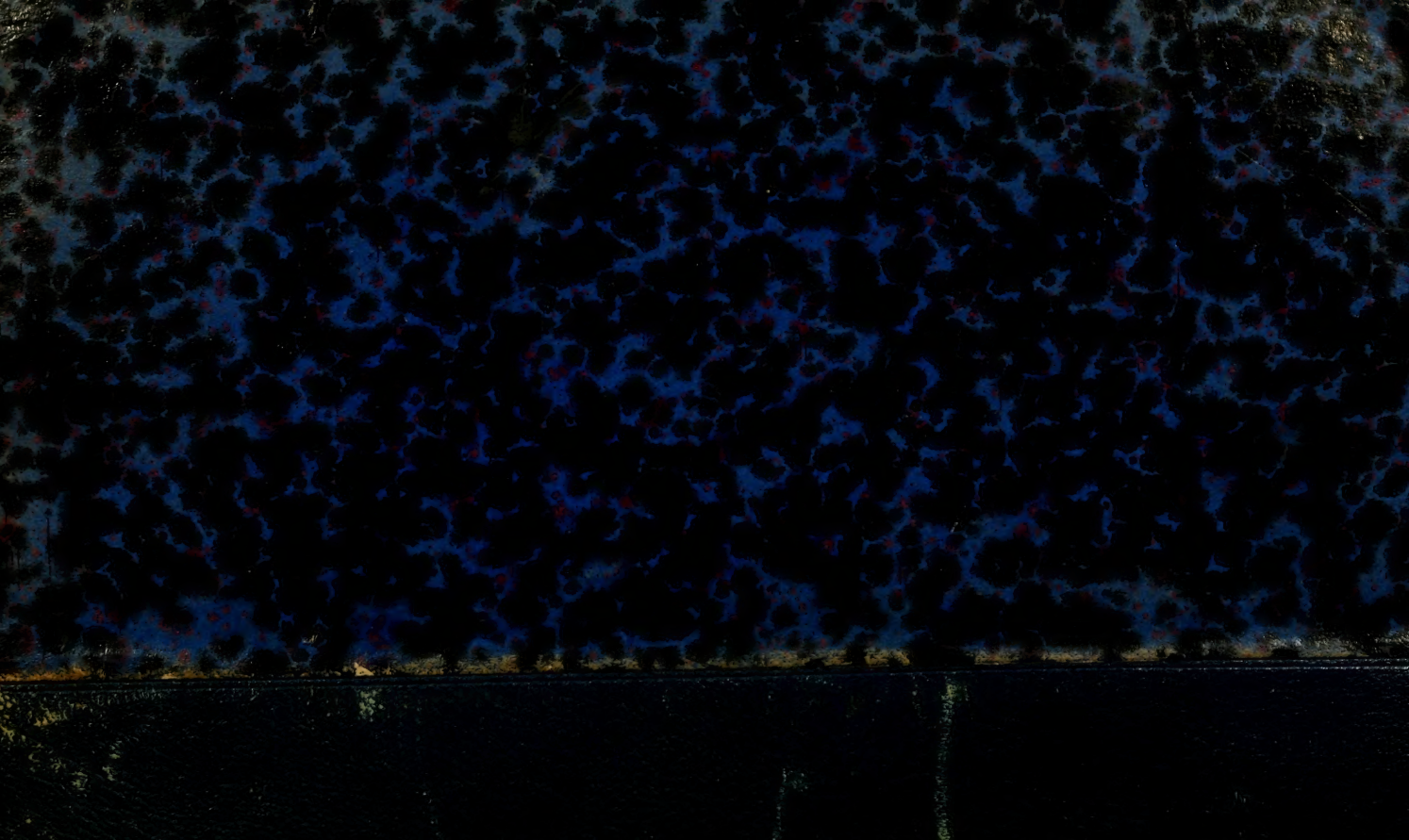

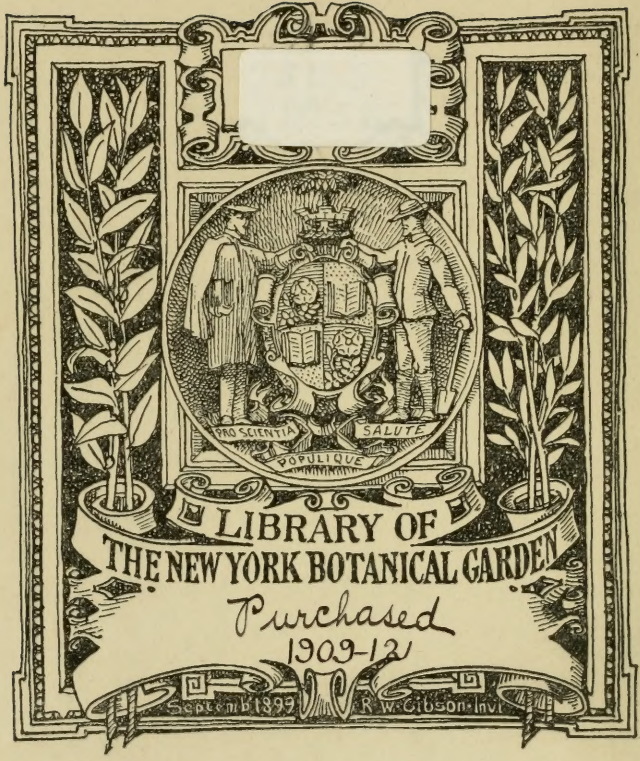
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The Mendel Journal

October 1909

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The
Mendel Journal

No. 1 October 1909

PROLOGUE.

Mendelism is a subject which has come to stay and to play an important part in human affairs. In Agriculture, in Horticulture, in the Prize Pens, and in Sociology, its voice will be heard. It will not be as a voice in the desert, but as a world-vibratory one, uttering its pronouncements, admonitions, and definitive conclusions, based on the solid and unshakeable ground of accurate experiment, wherever culture and life come into contact.

A subject like Mendelism, full of complexities, and liable to be misunderstood by the careless, the hasty, and the unwary, necessarily runs many dangers among the shoals of human affairs. There are the dilettante philanthropists and scientists, and social reformers; and there are newspaper writers possibly seeking to ingratiate themselves with their numerous readers, by assuming the garb of "gods who destroy false idols," which they themselves first set up. Not Mendelism, nor its progress, but the application of it to human affairs, is endangered by them.

A great subject like this, one with not only an academic importance, but clearly of great practical importance also, and having a bearing upon some of the greatest phases of human activities, so far as

its presentation and promulgation are concerned, has two courses open to it. These two alternatives are inevitable. Mendelism differs in this respect from subjects which are purely academic, because these are only presented and discussed in scientific societies and in their journals. But Mendelism is a subject which comes into the category of the " Humanities " ; it will be discussed in various circles, by people more or less well acquainted with it, and more or less antagonistic to it. Must its wider promulgation necessarily be left wholly to them ? We think all will agree that a better alternative lies before us.

It is better that Mendelism shall be presented to a wider public by men who believe in its truth, who foresee its future, and who recognise their responsibilities in the work they do. That is one of the objects with which the " Mendel Journal " starts upon its career, and which forms the far distant beacon-light towards which it will consistently steer.

But it has another object. It is to gather for the Science of Genetics a harvest rich in facts relating to human pedigrees and the inheritance of normal characters as well as of peculiarities. To find these the seeker must quit the experimental garden and the cloister, and he must pass out into the world of his fellows. From them shall the grain be gathered in order that it may be garnered in these pages. A golden field, ripe for the harvest, awaits the coming of the Mendelian reapers. We appeal to all who are acquainted with families in which peculiarities and markedly contrasted normal characters, have run

from one generation to another, to send details to the Editor of this Journal. Such contributions will be treated in the strictest confidence, they need not necessarily be published, and will not be published without the consent of all who are concerned, and then only in a form approved by them. While it is clear that medical men have many unique opportunities for acquiring knowledge of pedigrees of this kind, it is hoped that contributions from laymen will also be forthcoming.

In future numbers of this Journal, prominence will be given to matters pertaining to agricultural and horticultural practices and problems. It is also contemplated to make it a medium by which authoritative advice and direction may be given in the form of answers to questions upon matters of general interest, relating to problems of cattle, cereal, and plant breeding. In this way, it is hoped that the Journal may become a medium of great value to all who are engaged in the breeding of live stock of all kinds, and to those who are concerned in the production and fixing of new varieties of flowers, or of leguminous and cereal stocks. It is certain that much money and time have been wasted in the past, owing to the haphazard methods and erroneous ideas which were employed in agricultural and horticultural practices.

It is perhaps not too much to anticipate that this Journal may at least be more valuable than a Royal Commission and as competent as a Government Department, to advance the scientific treatment of

matters of the greatest commercial importance to the country.

Any questions directed to the Editor, bearing upon the scientific breeding of cattle and plants, will, as far as possible, receive full consideration in the next number of the Journal.

Business communications should be addressed to the Manager, Office of the Mendel Journal, c/o Messrs. Taylor, Garnett, Evans, & Co., Ltd., 54, Fleet Street, E.C. Literary contributions, or questions relating to the scientific breeding of cattle and plants, or pedigrees of human families, should be addressed to the General Editor.

ORIGINAL RESEARCH.

(1) Parthenogenesis in Nicotiana.

By ROSE HAIG THOMAS.

I NOTICED last year and this summer that in several different cross fertilisations, made between different species of Nicotiana, some plants bearing only the characters of the ovule parent were produced along with others which were apparently F_1 hybrids. Upon the first and second occasion when I obtained these results—which occurred with reciprocal crosses of *Nic. Sylvestris* × *Nic Affinis*.—I thought they must be due to accident, for muslin bags had been used, and it was possible that pollen from adjacent flowers of the same plant may have gained access through the bag. I repeated the experiment a third and a fourth time, with every precaution, such as the use of wax-paper bags and the cleansing of the pollinating instruments in spirit before use. At both repetitions of the experiments the same phenomenon was manifested. I then began to suspect the cause, and determined to test Nicotiana for parthenogenesis.

In the first few trials the method employed was to cut off the anthers alone, but later, after success attended these I cut off both stigma and

anthers; the result remained the same, the buds developed into full bloom, the corollas withered, the capsules set, the seed in due time ripened, and if left on the plant long enough the capsules split open and shed their seed.

My first trial for parthenogenesis was with *Nic. tabaccum Cuba*, raised from some seed I gathered from a plant in the well-known garden of Casa Loring, near Malaga. The gardener there told me that this plant had grown from seed brought over from Cuba, and that it had been gathered from the finest tobacco plants grown in the island. This plant is taller than other *N. tabaccums*, and is 6½ft. to 7ft. in height, and the stems are very thick; it flowers at first in a terminal cluster and afterwards axially. The limb and tube of the corolla are pure white; the corolla is sometimes four petalled with four stamens, sometimes five petalled with five stamens, and both forms are found on the same plant. It is a freely self-pollinating plant, for under protection from insects it will seed every blossom.

On July 15th, 1909, I cut off all the anthers from five young green buds on a spray of *Nic. tab. Cuba*, and covered them with a wax-paper bag, which was wired on in the usual way. At the same time all the other buds and blossoms on the spray were removed. On July 24th the spray was uncovered and it was found that only one ovary had failed, the other four capsules having set seed. One or two tiny buds were sprouting; these were pinched off, and the bag replaced over the seed. I at once started a

second experiment on the same plant to confirm this first one, and proceeded in exactly the same manner. On the same day I experimented with another plant, using the same precautions. This plant was a hybrid of *F*1 *N. Tabaccum Cuba* × *N. F*1 *P.** (*N. Sylvestris* × *N. Sanderæ* × *N. affinis F*1 *R.**) On August 1st, the eighth day after, all the capsules were found set on both these plants. I now determined to test every Nicotiana species and variety and hybrid flowering in my greenhouse and garden, and succeeded in setting parthenogenetic seed on the following :—

Species.	Number of Successful Experiments.	Notes.
Nic. Suavolens ...	1 spray on 1 plant.	—
Nic. Sylvestris ...	3 sprays on 3 plants.	—
Nic. Sanderæ ...	2 capsules on 2 plants.	—
Nic. Tabaccum Cuba..	5 sprays on 2 plants.	Parthenogenetic seed sowed 9th Sept., germinated on the 21st Sept.
Nic. Tab. Mirodato ...	1 spray on 1 plant.	Asia Minor; seed obtained from the Board of Trade.
F2 Nic. Sylvestris x Nic. Affinis ...	2 capsules on 1 plant.	This parthenogenetic seed sowed 4th Sept., germinated 12th Sept.
F1 Nic. Sylvestris x Nic. Tab. Cuba ...	2 sprays on 2 plants.	—
Nic. Tab. Cuba x F1 P.* (Sylvestris x F1 R.* Sanderæ x Affinis)...	3 sprays on 3 plants.	—
Nic. F2 P.* (Sylvestris x F1 R.* Sanderæ x Affinis)	4 sprays on 4 plants.	—
Nic. F1 P.* Sanderæ x Affinis	1 capsule on 1 plant.	—

* R. = red. * P. = purple.

The first parthenogenetic seed I examined was that of *Nic. Tab. Cuba* ; half the seed was round, full, sound looking ; the other half flattened, poor, little likely to germinate. The parthenogenetic seed was compared with selfed seed of the same variety ; in this latter nine-tenths was full, sound seed. It seems possible that in this variety all the ovules are not capable of parthenogenesis, and that those not set might have proved fertile to a pollination either self or cross. But in the hybrid *Nic. Sylvestris* × *Nic. affinis* F_2 the parthenogenetic seed was all round, full, and sound.

In order to ascertain the condition of the pollen in the Nicotiana buds, within about twenty-four hours of expansion, I gathered from *Nic. Sylvestris* × *Nic. Tab. Cuba* F_1 an unopened bud $2\frac{1}{2}$in. in length, and split it open ; the style was two-eighths of an inch longer than the stamens, and the anthers were green and solid. I cut one and placed the section under the microscope ; the pollen was miniature, and floated in a colourless liquid that dried up after a quarter of a minute's exposure to the air. As the anthers dehisce at the opening of the corolla, the development of the pollen is probably very rapid towards the end.

The observation of the immature condition of Nicotiana pollen in the young bud at a time subsequent to that chosen for the removal of the anthers and stigma testifies both to the ease with which, in this plant, trials for parthenogenesis can be made, and to the reliability of the experiments.

The cutting off of stigma and anthers in the earliest stage of the young bud does not in the least affect its development; the tube grows to its full length, the corolla expands to its full width before withering, and there is no more delay in setting the capsule than under normal selfing occurring on other sprays of the plant.

In these attempts to obtain parthenogenetic seed, failures resulted in some of the hybrids, but these were not more numerous than the failures from attempts to self the same hybrids.

The fact remains that parthenogenesis was discovered in ten species, varieties and hybrids of Nicotiana, and it is possible will be found in all of them if the right period is chosen for the trial, *i.e.*, when the plant is beginning to go off its fullest bloom. Amongst the hybrids I found success more likely to attend a test made at this stage, and also when that test was made on a spray which had already seeded one or two of the lower blossoms. Amongst the species and varieties this is not so necessary. In the Tabaccums success was unfailing.

When we remember the wide geographical distribution of Nicotiana, the occurrence of parthenogenesis in an Australian plant, like *N. Suavolens*, in the Tabaccum varieties, and in South American species, points to the conclusion that it is an ancient character in Nicotiana, and had developed in this genus before the separation of the possible land connection between Australia and South America. Or, if that conclusion is questioned, we are led to infer that parthenogenesis has arisen independently in different species of the genus.

An important consideration which this discovery of parthenogenesis in Nicotiana opens to discussion, is the supposed hybrid nature of *N. Sanderæ*. This plant is said to be a hybrid of *N. Forgetiana* × *N. affinis*. The former species was brought by one of Mr. Sander's travellers from South America, and it is said was then crossed with *N. affinis*. But the true history of this hybrid appears to be shrouded in mystery.

In the light of these observations on parthenogenesis in Nicotiana, it is conceivable that Sander may have obtained amongst the F_1 offspring from his cross of *N. Forgetiana* × *N. affinis* not only some true hybrids which he possibly destroyed, but also some individuals derived from the parthenogenetic seed of *N. Forgetiana*. He may have selected these and sent out the seed as the type of the supposed hybrid *N. Sanderæ*. In other words, *N. Sanderæ* may simply be *N. Forgetiana*. If this is so, it will explain what is, from the Mendelian standpoint, the remarkable fact that the supposed hybrid *N. Sanderæ* breeds true to seed, for this is a phenomenon which is not expected to occur, on Mendelian principles of gametic purity and segregation.

Parthenogenesis in Nicotiana perhaps explains a fact which I have frequently observed, namely, when *Nic. Sanderæ* is growing alongside of other varieties of Nicotiana in the open it always seeds true. This remark applies equally well to other Nicotiana which I have grown.

THE MENDELIAN COLLECTION OF HUMAN PEDIGREES.*

(1) Inheritance of Suicidal Mania.

The Families of A and B. Pedigree Chart I.

BY GEO. P. MUDGE.

I am indebted to Miss Gertrude Flumerfelt, who was recently one of my students at the London School of Medicine for Women, for the facts of this pedigree. I cannot sufficiently express my obligation to her for the interest which she manifested in this case, and for the trouble and care which she took in elucidating the various facts.

The Facts of the Pedigree.

This pedigree was constructed in 1907, by enquiry of some intimate friends of the B family.† There are two families concerned, and they have apparently lived for many generations in an English village. Both families are very respectable, and their members as a whole are moderately well off, while some members have been wealthy farmers.

As already indicated, two families are primarily involved in the pedigree, namely, the A and B families. There exists a tradition in the village in

* The Editor will be glad to receive from medical men and others, family pedigrees showing the transmission of disease, peculiarities, abnormalities, or of marked abilities, but not necessarily for publication. See Prologue.

†The symbolic letters employed to indicate the two families concerned do not in any way reveal their identity. The two first letters of the alphabet have been chosen for the necessary purposes of description.

which they live that death by means of self-shooting belonged primarily to the B family, and death by self-drowning to the A family. The two families have intermarried, and among their descendants three forms of suicide are manifested, namely, the two original forms, by shooting and drowning, and a new form, by taking poison.

Before the present history commences, according to tradition, the two families were unrelated.

One peculiar and singularly significant feature of the case is the very characteristic mode of drowning adopted by the victims of the suicidal mania. It seemingly indicates that the manifestation of the mania is not a matter of opportunity or suggestion, but is an inherent impulse, which all external influences are powerless to check or destroy. It is indicative of an innate determination so pronounced that it would call forth our admiration were it manifested in nobler ways. The victims proceed to drown themselves by lying down, and then forcing their faces into a pool of water only a few inches in depth until death results.

Plate I.—"A Pedigree of Suicidal Mania."

C. M. = Cousin marriage.
D. = Suicide by drowning.
I. = Has been temporarily placed in asylum.
L. = Still living in year 1907.
P. = Suicide by taking poison.
S. = ,, ,, shooting.
P. = Parental generation.
F_1., P., P_1., P_2., and P_3., = The different generations.

The black symbols indicate those members who have manifested suicidal mania or whose behaviour has caused their friends anxiety.

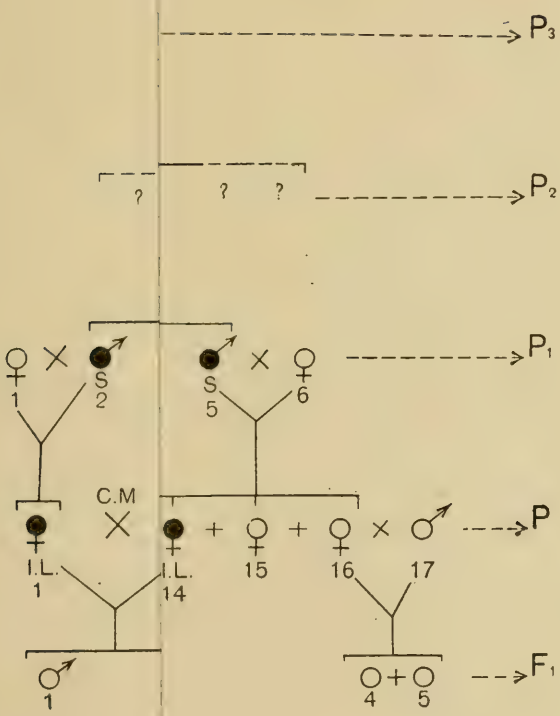
The ringed symbols indicate the normal persons.

