Explanation of Plates I. & II.

PLATE I.

FIG. 1. *Brachycerus cinnamomeus.*
2. ,, *suturalis.*
3. ,, *eximius.*
4. ,, *disjunctus.*
5. ,, *capito.*
6. ,, *omissus.*
7. ,, *rixator.*
7*a*. ,, ,, rostrum.
8. ,, *Faustii.*
8*a*. ,, ,, rostrum.
9. ,, *præcursor.*
10. ,, *phlyctænoides.*

PLATE II.

1. *Brachycerus albicollis.*
2. ,, *electilis.*
3. ,, *obtusus.*
4. ,, *draco.*
5. ,, *turbatus.*
6. ,, *gryphus.*
7. ,, *strumosus.*
8. ,, *mærens.*

III. *Pedigree Moth-breeding, as a means of verifying certain important Constants in the General Theory of Heredity.* By Francis Galton, F.R.S.

[Read February 2nd, 1887.]

It was suggested by Mr. Merrifield, in answer to my inquiries, that moths, especially those which breed normally twice in the year, would be very suitable subjects for a course of such experimental breedings as I have long desired to establish. My object at the present time is to obtain *data* for the revision and extension of a general theory of simple heredity, on which I have lately published memoirs,* and especially to test that portion of it which relates to Stability of Type. In addition to this, the experiments I propose would elicit incidentally many interesting results, some perhaps quite disconnected with the objects immediately in view.

The merits of moths as subjects of experiment are that the arrangements for breeding them in large numbers occupy comparatively little space, and involve comparatively little cost; their generations succeed each other quickly, and they undergo no change in length or shape of wing, &c., during their brief lives, so that the difficulties elsewhere connected with age and growth disappear; the specimens that are used in the experiments can be afterwards mounted in cases, and be labelled and preserved for future reference.

Mr. Merrifield has very kindly offered to commence the experiments for me, and trusts to be able to continue them for some years. I lay the proposed plan before the Entomological Society in hope of eliciting the suggestions and help of its members before the course of experiments is fairly begun.

* "Law of Regression," Journ. Anthrop. Inst., 1885; "Family Likeness in Stature," Proc. Royal Soc., 1886; "Family Likeness in Eye-Colour," Proc. Royal Soc., 1886.

The intention is to start from the brood of a single pair of moths (*Selenia illustraria* is suggested), and to trace the changes of some one characteristic,—say of the wing-length,—during many successive generations, down three parallel but contrasted lines of descent; the broods being reared all along out of doors, and under healthy and perfectly identical conditions. It will be convenient to distinguish these three lines of descent by the letters A, M, and Z. The A line of descent is to be produced by selecting and mating one (or more) pair of the longest-winged males and females that are found in the brood of the original pair. The progeny of this selected pair I will call A i. Out of the males and females in A i. one (or more) pair of the longest-winged will be selected. The progeny of this selected pair will be A ii. Out of the males and females in A ii. one (or more) pair of the longest-winged are again to be selected; their progeny will be A iii. This process will be continued for, it is hoped, at least six generations,—that is, for three years. Every moth in each of the broods is to be preserved, and those which become the parents of the succeeding brood are to be labelled accordingly. It is intended to mount the moths in an orderly series, separating the males from the females, and severally arranging them in the order of their wing-lengths, beginning with the longest and ending with the shortest.

The Z line of descent is to be produced by selecting and mating one (or more) of the shortest-winged males and females out of the original brood. Their progeny will be Z i. Out of these a selection of one (or more) pair of the shortest-winged males and females will be made, whose progeny will be Z. ii., and so on as before. Moths which are small, owing to deformity or obvious unhealthiness, should not be selected for breeding from.

The M line of descent is to be produced by selecting and mating one (or more) pair of medium-winged individuals out of the original brood; their progeny will be M i. Out of these a selection of one (or more) pair of the medium-winged males and females will be made, whose progeny will be M. ii., and so on as before. It will be understood that by medium-winged I mean with reference to the brood in question, and not with reference to the original brood. The use of the M line is to

afford a standard whence the divergencies of the A and Z lines in each generation can most suitably be measured. M will be affected, together with A and Z, by all the influences that affect the entire stock, and will therefore exhibit in an unmixed degree the A and Z *peculiarities.* It will easily be understood how important it is to attend to the requirement already laid down that the three lines of descent should be carried on under *identical conditions.*

The broods in the M and Z lines will be mounted in the same way as those in the A line. Therefore for every generation there will be three compartments in one or more trays, each containing (say roughly) 100 moths, and, as there are two generations in a year, the result of each year's breeding will be to fill six compartments.

After the sixth generation or thereabouts has been reached in each of the three lines of descent, it is further desired to proceed conversely, by breeding from medium specimens in each of the three lines, and again from medium specimens of their several broods, and so on until all trace of the A and Z peculiarities shall have disappeared from their respective descendants.

I have spoken of one *or more* pairs, because the moths do not emerge simultaneously, and yet they must be paired soon after they emerge. The ideal pair in the A line would be the very longest-winged male and the very longest-winged female of the entire brood, and in the Z line the very shortest-winged. In practice we must be content with an approximation to this. Two or three separate matings will have to be made between the most suitable of the brood at the time when they require to be paired, but the eggs that are to be preserved and reared will be those of only one or two of the most suitable of the trial pairs. The produce of different pairs ought not to be mixed.

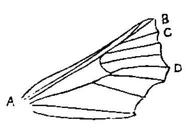
There will be little difficulty about the M pairings, as mediocrities are numerous.

The wing-length is the characteristic with which it is at present proposed to deal, as being more definite and easily measured than others. A similar treatment might be adopted in respect to other characteristics, such as the area of the wing, or the area of the patch of colour in the wings of such moths as may possess it.

A would in this case symbolise large areas, and Z small ones. Or the subject of experiment might be the depth of the general tint. Or, again, it might be the greater or less acuteness of the angle of the wing. Any variable characteristic that exists in both sexes and in all individuals can be treated in this way.

As there is no difficulty in treating the two sexes on equal terms in statistical inquiries by first transmuting all female measurements to their male equivalents [in human stature this is effected by adding one-twelfth or thereabouts; I use the multiplies of 1·08], so probably the different broods of dimorphous moths may be rendered comparable in the same way, and it may be found unnecessary in *Selenia illustraria* to confine the comparisons between spring-brood and spring-broods, and between autumn-brood and autumn-broods respectively. Thus the advantage of double-broodness, in giving two steps a year instead of one, will probably not be lost by experimenting on a dimorphous species.

Measurement.—The wing-length is to be measured from the root of the wing to its tip (that is, from A to B in the fig.). A has to be estimated from the run of the upper and lower margins, and can be determined with fair precision. B is very well-defined in a normal wing. Should, however, the tip be injured, the measurement is to be made from A to C, and stated accordingly. Again, if both B and C are injured, measure from A to D, and state so.



