing the period of pregnancy from 275 to 271 days. But I am very far from being satisfied with his data, especially with his mode of getting assurance as to the date of insemination, or, as he erroneously calls it, conception. He trusts, in my opinion, far too much in the mere statements of the females. So much is this the case, that I am disposed still to adhere to my own figure of 275 days as the nearest approach to a correct statement of the average duration of pregnancy.

Dr. Ahlfeld further tries to show that the majority of women are confined in the 39th week of pregnancy, —a statement quite in accordance with his previous conclusion regarding the duration of pregnancy, and, I need not add, not in accordance with the view of the duration of this state to which I adhere.

But prediction of the day of lying-in is an important practical matter, from whose arrangement all theory should be excluded. It is a valuable calculation of a quite empirical kind. Its successful performance does not necessarily depend at all on correctness of views as to the duration of pregnancy.

We cannot count from the beginning of pregnancy, or conception, as Ahlfeld pretends to do, because in no case do we know the day or the week in which it begins.

We cannot, except very rarely, count from a single coitus, or coitus only on a single day, because such cir-

cumstances seldom occur, and because, even when they are alleged to have occurred, we can very seldom obtain satisfactory assurance of them.

We almost invariably count from the last menses. The end of last menstruation is generally taken as the point to count from; and this is a rational proceeding, because cohabitation is, as a rule, suspended during the flow, and the female is not liable to be impregnated till after it has ceased. But, as I have already said, the calculation is purely empirical, and might, as is actually done by Cederschjold and Berthold, be made from the beginning of menstruation just as well as from the end of it. I adhere to the old plan because it is the old and generally-used plan, and because, therefore, the data from which the method of calculating the day of confinement has been elaborated have been made out upon it. Had we more numerous and more carefully collected data, based upon a system of counting from the beginning of menstruation, I should be ready to give up the old one and take the new one. Both systems yield the method of calculating on purely empirical, not on rational, grounds. Authors have committed grievous errors in vainly trying to combine empirical and rational grounds for this calculation. In the present state of science this is impossible. Only confusion can arise from so doing. There can be no objection to authors deriving evidence from this calculation for or against propositions in science; but at present science can lay down no grounds for the calculation other than the records of experience. Ahlfeld

is the ablest representative of such attempts; but in practice he comes in reality to simple dependence on the date of last menstruation.

I find that the 278th day after the end of last menses is the average day of delivery at the full time; and on this I proceed. No ingenuity can devise a superior plan of estimating, so long as the last menstruation forms the only generally available *terminus a quo*. The introduction by many authors of scientific views into the question of the best way of predicting the day of confinement, may be justly characterised as either at least unnecessary or else merely pedantic. Till I find a larger and more carefully compiled mass of facts than those of Reid and Merriman, I shall adhere to my method of calculating, based on the circumstance that 278 days is the average interval between menstruation and parturition; and in doing so I have science and common sense on my side.

The method which I recommend is confessedly a rough one. The calculation itself is always what is called a rough one. My method certainly is loose and erroneous to the extent of one day in certain cases, which I have specified at page 442.

Now, any practitioner can test this plan by his own experience, in a purely synthetical and reverse manner. He can try the plan, and then see how it has led him; whether it has led him and his patients into error or not. Since I adopted this reverse method of verifying my plan of calculation, I have found that Ahlfeld has already resorted to a similar test. It is only very

slightly different from the method by which the plan of calculating was developed. The difference is stated as follows :—Cases of delivery collated yield results on which the plan is founded : instances of prediction compared with the real events test the plan.

I shall now show what my predictions on this plan have come to. No one can hope to be an absolutely good prophet in this matter, but we can be as good as possible, as nearly right as may be. The predictions to which I shall immediately make reference were all written down before the events, and remain written. I have only 153 cases to refer to, all collected within several recent years. They are few, because I did not venture on the written-down prediction unless I was satisfied that I got good information as to the day of the cessation of the menses.

I need scarcely repeat, that in practice I do not predict a day, but a fortnight. I predicted a day in my note-book for my own use. These 153 predictions in my note-book I now analyse.

In 10 cases the day of confinement was exactly predicted, or about once in every 15 cases.

In 80 cases the confinement took place sooner than was predicted. The number of days of anticipation was, for the whole 80 cases, 590, or an average of above 7 days for each case.

In 63 cases the confinement took place later than was predicted. The number of days of protraction was, for the whole 63 cases, 535, or an average of above 8 days for each case.

In 63 cases, or more than one-third of all, the time of confinement was successfully, though not exactly, predicted, the birth occurring not earlier or later than 4 days from the predicted day.

The average error was about $7\frac{1}{2}$ days,—a circumstance which indicates that the prediction should not state the week of confinement but the fortnight of confinement, there being generally an error of a little above 7 days on the one side or the other of the ascertained average day.