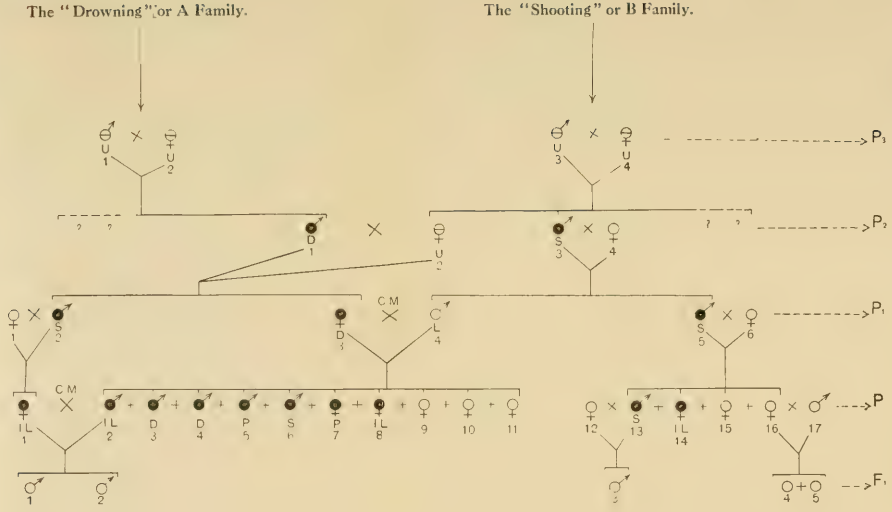
The ringed symbols marked with a transverse line represent unknown persons.

The numbers are merely for descriptive purposes.

Pedigree Chart I.

The "Drily.




The "Drowning" or A Family.
The "Shooting" or B Family.
P3
P2
P1
P
F1
CM
CM

The tendency to manifest the suicidal mania developes, as a rule, between twenty and twenty-five years of age. There is one marked exception to the rule. It is the male member, No. 6 in the P generation. (Pedigree Chart 1.) He was a wealthy farmer, and was very much respected. He had apparently lived in his native village all his life. Notwithstanding that he had been very successful in his affairs, at fifty years of age, in the July of 1907, he committed suicide by shooting himself. An announcement of his death, accompanied by an obituary, appeared at the time in several of the weekly newspapers. It appears there was no reason for the suicide. But it is significant that throughout his manhood, according to the account of his relatives supplied to us, he was regarded as "crazy," though he was never confined in an asylum.

As is usual in constructing a pedigree of this sort, one finds that a few individuals are unknown, or the memory of them is lost, and we are perforce compelled to supply their place by hypothetical persons. These persons, however, we know must have existed. There are five of them in the present pedigree, and they are indicated by the cross line passing across the symbol which stands for them. They are Nos. 1-4 in the P_3 generation, and No. 2 in the P_2 generation. In the P_2 generation it is known that other members besides those shown in the pedigree existed, but all family account of them is lost, and I have not yet been able to trace them.

Details of the Individual Members.

P_3 Generation.

The mode of death of the four individuals represented is unknown. The existence of these four members is postulated. The family tradition is that the wife of Mr. A No. 1, and of Mr. B No. 3, were normal, and that the suicidal mania came down through the two husbands of this generation.

P_2 Generation.

In this generation Mr. A No. 1, and Mr. B No. 3, are known to have been brothers-in-law. Since Mr. B No. 3 was known to have married a woman No. 4, unrelated to either family, it follows that Mr. A No. 1 could only be brother-in-law to Mr. B No. 3 by marrying a sister of his. Accordingly, the woman Miss B No. 2 is postulated, since nothing is certainly known of her in the family or in the village.

Mode of Death.

Mr. A No. 1 .. .. Died by drowning himself.*
Mrs. A, *née* Miss B No. 2 Death unknown.
Mr. B No. 3 .. .. Shot himself.
Mrs. B, *née* ?—No. 4 .. Died a normal death.

P_1 Generation.

In this generation Miss A No. 3 is known to be a cousin of Mr. B No. 4, whom she married. He was still living in 1907, and was then 80 years of age, and had been sane all his life. This cousin marriage confirms the conclusion derived from the known

*In all cases but this one, drowning was known to be by the characteristic method already described.

existence of brothers-in-law in P_2, that the originally unrelated families A and B have intermarried.

Mode of Death.

Mrs. A, *née* ?—No. 1	Is known to have been normal.
Mr. A No. 2	Shot himself.
Mrs. B, *née* Miss A No. 3	Drowned herself.
Mr. B No. 4	Was still living in 1907 and then 80 years of age. Sane throughout life.
Mr. B No. 5	Tried three times to shoot himself and failed. Died a normal death. In the pedigree he is marked as an insane member.
Mrs. B, *née* ?—No. 6	Is known to have been normal.

P Generation.

In this generation, a second cousin marriage, resulting in a second intermarriage of the A and B families, has occurred. Some individuals of this generation, in addition to those who have actually destroyed themselves, have been temporarily placed, at one time or another, in asylums, because they became intensely melancholic and it was feared they too would destroy themselves.

Mode of Death.

Mrs. B, *née* Miss A No. 1 (cousin and wife of B No. 2)	Was temporarily in an asylum. In 1907 was 40 years of age and alive.

Mr. B No. 2 ..	.. Was temporarily in an asylum. In 1907 was 40 years of age and alive.
Mr. B No. 3 ..	.. Unmarried. Drowned himself.
Mr. B No. 4 ..	.. Unmarried. Drowned himself.
Mr. B No. 5 ..	.. Poisoned himself.
Mr. B No. 6 ..	.. Was crazy throughout manhood, but never put into an asylum. Shot himself in 1907 when 50 years of age.
Miss B No. 7 ..	.. Poisoned herself "because she vowed she would never see the marriage of her brother (B No. 2) to an A No. 1, his cousin."
Miss B No. 8 ..	.. Was in an asylum for a time. In 1907 she was becoming "queer" again.
Miss B No. 9 ..	.. Unmarried. Sane.
Miss B No. 10 ..	.. Married. Sane.
Miss B No. 11 ..	.. Married. Sane.
Mrs. B, *née* ?—No. 12 ..	Introduced by marriage; wife of No. 13; normal.
Mr. B No. 13 ..	.. Shot himself.
Miss B No. 14 ..	.. Unmarried, and was for a time in an asylum.
Miss B No. 15 ..	.. Sane.
Miss B No. 16 ..	.. Sane and married. Removed with her husband and two children from their native village.

Mr. ?—No. 17 .. .. Husband of B No. 16, and introduced by marriage. Normal.

F_1 Generation.

Master B No. 1 .. } Quite children. Sane up to the
Master B No. 2 .. } year 1907.

Mr. B No. 3 .. Is now 22 years of age and in 1907 was sane.

The two remaining children (Nos. 4 and 5) of this generation are quite young, and their name, sex, and age are not recorded. They have removed, with their parents, from the village, and the desire of the family is, I believe, to lose its identity. The children, none the less, constitute two of the most interesting members of the Pedigree, since they are the only members in this Pedigree both of whose parents are normal. Their future history is a matter of the greatest importance, both from the Mendelian and the medical standpoint. I am endeavouring still to trace them.

The Deductions from the Pedigree.*

We are not justified yet in saying dogmatically that this Pedigree is an illustration of Mendelian phenomena. But there are clearly some Mendelian indications. In the P generation all the individuals have passed the twenty-fifth year of life**, and

* We follow the example admirably set by Professor Pearson, in his "Treasury of Human Inheritance," in keeping our interpretations apart from the facts of our Pedigrees. Those who seek for the latter can thereby obtain them without wading through the former, which at this stage of enquiry must necessarily be wholly tentative.

** The youngest must be nearly forty.

it is between this and the twentieth year that as a rule the manifestations of insanity make their appearance. Yet there are some individuals who are still sane. Others have destroyed themselves or have been confined in asylums, in order to prevent self-destruction. This fact suggests a segregation of the morbid diathesis from the normal condition. But it is only right to add that melancholia is said to be a trait of the whole present family. What such a statement means is difficult to precisely understand. Whether this melancholia is to be regarded as a diluted form of the insanity of the family, or whether it is a distinct morbid diathesis of itself, or whether it is merely the melancholy that is not infrequently associated with phlegmatic temperaments leading a monotonous life, or whether, in this particular case, it may not be merely a neurotic boding, excited by a lonely country life and a knowledge of the family history, it is impossible to say. Many families have melancholic members, but suicidal mania is not manifested by them. The facts of the pedigree, I think, justify us in believing that if a member of the family has passed the twenty-fifth or thirtieth year of life without showing any peculiarity of behaviour, he is a sane member of that family. The existence of the individual, No. 6 in the P generation, who did not shoot himself until he reached fifty years of age, does not invalidate this statement, for, as is known, he was "cranky throughout his manhood."

The next feature in the Pedigree we have to consider in order to deal with it from a Mendelian standpoint is the view we ought to take of the condition of members Nos. 1, 2, 8, and 14 in the P generation. These individuals did not attempt to commit suicide, but their degree of melancholia was so great and their general behaviour such that, knowing the family history, their friends thought them safer in an asylum. In 1907 they were free citizens and were still living. In the Pedigree Chart we have indicated them, in the way their relatives and medical advisers apparently viewed them, as insane. If they are to be regarded as potentially afflicted with the family insanity, then they bring the number of insane members too high, *if the case is regarded as one of simple Mendelian segregation of insanity and normality.* If, however, they are to be regarded as merely extremely melancholic persons, but not insane, then the Mendelian expectation will be 7·5 of those afflicted and 7·5 of those not afflicted. The result—*upon this view of the nature of these members*—is 6 : 9 respectively.

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(2) A Pedigree of Tuberculosis.

The Family of C. Pedigree Chart 2.

BY GEO. P. MUDGE.

The following Family history was brought to my notice, in response to some enquiries which I made, in the early part of this year. The information was

vouchsafed by a member of the family, the No. 4 in the F_1 generation. The family is a well-to-do Irish one. It is chiefly remarkable in the apparently sporadic appearance of tuberculosis in the present or F_1 generation and in the manifestation of that disease, as a rule, at between 19-20 years of age. The family is one of such social position that it excludes the factors of malnutrition and unhealthy surroundings as an operative environment in the causation of the disease.

The Facts of the Pedigree.

P_1 Generation.

No. 1. Retired Colonel. Death unknown.
„ 2. Died of senility at 80 years of age.
„ 3. „ „ 70 „ „
„ 4. „ at childbirth.

P Generation.

All normal except No. 5, who died of cancer.
Nos. 4 and 6 are sisters, and No. 2 is the sister of No. 3.

F_1 Generation.

No. 1. Died at 18 years of age from rapid pulmonary consumption.
„ 2. Still alive. Has suffered from ulcerated throat.
„ 3. Died at 19 years of age from tuberculosis of stomach.
„ 4. Quite normal.
„ 5. Quite normal, but has gout in one finger joint.
„ 6. Still alive, but had tuberculous neck glands extracted at 19 years of age.

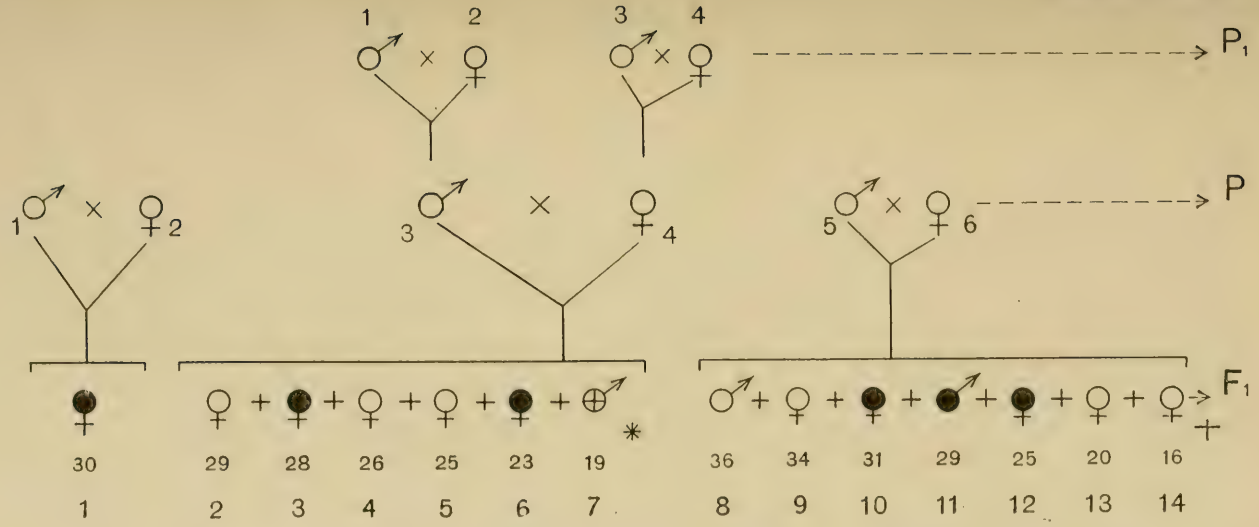

Plate II.—"A Pedigree of Tuberculosis." The larger numerals 1, 2, 3, &c., are for descriptive purposes alone.

The smaller numerals above them indicate the age the individuals to whom they relate would have reached in the year 1909, had they lived, or in case of those still living, their age in that year.

The black symbols indicate the individuals who have been the subjects of tuberculosis or have died from it. The ringed symbols indicate normal individuals. The symbol with cross lines, indicates a possibly potentially tuberculous person.

* There were in addition six still-born children.

† ,, ,, ,, seven ,, ,,

No. 7. Suffers from cough, is very susceptible to cold, very slight in build, is losing flesh, is 19 years of age, but looks only 15, and has no moustache.

„ 8. Normal.

„ 9. Delicate : suffers from headache.

„ 10. Died at 19 years of age from tuberculosis of vertebral column.

„ 11. Died from tuberculosis of lungs. First manifested between the 19th and 20th year.

„ 12. Died at 13 years of age from rapid pulmonary consumption.

„ 13. Quite normal.

„ 14. Normal, but suffers from adenoids.

In addition to the individuals here given, there were in the left hand Family (members Nos. 2 to 7), six more children which were stillborn, and in the right hand Family (Nos. 8 to 14), there were seven stillborn children.

Deductions from the Pedigree.

One fact calls for observation. In the P generation, Nos. 2 and 3 are sister and brother respectively, and Nos. 4 and 6 are sisters. Tuberculosis has appeared in both the branches to which Nos. 4 and 6 in part gave rise, and it has also appeared in both the branches which owe their origin to Nos. 2 and 3. Nos. 3 and 4 and 2 and 6 may therefore be tentatively regarded as D R's, since clearly the appearance of themselves and their members in a family has resulted in the manifestation of tuberculosis in that family.

If we regard the capacity of contracting tuberculosis as due to the absence of some factor which is present in normal individuals, then the parents Nos. 3 and 4 and 2 and 6 may be symbolised as N n, where N = normality, and n = absence of normality = tubercular predisposition. We shall then expect in the offspring of Nos. 3 and 4, one tubercular to three normal individuals. If we bear in mind the fact that the young man, No. 7, suffers from cough, is very slight in build, is losing flesh, and looks younger than he really is, and if we further remember that he has only just reached the age, namely, nineteen, when the disease first manifests itself in this family, there can be little doubt that the prognosis in his case is a grave one. If we then regard him as potentially tubercular, that will give us 3 : 3, instead of the expected ratio in this particular branch.

If we make similar assumptions with regard to the constitution of the two parents Nos. 5 and 6, we find that in their offspring there are 4 normals to 3 tuberculates, instead of 5·2 to 1·8 respectively. Taking the whole F_1 generation together and including No. 7 among the tuberculates, we have 7 normals to 7 tuberculates, instead of 10·5 to 3·5 respectively.

In regard to this discrepancy of numbers, we must remember there were thirteen children who were stillborn. Had they lived, it is possible the ratios may have been more accordant; they may have also been more discordant. We have yet to learn what is the relationship between the tubercular

diathesis and the "struggle for existence" during intra-uterine life. We have also yet to learn whether the people who are N N are more resisting in their powers than those who are N n. For of the 15·7 persons who are expected to be normal in this Pedigree, only one-third will be N N in constitution, and the rest N n. We are not justified in believing that both types will necessarily be identical in resisting powers. The one may resist disease under all conditions, and the other only contract it under exceptionally bad conditions, leaving it to the persons of n n constitution to fall victims under even the best conditions, foredoomed to die of tuberculosis. And, as a matter of fact, No. 6 in F_1 has had only a local glandular attack and is now apparently quite well. Is she an N n? If we take her and her brother, No. 7, out of the list, then we have 9 : 5 where 10·5 : 3·5 is expected.

The young man No. 7 is from the modern pathological standpoint an interesting case, or rather will be, if tuberculosis declares itself in him. Can the hidden weakness in his constitution be stayed by an opsonic substance?

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(3) A Pedigree of Human Hybrids.

Segregation of European Skin Colour in a Quadroon Fraternity.

BY GEO. P. MUDGE.

I am indebted to the courtesy of Mrs. Haig Thomas, who very kindly passed on to me some

information supplied by her cousin, Colonel H. de H. Haig, R.E., for the following extremely interesting case of the segregation of European skin colour in a generation of quadroons. To Colonel H. de H. Haig I am under a very great obligation for the interest he manifested in my enquiries and the great trouble he took in the endeavour to ascertain the answers to them. He very kindly sent me a photograph of a group of persons in which three of the ladies, who belong to the pedigree, appear.

The facts of the pedigree are given upon the authority of Colonel H. de H. Haig, who was personally acquainted with all the persons (with three exceptions) who appear in the pedigree. I am not of course permitted to mention names, nor can I describe the geographical locality to which these events relate. It is, perhaps, permissible to say that they did not occur in the West Indies.

The Facts of the Pedigree.

The Parents = P Generation.

The father of the family was almost certainly a yellow mulatto. His parental origin is not known for certain. His skin was light yellow brown; it was the "colour of leather called nut-brown" and "lighter than a new brown boot." His hair and nose were quite negroid, and his lips slightly so.

The mother was an Englishwoman, and a daughter of English parents of good social position.

The mulatto father came to England to study medicine, and upon qualifying, he married the English

lady and returned to his birthplace for the purpose of practising his profession.

The Children = F_1 Generation.

There were seven children. I will describe them in order of age.

No. 1 is a daughter. "Her hair and nose are unmistakably negroid. Her skin colour is like her father's, but lighter. Her lips are not noticeably negroid. She is very easy going and good tempered."

Nos. 2 and 3. Both were daughters. "The skin colour of both is indistinguishable from that of ordinary English girls; it is quite white. The hair of both is typically European in form, there is no trace of the negro crimp, and in both it is dark brown in colour. The two girls are very beautiful and have married Europeans, both of them officers in the Army."

No. 4 is a son. He was not seen by Colonel H. de H. Haig, but he is known to have both skin colour and negro hair like the brother next to be described.

No. 5 is a son. "He had negro hair; the skin colour was like his father's but lighter, though darker than that of his eldest sister."

In addition there are two other daughters whom my correspondent has not seen. One of these is married, but it is not known if she has children.

The Grand-children = F_2 Generation.

Both the married daughters Nos. 2 and 3 have a family. One has a son and a very pretty daughter,

and the other has one son and three daughters. These six children have not been seen by my correspondent.

The Deductions from the Pedigree and General Observations.

There is one fact which I think needs particular notice here. In considering this pedigree, we are bound of course to face certain possibilities. Many Europeans were resident in the island. But it needs only the most casual glance at the photograph to render it at once clear that the likeness between the three sisters is very pronounced. There can be not the slightest doubt, I think, that they are sisters by the same parents. I have shown the photographs to several friends and they concur in my belief. Of course the resemblance is much greater between the two white sisters than between them and the coloured one. But even between them there is, in the form of the eyelids and the eyebrows, a strong suggestion of sisterhood.

If this were a Mendelian phenomenon of the simplest order, such as Professor Pearson has imagined possible, we should expect that the offspring of such parents would be in equal numbers "whites" and coloured persons. There are actually 2 "whites": 3 coloured persons plus two of unknown type. But it is clear that this is not a simple type of Mendelian phenomenon, because the coloured children are not alike in respect of skin colour or hair characters, and they are not exactly like their father. It is clearly a more complex type of Mendelian

inheritance. But whatever the nature of its complexity it is obviously a case of Mendelian segregation of European skin colour in the gametes of a coloured father. It is not necessary to consider the general problem in detail here, because it is fully considered in a later article in this Journal entitled "Skin Colour in Human Hybrids," pp. 163.

PAPERS READ TO THE MENDEL SOCIETY.

THE EVOLUTION OF MAN.