In performing the measurement with a pair of compasses (or other scale) it is necessary to bring the points very close to the moth, else a slight change in the position of the eye will give discordant readings; and if the eye is not moved at all the readings will be sensibly too small. A magnifying-glass is desirable. For measuring living but sluggish moths, I have contrived a pair of compasses that work well and quickly when making the ordinary measurements, and which carry a magnifying-glass. I will not, however, now give a drawing of it, because I want to assure myself of the handiest form of the instrument before doing so. Its principle is this. Imagine a pair of fine-pointed scissors with very long arms,—say five times as long as the blades; the points of the blades represent the compass-points. At the end of one of the arms a scale is pivotted; its free end runs over a pin at the end of the other arm. As the distance between the pivot and the pin is always five times that between the compass-points, the scale is five times the natural size, and can be read off easily to half millimetres, the pin acting as index. In using these compasses the forefinger slightly presses the scale against both the arm on which its free end rests and the pin over which it runs. When the compass is satisfactorily adjusted an increased pressure is sufficient to clamp the scale, and it can then be read off. I have also tried the glasses from one of the tubes of an opera-glass, with a lengthened interval between them, so as to form a microscope of very long focus, say 18 in. This was fixed on a light rod that carried a millimetre scale, set across its free end at a trifle less than 18 in. from the object-glass. On approaching the scale to within half an inch of any small object, that object and the scale are both in fair focus at once, and they are sufficiently far from the eye to render the error of which I spoke of little or no importance.

For the accurate measurement of dead moths I have a much better instrument under construction, in which I use a small microscope with cross wires in the short limb of a pentagraph, and use the long limb both for setting the microscope and for reading off the measurements.

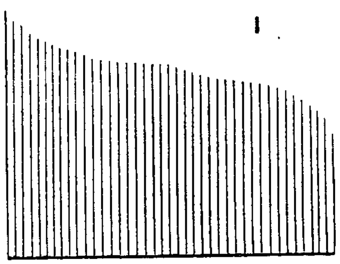
The details of the whole procedure are settled thus far only provisionally, as it is reasonable to hope that much has yet to be gained from the past experience of others, and more by the earlier stages of the experiments themselves, so far as they are new to experience. I have considerable hopes that many persons may feel disposed to work with me, for I am sure it will be accepted as an obvious truth by all, whether they may interest themselves in the technical explanations that follow these remarks or not, that sets of broods of pedigree moths, all of whose direct ancestors, male and female, and all of whose uncles and aunts, great uncles and great aunts, and so on for at least six generations, are preserved in convenient trays for reiterated study, would form a collection of first-class importance for hereditary investigation.

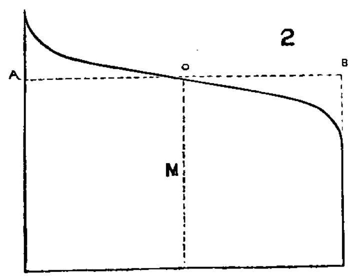
It would be well if in each case of experiment more than one stock could be reared, descended from the same original pair of moths, but in different places, in order that the male and female pupæ may be occasionally interchanged and the moths cross bred, by which the evils of too close interbreeding would be diminished. I sincerely hope that any practical entomologist who may be disposed to further these experiments will communicate with me at 42, Rutland Gate, London, S.W., or with Mr. Merrifield.

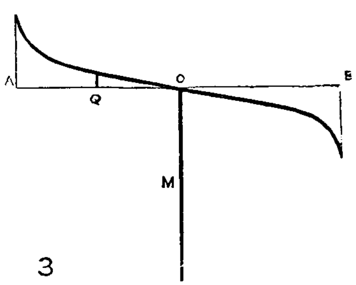
APPENDIX.

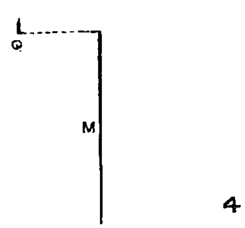
ON THE PROPOSED METHOD OF DEALING WITH THE OBSERVATIONS.

After the results of the experiments have been obtained they will be treated by me in somewhat the same minute and technical way that the data I now possess were treated in the memoirs









already referred to. It may perhaps be well to give here a brief and partial account of the guiding principles.

When we represent the wing-length of every specimen in a large brood by a corresponding vertical line, and arrange these lines in an orderly manner according to their lengths, and at equal distances apart upon an horizontal base, we shall obtain a figure like that shown in fig. 1. Here, however, only some twenty vertical lines have been drawn, sufficient to indicate what is meant. It is not necessary to make a larger or more minute drawing for the sake of mere explanation. The variability of wing-length, as indicated by the difference between the lengths of the longer and shorter lines, has been purposely exaggerated to make the meaning of the figures more clear.

In fig. 2 these upright lines had been enclosed within a dotted boundary whose vertical sides should lie at an interval of one-half space before the first and beyond the last of the lines respectively, and whose upper portion is a smooth curve drawn with a free hand, touching the tops of the vertical lines; then the vertical lines are supposed to have been rubbed out, and only the contour or "scheme" to remain. This scheme contains, in a most compact form, the measurement of every individual in a brood, or in a population however large. A dotted vertical line, or ordinate, M, is drawn to the curve from a point that bisects the base, and a horizontal line is drawn through the point O in the curve where this ordinate, which is the "median" of the curve, meets it. The median is practically the same as the average, and it is clear from the construction that its value is quite independent both of the width of the scheme and of the number of individuals to which the scheme refers, so long as they are fairly numerous. The horizontal line A O B is the "axis" of the curve; it divides it into symmetrical halves. In fig. 3 the nearer half of the axis A O is itself bisected, and an ordinate Q is drawn to the curve from the point of bisection. Q is what I call the "Quartile" of the curve.

In fig. 4, M and Q are all that remain, and they are all that we are mathematically concerned with. When they and the interval between them are given the whole of the scheme can be calculated; but the interval between them is unimportant for the objects in view. It does not in the least matter on what horizontal scale the scheme is drawn, as the values of Q M and other ordinates at different *fractional* divisions of the axis are independent of the horizontal extension. Q and M are the only values in which we are interested, and it is with these that I work.

It has been abundantly shown by many, from Quetelet onwards, and all my own many statistical inquiries confirm the view, that the curve in the above figure* is, or at least tends to be, of as definite a character as an ellipse or any other familiar curve; that it equally admits of mathematical definition, and that it possesses peculiar properties of its own that are of the highest importance to statistical inquirers. Just as an infinite variety of ellipses may be drawn on the axis A B, differing from one another in their extension above and below A B, but otherwise preserving the well-known proportion of an ellipse; so may an infinite variety of *curves of normal variability* be drawn on the axis A B, differing from one another only in the amount of their extension above or below it, as measured conveniently by the length of Q. They will all maintain the proportions shown in the table below, which refers to the middle nine-tenths of the curve. The twentieth part at either end is sure to become irregular. O A is supposed in the Table to be divided into 50 parts, the division at O counting as 0°, and that at A at 50°; then, when Q is taken equal to 100 units, the several ordinates drawn from the principal divisions on the axis are of the lengths shown in the table. For any other value of Q all the tabular values must be changed in uniform proportion to the new value of Q.

Division.	Length of Ordinate.
0	0
10	38
20	78
Q 25	100
30	125
40	190
45	244
50	indefinite

As the curve is symmetrical the same measurements apply to either half of it, but in the one half they are made from the axis upwards, in the other half they are made downwards.