But the most interesting result of these figures is the answer to the question, Can the calculation be improved? and the answer is, that it is, for practical purposes, perfect, or as nearly so as the present state of science permits. This near approach to perfection is shown, firstly, by the observation, that the errors on either side of the predicted day are nearly equal. If the errors on either side were exactly equal, then the calculation would be perfect; for it would thus be shown that, for the mass of cases, the exactly most probable day of confinement had been hit upon. In my 153 cases the excess of error is on the side of anticipation. This excess is 55 days. Now, 55 days for 80 cases is less than a day of average error; and as our prediction does not pretend to even the accuracy of a day, the error may be truly regarded as trivial.

There is another, far more precise, and the only true way of analysing these or like results with a view to ascertaining whether the calculation is the least erroneous possible. This method consists in ascertain-

ing, not the average error on each side of the true point, but in observing the amount of error in each successive day on either side of the true point. This method, pointed out to me by Professor Tait, has been kindly also carried out by him, and I here give his note containing the details of it.

This note is of some value, even in connection with the small number of cases which I have for analysis by this method, which is founded on the theory of probabilities. Were my cases ten times as numerous, it would enable us to arrive at final results. But I make careful mention of the method here, chiefly because of its extreme value as a suggestion for the use of future investigators. When applied to a sufficient number of instances it forms the only exact means of testing any plan of calculating. It not only tests such plan, but gives, when worked out by the

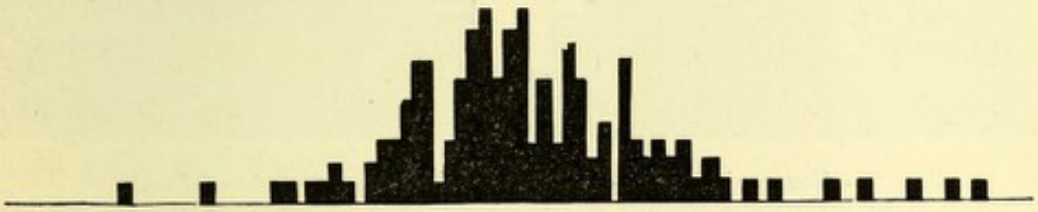


Fig. 1.—Showing Dr. Matthews Duncan's data Graphically.

method of plotting used by Professor Tait (see Fig. 1), a correct view, at a glance, of all the errors in defect or in excess; and not only this, but also, with equal facility, a correct view of the importance of the errors. In addition to all these advantages of this method, which, so far as I know, has not yet been applied to the subject on hand, there is another, that, from a suf-

ficient number of observations exactly made, it will enable us to elicit with certainty the true plan of calculating. It will not only show errors in an old method: it will show also how to avoid them; how to correct the old method.

The first figure here given is a mere ocular view of the errors. It puts the variations in my 153 cases not in a new light so much as before a new sense; not before the eye in written words, but before the eye in represented masses.

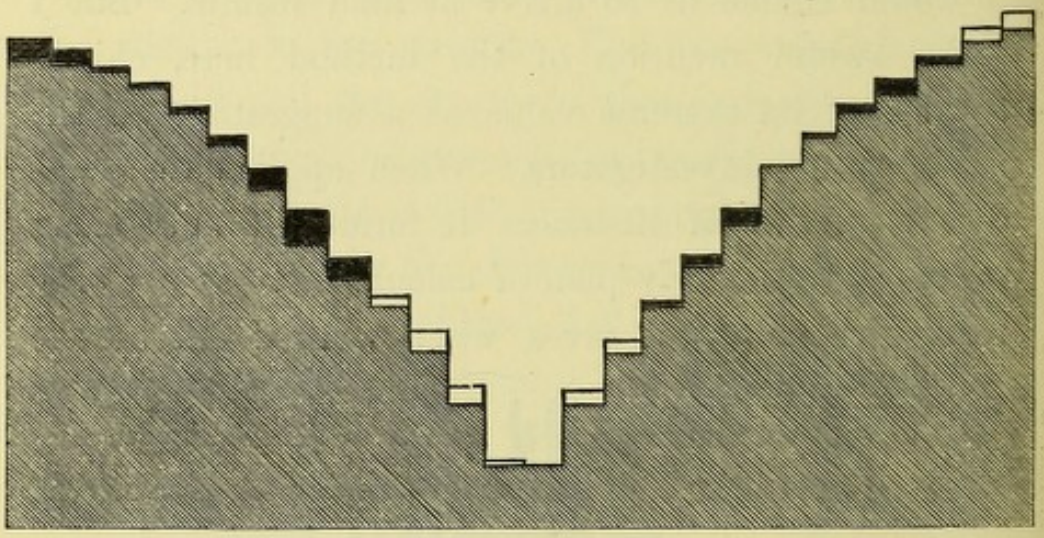


Fig. 2.—Comparison of the Data with the ordinary Law of Error.