(*An Address delivered to the Mendel Society, February*, 1908.)

By J. T. CUNNINGHAM, M.A., Oxon., F.Z.S.

THE present paper is an attempt to consider some of the modern conceptions of Biology in relation to the human species. With regard to species in general, the Darwinian theory assumed that the differences between species were differences of adaptation, that specific characters were useful, that species were adapted to different modes of life. It has, on the other hand, been maintained by later zoologists that in the vast majority of specific characters there is no evidence of such utility, or of correlation with useful characters. The most eminent systematists distinguish now, as those of pre-Darwinian days did, between diagnostic characters, which are of chief systematic value, and adaptive characters, which for purposes of classification are often rather misleading than significant. The more useless a character is the more valuable it is as an indication of affinity. One modern school of

evolutionists, recognising the uselessness of diagnostic characters, holds that they have not been evolved by selection, but have arisen spontaneously as mutations; and, with the usual tendency to carry a doctrine to extremes, they maintain that all characters are independent of utility, all arose as mutations. The American investigator, Dr. T. H. Morgan, has published a book specially devoted to this doctrine, in which he endeavours to show that adaptations do not really exist, that mutations have occurred which could only survive under special conditions of life, which in some cases the modified creatures have found, so that habits have been determined by structure, not structure by habits. Thus in the short period of half a century we have had the swing of the pendulum of biological opinion from one extreme to another, from the belief that all characters were adaptive or useful to the belief that none were adaptive. In the meantime the common-sense view has persisted that some characters were useful and some were not, and that the former were easily modified by conditions of life, the latter unaffected by such conditions. It must at any rate be admitted that usually in studying any group of animals we can certainly distinguish between characters which have no visible relation to the maintenance of life and others which are necessary or advantageous to that purpose; and it is therefore possible to consider the origin of these two kinds of characters separately.

The human species, in spite of the attention devoted to Anthropology, and although it is to us the

most familiar species, has perhaps been less studied from the zoological point of view than any other. It is also from this point of view the most difficult, partly because it is our own species and we cannot get far enough away from it to see it in true perspective, partly because it has had such an exceptional history, having spread over the whole earth and become largely independent of physical conditions; that is to say, it has attained to a great extent the power of making artificially uniform conditions which render it independent of differences of climate, geographical features, and differences of fauna and flora in different habitats. The first question to consider is whether man is a single species or several, and what is his relation to other species. This question, as well as most of the others which I propose to consider in this paper, has been discussed with his usual thoroughness and judgment by Darwin in his "Descent of Man," so that I am really only trying to see whether we know any more about these problems than Darwin taught us.

The chief peculiarities of man, as compared with his nearest allies, the anthropoid apes, are all adaptive and useful characters, namely, the erect position, the structure of the hand and foot, and the faculty of articulate speech. Associated with the possession of language are the size and differentiation of the brain, especially of the cerebral hemispheres, and the correlated size and shape of the cranium. The reduction of the jaws, teeth, and face generally is also a characteristic feature, and adaptive to the

diminution in the use made of the jaws and teeth in feeding and fighting. If man is regarded as a single species, then he affords a conspicuous instance against the doctrine that specific characters are not adaptations, but it must be remembered that the contention is not that no specific characters are adaptive, but that in a vast number of cases several species are distinguished and named which live in the same district under the same conditions, and that where they live in different habitats there is no evidence that the characters correspond to differences in the mode of life. On the other hand, there is no reason why a single species should not become adapted to some peculiar mode of life, but then it would be a matter of opinion among systematists whether it should not be placed in a separate genus.

Before proceeding further with this part of the subject, it is interesting to consider the origin and nature of these adaptations. While others have been disputing whether acquired characters are ever inherited and whether adaptations are due to the inheritance of acquired characters, Dr. Archdall Reid has made the brilliant discovery that such adaptations as those which distinguish man from the anthropoid apes are not inherited at all, but are acquired by every individual in the course of his development. Inborn or congenital characters, he says, are developed by the stimulus of nutrition alone, acquired characters are developed by the stimulus of use. Modifications acquired as a result of use and disuse are clearly never transmitted, because they never develop except in

response to the same stimulation as in the parent: "Plainly, then, that which is transmitted to the infant is not the modification, but only the power of acquiring the modification under similar circumstances —a power which has undergone such an evolution in high animal organisms that in man, for instance, nearly all the developmental changes which occur between infancy and manhood are attributable to it."* Now, while it must be admitted that it is very important to ascertain how far characters are developed entirely as the result of the constitution of the germ-plasm, and how far they require an appropriate stimulus, I think Dr. Reid attributes excessive importance to the latter factor. We know that a child only learns to speak by hearing speech, but we also know that monkeys and dogs do not learn to speak under the same conditions. The difference, therefore, between man and his nearest allies is not in acquirement, but in hereditary constitution. Indeed, Dr. Reid admits as much, but he puts the fact in other words, and here, as in much else that he has written, it seems to me that he imagines he has discovered something new when he has expressed what was known before in different and unnecessarily abundant language. He says that since "Nature" has endowed animals with the power of making not all possible acquirements, but only certain fixed acquirements that are commonly useful to the species, therefore species differ not only in characters which are inborn, but also in those which are acquired; for

* Principles of Heredity, 2nd Edition, page 35.

instance, the forelimbs of both ox and man grow greatly in response to use, but the lines of growth are very different. Exactly. Of course, Dr. Reid assumes that the difference in the power of making acquirements is due to natural selection. The power of growth in response to exercise resides, he says, not especially in the parts which are most used, as joints, teeth, or tongue, but in the parts in which it is most useful ; in other words, in those parts where it has been evolved, not by use, but by natural selection. It would be difficult to compress a greater number of fallacies into such few words. The chief fallacy lies in the word use. Use of a muscle means contraction, and contraction causes growth of muscle ; but it is obvious that joints do not contract, and that a joint has no size. The fact that joints can be developed by use is proved by their actual formation occasionally in neglected fractures. It is also obvious that the tongue being a muscular organ is developed by exercise not merely in absolute size, but in the complexity and precision of its movements, as in the muscles of the hand ; otherwise we could not learn to speak. In fact, it is precisely because the power of acquiring certain structural adaptations resides in those parts which are used for certain purposes that Lamarckians conclude that the power to acquire and the acquirement are due to the same causes ; in other words, that the hereditary or congenital factor and the acquired factor in adaptations are both due to external stimuli. The contrary view is mere assertion based on no evidence. What evidence, for

instance, is there that the ancestor of man possessed a variation in the power of acquiring the upright position, independently of the attempt to walk on his hind legs ?

Dr. Reid supposes that this power of making acquirements is greatest among the higher animals, and little or not at all present in the lower animals and plants. He instances the frog, and expresses his belief that a tadpole enclosed in a hole or crevice, if supplied with food, would develop into a perfect frog, and that this is possibly the explanation of those cases reported in the newspapers from time to time of perfect frogs found enclosed in stone in quarries. It is unnecessary to discuss seriously this suggestion ; it will be sufficient to consider how much foundation there is for the dictum of Dr. Reid that the frog's body gains nothing from use, and its mind almost nothing from experience. This implies that the metamorphosis is entirely due to heredity and not at all to stimulation. It has been proved, on the contrary, that aquatic larvæ of Amphibians can be made to retain the larval state by forcing them to breathe in the water and not allowing them to breathe air, so that in this case, as in many others, the development is partly due to acquirement in Dr. Reid's sense of the term. Dr. Reid contrasts with the supposed development of the frog the alleged fact that if the limb of an infant be locked by paralysis or by a joint disease it does not develop into an adult limb, but there is every reason to believe that the same statement would be true of the frog.

We must conclude, then, that man differs from the anthropoid apes chiefly in adaptational characters, and that these characters are inborn or congenital. They are congenital in two senses; firstly, in the sense that they develop to a certain degree under what Dr. Reid calls the stimulus of nutrition, by which he means nutrition, moisture, heat, and oxygen, the essential conditions of all development and all life; secondly, that they attain their adult development from a hereditary tendency to certain modes of use and function, and from a degree of exercise which would not produce the same development in any other species. The new-born infant differs from the adult man, but it also differs from the new-born ape in all essential human characters, and that adult has acquired structural peculiarities which no ape could possibly acquire from any stimuli in its own lifetime. Obviously these are not merely specific characters, and man is not merely a species of a wider genus. Adaptational differences are characteristic among other animals of a genus, or of a family, or of larger groups. For example, among the mammals the orders are distinguished by differences of adaptation, *e.g.*, the Cheiroptera and Carnivora; but within a single order a family may be so separated, as in the case of the mole family. It is not easy to find a genus so distinguished. Man thus appears to have the rank of a family. The condition of the hair in man might possibly be regarded as a diagnostic character which is not adaptive; if the absence of hair on the body be explained by uselessness, still the

special development on the head looks like a non-adaptive feature. It is difficult, then, to regard man as merely a genus of anthropoid apes.

On the other hand, we do not find that man can obviously be divided into a number of distinct species as other families of mammals can, or as even a genus can be divided. There are distinct races of man, and the question is whether these correspond to species among other animals. To discuss this question we have to consider the diagnostic characters of these races. Darwin considers them very carefully in the work I have already mentioned, and comes to two remarkable conclusions which are of chief importance in relation to the object of this paper—firstly, that these characters graduate into each other so that the races cannot be absolutely defined; secondly, that they are in no sense adaptational. He says that, so far as we can judge, none of the differences between the races of man are of any direct or special service to him, nor can they be accounted for in a satisfactory manner by the direct action of the conditions of life, nor by use and disuse, nor through the principle of correlation. He then proceeds to enquire whether they can be explained by sexual selection. He concludes that this process will not explain all the differences, but that there is a residuum which must provisionally, at least, be regarded as due to spontaneous variations which have become constant and general without selection. Thus we find Darwin in this case compelled to adopt the view which in my opinion still holds good in man

as in other animals, that there are two categories of characters, namely, the adaptive and the non-adaptive. The latter are of the same kind as those which are called mutations by modern biologists, while the former, in my opinion, are directly due to stimuli. Where the stimulus is functional, the modification is such as to render organs and structures more fitted for the functions; but certain conditions may produce a direct result which has no connection with function, and which is, therefore, not in the ordinary sense useful or adaptive; for example, the absence of light stimulus causes the absence of pigment from the lower sides of flat fishes, but this character is neither useful nor adaptive. The real distinction between the two kinds of characters according to my views is that those of one kind are due to external stimulation, those of the other kind are independent of external causes—the latter are mutations, the former may be called modifications.

Huxley's classification of the races of mankind is a somewhat simple one. He divides them into two primary divisions, resembling perhaps genera, namely, the Ulotrichi with woolly hair, and the Leiotrichi with straight hair. The Leiotrichi he subdivides into four groups: the Australioid, the Mongoloid, the Xanthochroi or fair whites, and the Melanochroi or dark whites. The characters in which the races differ are colour of skin, hair, and eyes, shape of cranium, whether brachycephalic or dolichocephalic, character of hair, projection of jaws, shape of features, especially nose and eye-apertures. The negro race

is one of the most distinctly marked, its characters being black-brown skin, woolly hair, prognathous, dolichocephalic skull, thick out-turned lips, flat nose. The type is most perfectly developed in equatorial Africa, as in Guinea ; to the north it has crossed with the Berbers, on the east with the Arabs ; to the south it shows reduced characters apparently without crossing. The Bushmen and Hottentots, while apparently belonging to the negro stock, are much lighter in colour, and this is some evidence that the black of the negro is originally due to the tropical sun. In the East Indian Archipelago is a type allied to the negro, but not identical, extending through the region called from its presence Melanesia, from Papua to Fiji. These have woolly hair, but the brow ridges and the nose are more prominent than in the negro ; the colour varies from black to chocolate. The Tasmanians, now extinct, are stated to have been an isolated colony of this race. The Andaman Islanders form a connecting link between the eastern blacks and the negroes, but although they have frizzled hair, which they shave off, they are brachycephalic, less prognathous than the negro, and the nose is narrower. The Australians are brown in colour with wavy black hair, dolichocephalic, having prognathous skulls with well-developed brows, and wide but not flat nose. Allied types are believed to occur in India, namely, the hill tribes descended from the primitive inhabitants, and in North-east and North Africa in the Nubians, Berbers, and the ancient Egyptians. The Mongoloid race seems to occur in its purest state in

Central and Northern Asia, and its features are : yellowish colour, long black straight hair, high cheek bones, short nose, and, especially, slanting eye apertures. The allied races are of immense extent and the features in many considerably modified either by variation or inter-crossing. The skulls in some of these are extremely dolichocephalic. In Asia the Mongoloid type is extended by the Japanese, Chinese, Siamese ; in Europe its invasions are represented by the Turks, Finns, Lapps, and Hungarians. The Malay race seems to be a branch of the Mongoloid, and extends over Java, Sumatra, Borneo, and can be traced to New Zealand and throughout Micronesia and Polynesia. It is supposed that the Maoris and the islanders of the Pacific, differing considerably from each other in characters, have arisen chiefly from crossing between a race allied to the Malays and the darker Melanesians. The inhabitants of the whole of America, North and South, seem to belong to one main type supposed to have been derived from the Mongolian. The uniformity of the American Indians, as compared with the diversity of types in the Old World, is one of the most striking facts in anthropology, and is most probably explained by the view that America was populated from a single race, the Mongolian, from Asia, within a period so comparatively recent that no great divergence has been developed. Lastly, we have the white men, whose home is chiefly in Europe, and who include a great variety of subordinate types.

The type with blonde hair and blue eyes is found chiefly in the north, *e.g.*, in Scandinavia and North Germany, but representatives of it are found in North Africa and in Western Asia. There can be little doubt that this is a pure type, but Huxley suggests that the Melanochroi originated from the mixture of the Xanthochroi and Australioids of India and North Africa.

I can only pretend to offer a few suggestions on the characters which distinguish the races thus rapidly surveyed. Mendelians will assume that they are all simple mutations, but this does not seem to me to be a reasonable conclusion. Some of them, as in other groups of animals, are differences of degree in those adaptive characters which distinguish man from the apes. For example, prognathism and the size of the cranium and brain. The decrease in prognathism is obviously associated with the degree and duration of civilisation, so that this character in negroes and Australians is adaptive. Perhaps the same may be said of the extreme dolichocephaly associated with a sloping forehead in the same races, but we cannot say that the men with the shortest skulls are the most civilised, for some of the least civilised Mongolians are more brachycephalic than the English. In my opinion, there is good evidence that dark or black skin-colour is correlated with the light and heat of the tropical sun. It may be objected to this that the American Indians of the tropics are not black like the negroes, but they are known to be considerably darker than those of the north. At

anyrate, we have the following facts: that no very dark race occurs in temperate climes either north or south, that the negroes of equatorial Africa are distinctly darker than the Bantus and Hottentots of South Africa, and that the negroes of North America have become lighter since their importation. I have sometimes thought that perhaps modifications due to stimuli might differ from mutations in not exhibiting Mendelian segregation, but the evidence so far as we have any is contradictory, for while crossing of negroes with whites always gives intermediates in all degrees of mixture, we have a constantly repeated segregation when dark whites and fair whites interbreed. Eimer mentions this as especially conspicuous in South German villages, where the inhabitants continually intermarry, and yet pure blondes and dark children occur constantly in the same family. This may be typical Mendelism, the dark complexion being dominant and the blonde recessive; but it requires further investigation.* There are, however, many race characters which seem to be evidently mutations, since there is no evidence that they are useful or due to external conditions. As examples of these, we may mention the character of the hair with regard to curling, the direction of the eye-aperture, the prominence of the nose. We have little precise evidence concerning Mendelian inheritance in these. Mr. G. P. Mudge published

* Mr. C. C. Hurst has recently shown that dark eyes of any shade are dominant to blue eyes, and that the two characters segregate in Mendelian fashion.—*Proc. Roy. Soc.*, 1908.

lately some data concerning the inheritance of such features in crosses between Canadian Indians and Europeans, but it seemed to me that he found all the Indian characters segregating together in one individual, and that this could only occur in a much smaller proportion of cases than he stated. His evidence would have been more convincing if he had dealt with single marked features and proved that they segregated.* In negro crosses we have no satisfactory evidence of segregation in any character, whether adaptive or otherwise.†

A word or two may be devoted to the consideration of Darwin's suggestion that sexual selection may account for the non-adaptive character of human races. I have shown that where the characters are confined to one sex selection cannot be the cause of this limitation. Where a character is already unisexual, however, it may vary and remain unisexual, as, for instance, in the human beard. The question, then, is whether selection by the female is required to account for a difference in the beard, or whether the mutation might not establish itself without selection. In deer the antlers differ in different species in size and shape, and it could scarcely be suggested that the particular size and shape in a given species was due to the fact that they

* He did, however, state that segregation occurs between the blue eye, fair complexion, and light hair of a Scotsman, and the black eye, olive skin, and black hair of the Red Indian.—*Nature*, Nov. 7, 1907, p. 9, and Proceedings Royal Society of Medicine, Vol. II., No. 3, p. 124. Jan. 1909.

† This, of course, does not now hold true. See pedigree No. 3 in this Journal, page 24.—*The Editor.*

were the best for fighting, or the most admired by the female. But sexual selection might affect characters which were not limited in inheritance ; for example, the black of negroes might be due to the preference by either or both sexes for the darkest skin, but this is not a probable view.

In a short paper like the present I can only give a very imperfect outline of the subject, but I hope I have said enough to show that anthropology requires to be re-investigated from modern points of view. My own provisional conclusions are that man affords an example of a single species which has started a new group, which might become a genus or family. Other genera or families may have originated in this way by a single species adopting a new mode of life. The evidence does not seem to me to support the view that all human characters, adaptive and non-adaptive, can be regarded as mutations independent in their origin of habits or functional or other stimuli. The evidence seems to me to agree with the view I take of animals in general, that adaptive characters are due, not to selection, but to the effects of functional and physical stimulation, and that diagnostic characters are not adaptive and therefore not due to selection, but to blastogenic variation.

Mr. Cunningham's paper read to the Mendel Society in February 1908, was published in "Science Progress" in the following October. We are indebted to Mr. John Murray the publisher, for kind permission to reprint the article.

Biological Iconoclasm, Mendelian Inheritance and Human Society

A PLEA FOR THE OPERATION OF A MORE VIRILE SENTIMENT IN HUMAN AFFAIRS.

An Address delivered to the Mendel Society and to the Eugenics Education Society, in June, 1908.

By GEORGE PERCIVAL MUDGE.

No biologist who has lived in our community and watches its affairs, can feel that all is right in the modern sentiment that is guiding it. If he fearlessly faces the facts, and puts aside all prejudice arising out of mistaken ideas, he cannot fail to see that a continuation and extension of this sentiment will lead the nation—which is last upon the scroll of greatness—to its destruction.

Let us first consider the nature of the modern sentiment of which I speak. It is characterised by a wide range of various phases, but all of them may be expressed in one general formula, *i.e.*, "Preserve and procreate the unfit citizens, and hamper and discourage the fit." Let me illustrate the different ways in which this formula has been applied, by giving one or two examples taken from the statements and the conduct of the people who are endeavouring to apply it. When Canon Barnett says that the "weak of human society are so because of the shortcomings of the strong"; when Mr. Philip Snowden

says "he believes we can change human nature by education and legislation"; when Lady Warwick and Sir John Gorst demand relief for underfed school children from the Lambeth Board of Guardians; when certain well-intentioned but misinformed people demand that every elementary State school shall have an army of nurses to clean and bandage children's cut fingers; when Mrs. Sidney Webb impliedly advocates that the children of workhouse paupers can be made vigorous and strong and converted into self-reliant and independent citizens if only they are nursed by State employed nurses; when Mrs. Humphry Ward believes that the children of the lower social classes are going to be made happy, well ordered, contented, and healthy citizens if the State will but provide their recreation and their playing grounds; when an Act of Parliament decrees that public servants paid out of public moneys shall cleanse verminous persons; when certain medical men assert that the stunted weaklings whose parents live in one-roomed tenements are more stunted than those whose parents live in two-roomed tenements, and these more so than those of three-roomed tenements, and in consequence they conclude that our nation is undergoing physical degeneration along this road, and then further by implication recommend that the State (*i.e.*, the fitter citizens) shall provide four-roomed tenements; when one of the leader-writers of *The Times* asserts that "individuals, like races, are the product of the environment," implying a direct and moulding action of the environment;

and when the Glasgow Board of Guardians, at the expense of the ratepayers, send the children of reprobates, drunkards, criminals, prostitutes, and general failures to be boarded out and cared for among the intelligent, virtuous, and thrifty inhabitants of the western coast of Scotland, in the implied belief that they will inevitably become good citizens because of the changed environment—they are expanding this remarkable formula, and are endeavouring to teach the strange doctrine that sand can behave as granite if only both are beaten upon by the same tempestuous sea. Verily, the age of fairy tales has not yet passed away!