If the curve derived from a series of measurements of any variable characteristic is found on trial to conform fairly well with these proportions, it may be assumed that the characteristics in question vary "normally," that is to say, according to the recognised laws of chance, which specify the relative frequency of runs of luck of different lengths. Again, in so far as they vary normally, all the properties of the laws of normal variation may justly be assigned to them. It was by the use of these laws that my deductions were made.

* The curve actually used by these writers is of another kind and has another signification, but for all that it is the basis of the curve that I employ.

I have exhibited to this Society, as an example of the law of variability, a row of about 100 pods of sweet peas, the produce or brood of a single plant, which I had arranged edgeways, like the vertical lines in fig. 1. Their outline expresses very distinctly the peculiar shape of the curve of variability.

The object of preserving the entire brood of moths is to obtain careful after-measurements from which to deduce the values of M and of Q in each case. When this is done we shall be able to deal with each group in its entirety, and to submit it to mathematical treatment.

The data I have hitherto used in my inquiries were rarely derived from more than three generations, but the condition of statistical constancy in the peculiarities of a population, of which I will again speak, enabled me to extend their scope. They sufficed in this way to lead to many interesting, though perhaps only approximative, results. One is that each parent contributes, on the average, one quarter of the total hereditary peculiarities of the child, each grandparent one-sixteenth, and so on. In other words, that the two parents together contribute one-half, the four grandparents a quarter, the eight great-grandparents one-eighth, and so on, the whole heritage being thus accounted for. But when none of the progenitors besides the two parents are known, their implied peculiarities must be taken into account. They admit of being calculated, and have to be allowed for in the form of an increase to the hereditary contributions of the parents. It is found that each parent should in that case be held to contribute one-third; the difference between one-third and one-quarter (or one-twelfth) being the amount of the implied heritages. It is, however, highly probable from other considerations that, though this simple formula may be closely true for the parents, and nearly true for the grandparents, it may become sensibly and increasingly different for remoter progenitors. It is this that I want to investigate, chiefly through inquiries into Regression. Moreover, all theory concerning the cause and character of Stability of Type, and of much else of high interest in any general view of Heredity, must be based upon the facts of Regression, which such experiments as those proposed can alone, so far as I see, be likely to declare in a trustworthy way.

The laws of simple heredity, as I made them out, involve only five constants. These admit of being separately determined, and they are at the same time connected by an equation that serves to verify their observed values. The equation depends on the fact alluded to, that successive generations of the same population yield identical biological statistics, although each family, or brood, is full of variations, and although the "median" of each characteristic in each brood is on the average *always more mediocre* than the corresponding characteristic in the mean of the two parents. The first of these events, "fraternal variability," increases the variability of the population as a whole, and the latter event, which I call "Regression," decreases it; the two can be shown to counter-balance each other, and give rise to a position of stable equilibrium. The five constants are (1), the Median of the race; (2), the Quartile of the race; (3), the Quartile of the broods of the same parents, *i.e.*, brothers or sisters; (4), the Quartile of broods of a large number of *like* parents, mixed together in a single group; (5), the coefficient of Regression. The laws in which these constants play a part give calculated results that prove to be closely true to observation in the ordinary cases of simple heredity, where there has been no long-continued selection, but it does not at all follow that they will hold true for the descendants of a long succession of widely divergent parents. It is this that I want to test. The point towards which Regression tends cannot, as the history of Evolution shows, be really fixed. Then the vexed question arises whether it varies slowly or by abrupt changes, coincident with changes of organic equilibrium which may be transmitted hereditarily; in other words, with small or large changes of type. Moreover, the values of the Quartile in (3) and (4) cannot be strictly constant, and are probably connected in part with the value of the median, and require a modified treatment by using the geometrical law of error instead of the arithmetical one (Proc. Royal Soc., 1879). Again, the diminution both of fertility and of vitality that accompany wide divergence from racial mediocrity have yet to be measured, by comparing the A and Z broods with the M broods. It was assumed not to vary in the approximative theory of which I spoke.

IV. *Practical suggestions and enquiries as to the method of breeding* Selenia illustraria *for the purpose of obtaining data for Mr. Galton.* By Frederic Merrifield.

[Read February 2nd, 1887.]

It being necessary for the purpose of these experiments to bring up in a healthy state nearly all the individuals in every successive brood, instead of merely to obtain a fair number of cabinet specimens,—the usual object of larva-breeders,—more than usual care will be necessary to avoid dwarfing and casualties. No apology, therefore, is offered for submitting the following detailed suggestions,—the result of answers kindly given to many enquiries,—while at the same time further information is invited.

Selection of Species.—The species chosen should be variable in size and easy to rear, pair, and measure; and regularly double-brooded species have the great advantage of reducing by one-half the period required for bringing the experiments to an end. After fully weighing objections, the writer has determined to try a species of which he has considerable knowledge, *Selenia illustraria* (the Purple Thorn), adding to his preliminary experience by practising on the common *S. illunaria*, which appears a month earlier. Other double-brooded species are recommended, viz., the *Ephyras* (especially *E. pendularia*), *Drepana falcula*, and the *Closteras;* among single-brooded moths some of the common and easily bred Bombyces, especially *Hypogymna dispar*, and that variable, interesting, and easily reared Geometer, *Angerona prunaria*. Those who can get over the difficulty of pairing which the butterflies generally present would probably find the common and easily fed and double-brooded *Pieris brassicæ* very suitable.

It is much to be desired that the same species should be taken up by more than one observer, as in this way the accidental failure of a brood may be guarded against, and provision may be made for occasional cross-breeding to promote the vigour of the breed. Observers living in countries where there is a large choice of double-brooded insects, especially where the climate is such as to admit of three or four broods a year, have great advantages. Under such conditions probably some of the silk-producing Bombyces would be favourable subjects.

Apart from the bearing of the experiments on the theory of heredity to be elucidated, they can hardly fail to throw light on many problems in evolution, such as those which have been investigated by Prof. Weismann, as well as to furnish facts of interest in the life-history of the insects chosen for experiment;—such as the proportion of males and females, and the order in which they appear; the time of the day or night when they emerge from pupa; the time when they are most active on the wing, or in feeding as larvæ; the number of eggs laid; the duration of life in the several stages; the influence of temperature, moisture, and food-plant, &c.;—facts many of which have a general interest for entomologists, but which are rarely recorded with completeness and accuracy by those who only breed for cabinet purposes.

Attention is called to the importance, whatever species is chosen, of starting with a healthy original stock. Pupæ freshly dug or obtained from larvæ found wild, or eggs from moths caught wild, are preferable, as there need be no apprehension that they are suffering under defects engendered by feeding under artificial conditions or by interbreeding. In any case the origin or history of the stock with which the experiments are begun should be known. A supply from a foreign country does not appear to be objectionable, if the species obtained there is known to have the ordinary appearance, size, and habits of the native specimens.

The writer will be greatly obliged to any entomologist, having had practical experience in the rearing of larvæ, for any suggestions tending to ensure success in the experiments determined on. He would be particularly obliged by answers to the following enquiries:—How long can such moths as the *Selenias*, spring and summer broods respectively, be kept alive and quiet, and how can this best be done? Can any better plan be suggested than that of subjecting them to cold and darkness, and

what lowness of temperature will they bear without injury? Are the *Selenias* ever found feeding on flowers, or are they known to feed in confinement? Are the pupæ killed or injured by severe frosts? — Please address replies to 24, Vernon Terrace, Brighton.