The second figure is the important one. It requires a little explanation. The black and white parts are the errors which ought not to be—errors arising from imperfections in the plan of calculating. There must, of course, be many errors, in one sense, in these predictions of the day of confinement; but did the analysis of my 153 cases show no black and white, my plan would then be perfect. All avoidable error

would then be eliminated. A different plan of calculating might be discovered, but the present plan could not be farther improved. All this can be demonstrated by the laws of probable error.

If my method of calculating were perfect of its kind, the figure would have no black and white. In order to recognise how it would then stand, the student must not merely erase the black and white parts, and put the grey in their place. The white or blank represents excess, and white must be simply erased—the white parts entirely removed from the figure. But black represents defect; black parts, therefore, are not simply erased or removed from the figure, but the black is erased, and the general grey colour put in its place. The figure, as it then would be, gives the correct amount of error—the inevitable error.

The general appearance of the figure shows that in my 153 cases of prediction the amount of avoidable error is small. It gives at once such a view of the avoidable errors as would be very difficult and tedious to put into words.

Accumulation of cases will soon lead to the easy elaboration by this method of an absolutely correct method of calculating the term of a period whose length is indefinite.

"Your data," says Professor Tait, "though numerous in the sense of having been collected from your own observations, are rather scanty for the application of mathematical methods. I have, therefore, confined myself to a very simple species of interpolation, which

seems to be sufficient to extract from them their most important contents.

"When the numbers are plotted, as in the first figure, we notice some strange irregularities, the most singular of which are actual *minima*—7 and 14 days before, and 8 and 15 days after, your typical period. What these may mean (if they are *real*, and not due to mere defect of data) I cannot conjecture. If we suppose them due to defect of data, as I have no reason to doubt, there is still the curious fact that the errors in excess of the period are not merely more numerous than those in defect, but *they extend farther in time*. This must, I feel sure, be due to miscalculation on the part of some of the patients.

"By a tentative process, I find that all your numbers, irregular as they at first sight appear, with the exception of those last mentioned (which, for the reason given, I consider myself entitled to reject), accord fairly enough with the ordinary law of probability of error, provided we assign, as the true period, the second day before that given by your rule. Thus we obtain the following series, which is graphically represented in the second figure, white representing excess, and black defect, of observation as compared with calculation :—

Days . .	1	2	3	4	5	6	7	8	9	10	11	12	13	
Within . .	16	35	50	59	63	71	85	97	105	111	116	118	119	etc.
Beyond . .	15	34	47	56	65	76	90	98	104	108	115	125	130	etc.
Calculated .	15	30	44	57	69	80	90	99	106	112	117	121	124	etc.

The second and third lines are found for separate days by adding together each successive pair of your numbers, and the fourth is roughly calculated from the ordinary tables of Probability of Error. It would be easy to make the coincidence more exact, but the labour of the necessary calculation would hardly be justified by the extent of the data."

I shall now briefly compare some of my analytical statements with similar statements regarding the data of Ahlfeld, made in the elaborate paper already referred to.

Ahlfeld predicted the day of confinement by his own method in 1014 cases, of which he has the details in 915 instances.

In 30 of Ahlfeld's cases the day of confinement was exactly predicted, or about once in every 30 cases. My success was twice as great, the prediction in my cases proving exactly true about once in every 15 cases.

In 205 of Ahlfeld's cases the time of confinement was successfully though not exactly predicted—the event occurring not earlier or later than four days from the predicted day. This was a success in much less than one-fourth of his cases. My similar success was in 63 cases, or more than one-third of the whole.

Further, Ahlfeld points out that 465 of his cases, or less than one-half, showed not above 11 days of error in the prediction. Of my cases 120 showed not above 11 days of error in the prediction, or considerably more than two-thirds of the whole.

It is thus seen that, so far as the limited number

of cases can show it, my plan surpasses Ahlfeld's to a great degree.

I may add that, with a view to comparing his own plan with Naegele's, Ahlfeld calculated (not predicted) the day of confinement for 258 cases, of which he possessed all the necessary details, including, of course, the day of confinement. He found the average error to be, for his own method and for Naegele's, nearly 10 days. Mine was only $7\frac{1}{2}$. His own method proved a little more accurate than Naegele's.

Naegele's plan is to fix upon the seventh day from the first of the last menstrual period, and to predict the same day of the third next month, counting backwards.

Ahlfeld's plan seems to be to fix upon the seventh or eighth day from the beginning of menstruation as the day of conception, and to add to this 271 days.

Before concluding, it is necessary not to omit mention of a correction of one of my own practical recommendations. I have elsewhere said that the accoucheur may venture to predict the week of confinement, or to fix upon a day which is the middle of the week in which a woman is to be confined. Now, as the average error is about 7 days on each side of the event, it is evident that the accoucheur should not predict confinement in a certain week, but in a certain fortnight, or fix upon a day which is in the middle of the fortnight in which a woman is to be confined.*

* Spaeth, *Compendium der Geburtsk.*, s. 71, states the ordinary range of error as extending over 14 days.