This corybantic sentiment, so wildly solicitous of the well-being of the civically unfit, is being promulgated from every pulpit, by every newspaper, by every social reformer, by every political opportunist, and by every one of the great multitude of morbid and neurotic sentimentalists. It is a phase of hysteria permeating into the soul of our nation. Surely the worst thing any nation can do is to concern itself in frantic endeavours to save or pamper its weaklings and unfit. It is dangerous to ignore the clear teachings of science, that in the hereditary processes there is an irrevocable coming out at one end of the chain of generations of the characters that go in at the other; they come out not only as they went in, but numerically increased, because the individuals bearing them are multiplied in geometrical progression; they tend to spread ever wider in each successive generation. That which

started in the first generation as a pair of parents may in the seventh generation become a million individuals! We are not fully aware of the silent dangers that we incur in our endeavours to save the unfit and render their lot easier. Useless as they are, and unable to contribute to national resources or progress, they become an ever-increasing source of injury and harassment to the fitter citizens, who, either voluntarily or under compulsion, contribute to their maintenance. While the artificially-endowed unfit, who are being relieved in ever-increasing degree of their responsibilities, are faithfully procreating, the artificially-disendowed fit citizens, whose responsibilities increase as those of the other class decrease, are limiting the number of their children. For any nation, under any circumstances, this is a dangerous condition; but for a nation upon the walls of whose temples has been written, in the writing of contemporary events, the warning that in the not remote future she will be called upon to measure her strength with other nations upon the battlefield, it is nothing short of criminal and disastrous folly.

If our nation must depart from the *laisser-faire* policy, and make a positive effort to determine what the qualities of its citizens shall be, the only sound line of action is to endeavour to encourage and preserve the worthier citizens, and to discourage and eliminate the less worthy ones. And that result can be attained without any conscious effort, for those best fitted to survive under the conditions which reign on earth will do so by purely natural

processes, without any artificial and compulsory interference or help. And we may be sure that these processes are benign; biological teaching is clear on that point. By these processes, throughout the whole realm of nature, the maximum amount of happiness and vigour is attained, with the minimum amount of misery or pain. Human interference but decreases the former, while increasing the latter. It is better, in the faith of the greater religions, to accept our destiny.

I will come now to one of the principal objects of this address, *i.e.*, the refutation of the main assumption, sometimes expressed and sometimes implied, upon which this modern sentiment rests. It is tacitly assumed by the exponents of this sentiment that the qualities of the individual depend upon his environment; that he is vicious because of the viciousness of his surroundings, and good because his environment is made up of good influences. It is entirely wrong; there is no justification for it at all in the realm of fact. The very converse is true, for in social life the environment is the product of the individual, and not *vice versa*. The stunted individuals are not the product of a one-roomed tenement, but the one-roomed tenement is the expression of the inherent incapacity of this race to be able to do anything better for itself. They are not undergoing physical degeneration because they are living in a one-roomed tenement—many a better man has done that—but the one-roomed condition is the natural outcome

of their already-existing physical, moral, and intellectual degeneration. These degenerates are mutations, and breed true to their degeneracy. The qualities of an individual are determined within him; they are inherent, and depend upon the molecular architecture of the racial lines from which he has sprung. An individual is merely a piece of his racial stock; what he can do depends, not upon his environment, but upon the innate qualities which he has inherited from his stock. Let us observe that it is a potato plant, and not a dahlia, that grows out of a potato tuber; it is a dahlia, and not a potato, that grows out of a dahlia's tuberous root-stock. A change of soil will not alter this fact. One potato may be grown among an army of dahlias, but it comes up a potato plant in spite of that. The susceptible wheat plant falls a prey to the "rust," but the inherently immune plant growing in a field of "rusted" plants remains unattacked and healthy. From the fertilised ovum of a fish there is developed a fish, and not a bird. Transference of the bird to water, or of the fish to the skies, will not convert the one to the other. But the exponents of modern sentiment are preaching, either directly or by implication, that it will. It is a strange doctrine, and is so contrary to the common experiences of life, that it is marvellous it should ever have been believed. But I suppose there is no limit to the amount of delusion which the circumstances of a democracy, with the various appeals that are made to the uncultured and unculturable masses for their judgement, will allow to be imposed upon it.

Let me now proceed to describe one interesting case, to show how false is this doctrine of environment, before I pass on to the second main purpose of my address, *i.e.*, to demonstrate the irrevocableness and inevitableness of the transmission from generation to generation of those inherent qualities that are, for all practical purposes, the sole determining cause of what an organism will be and do. In the Tyrolese valleys and elsewhere there grow two plants known as the Summer Savory and the Flax. In the valleys both these plants are green in colour. This green colour is due to a substance called chlorophyll, and one of its inherent qualities is that it can only exist as chlorophyll within a certain range of light-intensity. In a too dull light it is not formed, and in a too intense light it is destroyed. But this chlorophyll is necessary for the existence of the plant; in its absence death ensues. Now, as we move higher up a mountain the sunlight becomes more intense, because there is a less thick layer of atmosphere, with its contained water particles, to absorb a certain measure of it. Consequently at a certain height the light-intensity will be sufficient to destroy the chlorophyll and kill the plant. The hypothesis of this modern sentiment demands that these two plants, placed under the same environment, shall behave in the same way. For, assuredly, if it is environment which determines the qualities or behaviour of organisms, then the same environment shall produce the same response in all organisms subjected to its influence. If this be not the as-

sumption underlying all efforts at social reform then what is the justification for them ? How, then, do these two plants react to a changed environment, when they are grown in the intenser sunlight that shines on the mountain slopes at a height of 2,195 metres, instead of in the shadier valley ? What justification, as living organisms, do they give us for those almost frantic efforts which are designated under the collective and misleading phrase, social reform—efforts which ostensibly aim at improving the environment of the weak and unfit on the assumption that thereby they will be made strong and fit ? No justification at all do we find in the behaviour of any living organism. Nothing, in fact, but condemnation of such efforts. For when these two plants are moved from an old environment common to them both, and are grown from seed and reared under a new and identical environment, do they both react in the same way to the same conditions ? Not at all ! The Summer Savory changee its colour, and becomes suffused over its whole surface with a purple colouration. Careful examination of the tissues of the plant shows that this new colour is developed in the most superficial tissues only, the internal tissues remaining unchanged. What is the meaning of this change of colour under the new environment of an intenser sunlight ? The purple-coloured sap which is thus called into existence possesses the power of absorbing certain of the rays of sunlight, and especially the particular ones which are most active in the destruction of the

plant's green colouring matter. The organism is thus protected from the harmful influences of an increased intensity of sunlight. Its green colouring matter—its chlorophyll—remains undamaged, and can continue to discharge those vitally-important functions that belong to it. This plant thus thrives, flowers, and seeds under the changed environment. But it is quite otherwise with the Flax. It does not develope any purple sap; it fails to respond in that way, or in any other protective way, to its changed conditions. It cannot even reach the flowering stage, for soon after it has passed the seedling stage its chlorophyll becomes destroyed by the intenser sunlight, and it perishes.

Here, then, are two organisms that give us a crucial test of the validity or invalidity of this remarkable environmental doctrine. And the answer is definite and condemnatory! What an organism can and will do does not depend on the environment, but on the qualities of the organism itself. It is one of the inherent capacities of the Summer Savory to respond to this particular kind of new environment, and to adapt itself to the changed conditions. It is one of the inherent defects of the Flax that it cannot do what the Summer Savory does; and the only result of applying the environmental doctrine to it is to kill it. This is a fact which is not without its social significance. It is a fact which is not without illustration in the case of Man himself. Let me give the illustration, for it is highly significant and entrancingly interesting. Certain missionaries,

supremely innocent of biological facts, rushed into some complex biological problems and endeavoured to civilise the Tasmanians. Acting under the impelling influences of mistaken ideas, but prompted by the noblest motives, they sought to change the environment of these people, and to give them what they imagined to be a better one. Their efforts were rewarded by the extinction of the Tasmanians! In something less than fifty years the whole race of this people was eliminated, not by war, or plague, or pestilence, but by the operation of those processes that flow from the application of that crude, theoretical, and sentimental conception of the power of the environment to make good out of bad that is the dominating influence of our own social efforts.

Some day we shall learn that the characters of men are relatively fixed and stable, and that they are the products of evolution under set conditions. As we find men, at any given period, in any given place, they are adapted to the particular combination of conditions under which they have evolved, and to no other. Arbitrarily change these conditions, as in the case of the Flax and the Tasmanians, and the organisms, who are thereby no longer fitted to the new conditions are destroyed. Leave them alone as Nature made them—"let the Tasmanians roam as they were wont, and undisturbed"—and they will thrive. Interfere with them, by setting up a dogma that rushes in where biological philosophy fears to tread, and the objects of our solicitude "become bewildered and dull, they lose all motives

for exertion, and get no new ones in their place," and ultimately they perish.

These are considerations which should be recognised before any endeavour is made to interfere with the conditions and natures of the citizens of our own communities. In the same way, but in lesser degree, that the inherent qualities of a Tasmanian are different from those of an Anglican Bishop, so are the inherent qualities of the lower social strata different from those of the upper strata in civilised communities. And just as the Bishop, with his higher aspirations and nobler sentiments, failed to recognise that the Tasmanian had not got them, and could not be made to have them, so certain well-intentioned, generous, impulsive, charitable, and philanthropic persons in the higher social scale fail to recognise that the aspirations and ideals of the lower classes are wholly different from their own. And just as with the Tasmanians, so in our own social efforts, disaster and destruction will follow any interference with those benign but merciless processes of Nature which allow only individuals adapted to their conditions to survive. The Tasmanians lived the life they did because their inherent desires impelled them to do so, and there was nothing in their physical environment inconsistent with them. But neither was there anything inconsistent in that physical environment with half-a-dozen other modes of living, had they chose to live them. Other ways, it is conceivable, were open to them, but they chose the particular one, and the only one,

which their inherent qualities made possible. The life they lived was the product of their desires—it was the outcome of their innate nature; they were not the product of their mode of life. They made their life, not their life made them. It is the same with our own social classes. The mode of life of the higher strata is the outcome of their inherent qualities in just the same way that the mode of life of the lower is the product of their inborn desires and capacities. Endeavour by altering the environment to compel a cultured and refined woman of the upper classes to lead the life of the slums, and we shall eliminate her race. But, clear and obvious though this is, social reformers fail to recognise the truth of the reverse process. Try to alter the environment of the lower classes, and compel them to give up their inborn habits and desires—which are the product of their line of evolution—and to lead a wholly different life, and we shall repeat the concluding chapter of Tasmanian history. The higher classes are the outcome of their evolution, and the lower of theirs. The existence of social classes is a natural fact, and the existence of different grades of social conditions is but the natural outcome of that fact. The social conditions are the products of the social classes, not the social classes the products of social conditions. It is in this matter that those medical men who are interested in social problems and social reformers have confounded cause and effect. And it is out of this confusion that modern social sentiment has sprung. So long as this sentiment

exists, and so long as it is operative in guiding and initiating social legislation, it constitutes a source of the gravest danger to the nation. It is time we awoke. It is time we turned our attention to Biology, and refused to heed any longer the "rope-dancers in the market places."

Let us pass now from the question of environment to consider inherent qualities and the way in which, once they have come into existence, be they good or bad, they irrevocably pass on unaltered through successive generations. An organism does not manifest a single quality alone, but is made up of a complex combination of many qualities. Some of these qualities, be they structural or psychical, are themselves not simple, but complex. Some of them are independent of others in the hereditary transmission, but others are correlated and always go together, in larger or smaller groups, from generation to generation. An individual, like his environment, is really an aggregate of complexities bound up within each other. This fact renders all questions of social interest extremely difficult to consider, and is the best justification of the *laisser-faire* attitude. If we depart from the certain ground of Nature, and artificially touch one link in the series of complexities that are interlinked and make up the tangled chain of life, it is not possible to forecast even the immediate, much less the remote consequences. They are always such as we never expected them to be. Nevertheless, certain main trends of this chain are sufficiently clear for practical

purposes, and can be seen with sufficient distinctness running through the phenomena of life. These are the facts that I would like now to bring to your attention.

When a tall person marries a short person, or one of sweet temper marries one of a bad temper, there are three conceivable possibilities as to the nature of the immediate offspring. We can imagine that tallness and shortness, or sweetness and badness, will blend with each other in each case, and produce offspring not as tall or as sweet as one parent, nor as short or as bad-tempered as the other. Let us call this the hypothesis of "blended inheritance." We can, however, imagine another alternative, and conceive that some of the offspring may be like one parent and some like the other, or that, under certain circumstances, all the offspring in one generation may be like one of the parents only, those which resemble the other parent, in the particular character considered, appearing in the next or later generations. In such a case there has been no blending of the two alternative characters, for they have passed on to the next or later generations distinct from each other, each retaining its own feature of distinction. Let us call this the hypothesis of "segregated or alternative inheritance." The third alternative is that the union of two characters in marriage shall produce an offspring showing a new quality, different from the two which by their union produced it. We may call this "diapheromorphic inheritance." There is yet a fourth possibility, which can, however, only occur with certain characters. The bodies of

the offspring may be a mosaic of the two characters of the parents. We may call this "mosaic, particulate, or pœcilodynamous inheritance."

I am not concerned now in discussing these types of inheritance, for it is to be observed they are all based upon the visible body characters. They are descriptive of the somatic features only in inheritance. My present line of enquiry will deal with somatic or body characters as incidental but still necessary features of consideration. It is with the gametic or sex-cell characters, which are, of course, invisible to us until they are manifested in the body cells, that we shall be essentially concerned. And I would like you to try and form a mental picture for yourself of an individual as a compound made up of a large number of sex-cells, which are surrounded by body cells, but which are in a sense independent of them. The body cells are the perishable cells; they live our life for us, and then die. But the sex-cells, in a sense, are the immortal cells, for they carry on our characters from the generation in which we live to all those which succeed us. Whatever the body cells may do, there is no evidence to show that the sex-cells respond to environmental influences, in the sense that their innate qualities can be altered. Indeed, their stability is a matter of vital importance, for without it there could not exist species that, throughout long periods of time and through a wide range of geographical distribution, retain their characters unaltered. There could be no survival of the fittest, if the qualities which made a race the fittest were not stable and, therefore, persistent.

The stability of the sex-cell is a fact that I would like you to grasp, because it is one of very great importance in considering social problems. What we have always to bear in mind is not so much a question of what the individual himself may be, but what is the nature of the characters which his sex-cells are carrying and transmitting to successive generations. And there is another fact of equal importance which we should endeavour to fully understand now, and this is that the body or visible characters of an individual are not necessarily a reliable indication of the characters which his sex-cells may be carrying. Three grey-coated animals, externally similar, may be carrying in their gametes (sex-cells) very different qualities, so far as colour is concerned. One may carry nothing but greyness, another greyness and albinism, and the third may carry greyness, blackness, and albinism. The offspring of these three apparently identical individuals would be very different. If these processes are at work in human life—and I shall presently show you evidence indicating that they are—clearly it is a matter of the widest significance, and one which all social reformers have overlooked. If a youth from the lower classes manifests a few superficial accomplishments—can, for instance, pass an elementary scholarship examination by a process of cram—but is carrying the civic qualities of his class, is it worth the while of a State to spend the money of better citizens upon that youth? Is it worth while to spend money in order that we may produce in an individual certain

artificial accomplishments which can always be had for nothing in the class immediately above that to which he belongs? In this next higher grade we not only obtain the artificial accomplishments, but in addition better innate civic qualities. What we are really doing by such methods is paying for an inferior stock when we have already the better one for nothing. We pay for a few putative accomplishments in an individual whose sex-cells are going to give us an inferior civic stock! That is one consideration which this new fact of heredity bids us heed.

We are going to deal, then, with the sex-cells. When the male sex-cell unites with the female sex-cell there is produced the first body cell, by the repeated divisions and growth of which a new individual arises. What this new individual will be, and what he will transmit to the next generation, depends not upon his environment, but upon what the two sex-cells, male and female, of his parents brought in. A sex-cell which carries leaden social instincts is not going to develope into a golden individual because he has been given a golden environment. From it a person of leaden instincts only will develope.

Let us appeal, then, to accurately-conducted experiments, and see how far we can ascertain some reliable facts as to what the sex-cells are really doing. When a sex-cell of a pure black rabbit is united with one of an albino rabbit, to what sort of offspring will it give rise? Without exception, whenever this cross has been made, black rabbits indistinguishable

from the pure black parent have been the result. The whiteness of the albino parent is temporarily lost to view; but it is not destroyed or swamped. It exists, and under the appointed conditions which Nature has determined and we have now learned it will reappear, pure and unaltered. Its association with blackness, in the processes of heredity, has not altered it. Environment, even of this intimate kind, has not changed it. For, if we cross the sex-cells of the brothers and sisters of this first generation, we find that two kinds of rabbits are produced, *i.e.*, black and albinoes. Blackness and albinism have separated out again, perfectly distinct. There has been no blending of these characters, but segregation of them.

We know that each member of the offspring in the first generation which results from uniting the sex-cells of a pure black rabbit with those of an albino must have been compound individuals, because each must have contained both blackness and albinism. They are, with respect to these two characters, impure individuals. We may call them hybrids, or heterozygotes*. But the feature of interest is that although they are impure, since they carry blackness and whiteness, they are indistinguishable from the pure black parent which carried blackness only. Something must have happened to the albinism, since it does not manifest itself in these hybrids. We are not sure what does happen to it, but it does not, for our present purpose, very much matter. We have got the fact that one of the

* A term applied by Prof. Bateson.

two alternative characters exhibits itself in the hybrid, and the other does not. We may speak of the character which thus shows itself to the exclusion of the other as the Dominant one, and the one which temporarily disappears as the Recessive character.

When these hybrids are mated with each other, or are mated back with either of their two aprents, or with individuals like their parents, blackness and albinism separate out again in this second generation. But we know from experiment that all the black individuals of the second generation are not alike. This is a very important fact, and its social bearing we have already glanced at (pp. 60 and 61). Some are pure black individuals, and others are hybrids; the one carries blackness only, and the other both blackness and albinism. And there is a certain proportion in which these types appear when hybrids are mated *inter se*. It is one pure black, two impure blacks, and one albino.

If the pure blacks be bred with each other, only pure black individuals are produced in the offspring. The dominant character breeds true. If the albinoes be mated with each other, only albino offspring are produced. The recessive character also breeds true. But if the impure blacks be bred together, then the offspring again consists in this third generation of different individuals in the proportion of one pure black, two impure blacks, and one albino. The hybrids do not breed true.

Now a moment's thought will show us that we cannot explain these facts on any assumption of

the blending of the characters carried by the sex-cells of the two parents. Had blending occurred, blackness as such, and whiteness as such, would have for ever disappeared. When blackness is crossed with whiteness, if blending takes place, we should expect to find a diluted blackness. And the diluted blackness of the first generation, when bred with diluted blackness, should on this hypothesis give no other individuals but those of diluted blackness; and it should do this for generation after generation. When crossed with albinism, a dilute black should give a diluter black, but not blackness as black as the original, and albinism as pure as the first albino introduced. It is clear, then, that the blended hypothesis of inheritance fails completely in this case. And it similarly fails in a large number of other cases, comprising a wide range of characters.

Will the hypothesis of segregation give us an explanation of the ascertained facts? Assuredly it does. Let us proceed to see how. The doctrine of segregation assumes as a fundamental proposition that when two sex-cells unite, one carrying blackness and the other albinism, these two qualities do not blend or fuse in the single cell which results, but remain distinct; and that at the ultimate cell-divisions by which the sex-cells of the new individual will separate out from its body cells, these two qualities will be found distinct from each other, and *carried in different sex-cells.* If the individual is carrying only blackness, then, of course, all its sex-cells will carry blackness. And, similarly,

if it is carrying only albinism, then all its sex-cells can carry albinism alone. But if it carries both blackness and albinism, then one-half of its sex-cells will carry one character, and the other half will carry the other.