APPENDIX.

General Treatment.—For the success of these experiments it seems very important to observe the principle that the insect should, all through its life, be subjected, as far as possible, to the natural conditions in which it exists, while protected from its natural enemies and from casualties. For example, *S. illustraria*, in a state of nature, lives in a much cooler, moister, and fresher air than it does when bred in-doors; its food-plant is exposed to similar influences, to a much stronger light, and its leaves are never in a flagging condition. On the other hand, the wild insect is subject to the depredations of birds, mice, earwigs, beetles, ants, ichneumon flies, and other predaceous and parasitic insects; and is exposed to injury by storms. These considerations point to the following precautions.

Larvæ.—Feed the larvæ, when practicable, on growing trees out of doors, confining them by "sleeves" of the material that most readily admits light and air, but is close enough to keep in the larvæ and exclude insect enemies; muslin, leno, and calico have been recommended for the purpose. This mode of out-door feeding saves some trouble, but does not dispense with frequent supervision, especially in rough weather. The sleeves should be frequently examined; snails and slugs will sometimes eat holes in them. Shelter may be necessary in stormy weather; for this reason, dwarf trees and trees in pots are advantageous. In town gardens a fencing of wire-netting is often necessary as a protection against larger animals. The sleeve should be opened frequently to remove the "frass" and shift the larvæ to fresh quarters, and folds in which the larvæ may get entangled should be avoided. When they are nearly full grown it may be expedient to remove them to a breeding-cage. The larvæ of *S. illustraria* seem to move about in the latter part of the afternoon, but not in general to feed till night. They should not be crowded.

Until the larvæ are so large that they cannot crawl through the interstices of the sleeve they may either be fed on a potted tree in-doors, or, perhaps more safely, in a glass-cylinder, such as is described in books on larva-breeding (Rev. J. Green's 'Insect Hunter's Companion,' Dr. Knaggs' articles in the early volumes of the 'Entomologist's Monthly Magazine,' &c.), or in a jar. A simple and handy one is a jam-pot, with the edge ground down smooth and covered with a piece of very fine muslin, held in place by an elastic-band, and having a piece of plate-glass laid over the top, and occasionally shifted aside for ventilation, and to prevent the formation of drops of water in which the larvæ may drown. Though the pupæ is described as subterranean, it generally spins up between leaves.

If growing trees are not available, the larvæ should be reared in a breeding-cage, standing in a cool airy place out of doors, well supplied with food standing in water-bottles, the necks tightly stuffed with moss, &c. The cage should have a thorough draught, and for the sake of the healthiness of the food-plant it is recommended that the top at least should be of glass. There may be an inch of fine light earth covered with a layer of moss, often renewed. All moss and earth should be baked to kill enemies. *S. illustraria* will eat birch, oak, ash, hawthorn, sallow, and alder, but the young larvæ sometimes will not take to all of these, therefore it may be expedient to try them with more than one, and when they are found to thrive on it they should be kept to it. It is stated to be best to cut their food from the same tree and the same side of it. *With the precautions suggested*, and a very frequent renewal of the food-plant, it is believed that the larvæ may be brought up in out-door breeding-cages almost as successfully as by "sleeving" them on growing trees. An occasional moistening of the food with soft water from a scent-spray is useful, especially if the breeding-cage is in an airy situation and the air is dry; but in this case particular care should be taken to remove the layer of moss with the frass and dead leaves collected on it, and at the first symptom of mould or mildew the process should be stopped. All the broods (long-, medium-, and short-winged) should be given the same food-plant and treated in the same way.

Pupæ.—As to the pupæ, it is believed the best way is to keep them out of doors, sheltered from rain, and laid in their slight cocoons on earth covered with moss, and prevented from drying up by placing the box containing them in a situation which will cause the earth to be slightly moist at bottom, or, if this cannot be arranged, by occasional watering with soft water from a scent-spray. Though these pupæ are generally found naturally in dry situations, they are there continually exposed to more or less moist air, and are in the winter rarely removed more than an inch or two from moist earth; and it has been found that if the leaves in which they are spun up become quite dry, the moth has a difficulty in coming out. The pupæ should be occasionally looked at, and a watch kept for insect enemies, including the larvæ of the Tineæ. It would be prudent to protect them against severe frost.

Moths.—Double-brooded moths are very apt to be influenced as to the time of their appearance by temperature. In an unusually warm season if kept out of doors, and in an ordinary season if kept in-doors, some of the moths will sometimes come out, or the caterpillars will spin up too soon, and the moths from them may come out as a third brood. Conversely, if the temperature be very low some of the larvæ of the spring brood will feed up very slowly, and some of the pupæ go over to another year, instead of coming out as a second brood. Any risk of this kind can generally be obviated by moderate watchfulness, and by moving the insects to a cooler or warmer situation, in or out of doors, as may be best.. In the South of England *S. illustraria* should appear from the middle or latter part of April through May, and the second brood (its pupa-stage lasting only two or three weeks) in August; any material departure from these dates that may actually appear, or be threatened by the rapid or slow progress of the larvæ, should be counteracted by shifting to cooler or warmer quarters.

Two pairs of each of the sizes (largest-, medium-, and smallest-winged), will probably be enough to produce the required number of eggs, and allow for casualties. After these pairs have been selected and have mated, the rest of the brood should be killed, set, and arranged in a drawer or store-box, according to size, the males and females separately; the breeding pairs, when they have laid their eggs, being set and put in their proper places with the rest, but labelled. Each successive brood will of course be kept separate from all the others. To ensure mating, the pair should be placed in a round bag of muslin, &c., over a fresh spray of the food-plant. A rather warm and moist air seems most conducive to activity in the winged state.

To keep moths long in a living state they should be in a moist air, and have access to honey diluted with water, best supplied by soaking little pieces of sponge in it. A single female of *S. illustraria* may lay 100 eggs or upwards. The female is apt to scatter her eggs over the bag if left in it; if transferred to a jam-pot and supplied with crumpled paper, she will *probably* lay in the creases, which can then be cut out and attached to the food-plant as the hatching period approaches. As it is necessary to preserve all, or nearly all, of a brood of 50 or 100 moths in an unpaired but healthy and vigorous condition till the whole brood is out,—a period which, under ordinary circumstances, may last several weeks, especially with the spring brood,—provision should be made beforehand for this purpose. The males and females should be separated in the pupa-stage (in all the species named they can easily be discriminated by the different appearance of the antennæ, aided by the different size of the abdomen). As soon as the first moth emerges the remaining pupæ should—without being exposed to a high temperature or to sunshine—be placed in a warm room, and the moths, as they emerge, be placed in a cool moist place, and there be kept in absolute darkness. A refrigerator, or a zinc cover constructed on the evaporating butter-cooler principle, may possibly have to be used here. For convenience in removing the moths without exciting them or injuring their claws, it seems best to place each pupa in a separate chip-box, having a black net lid. This is the course which the writer intends to follow. The moths can be roughly sorted by the eye, but for the sake of greater accuracy a pair of compasses should be used. (*See* the remarks on these in Mr. Galton's paper.) If the species to be measured is small or lively, it may be necessary to temporarily stupify it by placing it under a glass with a few drops of chloroform on blotting-paper.