CHAPTER VI.

PROTRACTION OF THE PERIOD OF PREGNANCY.

PROTRACTION beyond the common or natural term is a phenomenon which most obstetricians are now willing to admit. Protraction beyond the average term is a matter of course: it is only extraordinary lengthening of pregnancy, the child being alive, that is here spoken of. Although believing in its possibility, I am at the same time convinced that it is not so frequent an occurrence as late writers on this subject seem to think, and that many of the cases of this kind which are recorded have not sufficient evidence to support them. They are mostly based upon such signs as the disappearance of the menses, the sympathetic phenomena of pregnancy, and a physical examination of the uterus; all of which, it is needless to say, are abundantly liable to create misapprehensions and fallacious reasonings, and, singly or combined, can justify no absolute conclusion from them. The kind of evidence desiderated is that based on pregnancies produced by a single coitus, or when a last coitus is counted from: * but as protracted preg-

* See a case by Joynt. *Dublin Quarterly Journal of Medical Science*, vol. xlii. 1866, p. 380.

nancies are rare, such evidence must be very difficult of attainment. One great reason for discrediting the evidence of most of the cases recorded by authors is, that we hear nothing of great development of the uterus, or of large size of the child, or of the placenta, in such cases—results which, to say the least, might be expected. On the contrary, we find authors stating that in these so-called cases of protracted pregnancy the child is no bigger than usual, or is even smaller than ordinary. "Although in some of the cases of protracted gestation," says Dr. Montgomery,* "the child was of enormous size, it by no means follows that it should be so in all such instances; and, in point of fact, we find it expressly mentioned in some of them that the child was smaller than usual, as happened in one of Dr. Hamilton's cases; and Foderé says, that in three instances in which gestation was evidently prolonged, the children were under-sized and ill-thriven; while, on the other hand, the largest children are often produced where no extension of the term could have taken place." Dr. Burns also says† that "some causes which we cannot explain nor discover have the power of retarding the process (of gestation), the woman carrying the child longer than nine months; and the child when born being not larger than the average size." In further corroboration of these views the valuable observations on cows by Tessier have been cited, as showing that there was no marked

* *Signs and Symptoms of Pregnancy*, p. 82.

† *Principles of Midwifery*, p. 199.

coincidence of increase of size and weight of the fœtus with protraction of gestation. But this reasoning from analogy between the cow and woman appears to be very much overstretched, and there are evident reasons for expecting, *à priori*, that the period of gestation in woman should be limited on the side of protraction more than in the lower animals. Of these the strongest is based on a consideration of the adaptation of the well-developed nine-month fœtal head to the maternal passages, and the evils that are so well known to result from even slight disproportion between them. And, unless it be supposed that pregnancy is protracted for the special behoof of small and ill-developed children, it must be admitted that an extraordinary development of the fœtus is to be looked for in such cases. The acknowledged absence, then, of this extraordinary intra-uterine development is, for me, a strong evidence against the reality of many so-called cases of prolongation. On the other hand, the presence of this sign, in addition to others, is, in my opinion, powerfully corroborative of the supposed protraction in any instance. In illustration of this I may state that the best example I have met with of probable protraction occurred in a female who had borne several children, and who had previously always been correct in the calculation of the period of confinement from the cessation of menstruation. On the occasion in question she passed her calculated time four weeks, and before confinement, expressed her conviction, all the more strongly in consequence

of my incredulity, that she had passed her time a month. The labour was more tedious than usual, in consequence of the great size of the fœtal head. The child proved of very large size and advanced development. It weighed 10 lbs. 4 oz. The placenta was 2 lbs. in weight. Other cases similar to the above have come under my observation, some have been communicated to me by professional friends, and some are to be found recorded. Among these last I may cite the observation of Smellie, in which he says it was reasonable to suppose that the patient actually exceeded the usual term of gestation by four or five weeks at least. "Her labour was very tedious, though the pelvis was of a large size; but the child was very lusty, and the head squeezed into a longitudinal form. It was," he adds, "the largest child I ever brought into the world." *

In these cases the ordinary sources of evidence were confirmed by the evidently exaggerated development of the ova, the results of these protracted pregnancies. I have lately had under my care two cases in which gestation was, not without some reason, supposed to be prolonged, but which I reject from this category, because, although the ladies were in good health at the time of falling in the family way, yet the infants born were not at all larger than their

* For other cases see A. R. Simpson, *Edinburgh Medical Journal*, April 1864, p. 916; also Rigler, *Monatssch. f. Geb.* Band. xxxi. s. 324, 1868; also Cazeaux, *Traité de l'Art des Accouch.* 6me ed. p. 210.

former children. The ladies were sisters, and in each of them their calculation and mine was passed by nearly a month. The data founded upon were the cessation of menstruation and the occurrence of morning sickness. In both cases the respective nurses were residing with them for about a month before the supervention of labour.