Now it is clear that so long as a pure black individual is crossed with another pure black, sex-cells carrying blackness only can meet. Hence, only a black offspring can result. And in the same way, so long as an albino is crossed with an albino, there is only one kind of sex-cell which can meet, *i.e.*, an albinism-carrying one with an albinism-carrying one. But it is otherwise when an impure or hybrid black is mated with a similar individual. Here the two parents are both carrying two kinds of sex-cells, *i.e.*, those transmitting blackness and those transmitting albinism. Consequently, at sexual conjugation, a male sex-cell which carries blackness has an equal chance of meeting an egg carrying either blackness or albinism. And the same is true of the male sex-cell which carries albinism. If we agree to let *A* stand for albinism, and *B* for blackness, then *B A* will represent a hybrid individual. This individual will be black in colour, although it is carrying albinism, because we have seen that blackness is dominant and albinism is recessive. We may, therefore, graphically represent the possibilities in this way:—

$$A\,A \times B\,B \qquad \text{Parents.}$$

$$\mid \overline{B\,A + B\,A + B\,A} \mid \qquad \text{Offspring.}$$

If No. 1 *B A*, the sister, is mated with No. 2 *B A* the brother, then the *B* sex-cells of the first may meet the *B* and *A* sex-cells of the second, and give us *B B* and *B A* respectively. Similarly, the *A* sex-cells of No. 1 may meet the *B* and *A* sex-cells of No. 2 and give us *B A* and *A A* respectively. Thus we shall obtain 1 *B B* + 2 *B A* + 1 *A A*. That is, on the average in every four members of the offspring of two *B A* parents, we shall expect 1 pure black, 1 pure albino, and 2 impure or hybrid black individuals. And this is what experiment does give us.

We have spoken so far of the segregation of these two characters. But there is another feature which we should note, and it is this: Segregation of characters alone would not be sufficient to explain the experimental results. It is possible to have segregation and yet have both characters carried in the same sex-cell; they can lie side by side with each other in the cell. But it is a fundamental part of our proposition that any one sex-cell can carry one only of the two alternative characters. To this conception of the structure of the sex-cell we apply the term "gametic purity." Necessarily, gametic purity implies segregation. But segregation does not necessarily imply gametic purity. We may now widen the description of our principle of heredity, and call it the theory of segregation and gametic purity.

We have arrived, then, at this position: The sex-cells are the carriers of the characters of the race, from one generation to another. Whenever two alternative characters, such as tallness and

dwarfness, colour and albinism, short-haired coat and long-haired coat, hairy epidermis and smooth epidermis, blue eyes and brown eyes, and a large number of other alternatives are present in a race, each of the sex-cells can carry one only of these two characters, the other character being carried by another cell. And the distribution of these two characters among the whole mass of the sex-cells of any individual is such that one-half of the total number of them will carry the one character, and the other half will carry the other character.

And the outcome of this structure and arrangement of the sex-cells is that when two hybrids are mated, their offspring will consist of a mixture of individuals, some showing the dominant character and others the recessive. Those which exhibit the dominant character are not alike, some of them being pure dominants and the others impure or hybrid dominants. The proportions in which these types appear are one pure dominant, two hybrid dominants, and one recessive.

I would like now to ask your consideration of a type of mating which is very important from the social standpoint, and from the point of view of the student of human heredity. It is the mating where one of the two parents is a hybrid and the other a recessive. In our symbolic representation we may state it this way: $D\,R \times R\,R$. The result of such a mating is to produce only two kinds of offspring, being identical in regard to the two particular characters under consideration with the

two parents. That is, the offspring will consist, in the long run, of an equal number of individuals who are hybrids, or *D R*'s, and of pure individuals, or *R R*'s, who are carrying only the recessive character. The hybrid individual will carry both the dominant and recessive character, but will exhibit in his structural features or psychical conduct only the dominant one. That is, he is something different from what he appears to be. His influence on the race, in respect of his hereditary transmission, may be quite the opposite to that expected, if the expectation be based on his present apparent condition. This is a fact which social reformers and all others who aspire to interfere with Nature's processes would do well to clearly realise. It is easy enough to heedlessly interfere with the workings of Nature, and to misunderstand her, but it is impossible to escape the consequences of so doing.

If this conception of segregation and gametic purity be true of human life, it is easy to see how important a bearing it has upon the problems with which Genetics* and Eugenics are concerned. If we, as students of these branches of biological science, desire to see only our individuals of good civic qualities mated, and if we find upon further study that a certain proportion of our apparently good citizens are carrying in a recessive condition the antithetic bad qualities, which may become manifested in the next generation, clearly our line of

* For a definition of Genetics and Eugenics, see Appendix, p p. 110 and 112.

action or of social education must be modified. There is a pitfall lying across our path, and we must be careful to see it, in order that we may avoid the consequences of falling into it.

Before I pass on to consider the other main purpose of this paper, *i.e.*, the question whether the evidence justifies us in believing that the processes of segregation and gametic purity are operating in human heredity, I would like to make the task a little easier by tabulating all the possible types of mating which can occur. We are assuming for the present that human beings are composed of a number of alternative or unit characters. These are disposed in pairs, and each unit character in a pair is capable of replacing the alternative one in the processes of heredity. One of these unit characters we call dominant, and the other recessive. This assumption being tentatively accepted, we may classify human matings, so far as particular pairs of characters are concerned, in the following way, and from such matings offspring of the nature indicated will be expected:—

Parent.	Parent.	Nature and proportion of expected Offspring.
$R\,R$	$\times\ R\,R$	= All $R\,R$.
$D\,D$	$\times\ R\,R$	= All $D\,R$ and all visibly D.
$D\,D$	$\times\ D\,R$	= 1 $D\,D$ + 1 $D\,R$ = all visibly D.
$D\,R$	$\times\ D\,R$	= 1 $D\,D$ + 2 $D\,R$ (visibly D) + 1 $R\,R$.
$D\,R$	$\times\ R\,R$	= 1 $D\,R$ (visibly D) + 1 $R\,R$.
$D\,D$	$\times\ D\,D$	= All $D\,D$.

Now we are in a position to consider what is, next to the question of man's origin and destiny, one of the most intensely interesting of human questions: Do the alternative characters which make up human beings, and which decide what we

shall be and do, separate from each other during the processes and structural changes by which the sex-cells are produced, so that, individually, we represent the product of a sum of paired characters handed down to us by our parents? And what we shall be, therefore, will depend not upon what our ancestors as an aggregate were, but upon the gametic nature and structure of our parents? And since our parents can only carry between them á single pair of alternative characters of the same class, while all our ancestors between them may have carried many such pairs, it is clear that we individually cannot represent in any particular character more than two of our ancestors, while in some cases we may represent only one and carry in a recessive condition the alternative unit character of one other? Or, are we the product of the blending of all the ancestral alternative qualities that have been brought into our line by all our ancestors? So far as our evidence is definite at all, the answer for the latter question is negative, and affirmative for the former. I am far from asserting that our evidence is complete, or that it is of such a nature that the blended hypothesis of inheritance is yet altogether excluded. But I am prepared to maintain that, making due allowances for the circumstances under which the human pedigrees have been obtained, and giving proper consideration to the probable complexity of many human characters supposed to be simple, the evidence is very clearly suggestive—and in some cases it amounts to a scientific demonstration—that gametic

purity and segregation are the two processes which determine human heredity, alike in the transmission of normal and abnormal characters.

Let us pass on, therefore, to consider some of the evidence for segregation and gametic purity in man. I propose first to deal with the transmission of a normal human character, *i.e.*, eye-colour. Until Mr. Hurst, during the years 1905-1907, examined a number of families, separately and in detail, recording each individual eye-colour, it cannot be said that our knowledge of this matter was anything like definite or satisfactory. Previous records largely depended upon the observations of different observers, and upon a more or less popular and indefinite classification of eye-colours. And, moreover, the mathematical methods that were employed to deal with the ascertained data were of such a nature that the truth was rather masked than elucidated by them. As a matter of subsequent knowledge, they actually did miss the truth. Mr. Hurst made a personal examination of the eye-colours of one hundred and thirty-nine pairs of parents, and of six hundred and eighty-three of their offspring. He found that he was enabled to classify all eye-colours into two classes, which he called the "Simplex" and the "Duplex." These two types of eyes are quite distinct. The simplex type includes all the pure blue and pure grey* eyes, *i.e.*, blue and

* Pure grey eyes are simply blue eyes in which the iris tissues are a little more opaque, and so cause the blue to appear grey. Eyes which are blue in childhood may become grey in later life, owing to an increase of this opacity.

grey eyes which contain no visible trace of yellow or brown pigment. The duplex type includes the black, brown, hazel, green, and impure grey and impure blue eyes, *i.e.*, grey and blue with a little visible yellow and brown pigments present. He found that the simplex type behaves as a recessive to the duplex type ; so that, if one parent had pure duplex eyes and the other had simplex eyes, all the offspring would show duplex eyes. If both parents were of the simplex type, then all the children had simplex eyes. Since the duplex eyes are a dominant type, individuals who possess them may be either carrying duplex only, or they may carry as well the simplex type as a recessive. When both parents have duplex eyes, there are therefore three possible combinations which have to be considered. We may state them symbolically thus :—

$$DD \times DD,\ DD \times DR,\ DR \times DR.$$

That is, we may have a pure duplex married to a pure duplex, or a pure duplex married to a hybrid duplex, or both parents may be of the hybrid duplex type. If we turn back to the tabulation on page 69, we can see the nature and proportion of the expected offspring in these three kinds of marriages, on the assumption that the processes of gametic purity and segregation are operating in Man.

Let us consider Mr. Hurst's results. When both parents were simplex all the offspring, one hundred and one in number, belonging to twenty pairs of parents, were simplex also. That is, the recessive character breeds true, in spite of the fact that some

of the grandparents had duplex eyes. This is quite in accordance with the theory of gametic purity and segregation, and it is altogether discordant with any other theory of inheritance, whether biometrical (mathematical) or otherwise.

Turning now to the results when both parents are duplex, we have just seen that three combinations are possible. Of these, the two combinations $DD \times DD$ and $DD \times DR$ will give the same visible result, *i.e.*, all the offspring will show the duplex type of eye. But the third combination, *i.e.*, $DR \times DR$, will give a mixed offspring, in the proportion of three individuals having duplex eyes ($=1\ DD + 2\ DR$) to one having the simplex eyes. Examining Mr. Hurst's results, we find that our expectations are fulfilled, for of the fifty pairs of parents which he recorded, both of whom had duplex eyes, thirty-seven of them had a total offspring, one hundred and ninety-five in number, all with duplex eyes. They, therefore, illustrate the two first combinations. The remaining thirteen parents had a mixed offspring, of a total number of sixty-three individuals. Forty-five of these showed the duplex eyes, and eighteen of them the simplex eyes. In nearest round numbers the expectation is forty-seven and sixteen respectively. That is, Mendelian prediction and result are practically identical.

The next possible combination will be between parents having duplex eyes and those with simplex eyes. But the duplex parent may be one of two things—either a DD or a DR. So that from

the standpoint of the type of offspring which we may expect, we have two real combinations within this one visible one, *i.e.*, $DD \times RR$ and $DR \times RR$. In the first case all the offspring are expected to have duplex eyes, and in the latter one-half is expected to show the duplex eyes and the other half simplex. What are the actual results ? There were seventeen pairs of parents, of the nature of Duplex × Simplex, with a total of sixty-six offspring all showing the duplex type of eye. And there were fifty-two other pairs of parents of the same visible nature who had a total offspring of two hundred and fifty-eight, one hundred and thirty-seven of whom had the simplex eye and one hundred and twenty-one the duplex eye. The expectation is one hundred and twenty-nine of both types. There is thus a very near approximation of result to prediction.

Mr. Hurst's work shows two things quite clearly. It demonstrates the absolute segregation of these two types of eyes in human inheritance, and compels us to accept the conception of gametic purity as the only valid explanation of it. It is quite evident that the alternative hypothesis of blended inheritance does not explain a single feature of the phenomena. For if it did, why do blue-eyed (simplex type) offspring come from two brown-eyed (duplex) parents ? Or, why do they come from parents one of whom has brown eyes and the other blue ones ? Why do some brown-eyed pairs of parents give nothing but dark-eyed offspring, and other pairs give a mixed offspring of dark-eyed and blue-eyed individuals ? The con-

ception of blending does not explain it, while that of gametic purity and segregation enables us to form a mental picture of the main line of the processes concerned. The same segregation of eye-colour occurs when distinct races of mankind are crossed. Among my collection of human pedigrees I have several of an European (usually a fair-complexioned, light-haired, blue-eyed Scotchman) married to a Red Indian woman, and of their descendants. When the half-breeds from such a marriage marry an European of a certain gametic structure, the fair complexion, light hair, and the blue eyes segregate out again among their children.

I pass next to consider another character which we cannot regard as pathological, but yet is not normal. We may speak of it as abnormal. I allude to the quality of albinism. It is a condition in which there is a complete absence of pigmentation in the body.* It is known not only among the less deeply pigmented races of mankind, but also among the intensely pigmented races, such as negroes. Mr. Farabee has examined an interesting case of the hereditary transmission of albinism among negroes. An albino negro married a normally-pigmented negress. There were three sons, all normally pigmented. All three married pigmented negresses, and two of them had only the normally-pigmented offspring. The other son married twice, and in each

* The pigments of the blood, bile, and of muscle are, of course, excepted. It may perhaps be better, for practical purposes, to define an albino as an individual in whom there is absence of visible pigmentation.

case married a pigmented negress. But by his first wife he had one albino and five pigmented children, and by his second wife three albino and six pigmented children. The complete disappearance of the albinism in the first generation, and its reappearance in the second, shows that albinism in man, as in lower forms, is a recessive character to pigmentation. The results of these marriages are perfectly unintelligible on the conception that blending of characters occurs. For how, by any process of blending, can four albino children be produced out of a marriage of a normally-pigmented negro and a negress? The blending of black with black does not produce white! And how can we explain the mixed nature of the offspring—some individuals being albinoes and others pigmented—on the blending conception. A blend is expected to produce an uniform result. But the whole of the facts are explicable on the hypothesis of segregation and gametic purity. The albino grandfather married a normal negress. All three of their sons will be, therefore, hybrids of the nature $D\,R$, where D stands for the dominant character of pigmentation, and R the recessive character of albinism. So long as these sons marry only normal negresses (*i.e.*, $D\,D$'s), a pigmented offspring only is expected. That appears to have been the case with two of the sons. But if the third $D\,R$ son shall marry a negress who is also $D\,R$, then we expect in the offspring albinoes and pigmented individuals in the proportions of 1 : 3. This appears to be the kind of marriage that the third son contracted with both wives.

And we note that the number of albinoes and pigmented individuals, *i.e.*, 4 and 11 respectively, is as near as it can be to the expected 4 : 12 ratio. This case, then, must be accepted as demonstrating the existence of the processes of segregation and gametic purity in the hereditary transmission of at least one other human character.

Let us consider another case of albinism, but this time among Europeans. In a Swiss village there once lived an albino, Josephine Chassot. She never married, and for three generations back her ancestors were apparently normal people. But in her own generation she had ten cousins in the second degree.* Of these, three were albinoes, and the remaining seven were all apparently normal. All the normal ones appeared in one family, and all the three albinoes in another. In this latter family it is known that there were normal members, but the number is unknown. One of the normal cousins of Josephine married a normal woman, and had twelve children, eight being normal and four albinoes. Now this case, apparently remarkable because of the sporadic appearance of the albinoes, is entirely explicable on the Mendelian explanation of segregation and gametic purity. The families concerned lived in two Swiss villages half a league from each other, and we may suppose that at the time (*i.e.*, sixty years ago) travelling was difficult, and intercourse with more distant villages was more or less a rare event. Under such

* That is to say, they were the children of two pairs of parents, one of whom in each pair was a cousin of one of her parents.

conditions, consanguineous marriages are apt to be common. This we see in a marked degree in the Scotch Highlands, where the inhabitants of many villages are nearly all cousins, in nearer or remote relationship. We know that albinism is a recessive character in animals, and we have just seen in the negro marriage that it is so also in man. Individuals, apparently quite normal externally, may, therefore, carry albinism as a recessive character. So long as such individuals marry a quite normal person (one not carrying albinism recessive), albinism will never appear among the offspring. But if they marry another person similar to themselves—that is, one carrying albinism recessive—then albino children, as well as normal ones, will be born to them. So that it may happen that in a long line of ancestry, apparently normal all along, albinoes may quite suddenly arise. That appears to be the explanation of the case of Josephine Chassot. And the same explanation suffices for the familiar fact that albinoes are very often the offspring of a cousin marriage. In these villages, where we may assume consanguineous or endogamous marriages* were common, there must have existed a certain proportion of the villagers who were carrying albinism as a recessive character. When two such married—and in random selections this must now and then occur—albinoes are expected. This, then, being the Mendelian expectation with

* In this particular pedigree it is known that two normal brothers married two normal sisters, all of the same village, and albinoes appeared in the descendants of both.

regard to a qualitative result, how far is the expectation similarly corroborated by a quantitative result? The Mendelian expectation with regard to the proportion of albinoes and normal individuals is as 1 : 3. The total number of offspring concerned for purposes of calculation is twenty-four plus a few normals, the number of which is not recorded. The expectation, therefore, in round numbers is six albinoes and eighteen normals. The actual number is eight albinoes and sixteen normals. There is thus what the prediction requires, a large excess of normals over albinoes. And when we bear in mind that a few more normals, who are known to have existed, must be added to the actual result, we see how well result and prediction meet each other.

Dr. A. M. Gossage has quite recently made a study of some previously recorded cases of hereditary transmission of pathological characters in man, and he has produced a number of interesting facts. On the whole the cases which he has studied and republished confirm the Mendelian generalisation, though there are some exceptions which are discordant with it. These latter may, however, receive an adequate explanation when we know more of the particular diseases with which they are connected, or when we have been able to investigate a larger number of similar cases. But it seems quite clear from his survey of cases that segregation of alternative characters, such as some pathological trait and normality, does occur. The crucial test that such segregation has actually occurred is to be found in

the persistency with which a recessive character breeds true when once it has been segregated out from association with a dominant character. It is true that, with the exception of one remarkable case, which we shall consider later, we have no very long continuous pedigree of an extracted recessive character. But we have a number of short pedigrees, and when all their indications point in one direction, and almost invariably show that an extracted recessive character breeds true when the individual manifesting it is mated with another also showing the recessive character, we are justified in accepting this as clear evidence of segregation. And the justification is not lessened because a few exceptions occur. These may receive an explanation when we know what it is that determines which of two alternative characters shall be the dominant one, when we have tested the validity of past records by a more searching examination of future cases, and when pathologists and physiological chemists can tell us more of the nature of the factors which go to the making up of these pathological characters. Many abnormal characters are doubtless more or less complex in their nature. To treat such abnormalities as simple units is but to disguise the truth. When we know the nature of their complexities, the general trend of the whole evidence leads us to believe that they will fall into line with the general body of facts of Mendelian inheritance.

Some of the characters studied by Dr. Gossage are of suggestive interest. In a certain family

some of the individuals possessed hair presumably of the European type, but others had a "tightly-curled, short, woolly hair of the negro type." The facial features of these latter individuals were not in the least negroid. A tradition in the family attributes the woolly hair to a Mexican ancestor several generations back. We may assume it as probable that this ancestor was of the negroid race. If this is so we have a clean segregation of European facial characters from negroid ones, for none of the present family, thirty-six in number, manifest the least negroid trait. But in the random segregation of the various negroid and European characters that occurred in the sex-cells of this Mexican ancestry, negroid hair became associated with European facial characters. And apparently, the European alternative characters being recessive, once the European facial characteristics had been extracted they would henceforth breed true. Hence in none of the offspring that have descended from a Mexican ancestor who had European features but negroid hair, do we expect any negroid facial characters to reappear. But the negroid hair being a dominant character, this Mexican individual who bore it may also bear in his sex-cells the European type of hair as a recessive character. We should then expect to see both the negroid and European hair separate out in the succeeding generations, and be borne by separate individuals. And the curliness and woolliness must, on the whole, be as curly and woolly in one individual as another. And such appears to be the case in

this family. There have been three marriages of individuals who had woolly hair ($=D\,R$) with persons having European hair ($=R\,R$). The total number of individuals concerned is thirty-six. Of these, eighteen had negroid hair and eighteen had ordinary hair. The segregation is complete, and the proportions are in exact accordance with Mendelian expectation.