V. *Description of a new species of* Synchloë *from Kilimanjaro*. By Philip Crowley, F.L.S., F.Z.S., &c.

[Read February 2nd, 1887.]

Plate III.

Among some butterflies Mr. Watkins submitted to me the other day, received by him from Mr. J. M. Johnston, and collected by his brother Mr. H. H. Johnston during his visit to Kilimanjaro, I found a new species, which I propose to describe under the name of *Synchloë Johnstonii*. It is very nearly allied to *Synchloë hellica*, but differs from it in both sexes in having the wings more pointed at the apex, and in the costal margin of the fore wings being longer; the black on the apical area covers rather a larger space, and the four white spots in it are more definite. The marginal black spots in the hind wings of the male are larger than in *S. hellica*, and the spot near the inner margin of the fore wings in the female is only represented by a few blackish scales. The male type is in my own collection; the female in that of the Natural History Museum, which also contains a male. Expanse of wing, 1·9 to 2·0.

In the accompanying plate (No. III.) are represented the male, female, and under side of *Synchloë Johnstonii*, and also the upper and under side of the male of *S. hellica* for comparison.

Explanation of Plate III.

Fig. 1. *Synchloë Johnstonii*, male.
2. ,, ,, female.
3. ,, ,, under side of male.
4. ,, *hellica*, male.
5. ,, ,, under side of male.

V. *Report of Progress in Pedigree Moth-breeding to Dec. 7th*, 1887, *with observations on some incidental points.* By Frederic Merrifield, F.E.S.

[Read December 7th, 1887.]

Plate V.

It will rest with Mr. Francis Galton to describe at the proper time and place the results of the experiments in pedigree moth-breeding which I have commenced for him, if they should be carried to a successful conclusion; but in the meantime I am encouraged by him to write a sort of report of the progress hitherto made, and I think it is possible that the facts already observed may throw light on some points that are frequently subjects of inquiry and discussion in entomological and other periodicals. There are many of these points on which I have noted facts that may hereafter prove useful; but there are not many on which the observations made have been carried far enough to justify me in occupying the Society with them, and as to these I bring them forward partly in the hope of receiving suggestions from investigators qualified to offer them by scientific training and a lengthened experience, to neither of which I have any claim.

Having obtained an abundant supply of *S. illunaria* (*bilunaria* of the 'Entomologist' list) much earlier than of *S. illustraria* (*tetralunaria* of that list), I was led to try more experiments with the former than I had at first intended. I determined, in particular, to try the effect of forcing, partly in the hope that if success attended these efforts the period necessary to obtain pedigree results would be much shortened, and partly because I thought it would be interesting to know the effect that would be produced by forcing a rapid succession of short generations on an insect which in the natural state has in temperate climates only two generations, one covering four or five months mostly warm, the other seven or eight months mostly cold, each of these naturally alternating broods presenting such differences in size, colour, depth of hue, and, it is alleged, form, that until one was bred from the other the two were considered distinct species.

In describing my experience with the several groups successively experimented on I begin with those brought up under conditions most nearly resembling natural ones, as they will afford a convenient standard of comparison with such as were reared under more artificial circumstances. I therefore commence with those *illunaria* which were "sleeved" on growing trees. I am inclined to think that—except in the favourable circumstance that they were more effectually protected from enemies—the sleeved larvæ differed so little in their surroundings from wild-bred ones that they may be taken as fairly representative of the latter. I should, however, mention that there was one period of their lives during which nearly all the sleeved insects were subjected to a higher temperature than the natural one. In order to bring the moths out as closely together in point of time as possible, when the first moth appeared, the remaining pupæ were at once put into the forcing-box. I am not sure that this was necessary, especially with the summer brood of moths, for my experiments lead me to think that healthy individuals of this species, if kept in the dark, will live for ten days and more in summer, and for two or three weeks or more in colder weather, without any impairment of their functions, and only in rare instances will flutter so as to damage the tips of their wings enough to prevent convenient measurement.

Some preliminary explanations are necessary as to general treatment, and as to the sense in which I have used various expressions. My reason for being a little particular in these explanations is that any value such experiments as I am describing may possess depends entirely on a knowledge of the conditions under which they were tried. I have not knowingly burdened the narration with any statements, except such as seem to have some bearing or possible bearing on the results obtained. By "eggs," unless otherwise specified, I mean *fertile* eggs; and by the expression "fertile," as applied to the Selenias, I mean such as turn red, though many that go through the red stage and even the black one, which indicates that the young dark-skinned larva has been fully formed, often fail to hatch. As to the expressions

"larval" and "pupal" periods, I must explain that I found it impossible to observe, except on a few occasions, the actual date of pupation which, barring accidents, takes place inside a leaf carefully sewn together. But with the daily or almost daily, however brief, observation I was able to give, it was easy to see pretty well when a larva began to spin up, and consequently I have taken that time as the dividing line between the larval and the pupal periods. I found on several occasions, when the pupal period as thus defined lasted but eight or nine days, the larva remained in an unchanged condition for two days and more.

In my record I have found it expedient to note the period when "nearly all" had, as larvæ, spun up (*i. e.*, begun so to do), or, as moths, had emerged, because some 4 or 5 per cent., more or less, generally lagged behind the rest, from weakness of constitution I rather think. Excluding these laggards, I think the largest individuals of a brood were mostly to be found among or in point of time near to those that were longest in feeding up, and consequently in emerging. About 5 per cent. of the loss in my larvæ after I had first counted them after hatching may, I think, be ascribed to casualties, such as being squeezed or snipped or accidentally lost.

The pupæ were in all cases taken out of their cocoons and placed each in a separate chip box covered with black net, which was held in position by the rim of the lid, from which its top had previously been removed. These boxes stood on wire trays in crates and as the moths emerged were moved to crates kept dark by zinc covers standing in the cool room described later, near the window, almost always kept open, the sexes being in separate crates. I generally found the moths, especially *illunaria*, "out" when I came into the room in which they were kept, about 7.30 or 8 a.m., but some, perhaps 20 to 40 per cent., would come out during the day, rarely after 5 p.m. There is a very great difference between *illunaria* and *illustraria* in the resting position. The former rests with wings folded closely together over its back, as butterflies do. *Illustraria*, on the other hand, rests with the anterior edges of its fore wings at an angle of 60° or so to each other, the wings being all very much curved and the folds in them very wavy, and the abdomen brought into line with them, so that the insect has somewhat the appearance of a curled leaf with the concave side upwards. It would be interesting to know the position in which the other English species of the genus, viz., *S. lunaria*, rests. At first I fed the moths from little pieces of sponge dipped in very thin syrup, but I gave this up, as it seemed to promote mouldiness, and I do not think the moths lived any the longer for it. I never saw them feed, but had little time for watching them.