Such cases as those of the two sisters just mentioned, and numerous other so-called cases of protraction, are easily explained by supposing simply that the menstrual flux, which should have occurred about the probable time of the fruitful intercourse, was suppressed; or, in other words, that the decidua prepared for the ovum destined to be impregnated did not as usual throw off the bloody fluid. In these cases we must suppose either that the suppression for this one period arose from some ordinary constitutional cause, or, what is more likely, that the fruitful intercourse, occurring shortly before the ordinary menstrual period, anticipated and prevented it. This phenomenon we believe to be not very rare, and to be sufficient to explain away many cases of protracted gestation. In further illustration of this circumstance, we must be satisfied with referring to those cases of pregnancy after a single coitus taking place shortly before menstruation, the coitus producing, firstly, the partial or complete suppression of the menses at the approaching period, and secondly, the fertilisation of the ovum discharged in coincidence with the suppressed period. Some careful observations of this sort are

recorded by Raciborski, Montgomery, Marion Sims, and others.*

Till the question is brought near to a settlement by the accumulation of instances of protraction, in which only a single coitus could have fertilised the ovum, or in which a last coitus is counted from, we must rely on individual opinions and individual instances. I have already expressed my opinion, and may add that I do not know of an individual instance of certainly protracted gestation in which the production was small and light in weight. Before authors, as Hamilton, Foderé, and Montgomery, can justly ask the assent of the profession to their opinion, they must give the evidences or proof of protraction in the individual examples relied on. It is not sufficient that they express their opinion that they were evidently prolonged, in a case where their opinion regarding the resulting small children is contrary to what might

* Such cases admit of another explanation, which was suggested to me by a distinguished non-professional friend, but I fear it can be called only a possible explanation, for no observation of any kind has been made which can give it any security beyond that of a remote analogy. Physiologists are now, since the publication of Bischoff's monograph, aware of the peculiar dormant condition of the impregnated but undeveloped ovulum of the roe. The old puzzle as to the period of fecundation in the roe, and the dispute between the physiologists and the sportsmen, is well given by Boner in his *Forest Creatures*. Now my ingenious friend acutely suggests the possibility of a like condition in woman. Of course, if it occurred in woman, it could only be as a rare exception to the general rule, because it is well known that the human ovulum begins to be developed as soon as it is impregnated, in at least the immense majority of instances.

naturally be expected. I have already cited cases, equally well ascertained, opposing the view of Hamilton, Foderé, and Montgomery. The opinion of Tessier, and his observations on cows and mares, can only, by analogy, be brought to bear on the physiology of woman. But, their value being admitted, it is to be remembered that his facts number only eleven, and that no average weight of calves and foals is adduced for comparison with the special instances. Tessier's examples, besides, are not all quite pertinent, for they include two classes of cases—namely, when a small fœtus was produced after a prolonged gestation, and when a large fœtus was produced after a short gestation.*

As such opinions and examples have interest in the present state of the question, I shall add some more. And, first, I shall refer to a paper by Mr. Annan,† who in his turn cites an example of a large child following a long pregnancy, recorded by Dr. Collins of Liverpool. Mr. Annan gives three good instances, and expresses himself decidedly in favour of the view which I defend. Discussing difficult labours, Dr. Tyler Smith remarks, " The largest children I have met with in practice have been in cases where, from pendulous abdomen, the fœtus had been retained

* *Mém. de l'Acad. Roy. des Sc.*, 1817, p. 18. In his work, *Von dem Mangel, etc., der Gebärmutter*, p. 308, I find a passage indicating that Kussmaul admits the possibility of a small fœtus resulting from a prolonged pregnancy.

† *Edinburgh Medical Journal*, 1857, p. 712.

beyond the full term."* From Joulin† I make the following quotation:—"Manvie, vétérinaire à Epe, observa une vache qui porta près de 16 mois. Le volume du veau, qui pesait 61 kilog., rendit le part impossible; on abattit la bête. En 1831, on reçut à l'École Vétérinaire d'Utrecht, une vache qui porta 15 mois moins 2 jours. Numan constate une gestation de 11 mois $\frac{1}{2}$ chez une vache. Le veau qu'on fut obligé d'extraire par morceaux pesait 80 kilog. Enfin, Gronier a observé une vache qui porta 12 mois. Le petit, extrait vivant, avait acquis le volume d'un veau âgé de deux mois. Ces faits peuvent paraitre étranges, insolites; mais, je le répéte, on n'a véritablement aucune raison pour en nier la réalité. Louis, et les autres adversaires des naissances tardives, invoquaient à l'appui de leur opinion, l'ordre immuable des phénomènes de la nature. Ce sont là des phrases vides de sens; car on constate à chaque instant que la nature s'écarte de cet ordre prétendu immuable. On à dit que la cause des grossesses prolongées, dépendait du développement imparfait du fœtus qui sejournerait dans l'organisme maternel jusqu'a ce qu'il ait acquis toutes ses aptitudes à la vie extrautérine. Il est possible que cette opinion soit parfois justifiée, mais l'obstétrique comparée nous prouve qu'il est loin d'en être toujours ainsi. Et je crois que dans un cas de grossesse prolongée bien avérée, il ne faudrait pas s'exposer à subir les conséquences d'un développement

* *Manual of Obstetrics*, p. 449.