Another case in which a peculiarity of the hair is the character considered is one where a congenital tuft of white hair over the brow was hereditarily transmitted through six generations. Only a single member, the father, in each of the first three generations is known. And each of these possessed the white tuft. The father of the third generation married a normal person, and they had four children, two with and two without the tuft. The white tuft is a dominant character and the absence of it a recessive one. One of the normal offspring in time married a normal husband, and their children, three in number, were all normals. The two children with the tuft also married, and they had a mixed offspring, some of the individuals having the tuft and others not. Three individuals of this generation without the tuft married normal persons, and in their total offspring of eight members there was not a single one with the tuft. Thus in the total of eleven children born of four pairs of parents, one in each pair being an extracted recessive and the other a normal person, the dominant character did not appear. That is, the Mendelian expectation that the

recessive character would breed true has been fulfilled in fact. With regard to the proportions in which the two kinds of offspring derived from parents, one of which manifests the tuft ($=D\,R$) and the other does not ($=R\,R$), the actual proportion is nearly identical with that required by expectation. There are thirty-two individuals in the offspring, and the expectation is sixteen of each type. There are, actually, seventeen with the tuft of hair and fifteen without.

I have taken these two cases from Dr. Gossage's paper because they are of a nature which the layman can understand. They do not deal with pathological characters, but rather with what we may term peculiarities. But all the rest of the cases with which he deals are of a pathological nature. The phenomena of segregation and gametic purity are clearly manifested, but, as already stated, there are a few doubtful exceptions. Those who desire to study these cases will find them described in the *Quarterly Journal of Medicine*, Vol. 1, No. 3.

I would like now to ask you to consider two extremely important and interesting cases. The first one has reference to an abnormal condition of the fingers and toes. The peculiarities connected with them are correlated with others, but these we need not consider. The case has been worked out, with very great care and labour, by Dr. Drinkwater. The peculiarity is technically called "Brachydactyly"; more popularly we may speak of it as "short-fingerness" or "thumb-fingerness." The

fingers of the hand are apparently, like the thumb, made up of two joints, instead of three as in the normal hand. It is the middle one of the three joints which has to all appearance disappeared. There is some reason to believe that it has not actually disappeared, but that it is vestigal in size and has become fused on to the last joint. However, for practical purposes we may speak of the peculiarity as a two-jointed condition of the fingers.

The history of the family in which this peculiar condition is present is known for seven generations. The first three generations are incomplete, but the remaining four are complete and comprise a fairly numerous collection of individuals. The whole seven generations include one hundred and seventy-four persons. We may divide the marriages into two classes, *i.e.*, those in which an abnormal person married a normal one, and those in which an extracted normal individual derived from this family married a normal one from outside of it. The first type of marriage will be represented as $D\,R \times R\,R$, and the latter type by $R\,R \times R\,R$. The brachydactylous condition is shown by the observations on this family to be a dominant one. Now, what is the Mendelian expectation as to the offspring from these two types of marriage ? In the first type two classes of individuals, *i.e.*, short-fingered and normal-fingered, are expected in the offspring. And it is further expected that these two classes will be present in approximately equal numbers. Once more an appeal to the facts of human pedigrees confirms Mendelian

prediction. Both classes of individuals are present, and in very nearly equal numbers. There are eighty individuals from this type of marriage, and therefore it is expected that there will be forty each of the abnormal and normal individuals. There are respectively forty-two and thirty-eight. With regard to the second type of marriage, *i.e.*, $R\,R \times R\,R$, the expectation is that all the offspring will be normal. There are eighty individuals,* all normal.

The family which we have just considered is an English one. But a case of the same nature, dealing with an American family, was earlier published by Mr. Farabee. In this case the same principles are manifested. The extracted recessive (normal) character breeds true, and the offspring from a marriage of a parent with "short fingers" with one of normal fingers is composed of the two types of individuals, and in approximately equal numbers, *i.e.*, thirty-six of abnormal individuals to thirty-three of normal ones, the expectation being thirty-four and a half of each. These two cases of human brachydactyly, then, are clearly Mendelian in nature. Segregation and gametic purity explain the phenomena presented by them, while the blending hypothesis and the biometrical "Law of Ancestral Inheritance" completely fail.

I pass now to the second and perhaps one of the most important cases of a human pedigree. We owe its compilation in part to a French Army surgeon, the late M. Florent Cunier, and in part to an English

* In addition to these there are a few individuals known to have existed, but the sex and type is now not known.

ophthalmic surgeon, Mr. Nettleship. It is one of the largest known human pedigrees of its kind, comprising somewhere near a total of two thousand individuals, and it extends back through ten generations to the year 1637, when the individual Jean Nougaret, with whom it commences, was born. The peculiar hereditary character which affected one hundred and thirty-five persons of this family, and which manifested itself in each generation, is one which is known as congenital stationary night-blindness. The real nature of the disease is quite well confirmed, for Mr. Nettleship, with Professor Truc and M. Capion, have been able to examine fifteen night-blind subjects, belonging to the ninth, eighth, seventh, and sixth generations. The affected persons can see as well during the daytime as normal people. But at night time they can see only by candle light and by very bright moonlight. On moonless nights, or in very dark places such as cellars, they are blind. The affection is present from birth, and mothers are able to determine whether their babies are night-blind or normal by the apathy or interest manifested by them when objects likely to attract their attention are held up at night time. In this case of congenital night-blindness it is shown by the facts of the pedigree that the abnormal condition is the dominant one, and the normal one is recessive. The Mendelian expectation therefore is that when two extracted normal persons marry, or when an extracted normal person marries a normal individual, that all the offspring shall be normal. Is the Mendelian expectation

fulfilled in this case, too ? The answer is Yes. Whenever normal parents have married—and in this family inter-marriages between extracted normals have occurred—the offspring is always normal. There is altogether an offspring of one thousand six hundred and sixty-three normal individuals derived from the marriages of normal parents, and not a single abnormal person has occurred. As far as human investigation can be concerned, nothing can be clearer than the meaning of such a record. The segregation of the normal character, once it has occurred, is complete, notwithstanding that inter-marriages between extracted recessive individuals have occurred. And there are cases within this family where this pure breeding has extended continuously for seven generations. There is nothing to suggest that the recessive character will not continue so to breed for indefinite generations.

When we examine the records of the offspring derived from the marriage of abnormal with normal persons (that is, marriages of the type $D\,R \times R\,R$), we find the Mendelian expectation fulfilled in that the expected two types, *i.e.*, the night-blind person and the normal person, are present. But, however, the expected equality of the two is widely departed from. There are three hundred and sixty-four persons in the disease-bearing branches of the family, and it is expected that half of these will show the disease, and that the other half will be healthy. The expected proportion is, therefore, in round numbers 182 : 182. The actual result is 135 : 229,

or thirty-seven per cent. of night-blind persons instead of fifty per cent. Too much stress must not be laid upon this numerical difference. It must be remembered that segregation and gametic purity can exist notwithstanding that the proportions do not accurately accord with expectation. Factors of which we have no knowledge may be at work, disturbing these proportions in some families or generations, and leaving them undisturbed in others. No one with any real experience of living organisms, including human nature, doubts it is a fact which should be constantly held in view that living gametes within a living zygote are not marbles in a bag. The latter can be shaken up to ensure an uniform distribution of the black and white ones, but gametes and human nature cannot be so treated; we must accept them as Nature listeth they shall be. And this particular case is instructive, for there are good reasons to believe that the number of abnormals recorded does not represent the true number. The chief of these reasons arises from the promptings of a natural human weakness, *i.e.*, a reluctance to admit the existence of physical defects, and additionally there is upon the part of the women a very laudable desire not to jeopardise their chances of becoming wives and mothers. For the learned Curé who is familiar with the living descendants of this stock, and who has contributed to the construction of the pedigree, says: "It is considered as prejudicial to the establishment of children" to be afflicted with this disease, "and it is therefore apt to be concealed."

There is, indeed, one somewhat remarkable case where a woman kept her husband in ignorance of her condition for twenty years. Human nature being what it is, we must expect some deficiency in the numbers of the afflicted in this and other pedigrees.

The next case that I have to deal with is one for which I am indebted to the kindness of Dr. Drinkwater for his permission to use. It is a case of congenital asthma. There are altogether twenty-three individuals in the pedigree. The case is typically Mendelian. One individual extracted from a line in which the disease is present is free from it, and he married a normal person and had all normal children, three in number. The other extracted normal persons have not married. But four of those suffering from the disease have married normal people. The asthmatic condition is shown by the history of this pedigree to be dominant. We shall, therefore, expect in the offspring of these marriages an equal number of asthmatic and normal persons. There are, in fact, ten of each. Thus in the complete segregation of the normal and asthmatic characters, in the breeding true of the extracted recessive (normal) character, and in the proportion of the two kinds of offspring from pairs of parents one of which is asthmatic and the other normal, the case is a definitely Mendelian one. It is of some interest to note, in passing, that of the members who have married from this family four of them (two brothers and two sisters) are affected, and one only (a brother) is normal. This is a fact of some importance to students

of Eugenics, especially when it is additionally remembered that, in regard to another disease, phthisical persons tend to marry early. It is significant because physical defects seem to be no hindrance to marriage, but apparently, in this case, rather a recommendation.

The last case that I have for your consideration is one, it seems to me, of great importance, because it is very significant in many features. It not only affords us an example of hereditary transmission, but it gives us a concrete case where social sentiment and philanthropy having interfered, we can arrive at a definite judgement as to the pernicious effects of this interference. It is not an academic case that I have laboriously hunted for in the Archives of the Clinical Department of a great hospital, or in the volumes of a library. It is a case of the present time, that was reported in the *Morning Post* of May 25th, 1907; it is one that illustrates the prolific growth of unfitness when fed and watered by the mistaken kindness of the fit. It appeared in an appendix to the Ninth Annual Report of the Children's Committee of the Metropolitan Asylums Board. It is written by the Medical Officer to that Board. I ask you to note that fact, because medical men are largely responsible for the birth and promulgation of the modern sentiment which we are now considering. The general tone of the Report, especially the last paragraph, implies the possibility of converting inherent unfitness into fitness by a suitable environment. The parents in this particular case

demanded the return of one of their children from the Wandsworth Home of the Board, and the Medical Officer has appended the recommendation that "This is one of those cases which suggest most forcibly the advisability of having the control of children of this mental condition for a fairly long period." And he adds: "It will be interesting to follow his case in the future, and see whether his mental condition improves or deteriorates." Why does the Medical Officer recommend that this child shall be under his control for a "fairly long period"? Is it expected that any control, however prolonged, is going to transform the processes of heredity, or turn aside by a hair's-breadth the immutable processes of Nature? Let us examine the case and see how futile is the hope.

The father of this family had phthisis, and was insane; the mother was laid up for four months before the boy's birth with spasmodic paralysis, and afterwards lost the use of her legs for some months. The paternal grandfather died of phthisis, and the paternal grandmother, who is still alive, is in an asylum with mania and religious fancies. The maternal grandmother died of consumption (phthisis). Nothing appears to be known of the maternal grandfather, but, since the mother suffered from paralysis, it may be that it was transmitted from her father. Now let us examine the children of these parents, first prefacing our statement with the remark that meningitis in this case is a form of phthisis not attacking the lungs, but the membranes of the brain. No. 1,

a son, is said to be strong. No. 2, a daughter, suffers from meningitis, epilepsy, and tubercular (phthisical) knee joints. No. 3, a son, is afflicted with severe headaches. No. 4, a son, suffered a severe nervous illness, the nature of which is unknown. No. 5, a son, has had meningitis and paralysis, and is mentally dull. No. 6, a son, has had St. Vitus' dance. No. 7, a daughter, has suffered from meningitis, and is very irritable and subject to headaches. No. 8 (whether son or daughter is not stated) has suffered from paralysis and other nervous diseases. Nothing appears to be recorded of No. 9. No. 10, a son, is dead, and had paralysis in the legs.

No. 6, the boy who is afflicted with St. Vitus' dance, is the one whom it is recommended should be cared for out of public monies, by public officials, in public institutions.

Now we are chiefly interested for the moment in the Mendelian aspect of the heritage of this family. If we take a general view of the case we shall notice that we can reduce the pathological factors in the family history to two, if we express them in a general way. There is the tubercular taint manifested by phthisis, tubercular knee-joint, and meningitis. There is the nervous lesion indicated by mania, religious fancies, St. Vitus' dance, severe headaches, paralysis, and epilepsy. We may therefore say that we are dealing from the Mendelian standpoint with two pairs of alternative characters, *i.e.*, the tubercular condition and physical normality as one pair, and the nervous lesion and nervous normality as the other.

A possible Mendelian prediction for such a case is that four kinds of individuals will be present in the offspring. We may state them in the following manner:

1. Suffering from tubercular taint and nervous lesion.
2. Suffering from tubercular taint, but no nervous lesion.
3. Suffering from nervous lesion, but no tubercular taint.
4. Normal in both features.

And the proportions in which these four types may be expected are, in the order in which they are given: 9 + 3 + 3 + 1. That is, in a family of sixteen individuals, only one normal person is expected! And his normality will not be better than the normality of his race. In this family of ten only one is normal; the others all indicate some nervous lesion, or manifest both a tubercular condition and a nervous lesion. And thus far this case shows a resemblance to a Mendelian mode of inheritance. There is segregation manifested in the case of the normal son, in the production of individuals showing only nervous symptoms without tubercular ones, and in the appearance of both tubercular and nervous pathological features together in other individuals. But in a complex case of this kind many factors must be considered, and a larger number of families examined, before any degree of positive statement is justifiable. It is sufficient for the present that there is some indication of a Mendelian mode of inheritance, and that in other

human characters this mode has been clearly shown to exist.

Before we leave this case let us consider its social aspect. The fact of the hereditary transmission of the defects of this family is perfectly clear. That, at any rate, admits of no dispute. There is not an environment on earth that can make this stock other than what it is. Every effort that is made to save it, or to prolong it, so that the period of procreation of its individuals may be reached and lengthened out, is an effort which results in an increase of persons to whom life is a miserable and unclean burden. Every penny which is wrested from the earnings and productions of fitter citizens in order to rear an army of officials and palatial edifices for the maintenance of these unfit is by that amount reducing the number of persons to whom life means more or less of happiness and contentment. It cannot be otherwise. The penny cannot be spent upon both the unfit and the fit, any more than we can "eat our pudding and have it too." And it is interesting to note that what the child thinks of the pudding the philanthropist believes of the penny. Both suffer from a delusion, but the child is soon undeceived. His is a concrete matter, demonstrated in a few minutes. The philanthropist's problem is more superficially complex, and the avenging hand does not strike until as a warning it comes too late, because with its warning it also brings destruction.

Let us remember that in this family the paternal grandmother is still alive, and that the parents have

ten children. These two facts are important, because they demonstrate that physical and mental unfitness and civic uselessness are not necessarily accompanied by infertility, nor low fertility, nor shortlivedness. Indeed, there is here high fertility and longevity. When we bear in mind that there is good reason to believe that fertility is a character which is hereditarily transmitted, we have to recall the possibility that the grandparents on both sides may have ten children, that is, twenty altogether. And if these twenty marry, as the unfit always do, and they marry their "likes," as they also nearly always do, and if each of these twenty pairs of parents have ten children, we are faced with the appalling possibility that from this family alone there are living in our present generation two hundred individuals carrying or manifesting the tubercular, paralytic, and epileptic diatheses! And if we go forward to the next generation there may be two thousand of them, and in the third generation twenty thousand of them! An army of epileptics, paralytics, and tuberculates!

It may be asserted that I am indulging in wild and sensational alarms, and that this sort of thing has never occurred and can never happen. It is so easy for any nation to play the part of the traditional ostrich, for the sands of self-delusion are loose and soft. And it is hard to be brave when problems that can be put off for some one else to solve call upon us to solve them now, before they become so bad that their remedy must be heroic. They who assert that these things can never happen, and that a great army

of useless citizens cannot be reared in a short time, have either never troubled to ascertain the facts or have ignored them. There is the well-known German case, for instance. In this example seven hundred and nine descendants of a particular woman could be traced. She was a drunkard, a tramp, and a thief. Of her descendants, seventy-six were convicts, seven were murderers, one hundred and eighty-one of her female descendants were prostitutes, one hundred and forty-two were tramps and beggars, while sixty-four lived on charity and one hundred and six were illegitimate. In the course of three generations this woman and her descendants cost the State, in trials, prisons, and workhouses, a quarter of a million of money! There is another case of a like nature, from Canada. On the Upper Hudson some generations ago there existed a child of the "gutter type." She ultimately became a "prolific mother of a prolific race." Besides a large army of idiots, imbeciles, drunkards, paupers, and prostitutes, the country records show two hundred of her descendants who have been criminals.

And this is all that our philanthropy and modern social sentiment achieves! It rears great armies of unfit individuals, and it discourages through taxation and in other ways the increase of the fit. While the useless are pampered the useful are harassed. While it is becoming increasingly easy and profitable to be unfit and yet live, it is becoming proportionately difficult to be fit and yet enjoy the benefits of our fitness. Freedom and licence are being granted to

the lower classes, and liberty is being restricted for the individuals of the classes above.

The whole influence of this modern sentiment is trending in the wrong direction. It sets out with the belief that it can effect the salvation of the unfit by improving their environment. It will end by achieving the destruction of the fit, without having even gained the salvation of the unfit. It commenced its operations by creating an army of grand-maternal officials to guide the footsteps of the weak, the stupid, the inane, the imbeciles, and the physical and moral wrecks. It will end—indeed, it has begun to end—by the creation of an army of lower-grade officials who are rapidly degenerating into petty tyrants, who seek to restrict the successes of better citizens that have achieved what they could not, and have excited alike their cupidity and their vanity. And this, it appears to me, is the social aspect which the case of the Metropolitan Asylums Board presents to us. Throughout history every effort to save the unworthy has reacted to the detriment of the worthy. It is time we looked more seriously both at the teachings of History and Biology.

A popular impression exists that by marriage it it possible to minimise in the next generation the evil qualities of one of the two partners by the good qualities of the other. It is a belief which is based impliedly upon the conception that underlies what is termed blended inheritance. Its faith is rooted in the assumption that the offspring represent a blend of their two parents, and of their more distant ancestry,

too, through the channel of their parents. It is implied that the evil propensities of the one will be diluted, in the processes of heredity, by the goodness of the other. It seems, however, to be forgotten that, even if it be true, the goodness is diluted too. But if the processes of segregation and gametic purity are operating in Man, there is an end of this belief. For no marriage of fit with unfit, of virtue with vice, of truth with falsehood, of courage with cowardice, and of physical vigour with physical weakness will result in any diminution of the alternative bad qualities. Marriage will merely procreate and multiply the individuals who manifest them. When Friedrich Nietzsche, in his "Thus Spake Zarathustra," wrote the three following paragraphs in the chapter "Of Child and Marriage," he was nearer than he imagined to the truth, and he anticipated, in a few words, the Eugenic doctrines of the present :—

"Worthy and ripe for the significance of earth appeared this man unto me, but when I saw his wife earth seemed unto me a madhouse."

"Yea, I wish the earth would tremble in convulsions whenever a saint and a goose couple."

"This one went out for truths like a hero, and at last secured a little dressed-up lie. He calleth it his marriage."

The follies and impulses of youth are proverbial; even the youth who ultimately may blossom into genius or to a high degree of civic worth, and in his maturity may aspire to high ideals, is not free from the errors

incidental to his age and temperament. Students of the Divorce Courts are familiar with examples that serve to illustrate the disastrous consequences that as often as not follow in the train of early folly. When a youthful member of a noble family, forgetting, in his ardent impulses, the traditions and glories of his stock, marries a ballet girl; or when a young man of impulsive moods but of good parts otherwise, and descended from a family of good social position, and who will later develope all the aspirations that belong to his social class, accepts a partner without any solid qualifications and who has temporarily ascended from a lower social level, and will manifest the instincts and inherent defects of her class; or when a young man, who because of the stock from which he springs, will later on in life become a more serious citizen, marries a "dressed-up doll"; or when a woman, yet in the vernal stage of life and of refined instincts, consents under parental pressure to marry a rich man of coarse habits whose years are strewn with the sere and yellow leaves, no Heaven-made conception of the nature of human marriages can avert the inevitable consequences. It is a serious question for the student of Eugenics whether a youthful error of judgement in marriage, by which "a dressed-up lie" and "a goose" is chosen as a mate, should be visited with permanent and lifelong punishment. The time has passed when the dogma of "heaven-made marriages" should be allowed to operate to the detriment of much that is best or good in the community. No marriage that declares by its results its obvious inequalities and

disastrous disabilities should be permanently maintained. If one of the two partners is capable and the other grossly incapable, if one is intelligent and thrifty and guided by high ideals, and the other is foolish, thriftless, and incapable of rising above the lowest aspirations, then the marriage, as in a part of the days of Rome, should be possible of mutual dissolution. For to permanently bind a good citizen to a bad one is waste of the good civic material.