When "nearly all" the moths had emerged they were measured on their under sides, the wings being folded together over their backs. The length of the fore wing was measured from its tip or extreme anterior point (B in Mr. Galton's figure, *ante*, p. 22). The other or shoulder extremity is not so easily ascertained or described, and at first the search for it gave me some difficulty; but after a certain amount of practice I found that when a strong light fell obliquely along the wing in the direction from the tip towards the shoulder, it brought out a little dark transverse crease, in some cases shortened almost to a point, between the root of the hind wing (which, viewed from the under side, of course overlies the fore wing) and the body, and this crease I made my other terminus, taking the precaution of always laying the insect to be measured in the same position. This was done by fastening on the surface of a sheet of cork two strips of the same at about five-eighths of an inch apart, so as to leave a shallow flat groove of that width between them, and laying the moth on its side in this groove, a thin wedge of wood sheathed with zinc being pushed along the groove so as to support the wings, especially their outer edges, and the wings being held down with the usual cork setting-bristle. A pair of screw-compasses was then taken, one leg fixed on the zinc at the tip of the fore wing that lies uppermost, the other leg adjusted to the crease by turning the screw, and the length was marked off on a millimetre scale. The habit of the Selenias to bend their wings backwards when at rest facilitated the task, but I found chloroform indispensable; applied in the form of vapour by a few drops on blotting-paper under a bell-glass just long enough to produce insensibility, it did not seem to hurt the insects in any way. The use of a pair of spectacles strong enough to bring my eyes to see clearly at five or six inches distance from the object was sufficient to enable me, as I judge, to *estimate* differences amounting to the tenth of a millimetre, and I do not think all sources of error taken together would much exceed a quarter of a millimetre. It was not, however, until after I measured my sleeved *illunaria* on the 22nd July that I attained to this amount of accuracy, and therefore my earlier measurements must be taken as only approximate; but I think the *general* results are not far wrong. I should add that the "crease" cannot always be found, especially where the moth is very hairy; experience will tell the observer where it should be, and if the same person always measures, not much addition need be made to the percentage of error on account of the absence of the "crease." All measurements are of one wing only, so that the "expansion of wings" would be double the measurement given, plus about 3·5 mm. for the width of the body between the wings at the point measured. The "expansion of wings," however, measured from tip to tip of a moth set in the English fashion, would be about 1 mm. less than double the expansion of the single wing, owing to the inclination downwards and forwards.

After the moths had been measured, they were paired off in cylindrical muslin bags kept open by wire frames, each about 8 or 9 inches by 5; these bags, except where otherwise stated, were kept on a shelf outside the window of a cool room facing W.N.W., and protected from heavy rain; and there the moths laid their eggs, generally scattered over the muslin, and preferably in folds. I gave up inserting sprigs of the food-plant, as I found they rarely took any notice of them. The eggs, which will bear rough handling, were detached by hand or by the back of a knife, &c.

Nearly all the facts recorded are from my own personal observation, as I did not leave home for more than three or four days at a time, except during the last ten days of September and less than a week at the end of October, and on these occasions I had an efficient *locum tenens*, who had acted as my assistant at other times.

I have a more or less full record in most cases of the number of eggs laid, the number hatched, the number of moths that pupated and of moths of each sex that emerged, with dates and measurements, all of which may be useful for reference before the experiments are brought to a close, and which will, I hope, be dealt with by

Mr. Galton, so far as they bear on his studies in heredity. In this paper I propose only to give a *résumé* of facts observed in the different broods, for the information of those who are interested in investigations of this nature. All the moths have been preserved, and are labelled, and I have brought with me some specimens of the various broods. It will be seen that in the case of the forced *illunaria* I have had to do with as many as five successive generations in the year. In speaking of a "generation" or "brood" I reckon it as beginning with the egg; in this sense I have had to do with the first generation only in its latest, winged, stage. It is proper to remember that the succession of broods would have been still more rapid than it has been had I paired off the moths as soon as I had a couple; the delay necessary for making a selection added about a week of time. I have had actual experience of the following periods—egg 7 days, larva 16 and pupa 8 days, pairing and laying 2 days, total 33 days; and I am satisfied that it would be possible to run a generation through from egg to egg in 35 days.

I exhibit a diagram,* which will be a guide to the observations that follow, and will save much detailed description. It shows the connection of all the broods reared, and marks the extreme duration of life in the egg, larva, and pupa, and the duration of life in the moths from the time that the first appeared until the selection was made for breeding from. The moths so selected generally lived from 7 to 14 days; the others were killed and preserved.

THE EXPERIMENTS WITH S. ILLUNARIA.—The spring of 1887 was, as all will remember, a singularly cold and backward one. No *illunaria* were taken for me till 12th April. I bred from two females taken near Brighton on the 29th April and 2nd May respectively by Mr. A. C. Vine, who kindly gave them to me, and from two females taken on the 2nd May in the New Forest by Mr. Charles Gulliver, of Brockenhurst. They laid from 48 to 133 eggs each. Some of the eggs laid by them were used for preliminary trials. There were 271 left. I divided each of the four batches into three, and, mixing together one-third from each batch, obtained three lots of 90, 90,

* See Plate V.

and 91 eggs for sleeving, bottling, and forcing respectively.

SLEEVED ILLUNARIA.—*Preliminary.*—The eggs were placed in sleeves on young birch-trees not exceeding three feet in height. Though the trees were only planted last December they were in so good a condition for moving, and were so carefully removed, that the summer foliage seemed scarcely checked by the operation. My back garden, in which they were planted, is a cool one, shaded by a tall house on the E.S.E., and by a wall of five to six feet along the S.S.W. side, and the trees were mostly planted very near this wall. At midsummer they received no sunlight except between 10.30 and 1.30, and during most of this interval it was partial. These retarding conditions were perhaps somewhat counteracted by the protection afforded by the sleeve from wind and from all but heavy rain. The sleeves were made of "Victoria lawn," kept from collapsing by three split cane-rings sewn in. There can be no doubt that sleeving is the least troublesome way of feeding larvæ that require no earth; the only trouble I have found is in shifting them while young from one sleeve to another, but any loss in the process was prevented by spreading a slit newspaper on the ground below. My provision of growing leaves being small, I frequently supplemented it with fresh-cut twigs of birch, willow, or occasionally rose, dropped into the sleeve. Both *illunaria* and *illustraria* are very accommodating feeders; they will eat most forest-trees and shrubs, including brambles, and will also eat evergreen honeysuckle (*L. brachypoda*), the variegated Japan honeysuckle, and the small-leaved evergreen *Cotoneaster*; and three or four out of a score survived a diet of ivy. Mine seemed to prefer willow to everything else. When autumn came they appeared to like the leaves that were beginning to turn yellow as much as those that were still quite green. In the autumn my supply of growing foliage became exhausted, and, when the larvæ had mostly entered on their last skins, I moved them into breeding-cages: these had glass tops and ends, and finely perforated zinc sides, and the food in them stood in bottles of water. Little as the ventilation was I found that in the dry weather, of which we had so much last summer and autumn, the food in them dried up very rapidly, and I provided the sides with coverings of varnished paper. The effect of these was that water usually stood in drops about the glass inside and sometimes ran down the sides, but the larvæ seemed none the worse for this. The dwarf sleeved trees were protected from birds, &c., by a cylinder of $\frac{3}{4}$-in. wire-netting, with a hinged top of the same, and from slugs by an outer ring made of a strip of perforated zinc 6 in. wide, any slugs within the ring being caught by greased cabbage-leaves. When my first sleeved brood was reared I put the eggs in the sleeve to hatch, but I afterwards adopted the plan of hatching them indoors, and putting the young larvæ in the sleeve when a few days old. I judged it best not to crowd together young larvæ of different ages; I am not sure the larger ones do not under such circumstances sometimes eat the little ones. By the time they have changed their second skins no naturally solitary larvæ can be more tolerant towards one another. Having made these explanations, I will shortly describe what happened to each successive brood, referring also to the tabular statement appended.