† *Traité Complet des Accouchements*, p. 456.

exagéré du fœtus, surtout chez une femme dont le bassin aurait des proportions médiocres. Dans ces circonstances on pourrait poser avec Silbert (d'Aix), la question de l'accouchement provoquè. C'est un point pratique qui merite l'attention."

There is a large field here open for research, not only as to the connection of protracted pregnancy with development of the child, but on the connection of the age of the mother and of the number of the pregnancy with the duration of the process and the development of the resulting progeny—topics on which I have made some remarks in an earlier part of this volume (see page 65), and to which Frankenhœuser* has made a valuable contribution, which supports by statistics the view which I maintain.

The evidence of highest value which we possess in regard to the subject of the extraordinary prolongation of pregnancy is founded upon cases where pregnancy resulted from a single, or is counted from a last, coitus. The results of these cases go far to establish the well-founded opinion of Dr. Montgomery, that the cases most deserving of confidence are those in which the usual term was not exceeded by more than three or four weeks. But the cases referred to give us the interval between insemination and parturition, a period which I have elsewhere remarked requires a correction, which physiology has not yet enabled us to decide, for the possible interval between insemination and conception. In a practical and medico-legal point of view,

* *Jenaische Zeitschrift für Medicin, etc.*, 1867, § 185.

however, the interval obtained is of great importance. In the collection of cases of this kind (see p. 435), the longest duration found is in one case where the period was 293 days. The other cases of protraction will be observed by a reference to the table.

The theory of the duration of pregnancy is still unknown. Some authors, believing that labour comes on at the tenth menstrual period, explain the protraction by the female's having a longer menstrual interval than usual, ten of which will make up a period exceeding the usual term of pregnancy. Others have supposed that, from some cause, a female might miss the usual period, and go on to what would have been the next menstrual period, had she not been impregnated. Others have connected it with tardy development of the fœtus, with the influence of depressing emotions, etc. But all these are as yet mere hypotheses.*

* See also footnote, p. 462.

CHAPTER VII.

DR. MONTGOMERY'S OPINIONS.

In the *British and Foreign Medico-Chirurgical Review* for July 1854 there appeared an abstract of a paper, by Dr. Gustav Veit, professor of midwifery at Rostock, on the duration of pregnancy and other subjects. I have had no opportunity of seeing more of this essay than is contained in the midwifery report of the above journal. Dr. Veit has tabulated forty-five cases from Reid, Montgomery, Girdwood, Rigby, Lockwood, Lee, Desormeaux, Dewees, Beatty, Skey, M'Ilwain, Ashwell, Cederschjöld, and others, in which the date of impregnation appeared to be fixed by a single coitus. From this table it is found that the average interval between insemination and parturition (commonly called the duration of pregnancy) is 276·93 days.

Dr. Veit also collected a mass of observations in regard to the interval between the end of menstruation and parturition. From this collection he ascertained that the average extent of this period is 278·5 days.

These, and like conclusions, though widely published, had been subjected to nothing that could be called adverse criticism till the recent republication of

Dr. Montgomery's great work on the *Signs and Symptoms of Pregnancy.* This contains an essay on the period of human gestation, in which it is assumed, we shall see on what authority, that the natural period of gestation is forty weeks, or 280 days; and in which the following statement of the immortal Harvey is pronounced to be erroneous. We quote from Montgomery.

"The words of this illustrious man are these—'*Unquestionably the ordinary term of utero-gestation is that which we believe was kept, in the womb of his mother, by our Saviour Christ, of men the most perfect; counting, viz., from the festival of the Annunciation, in the month of March, to the day of the blessed Nativity, which we celebrate in December.*' This is a period of 275 days only; he then goes on to state, what does not appear to have any very obvious connection with the fact referred to, but is indeed rather at variance with it."

"'*Prudent matrons,*' he says, '*calculating after this rule, as long as they note the day of the month in which the catamenia usually appear, are rarely out of their reckoning; but after ten lunar months have elapsed, fall into labour, and reap the fruit of their womb the very day on which the catamenia would have appeared had impregnation not taken place.*'"

I defend the opinion of Harvey on the ground of the data afforded by Dr. Montgomery, and shall show that his assumption of 280 days as the natural period of human gestation is, to say the least, unfounded.