I am aware that the problem is a complex one, and it is not simplified by the possibility that the incapacity to make a good judgement in the choice of a mate may be an inherent quality, and not always a passing aberration of youth, and will be therefore hereditarily transmitted. But in spite of that, perhaps it would be wiser to give an individual at least one opportunity of rectifying a youthful misjudgement.

I am treading, I know, upon dangerous ground, and I hope that I shall not be misunderstood. I trust that nothing which I have said can be construed into implying any sympathy with those ideas of promiscuous marriage that in certain quarters are being preached by men whose fanaticism or desire for notoriety is greater than their knowledge of either the history or nature of mankind. I, for one, repudiate all such ideas. But the sanctity and holiness of marriage may be destroyed when earthly considerations and ideals are ignored by a dogma that has no justification in true belief or in truth.*

* See Appendix, pp. 113 and 114.

If now we take a general survey of all the cases which we have considered, we shall agree, I think, that the evidence is pretty clear in regard to some of the qualities of man that there is segregation in their hereditary transmission from generation to generation. There is reason to believe that future investigation will widen the number of human characters which so behave. There is no reason to believe that the mental and moral qualities of man are transmitted in any way different from the physical characters. Indeed, the evidence indicates that they are hereditarily handed down in the same way that the physical ones are. Without citing more recently-investigated cases, it will suffice to point out that it is a historically-known fact that the eloquence of Marcus Antonious was a hereditary gift, and that the high ability of Brutus' family extended through many generations, and was manifested in the discharge of the duties of the many great posts which that family occupied during successive generations.

If, then, it shall be ultimately shown that segregation of alternative characters and gametic purity is the rule of man's hereditary processes—if the clear indications of the limited cases already known become a generalised expression of a general process—and justify us in saying that segregation and gametic purity express a natural law of man's heredity, what bearing will this have upon all social questions and upon questions of even wider moment ?

Looking over the cases which we have already

considered, and substituting for such physical characters as short fingers, curly hair, and stationary night-blindness certain bad civic qualities such as those which characterise the loafer, the wastrel, the congenital drunkard, the habitual criminal, the congenitally tuberculous, the mentally deficient, the thriftless and generally incapable persons, and others, how will the problem work out? When we recall with what persistency the peculiarity of stationary night-blindness and short-fingerness passed on through the generations—passed on for two hundred and seventy years in the case of the former character, and in the latter case for about one hundred and eighty-nine years—without any known alteration, it is time that we began to consider what types of citizens our philanthropy and charity are breeding. It is time we faced the problem. It is time we learned how little environment can do, and how much the inborn qualities determine all for us. In the multitudinous efforts which are being made for social reformation it is to be hoped that the beauties of character will not be forgotten, and that the road to success will not be made too easy, nor the road to destruction too difficult. If by our social efforts we are breeding, rearing, and accumulating an undesirable stock that cannot or will not work, that cannot or will not tend its children, that cannot or will not be sober and thrifty, that cannot or will not educate and feed itself, that cannot or will not attain an intellectual and moral level that is necessary for social life, then we are advancing along the road that

brings destruction to the fit in order that the salvation of the unfit may be attempted.

There can, I think, be no doubt that those circumstances which are attendant upon the workings of a democratic state of society tend to create and to fan into larger size a flame of emotional sentiment that becomes wildly excited over the imagined or exaggerated sufferings of the weak, incapable, and generally unfit. We have only to listen to the hustings speeches of the politicians, especially to those of the type delivered by Mr. Winston Churchill and Mr. Lloyd-George, to see how pernicious are the workings of democracy when the leaders are unworthy of their position.

The same type of morbid sentiment and craven fear that gave the mob of Rome its free circuses and free bread, that gave to the Athenian multitude expensive theatrical shows free of cost to themselves, and placed needy adventurers and profligate demagogues of the type of Cháres in positions of executive and legislative power, is to-day in our own community increasing the privileges and rights, and decreasing the civic responsibilities of the lower classes of society, and simultaneously is placing power in the hands of men who either represent these classes or are sprung from them. Greece and Rome possibly fell in ignorance. There may have been none among their counsellors who saw the importance of the facts of heredity, or, even more probably, did not recognise that such facts were part of the operations of Nature. If England has reached her zenith it cannot be said

that her decline is due to ignorance. Rather, it must be said that it is coincident with a period of great activity and advance in biological knowledge. To-day the truth of Evolution, the Processes of the Survival of the Fittest, and the fundamental facts of Heredity are clearly grasped and understood by biologists. At no period in human history has knowledge been so extensive, so accurate and so carefully generalised as now. No one acquainted with human life and with lower animal life doubts the identity of the natural processes that act upon both. Man's ethical and æsthetic qualities are not new creations—only elaborations of qualities present in the living kingdom long before he came. And beneath his ethical and æsthetic nature lies the animal. In the long run the animal instincts, hunger and procreation, determine man's conduct. We need only recall the French Revolutions to remind us of the fact. Let us not forget the guillotining until the headsmen sank worn out; followed by the fusillading of little children—" the wolflings " of Marat, " who might grow to be wolves "—and of women with children at the breast; of wholesale drownings, of women stripped naked before they were drowned, and of mothers dragged to the guillotine to witness the slaughter of their innocent children. Truly did Carlyle say, " Cruel is the panther of the woods, the she-bear bereaved of her whelps; but there is in man a hatred crueller than that." The angelic gloss on man is but a surface-coat painted in times of peace and prosperity, but cast off in times of distress and

hunger. We must not forget the recorded facts of ship-wrecked and starving men casting lots for their next meal, and feeding upon him whom chance had marked. Neither must we forget the County Courts, nor the now historical episodes of the Marshalsea. "Give me the pound of flesh which is due to me, or die for aught I shall care," is the general formula that designates the doings of mankind, even in normal times. I do not regret that it is so. I do not, in even small measure, denounce Shylock. I have come to recognise that the weak demand the "pound of flesh" from the strong whenever they can, no less than the strong demand it of the weak. Be careful, therefore, philanthropists and social sentimentalists, that in your frantic haste to procreate and preserve the civically unfit you are not bringing into being a great herd whose demand for its "pound of flesh" shall be none the less emphatic because it cannot earn the value of that pound, or, having earned it, finds that others, stronger and fitter, have already consumed it. It is a dangerous problem, this of the multiplication of organisms, with that philogamic passion at their rear ever unresisted, driving onwards. It is dangerous even if the multiplication is that of the fittest. But if for a while you shall aid the maintenance and increase of the unfit and unfittest it means ultimately an annihilation on a scale passing beyond all comprehension and transcending all conception. It is therefore wise to heed how far the hour may be distant when by the voice of the great shipwrecked herd the casting of the lot may be

demanded, and in spite of your philanthropy and mistaken kindness you shall serve as food, to stay for a moment the hunger of that wretched and helpless mob that your misguided sentiments called into a larger existence. For, as Vergniaud said of the execution of the Girondins: "The Revolution, like Saturn, is devouring its own children," so it may be said of your sentiment that it will devour you and yours.

If, in defiance of the teachings of biological science, modern sentiment persists in breeding and rearing a helpless race that cannot by its own efforts satisfy its hunger, or if it will persist in rearing a race that manifests criminal and brutal instincts; if, in order that the lower democracy may be appeased, the unfit, the sluggish, the lazy-rapacious, and the cunning demagogues are in turn extolled, bribed, and maintained in life at the expense of the fit and worthy, then England, in spite of her greater knowledge, has commenced to follow the path that led to the destruction of Rome and Greece.

If the ruling classes of this country, the Aristocracy, have forgotten their duties, which is to face and enforce the truth of their generation, or have lost their courage, then it must fall to the more vigorous and less æsthetic portion of the Intellectual Classes to take their place. The danger is clear and eminent enough. With the Lower Classes increasing their birth-rate, while the Middle and Upper Classes are remaining stationary; with certain well-intentioned but mistaken members of the higher social classes

making remarkable efforts at much personal sacrifice to lower the infantile mortality of the lowest strata of society; and with workhouses, infirmaries, and lying-in hospitals using baby incubators to fan back into existence the weakly lives that benign Nature, in the noblest interests of her race, demands shall perish, there is no need of a vivid or expansive imagination to picture the end of a nation that has become thus far decadent. It seems that we, as a nation which have so long professed a belief in a merciful and loving God, have forgotten the old cry of resignation, "Thy will be done," that brought comfort, courage, and virility to Englishmen of the past when the processes of Nature eliminated their weak and sickly. They understood not these processes, and in ignorance resigned themselves in a noble faith that all was working towards a beneficent end; and, in their resignation, found their salvation and that of their race. The generation of to-day believes it understands these processes, and in the arrogance of its belief has played the coward and traitor to its heritage. In its sentimental interference with the workings of Nature it is leading the community to its destruction. In its attempts to be superhuman it has become inhumane. In its efforts to save life it has increased the number of individuals to whom existence means misery and pain, and is decreasing those to whom life means health, happiness, and progress. It recalls to my mind some of Wordsworth's lines in "The Excursion":—

> "Vain-glorious Generation! what new powers
> On you have been conferred? What gifts, withheld
> From your progenitors, have ye received,

Fit recompense of a new desert? What claim
Are ye prepared to urge, that my decrees
For you should undergo a sudden change;
And the weak functions of one busy day,
Reclaiming and extirpating, perform
What all the slowly-moving years of time,
With their united force, have left undone?
By Nature's gradual processes be taught;
By story be confounded! Ye aspire
Rashly, to fall once more; and that false fruit,
Which, to your over-weening spirits, yields
Hope of a flight celestial, will produce
Misery and shame. But wisdom of her sons
Shall not the less, though late, be justified."

The generation in which we are living seems to be fond of poetry of an emotional kind. Let me address to it a few more lines of a type of poetry which is less sentimental, but more truthful:—

"The Moving Finger writes: and, having writ,
Moves on: nor all your Piety nor Wit
Shall lure it back to cancel half a Line,
Nor all your Tears wash out a Word of it."

It would be well if we endeavoured to understand the real significance of these lines in relation to the doctrines of modern sentiment. In more than one way it points to us the irrevocable consequences of every attempt that is made to interfere with the beneficent workings of Nature. The "Moving Finger writes," and then "Moves on," is but a poetical way of warning us that for every committed act, social as well as individual, there are both immediate and multiplied remoter consequences, the misery entailed by which cannot save us from their inflexible operation, nor all the tears engendered by them wash out a single punishment which they inflict.

We all remember as children reading of the traditional King Canute, who, at the instigation of his flattering courtiers, sat on the sea-beach and bade

the tide recede. He bade the immutable processes of Nature that determine how and when and to what extent the tides shall rise and fall to cease. But no decree of man or king can alter by the fraction of an inch the rising of the tides. It would be well if we recognised that human life is controlled and determined by processes that are as immutable and as merciless as those which govern the tides or determine the movements of the planets. We can learn to understand them and to obey them, but we cannot destroy or diminish them.

Unfortunately there exists to-day a Canute who is not traditional, but figurative. We may regard him as the symbolic emblem of modern sentiment. He sits upon the sea-shore of current deeds, and, looking forth upon the rising events of Futurity, demands that they shall cease to rise! The hour of his disillusionment is not far off; it is nearer than he imagines, for the shore upon which he sits shelves less than he believes. By not the fraction of a second will the tide of Nature's processes be delayed or hastened in its rise upon the Future of the Nation.

What we sowed yesterday is to-day germinating, and will be reaped to-morrow. There is time, even now, to dig up the sprouting tares before their harvest shall have destroyed the wheat. Who, with hoe and rake in hand, will go forth and lead the way? Will the natural rulers of the Nation, the Aristocracy, do it, or must the more vigorous and virile of the Intellectual Classes—the Upper Middle Strata of Society—ignoring its poetically æsthetical section, usurp their position and discharge their functions?

APPENDIX.

Added February, 1909.

Note to p. 68.—By " Genetics " is understood that branch of Biology which studies the phenomena of heredity. Quite recently, owing in part to a rediscovery of Mendel's generalisation and in part to the large and increasing volume of experimental evidence in corroboration of it, the study of heredity has now for ever emerged from the chaos which marked its previous condition. An appeal to accurately conducted experiments is the soundest and safest method by which the problems of heredity can be solved. It may be urged, indeed has often been urged, that we cannot apply the experimental method to man. But so far as he is concerned, experiments on a vast scale have already been performed. It is not the want of experiment that we lack, but the proper and accurate recording of the results. Blue eye with blue eye, brown eye with brown eye, and brown eye with blue eye have been mated together in numbers that dwarf our most colossal experiments—even those of Prof. Bateson with an offspring of twelve thousand five hundred chicks—and yet the only accurate analysis and record of these facts which has been made is that by Mr. Hurst of quite recent date. North American Indians and negroes have been crossed with Europeans many hundreds of times and their progeny have intermarried and been married back to the original European or coloured stock still more frequently, and yet nowhere is there

to be found a scientifically recorded statement of the facts. The material exists, but it wants scientific analysis and recording. If this address should be read by any persons of leisure, or by medical men, who may at any time be resident in Canada, and who may become acquainted with families where intermarriage between Europeans and Red Indians has occurred, they may render services of the greatest value by taking photographs, both full-face and side-view, of the grandparents, parents, offspring, and of as many collaterals as possible, and recording as carefully as can be the colour of the skin (both that exposed and that protected), the colour of the eyes, and the colour and nature of the hair of each individual. More valuable still would it be if a lock of the hair of as many persons as possible were obtained. If it is not possible to obtain complete pedigrees, then partial ones will serve the purpose, provided that the nature (whether half-castes or otherwise) of the different individuals is accurately ascertained. A letter addressed to me at the London Hospital Medical College, London, E., will always reach me. In the same way marriages between negroes and Europeans and the various marriages of the mulattoes could be recorded. In the case of mulattoes, quadroons, and octoroons it is desirable, if possible, that observations as to differences in tints in each class should be recorded. It is also very essential that not only skin colour, but hair colour, hair texture (whether European or woolly), and eye colour should be recorded. Other facial features will, of course, be recorded in the photo-

graphs. The results of crossing albino negroes with normally pigmented members is also urgently required. The same considerations hold with regard to human albinism generally. Much of the published records are incomplete and not precise enough. The case of Josephine Chassot described on p. 77 is an example of this. She is described in the original publication as "affected with albinism," as having "pink eyes (pupils)," but "with peculiar lilac-coloured irides," and as having a "very white skin." But nothing is said about her hair colour. When described in comparison with her sister, who is stated to be dark, she is said to be fair (blonde). Such a description may mean that her hair is flaxen or white. The other albinoes in this pedigree are simply described as "albinoes," or as being "affected with albinism." But as to whether they are complete albinoes having no pigment at all, or are so-called partial albinoes having flaxen hair, we are left to infer.

Note to p. 68.—A definition of "Eugenics" by its author, Mr. Francis Galton, may be found in "Nature," Vol. LXX., p. 82, 1904. "Eugenics is the science which deals with all influences that improve and develope the inborn qualities of a race." The essential basis upon which "Eugenics" is founded exists in the consideration of certain postulates which all will accept, *i.e.*, "that it is better to be healthy than sick, vigorous than weak, well-fitted than ill-fitted for their part in life. In short, that it is better to be good rather than bad specimens of their kind,

whatever that kind might be." "The aim of Eugenics is to represent each class or sect by its best specimens, causing them to contribute *more* than their proportion to the next generation ; that done, to leave them to work out their common civilisation in their own way."

Mr. Galton thinks that learned and active societies desirous of promoting Eugenics might proceed by the following methods : " (1) Dissemination of a knowledge of the laws of heredity so far as they are surely known, and promotion of their further study. (2) Historical enquiry into the rates with which the various classes of society (classified according to civic usefulness) have contributed to the population at various times, in ancient and modern nations." Mr. Galton then adds : " There is strong reason for believing that national rise and decline are closely connected with this influence." " (3) Systematic collection of facts showing the circumstances in which large and thriving families have most frequently originated. (4) Influences affecting marriage. (5) Persistence in setting forth the national importance of Eugenics."

With reference to the third and fourth of these proposals—and which may also partake of the nature of a note to pp. 99 and 100—I would like to add that I have for some time now been endeavouring to collect facts bearing on the causes of what we may call " unhappy or incompatible marriages," and I am desirous of obtaining further information from those who are in a position to give it. Names are not

necessary, and all information will be regarded as confidential. There is reason to believe that in some cases these marriages are the result of an inherent incapacity upon the part of one of the two partners to make a felicitous choice. An inherent incapacity of this sort would, therefore, because it is inherent or congenital, run through members of the same family, and would be manifested by that family having a larger proportion than usual of such unhappy marriages. If any one of my readers is acquainted with families of this kind, I shall be glad if they will communicate with me.

In other cases, my present information suggests that these marriages are the result of a too early union. At a certain age, say, from twenty to twenty-five, a pair of individuals may possess temperaments and capacities that are more or less compatible and could harmoniously exist together. But at a certain later age, let us say about twenty-six to thirty, one or both individuals undergo a change in temperament, character, and ideals, perhaps as divergently as it is possible to conceive, and the marital state becomes incompatible and impossible. In this connection, it is a fact of some interest, which I have no doubt others have observed, though perhaps they may have given it no more than passing consideration, that there exist people who instead of becoming wiser with experience, become more foolish, and who instead of becoming more proficient become less so, as they pass on towards their primal years. If such persons passably discreet, tactful, and proficient at the

time of marriage, for the positions which their youthful years are likely to occupy, are married to partners who improve in all qualities as time advances, it is clear that the barrier of incompatibility must widen with the passage of each year. I do not suppose that these cases are very numerous, but still they exist.

And it seems to me a very important matter, because I think that these changes of which I speak are inherent or inborn in their nature, and can no more be avoided than the appearance of the antlers and of the fighting instincts of the stag when it has reached its maturity; and cannot be commanded to stay their appearance any more than can those sexual instincts and secondary sexual characters which inevitably appear in human beings at a certain stage in their developement. This appearance of new characters and new qualities at different stages in the lives of individuals, and of one kind for one person and of another kind for another, is of far wider social import than its application to married life. Its very existence vitiates a great deal of the medical evidence that was given before the Committee on Physical Deterioration and Degeneration. This latter question is not so much a medical one as it is a biological one, and the medical evidence, though valuable, is not the most important that can be adduced, and, in fact, in many of its conclusions it is erroneous and in its nature entirely misleading.

Note to pp. 102, 104, *and* 106.—While this address has been passing through the press, four events of some

significance have happened. One of these is the earthquake of Messina and its attendant incidents. Among these incidents there is one which is, in history, of such ordinary occurrence that in our familiarity with it we treat it with an altogether undeserved indifference. I allude to the fact that among the stricken and starving inhabitants, " fierce fights with knives for bread " occurred. And, after some seeming order had been restored, the distribution of food by the authorities took place in the presence and under the protection of an armed guard. Where hunger presses and it cannot be satisfied, blood will inevitably flow.

The second incident is the occurrence of a fire at a cinematograph exhibition in the East End of London. It was apparently quite a simple and not very dangerous affair. The cinematograph film caught fire and made a flare altogether out of proportion to its possibilities of danger. According to the newspaper reports of the operator's statement of the accident, the cinematograph machine was enclosed in an iron box. None the less, a panic seized the audience, and the women, in their frantic endeavours to save themselves, trod down the children, and the men trod down both women and children in their equally frantic efforts to save their threatened lives. " Each one for himself and destruction to the hindmost," expresses this event. And within the past two years there have been a succession of similar events.

The third incident is a " nine-days' wonder." It

ought rather to be an iconoclastic fact ever fresh in our memories. Whenever it is recalled it will probably be spoken of as the " Tottenham slaughter." Posterity quickly forgets. In a twelve-months' time the affair will have sunk into oblivion, in company with a great many more unpleasant facts that ought likewise to be remembered. Let me, therefore, record its essential details. A Russian outlaw had sought the hospitality and protection of English laws. He had lived in this country apparently for some months, but for a fortnight of this time he had been employed in an English factory at Tottenham, but gave up the work " because it was too hard." During the interval he had learned that the wages for the employés were brought from the bank in a motor-car at half-past eleven o'clock every Saturday morning. On the Saturday morning of January, the 23rd, 1909, this outlaw, in company with a companion, who was similarly an Anarchist outlaw from Russia, waited at the appropriate hour outside the entrance to the works. Both men were armed with revolvers and with pocketfuls of cartridges. Their intention, therefore, was quite clear and premeditated. As soon as the messenger had alighted from the car with the bag of money, it was seized by one of the desperadoes, and in the struggle which ensued the messenger was fired at and wounded. The assassins then made off, and endeavoured to clear a path of escape for themselves by indiscriminate and wild firing, right and left of them. The hue and cry was raised, and chase was given. Some of the pursuers were on foot, some

on horseback, and some in a motor-car. The pursued were chased for a distance of five miles, making their way towards Epping Forest. In the course of their reckless flight, they boarded an electric tramway, fired at and wounded some of the passengers, and one of them compelled the driver to proceed at a rapid pace by holding a revolver at his head. While this one was thus engaged at the front of the car, the other was at the rear, and in an indiscriminate fashion was firing right and left. At a certain point they left the car, which they then believed to be nearing a police-station, and boarded a milk-cart, which they drove in another direction at a furious pace. This they ultimately left, and then took to the fields. Soon after this one of the two was brought to bay, and he shot himself, but not mortally; he was taken prisoner, and subsequently died from meningitis in a hospital. The other effected his escape by an almost superhuman effort, and finally took refuge in a small house. Here he was at last shot by one of the policemen who had taken part in the chase and who had borrowed a revolver.