Second generation (first summer brood).—From the 90 eggs I reared 34 male and 23 female moths, together 57, none of them being cripples. The eggs were rather more than three weeks hatching; the larval period averaged 38 days; the pupal period of the first moth that emerged was 13 days. The pupæ were forced from the time the first moth appeared—15th July—and the last came out 25th July. May and the early part of June were very cold and dry. I paired off 9 couples, 7 of which laid fertile eggs. I bred from the largest pair (A) a medium-sized pair (M) and the smallest pair (Z).

Third generation (A 1, M 1, Z 1).—These eggs hatched in 7 or 8 days; the larvæ averaged 50 to 60 days in feeding up. I have obtained from them the following pupæ, now passing the winter out-of-doors, of A 1, 101; of M 1, 64; of Z 1, 60. As the sleeved food was in danger of falling short, on the 13th September, when a few were beginning to spin up, I transferred the larvæ from the sleeves to breeding-cages; and on the 15th October these breeding-cages were brought indoors to hurry on the remaining larvæ before their food-supply should fail. All were in pupa by the 25th October.

Bottled Illunaria.—*Second generation.*—These were brought up on cut food in Bordeaux plum bottles, covered with muslin, plate-glass being laid over the top and slid away when the moisture inside the glass was excessive. The 90 eggs were three weeks in hatching; the larval period averaged 30 or 31 days, the pupal 14 days. I bred 31 males and 32 females, together 63; no cripples. They were distinctly larger than the sleeved ones. When the larvæ were about half-grown (on 18th June) I transferred half of them to an outdoor breeding-cage. The only difference I found in the moths so treated was that they were about two days later, and were smaller, *viz.*, the male averaged 17·60 instead of 17·70, the females 19·00 instead of 19·50. The weather was so warm most of the time that there could have been little difference in *temperature* between the two batches; but it was very *dry* weather out-of-doors, while in the bottles a moist atmosphere prevailed. I did not think it necessary to continue this brood.

Forced Illunaria.—*Preliminary.*—The forcing boxes were two, their inside dimensions about 2′ 4″ by 1′ 8″, and 2′ in depth. They were of wood, with glazed lids set on a slight inclination forwards, and ventilation capable of being closed, and were warmed at the bottom by a zinc cistern, under which was a gas-jet. One had glass also in front and partially at the ends. The temperature was generally from 70° to 80° Fahr., but occasionally (more especially when the sun shone into the room in the afternoon) it rose to 90°, and sometimes at night it fell to 60°. In the summer it was generally some 15° higher than the air of the room. Until the larvæ were about half-grown, and sometimes till they had spun up, the forced larvæ were brought up on cut food in bottles, or else in glass cylinders having a sloping sheet of muslin at the bottom, with a hole in it for the neck of a bottle containing food. When half-grown they were generally transferred to breeding-cages placed in the forcing-box; in both cases the atmosphere was quite a moist one. The forcing did not begin till 28th May, when the eggs laid by the wild-bred moths were on the point of hatching.

Second generation.—From the 91 eggs I reared 25 male and 33 female moths, together 58; no cripples.

The hatching (not forced) lasted about 3 weeks, the larval period 18 or 20 days, the pupal 8 or 10 days. The moths were larger than the sleeved ones, but not so large as those that were bottled. On 3rd July I paired off two of the largest (A), 5 of medium size (M), and 2 of the smallest size (Z); and from all of these, except one M and one Z, I had fertile eggs, which I bred from as follows:—

Third generation (eggs not placed in the forcing-box till earliest of them about to hatch).—The 3 broods did not vary much in their rate of progress—the M's were 2 or 3 days behind the A's, the Z's 2 or 3 days later still: the larval and pupal periods together were about the same as in the second generation. From 205 A 1 eggs I bred 68 male and 61 female moths, together 129, six cripples; from 115 M 1 eggs, 35 males and 53 females, together 88, one cripple; from 107 Z 1 eggs, 16 male and 14 female moths, together 30, 3 of them cripples, and two so weakly that they died before they could be paired: many of the Z 1's died as larvæ. The A 1's comprised the largest I had yet bred; I did not average them: the M 1's (averaged by taking every alternate one of each sex in the order of emergence) were slightly larger than the average of the preceding generation: the Z 1's considerably smaller. I paired off 4 of the largest couples among the A's, 4 average couples of the M's, and 10 couples of the Z's. None of the A's or Z's laid a fertile egg: 3 out of the 4 M's laid fertile eggs, and from one of these pairs, paired 16th August, I had 210 eggs, which I bred from as follows:—

Fourth generation, M 2.—These were not only slower, but straggled more in their feeding up and emergence than the earlier forced generations had done. The first spun up 23rd September; by 8th October nearly half had done so; and on the 1st November all had done so except two, which soon after died. Many larvæ died in pupating, and a few before. I have some reason to think this was owing to their having been made too hot at one time. The first moth appeared 2nd October; by the 3rd November 60 were out, and on the 7th the last appeared; but 3 or 4 are still in pupa, one or two of them certainly being alive. 36 are males and 25 females; 3 were cripples, and 3 more died before they were paired off. The hatching occupied about 10 days (eggs not in the forcing-box till the first eggs were about to hatch), the larval period ranged from 26 to about 59 days; the pupal period seems to have been about 12 days. After the early alarm they were kept rather cooler than the preceding forced generation had been, as, with the advent of cooler weather, I found it difficult to keep up a high temperature without making the bottom of the forcing-box very hot. The average size had again risen on that of the preceding generation. On 23rd October I paired off 6 couples, keeping them in the forcing-box, and 4 of them were fertile. The largest pair laid 170 eggs (called M 2, A 1), the medium-sized 210 (called M 3), the smallest 80 (called M 2, Z 1).

Fifth generation.—About 86 of the first, 169 of the second, and only 24 of the third hatched. The numbers are now about 82, 151, and 21 respectively. I am feeding them up on rose and evergreen honeysuckle, and the most forward are nearly full-grown, as will be seen by the living specimens I exhibit. I have made an improvement in my forcing-box, so that I can keep up a more equable temperature without danger of roasting those which are near the cistern, and I keep it at about 70° to 80°.