The period generally recognised under this name, and discussed by Dr. Montgomery, does not measure the real duration which extends from conception to parturition, but that other period extending from fruitful connection to parturition. It is this latter of which we now discourse.

Dr. Montgomery describes the natural period of human gestation as 280 days. Now there is no such thing known to obstetricians to exist in nature as a natural period of pregnancy, measuring a certain number of days. This may be considered absolutely demonstrated. The interval between fruitful coitus and parturition is known to us only as a variable period, of uncertain length, not merely in different individuals, but even in the same individual on different occasions. So far is Dr. Montgomery from having any authority for fixing 280 days as the natural period, that, in his own laborious collection of fifty-six cases, in which, he says, the day of fruitful intercourse was known, there are only four in which parturition certainly occurred on the 280th day. Obstetricians can only speak with propriety of an average duration. This is attainable by striking it from the largest collection of well-ascertained cases. This average is the nearest approximation that can be made to what may be called the natural period of gestation. The data afforded by Dr. Montgomery for arriving at this mean or average, or nearest accurate general statement of the interval between fruitful connection and parturition, the duration of pregnancy, are of different degrees of value.

The most trustworthy and valuable are undoubtedly those cases of pregnancy which date from a single coitus. They number twenty-five, and their respective durations are as follow:—263, 264, 265, 265, 267, 270, 271, 272, 273, 274, 274, 274, 274, 274, 275, 275, 276, 276, 275 or 277, 277, 278, 280, 280, 287, 291 to 293. Of these twenty-five cases the mean is 274 days. The best data accessible to Dr. Montgomery, then, give 274 days as the duration of pregnancy, not 280.

It appears to me that the next most valuable data for settling this point are to be found in his table of thirteen cases, dating from the day of marriage. The intervals between marriage and parturition in these cases were as follow:—261, 265, 268, 269, 270, 271, 271, 271, 272, 273, 274, 279, 291. In regard to these Dr. Montgomery himself says:—"The average interval between the day of marriage and that of labour was 272 days q. p., or thirty-nine weeks, minus one day; or, if we deduct the last case, which went to 291 days, the average interval would be $270\frac{1}{3}$ days." Where, then, one naturally exclaims, are the grounds for saying that the natural duration of pregnancy is 280 days? This group of cases is interesting as including only primiparæ, a circumstance which probably accounts for the special shortness of pregnancy in them.

Dr. Montgomery's work presents us with another table of data. It consists of fifty-six cases, in which, he says, the day of fruitful intercourse was known. Now, to us, this table, at first sight, and before esti-

mating the results of it, appears to be of less value than either of the two former. Every case, almost, is invalidated because we do not know the authority or grounds upon which it is said that the day of fruitful intercourse was known. We do not know even the observers' names. Dr. Montgomery has laboriously collected cases of protracted pregnancy, all of which, so far as available for this table, find place in it. The whole weight and importance of it is contributed by the distinguished obstetrician's name that publishes it. That authority is undoubtedly of the very highest, but can scarcely be communicable to cases derived from a promiscuous set of observers, whose reasons for decidedly fixing on a single day are not given. In an exact investigation like this all cases should be rejected except those dating from coitus on a single day. But let us examine and see what this table affords towards the solution of the question. Omitting six cases where a single day is not given, we have fifty where the interval between fruitful intercourse and parturition is said to be as follows :—242, 258, 258, 263, 265, 267, 267, 267, 267, 268, 269, 269, 272, 273, 273, 274, 274, 275, 275, 276, 277, 277, 278, 278, 279, 279, 279, 279, 279, 280, 280, 280, 280, 281, 283, 283, 284, 285, 286, 287, 287, 287, 288, 290, 291, 291, 292, 293, 293, 297. Of these fifty cases, all those satisfactorily known to Dr. Montgomery, the mean duration is 277 days. This table, framed under the conditions above described, yields a result opposed to the dogma of its author. Where, then, is the authority for stating

280 days as the natural period of gestation? It is nowhere.

We agree with Dr. Montgomery in his opinion that there is no other satisfactory method of arriving at the solution of this question but the one we have just followed—viz. the collection of well-ascertained facts and their analysis. "Independently (says he) of the very few cases in which we have satisfactory evidence of conception following casual intercourse, or perhaps a single coitus, we have no certain means of knowing exactly the commencement of gestation, and are obliged to form our calculation on one or other of three very fallacious grounds;" which he then proceeds to consider.

In the vast majority of cases the calculation of the day of confinement must be made from the termination of the last menstruation, for reasons which are well known. The average time to which a woman goes, after the last appearance of the menses, is 278 days (a period shorter than Dr. Montgomery's duration of pregnancy). This average is obtained by the collection of single observations and their subsequent analysis. If, then, we wish to ascertain the most probable day of a woman's confinement, we add 278 days to the last day of the last menstruation. The method of doing this, without a periodoscope, I have already shown.