ILLUNARIA.—*General results.*—Without venturing any opinion on many of the questions suggested by an examination of the facts above detailed, until more facts have been accumulated, I may advert to a few of them. It seems to be established that *S. illunaria* forces well, and there is evidence that the average size of forced specimens is larger than that of the insects reared on growing trees, and tends for a time to increase from generation to generation, notwithstanding close interbreeding. I am not satisfied that the fertility has been diminished by the process of forcing; but it does at present appear as if extremes in size, especially in the direction of smallness, have a tendency to be sterile, and I think it prudent to select the breeding pairs from some point quite short of either extremity in the scale of size. There is another fact established as to the summer broods of *illunaria*,—all of which that I have reared, I need hardly say, are in appearance of the summer type, *Juliaria*,—*viz.*, that, in accordance with the usual rule with the *Geometræ* inhabiting this country, the female on the average is larger than the male, and decidedly so. This will appear clearly by the tabular statement I refer to. My own personal experience, which is confirmed by trustworthy information I have lately received from several quarters, is that in the spring brood the case is reversed, so that the male is decidedly the larger; at all events, it seems certain that the spring female has no excess of size approaching to what she shows in the summer brood. In this connection I venture to call attention to the following points :—(1), of 272 *Geometræ* described in Stainton's 'Manual,' only 16 are recorded as appearing in the five months from November to March; (2), *illunaria* in its spring emergence is one of them; (3), of the remaining 15, 9 have apterous or quasi-apterous females (there being only two other apterous females among the 272, and these two appear in April and October respectively); (4), another of the 15 (*H. pennaria*) has the wings of the female strikingly smaller than those of the male. Is it possible that the relative size of the female in the spring emergence of *illunaria* is a step towards the condition of apterousness, or, it may be, a remnant of it? So far as I have had means of judging, *illustraria* and *lunaria* do not show such a difference between the sexes according to the season of emergence, but their spring broods are much later than those of *illunaria*, which (unless *Tephrosia laricaria* (*biundularia*), another of the 16, of which I know but little, resembles it in this respect), is unique among double-brooded English *Geometræ* in producing its early brood in a winter month.

S. ILLUSTRARIA. — Mr. Barrett kindly sent me eggs from a female taken in Norfolk in May, and Mr. Gulliver, of Brockenhurst, supplied me with some larvæ beaten in the New Forest. From these two sources I bred 9 males and 12 females, and, though the variety in size was not very great, I selected a large (A), medium (M), and small (Z) pair, the eggs from which I sleeved; and from them I have three batches of hybernating pupæ, *viz.*, A 116, M 103, and Z 78. I reared several mixed broods in the forcing-box, with some remarkable results, which I hope to follow up.

CONCLUDING REMARKS.—I shall be very glad if the account I have given of the experiments with the Selenias, and of the ease with which they can be bred, should lead others better qualified than I am to take up the subject; and I shall be glad to supply eggs of any race bred. The remarkable changes which the larvæ undergo in appearance, attitude, and habits, so well described by Mr. Poulton; the perfection to which the imitation of jagged twigs has been developed in them; the great variation in size of individual moths, especially in the spring brood, and in shape; the richness and variableness of shading and colour in the wings, and their unusual positions when at rest, apart from other points to which I have already called attention, make them a very remarkable group, and they ought to have an interesting family history. The experiments I am trying with *illunaria* and *illustraria* will leave abundant scope for other investigators who may direct their attention to these two species, and a very interesting species, *S. lunaria*, remains. *Tephrosia laricaria*, which is stated to resemble *illunaria* in having an early spring and summer emergence, and in the smaller size and different appearance of the latter brood, would also be an interesting species to work up. As to *illunaria* and *illustraria*, may I suggest that practical entomologists would be promoting the investigation by preserving any specimens they may meet with next spring, or a fair sample of them, for comparison with the numbers I expect to bred? I should be particularly obliged by being afforded any opportunity of seeing, and, if judged expedient, breeding from, specimens of either species from Scotland or Scandinavia, where they are stated to be single-brooded, or from Ireland, Wales, or Central or Southern Europe.

[*Note as to Measurement.*—I find it is practicable, without piercing the insect, to measure the *expansion* of wings of the chloroformed insect by setting it temporarily, with cork setting-bristles, on a *flat* setting-board covered with paper ruled in square millemetres, and after trial I recommend this mode decidedly as the more safe and certain in its results. The tips of the fore wings should be as widely separated as possible, so that the front edges of these wings will be nearly in a straight line.]

TABULAR STATEMENT OF FERTILE EGGS LAID, AND MOTHS REARED, WITH MEASUREMENTS.

Eggs.	MOTHS.						Larval & pupal period. Days.
			Largest.	Smallest.	Diff.	Average.	
	First Generation. Wild.						
	♂	1				20·20*	
	♀	4	22·40	18·10	4·30	20·20	
		5				0·0	
	Second Generation. Sleeved.						
	♂	34	18·70	16·70	2·00	17·35	
	♀	23	20·30	16·30	4·00	18·24	
90		57	1·60	·40		·89	51
	Bottled.						
	♂	31	18·70	16·60	2·10	17·65	
	♀	32	20·70	18·10	2·60	19·26	
90		63	2·00	1·50		1·61	45
	Forced.						
	♂	25	19·00	15·90	3·10	17·54	
	♀	33	20·80	17·90	2·90	18·95	
91		58	1·80	2·00		1·41	30
	Third Generation. Forced. A 1.						
	♂	68	19·60				
	♀	61	21·10				
228		129	1·50				30
	Forced. M 1.						
	♂	35				17·88	
	♁	53				19·20	
115		88				1·32	32
	Forced. Z 1.						
	♂	16	16·90	15·40	1·50	16·15	
	♀	14	18·30	15 60	2·70	17·57	
107		30	1·40	·20		1·42	34
	Fourth Generation. Forced. M 2.						
	♂	35	19·80	17·00	2.80	18·41	
	♀	26	20·90	18·20	2·70	19·40	
210		61	1·10	1·20		·99	52

* Average of five males taken in spring, 21·50; of five females then taken, 19·90; difference in favour of male, 1·60. In all the later generations the difference is ·89 to 1·61 in favour of female.

Explanation of Plate V.

The explanation of this Plate will be found at p. 128.

VI. *Life-histories of* Rhopalocera *from the Australian region.* By Gervase F. Mathew, Staff-Paymaster, R.N., F.L.S., F.Z.S., &c.

[Read December 7th, 1887.]

Plate VI.

During a period of more than three years spent in cruising off the coasts of Australia and New Zealand, and amongst the islands of the Western Pacific, I devoted as much of my leisure time as I was able to in collecting Lepidoptera, and working out, to the best of my ability, the life-histories of such Rhopalocera as it was my good fortune to obtain the larvæ of. In doing this there were many obstacles to contend with, such as the constant change of locality, the shortness of our stay at the different places visited, and the difficulty of preserving fresh, for any length of time, the various food-plants for the sustenance of the larvæ.

For many years I have taken the liveliest interest in rearing Lepidoptera from the egg or larva, and noting the habits of the different species in a state of nature, and have often regretted, when perusing descriptive works on exotic butterflies, that so little has been written concerning their earlier stages, or so little said as to the general habits, localities, times of appearance, &c., of the species described. If, when practicable, such information were furnished, the books would be infinitely more valuable; and would, I feel convinced, tend to attract many more to the study of these charming creatures.

Melanitis leda, Linn.

In Australia, I have met with this species at Cooktown, Brisbane, and Thursday Island, and Mr. Masters informs me that he has taken it near Sydney. It probably occurs, in suitable places, in all tropical parts of Australia. In the Western Pacific I observed it at Fiji,