Dr. Montgomery gives no specific directions for making this important calculation. But it appears, from some passages occurring incidentally in his essay,

that he adopts the following plan. Some day is selected after last menstruation as the most probable day of fruitful intercourse, and 280 days are added thereto. As the selection of this day must be, in almost every case, made on the most worthless and insufficient grounds, the resulting calculation must be similarly characterised. Besides, if there be any truth in the statistical data of Dr. Montgomery, and their analysis given above, which is partly his own, then this plan of his must lead to a putting off of the probable day of confinement to far too distant a time. For instance, we have in the table of observations dated from the day of marriage, thirteen cases on Dr. Montgomery's own authority. Now, in these, as already stated, the women went on an average only 272 days from the day of the nuptials. If a probable day of fruitful intercourse after marriage had been selected, and 280 days added thereto, in these cases, such a plan would have evidently led to a mass of errors in the way of putting off the predicted day of confinement far too long.

I may here mention that with the subject of this important calculation or prediction Dr. Montgomery has confounded the question of the interval between insemination and conception. If such an interval existed, he says, "we should have no means of calculating the period of gestation with anything like an approximation to accuracy in any case." Now, if there be an interval in nature between insemination and conception, we must adopt it, whatever results it

may lead to. If it truly exist, it can lead only to true and good results. It is not considered probable by Dr. Montgomery that any interval, or an interval of any importance, does exist. The highest authorities, however, on such a point, are unquestionably very strongly in favour of the belief in its existence, and its being of considerable extent, say several days. But in truth this question of a possible interval between insemination and conception has nought to do with the calculation of the date of confinement. Its truth or untruth does not affect such calculations, and no author but Dr. Montgomery has, so far as I know, discussed the two points as connected with one another in any way tending to modify practical precepts.

CHAPTER VIII.

HARVEY'S OPINIONS.

GREAT men often seem to arrive at the truth, even in circumstances of complication and difficulty, by some process so simple that it appears like an operation of instinct. The immortal Harvey's expressed opinions in regard to the duration of pregnancy, and the calculation of the day of confinement, bear this character, for we cannot discover the grounds on which he arrived at results so nearly identical with those of modern science.

The interval between the festival of the Annunciation and the day of the blessed Nativity is that adopted by Harvey as unquestionably the ordinary term of utero-gestation. This is a period of 275 days, Lady-day, or the festival of the Annunciation, being on the 25th of March, or 84th day of the year, while the day of the Nativity is the 25th December, or 360th day of the year. It is remarkable that the largest recent collection of cases made on certain or on the best grounds give also an average result of 275 days. Harvey, it will be observed, does not speak of any natural term, but only of the ordinary term, his correct appreciation of which is clearly indicated.

Harvey guards also his rule for calculating the day of confinement from being considered exact by saying that those prudent matrons who follow it "are rarely out of their reckoning." His statement is that after ten lunar months have elapsed from the commencement or appearance of last menstruation, they fall in labour the very day the catamenia would have appeared had impregnation not taken place. If the usual or average computation of the menstrual periods and intervals is adopted, the period of Harvey is 280 days, including the number of days of the last period. Ten times the usual interval and period of discharge—that is, ten times 28—gives 280 days; but as this includes the last period, of course the three, four, or five days of that period have to be taken from the 280 days, if we wish to find the interval he allowed between the end of last menses and parturition. Thus Harvey gives prudent matrons only an approximative calculation. The interval between last menstruation and parturition, according to him, is something a few days less than 280. The average time found by modern calculations, as stated in a former chapter of this part, is 278 days, with which Harvey's rules are as nearly in accordance as can be expected in a subject altogether incapable of any exact statement.

Dr. Montgomery's objections to Harvey's opinions are founded on the assumed accuracy of his own natural period of pregnancy—namely, 280 days after conception. We have already shown that this period is assumed on insufficient grounds, and that, as the day

of conception is never known, we must seek some other method of calculating the day of confinement than any founded on the supposed day of such an occurrence. Without seeking to disparage the very high value and authority of Dr. Montgomery's writings, we publish these comments, believing that they demand modification of the views enunciated in his essay on the period of human gestation.

In conclusion, we venture to state the following propositions :—

1. That the interval between conception and parturition (the real duration of pregnancy) has not been exactly ascertained in any case.

2. That the average interval between insemination and parturition (commonly called the duration of pregnancy) is 275 days.

3. That the average interval between the end of menstruation and parturition is 278 days.

4. That the intervals between insemination and parturition, and between menstruation and parturition, have no standard length, but vary within certain limits.

5. That while absolute proof of the prolongation of real pregnancy beyond its usual limits is still deficient, yet there is evidence to establish the probability that it may be protracted beyond such limits, to the extent of three or four weeks, or even longer.

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