In the course of this bloody fracas, a policeman and a small boy were shot dead and twenty-three persons were wounded. One important moral we may draw from this event: The civically unfit under an autocratic régime are similarly unfit under a democratic one, even though it be pervaded with a benevolent sentiment. But the autocratic régime knows how to get rid of them, and the democratic one how to receive them.

Here, then, in our midst, modern sentiment allows a type of criminal not only to exist, but also to breed ; and this type is one so devoid of all sense of civic responsibility and so destitute of the rudiments of social instinct, that the individuals which constitute it are deliberately prepared to ruthlessly and indiscriminately murder all who shall endeavour to frustrate their criminal desires ; and even children are not excluded.

Do not let us delude ourselves by believing that only Russian Anarchists can perpetrate these deeds. This type of criminal is a mutation common to all nations and to all races of mankind. They are not the product of any social conditions any more than the short-legged and long-bodied ram which gave rise to the Ancon race of sheep was the product of special farm conditions. They are germinal mutations, and as such will breed true to their mutant character.

The fourth event occurred on Monday, the 11th of January, 1909, at half-past seven o'clock in the morning. The Democracy of France has been permeated, in common with other Western European nations, with the spirit of sentimental regard for all types of civic unfitness which we have been considering. And France has been paying the penalty. For, encouraged by the knowledge that this morbid sentiment would protect murderers from the guillotine, and would only send them to expiate their crimes beneath the genial climes and the cocoanut trees of French Guiana, with a profusion of tobacco to smoke and of abundant leisure, a gang of desperadoes

had terrorised the North of France by a great number of motiveless and ruthless murders. The leader of this band had committed two hundred and fifty such murders, and he was tranquilly playing cards and contemplating the happy hours he would spend under the cocoa-palms, when he and his companions, three in number, learnt the fact that even the stupidest sentiment must some time come to an end. For at last, craven fear had attained what common-sense could not, and these murderers were condemned to death. An enormous crowd assembled to witness the execution. Throughout the proceedings the crowd did not cease to raise shouts of "Death, death!" The crowd was "delighted," "exultant," and "excited." France has, through the agency of her sentiment, bred her criminals and unfit, just as we are doing, and now she must meet the situation by a ruthless vengeance and wholesale destruction, just as we shall have to, or herself sink into "death vomited in great floods." *Laisser faire*, which leaves all unfitness to reap its own destruction as fast as it arises, achieves the end more humanely and much quicker.

I have appended these four cases in order that we may be reminded what will be the conduct of the human multitude when the day of adversity and stress arrives. Unless a nation is composed of reliant and self-supporting individuals this day of adversity is inevitable. For so soon as our morbid sentiment has reared a shipwrecked horde of helpless incapables of such dimension that the fit cannot or will not any longer bear the burden of its support, then the day of

reckoning will be with us, and " all our piety and wit and knowledge will not cancel by half a second " the casting of the die, nor all our tears, nor lamentations, nor unspeakable anguish of our womenkind and our children will avert by a single drop the streams of blood that will wash the streets of our great cities. It is not well to forget Nantes of the 14th of December, 1793. The Girondins came to regenerate a stricken France. They were men of talent, of courage, of constitutional principles, but the great impotent herd of the Sansculottes guillotined them by the verdict of a " Patriot Jury " of " *terminer les débats* " for their services to their country. Are we sure that we are not breeding a race of Sansculottes who will demand license for themselves and terror and destruction for every good citizen ? The beauty, courage, nobleness, and unspeakable pathos of Jeanne-Marie Philipon as she appeared before the blood-besodden crowd of the lower French Democracy did not save her, nor will it, under like circumstances, save the best and highest of our women from similar bloody and blindly vengeful deeds, when once our sentiment has called into being a multitude that is helpless to live by its own efforts and meets all resistance to its demands for the confiscation of the property of worthy citizens by bloody and ruthless executions. It does not matter whether the adversity which excites these deeds is due to the corruptions of a Royal Court, to the impotence of a brutal multitude, or is the product of an excitable and morbid sentiment. Adversity, cupidity, and hunger are not altered in their nature

or their effects by the method of their origin. And the lower the social class we rear and pamper, the more animal and the more hæmal will its instincts and its acts become.

England, then, should beware of that fatal lethargy which rests on the belief that evil and destruction cannot visit a nation that was once—and may yet remain if she so wills it—the greatest of the recorded Ages. Upon the walls of her Temple the inscription of her doom is being written. Shall it be erased before it is completed, and in its place the motto of virility inscribed: "England shall be Fit and Strong?" If that is the nation's desire, the day of preaching is done, and the hour of deeds has come. For, on the dismembered Empires of the Past, the Warning Angel of Historical Experience stands, pointing the way to England's safety. The admonitions that rise anew from the ruins of Greece, of Rome, and of Venice, bid us desert the road of a too widened Democracy, and turn to that which leads towards a wise, an understanding, and a broadened Oligarchy.

Never to any country came the opportunity that lies at England's feet to-day. With an Artizan Class unequalled for skill by any rival; with commercial organisers of great resources, subtlety, and boldness of methods; with a Middle Class eager for enterprise and gifted with productive capacity; with a Literature studded with gems as precious as any the world contains, and the names of whose poets and prose-writers are honoured in every seat of culture

throughout the world; with a Scientific Hierarchy the greatest of its Age; with a Landed Gentry, the blood of whose sons has been shed in their country's welfare on the torrid deserts, fertile plains, and frozen snows of every Continent, and whose deeds of valour, codes of honour, and conceptions of duty have furnished an example to every age that shall look back to them for inspiration; with an Aristocracy that has succeeded beyond all others in the government of diverse peoples, England is blessed with that combination of qualities that should render her invincible against the world. And yet, mighty though she inherently is, sound in sinew, strong of intellect, and prolific of wealth, she lies prostrate before the fetich of a lower caste Democracy; her splendour sullied, her energies paralysed, her culture forgotten, her knowledge neutralised, and her traditions ignored, amid the blatant din of government by untruthful mural posters, by uncouth and pyrocephalic demagogues careless of veracity, and by newspaper placards that shamelessly and ostentatiously offer bribes to the lowest and least worthy of English society. Truly the hour and its running sands call for leaders who, like Pitt and Wellington, the Aristocrats, shall think of country before salary, of honour before the acclamation of a multitude, of principle before expediency, and of the preservation of the Fit, not the bribery of the Unfit; who shall put Biology before Sentiment, and, wedding the former to History, shall build anew, with the aid of a more virile people, the foundations of a yet greater Empire. But if she is to accomplish

this, then in the immediate self-denial of a mistaken sentiment and in the struggle with the errors of to-day, she must sow the seed that shall ripen into the glory and the welfare of to-morrow. The biological and historical hour-glass alike warn her, that the sands of her greatness have nearly run their course. Is England still great enough to stem the Tide and turn back the Flood? If so, her rejuvenated but broadened Oligarchy shall proclaim the answer.

MENDELISM AND SEX.

An Address delivered to the Mendel Society, 29th March, 1909.

BY

C. C. HURST.

THE question of the determination of sex is an old one. It is a problem which has been much debated along various lines of inquiry. Until recently it not only eluded all solution, but gave no promise of solution.

Recent Mendelian experiments, commenced about the year 1900, had not, however, been very long in operation before the conclusions to which they led afforded clear suggestions that the problem of sex could be investigated by the same methods and with the aid of the same directing principles. Mendelism, in fact, has provided the key by which the question can be accurately and experimentally answered. Much however remains to be done, for the knowledge which we now possess is to be regarded as in the nature of a right beginning rather than as a final solution.

Until quite recently it was generally believed that external conditions determined the sex of

individuals. It was assumed that if we knew these conditions we should be able to decide at will the sex of the unborn young. Acting under the influence of such a belief, several investigators designed and carried into operation various experiments that were intended to show the part which different factors in the environment played in the determination of sex. The well-known experiment of Yung with tadpoles was of this class. In this experiment, certain tadpoles were fed on very nutritive and abundant food, while others were fed on less nutritive and limited material. It was found that there was a larger percentage of female frogs derived from the former and of male frogs from the latter. The conclusion thus suggested itself that females were determined by excessive anabolic or building up process of nutrition, and males by that antithetic process in which anabolism only just keeps in excess of the katabolic or breaking down processes. But there were several sources of error in such an experiment. The most serious one is indicated by a similar experiment with the caterpillars of certain butterflies. In this experiment it was ascertained that under-feeding did not result in the production of an excess of males, but in the elimination of the females. There being a heavy mortality among the females, it is a natural consequence that there appears to be an excess of males. But it is apparent and not real. It is, therefore, in reality, not a case of sex-determination by environmental influences, but one of survival

of the fittest. The males are more resistant to the harsh effects of a low nutritive diet than are the females. Moreover, the interesting case of the bee should have been sufficient to expose the fallacy of the belief that the environment can determine sex. The queen bee lives under special and uniform conditions, and she is fed on highly nutritive material. If conditions determine sex, then she should produce offspring definitely predominating in one direction with respect to sex, for it should be mainly feminine or mainly masculine. Moreover, if high nutrition determined the formation of females, then the parthenogenetic eggs of the unfertilised, but specially fed, queen bee should be mainly or wholly female. But the reverse is the case. The unfertilised eggs all produce male bees or drones, while females or workers are alone produced from the fertilised eggs. The simple fact that the act of fertilisation thus determined the sex of the individuals arising from fertilised eggs is by itself sufficient to show that sex is determined within the germ-cells, and is not dependent on environment.

It is recognised now, by those who are engaged in the experimental investigation of the question of sex and of cognate problems, that sex itself, like other qualities, is predestined in the germ-cells, and is definitely determined by fertilisation. And, when it has thus been predestined and determined, external influences cannot alter it. It is, however, possible that in particular cases the proportions in which the sexes may appear are determined by

environmental influences. In the case of the butterfly caterpillars mentioned above, certain conditions associated with semi-starvation, while they do not determine sex itself, do determine the proportion of males and females, by unduly eliminating the latter without influencing the former. But this difference in the resisting powers of the two sexes is probably exceptional, and, as a rule, the sexes are similar in their inherent responsive powers towards special environmental conditions.

In the majority of cases, where we are in possession of sufficiently accurate statistics, it appears that as a general fact the sexes are produced in equal numbers. This is so for Man, for the lower animals, and for unisexual plants. The production of the two sexes in equal numbers is a significant fact from the Mendelian standpoint. It had no meaning whatever in pre-Mendelian days; but now its interpretation is clear. The individual distinctness of the sexes is also a significant fact. It indicates the complete segregation of maleness and femaleness. This segregation of the sexes and their occurrence in equal numbers at once suggested the well-known Mendelian ratio of 1 : 1. This ratio is the result of mating a Mendelian hybrid* with an individual carrying the recessive character, and it indicates that one of the two sexes is a dominant to the other. If we use the symbols which have been previously described,* and if we tentatively regard the female as a dominant hybrid and the

* For definition and explanation see pages 63 and 67.

male as a pure recessive, or *vice versâ*, then they will be respectively symbolised as *D R* and *R R*. A reference to the table of matings on page 69 will show that the expected offspring from a *D R* parent mated with an *R R* one, will consist in equal numbers of *D R's*, which in this case will be females—if we regard the female character as dominant—and of *R R's*, which will be males. Thus far, then, the two general facts already known to us, namely, the segregation and numerical equality of the sexes, strongly suggest that sex is predestined in the germ-cells and is hereditarily transmitted in accordance with the Mendelian principles of gametic purity and segregation.

It was not however until quite recently that experiments specifically designed to answer the questions presented by sex have, by their results, extended the suggestion into proof. Among the most important and interesting of such experiments we must place those of Professor Correns with two species of the Bryony plant. In the species known as *Bryonia dioica* female flowers are found on one plant and male flowers on another; the two sexes are borne on different individuals, so that any particular plant is either male or female, and not hermaphrodite like the majority of plants. The other species, named *Bryonia alba*, has the sexes borne on different flowers but on the same plant. Each individual plant is therefore hermaphrodite, bearing both male flowers and female flowers. When the flowers of a

female *B. dioica* are crossed with those of a male *B. dioica*, the offspring consist of a mixture of male and female plants.

The most interesting crosses, however, are the reciprocals between the hermaphrodite plant of *B. alba* and the two unisexual plants of *B. dioica*. When the flowers of the female plant of *B. dioica* are crossed with the male flowers of *B. alba*, all the individuals in the offspring are female plants.* But the reciprocal cross, strangely enough, gives a very different result. For, when the female flowers of *B. alba* are pollinated with pollen from the flowers of a male *B. dioica*, the offspring consist of male and female plants in approximately equal numbers.

Correns endeavoured to interpret these results in the following way. Let us take first the cross of male *B. dioica* with female *B. dioica*. The result, as we have seen, is a mixture of males and females in equal numbers. We will for the moment assume, as Correns did, that maleness is dominant ; that the male plants are Mendelian hybrids and therefore carry two kinds of pollen-cells in approximately equal numbers, one half bearing the character of maleness and the other half that of femaleness ; and that the female plant, being recessive, must be pure with regard to femaleness, and all its egg-cells will therefore carry femaleness alone. It is clear that a cross of the

*A few exceptions—2 males with 589 females—are said to occur, and a few of the females bear occasional male flowers.

nature which we are now considering will resolve itself into the simple Mendelian one of *D R* by *R R*.* The *D R* here represents the male plant and the *R R* the female plant. The symbol *D* in this case stands for maleness, which is dominant, and *R* for femaleness, which is recessive. Since the female plant bears only "female" egg-cells and the male plant bears both "male" and "female" pollen-cells in equal numbers, it must happen in the random meetings resulting from pollination that one-half of the "female" egg-cells will be fertilised with "male" pollen-cells which will give us *D R* offspring, and one-half with "female" pollen-cells which will give us *R R* offspring. And, since maleness is dominant, then the individuals which result from a fertilisation of *R* egg-cells by *D* pollen-cells will be hybrids and will be visibly male. Thus there will be produced equal numbers of hybrid males (*D R's*) and of pure females (*R R's*). So far, then, the Mendelian interpretation is in accordance with the experimental facts, and it gives them an intelligible unity.

Let us pass next to consider the crosses between the two different species. As we have already seen, they resolve themselves into crosses of the two following kinds :—

	Nature of Cross.			
	Bryonia Dioica. (Unisexual).	×	Bryonia Alba. (Hermaphrodite).	Result. Offspring.
(1)	Flowers of female plant	×	Male flowers	= All females
(2)	,, ,, male ,,	×	Female flowers	= Males and females

* See page 69, where such a cross is explained.

Now, as we have already pointed out, the individuals of *Bryonia alba* are hermaphrodite, bearing both male and female flowers. In other words, as individuals they are not differentiated with regard to sex. We here meet with a new kind of cross, different from that between the unisexual individuals of *B. dioica*. When the unisexual individuals of this latter species are crossed with each other, we are really dealing with the crosses of two differentiated individuals, in each of which the one sex has segregated from the other. But in the cross of *B. dioica* with *B. alba* we are dealing with one where a sexually differentiated individual is crossed with an undifferentiated one. The results show that we may regard differentiation and non-differentiation (absence of differentiation) as definite characters capable of hereditary transmission. Hence we are here dealing with a cross between a differentiated unisexual plant and a non-differentiated hermaphrodite one, the unisexual character being apparently dominant.

Let us first consider case No. 2 in the table above. Some of the pollen-cells of the male flower of *B. dioica* are carrying maleness and others are carrying femaleness. The egg-cells of the female flowers of *B. alba* are carrying undifferentiated hermaphroditism. In a cross of the kind we are considering, two results may happen. For both the male and female carrying pollen-cells of *B. dioica* will meet the egg-cells of *B. alba* carrying the hermaphroditic character. In the former case there will be produced fertilised egg-cells bearing the characters of

maleness and hermaphroditism. These cells when they develop into individuals will produce unisexual males, because both unisexuality and maleness are dominant. The second possible result will be attained in the latter case, when fertilised cells having the composition of femaleness and hermaphroditism are formed. These cells will give rise upon development to unisexual female individuals, because unisexuality is dominant over hermaphroditism. Thus two kinds of individuals are expected on this interpretation, unisexual males and unisexual females. And the experimental results do, as we have seen, confirm this Mendelian expectation.

Now we will take the reciprocal cross as shown in case No. 1 in the table. Here again we meet with the undifferentiated hermaphroditism of *B. alba*, but now carried in its pollen-cells. In this cross, then, only one result, and not two, as in case No. 2, will occur: the egg-cells carrying femaleness alone of *B. dioica* will be fertilised by the pollen-cells of *B. alba*, which are carrying hermaphroditism alone. And since unisexuality is dominant to hermaphroditism, all individuals produced by this cross will manifest the unisexual female character. Hence, our Mendelian interpretation expects females only to result from such a cross. And experiment has shown that such is the case.

While the Mendelian interpretation devised by Correns does undoubtedly enable us to give an

intelligible statement of the probable processes at work, it is not altogether free from objection. The assumption of the undifferentiated nature of both the pollen and egg-cells of *B. alba*, with the further necessary assumption that these have a composition different from those of *B. dioica*, is one that would require a great deal of evidence in its favour before we should be justified in finally accepting it.

But there is a simpler interpretation which quite as adequately fits the facts and does not involve the serious assumption necessitated by Correns' theory. It is the interpretation of Professor Bateson. In his scheme we regard not the male, but the female, as being dominant. The pollen-cells of *B. dioica* are regarded as homozygous carrying maleness alone, while the pollen-cells of *B. alba* are also regarded as homozygous but carrying femaleness alone. The female or egg-cells of both species are regarded as heterozygous. Individuals of both species will possess two kinds of egg-cells, and these will occur in approximately equal numbers. One half of them will carry maleness and the other half femaleness. We shall then expect that when the egg-cells of *B. dioica* are fertilised with the pollen-cells of the same species, there will be produced equal numbers of male and female offspring. For when a pollen-cell which carries maleness meets an egg-cell which, as we have postulated, carries maleness alone, clearly the resulting offspring from such a fertilisation must be male. But when the second kind of egg-cell, that

which carries femaleness, meets a pollen-cell carrying maleness, it is clear that if femaleness is dominant, the resulting offspring will be female. But whereas the male is pure maleness, the female is a hybrid, carrying both maleness and femaleness.

Similarly, the offspring resulting from fertilising the egg-cells of *B. dioica* carrying either maleness or femaleness with the pollen-cells of *B. alba* carrying femaleness alone, are expected to consist of heterozygous (hybrid) females and homozygous (pure) females in equal numbers. That is, so far as visible characters are concerned—for we cannot distinguish these two kinds of females by inspection—the offspring are expected to consist entirely of females. And experiment shows that such is the case.

In the reciprocal cross of the egg-cells of *B. alba* carrying either femaleness or maleness fertilised with the pollen-cells of *B. dioica* carrying maleness alone, it is expected that one half of the offspring will be heterozygous females and one half homozygous males. And again experiment confirms our expectations, in so far that the offspring does consist of an equal number of males and females.*

The application of Mendelian methods and interpretation to the consideration of the experimental results does, therefore, give us an intelligible and consistent statement of the ascertained facts. No previous theory has ever accomplished that. And we may, I think, feel assured that at last we are on the right road towards the elucidation of this difficult but interesting problem.

**It would be interesting to know whether B. alba when self-fertilised produces female plants as well as hermaphrodites, as it should do according to this scheme.*

There are reasons for believing that the different factors which determine the hereditary characters of organisms are carried by certain nuclear bodies called the chromosomes. In form they are sometimes rod-shaped and sometimes horseshoe-shaped, and they make their appearance within the nucleus just before cell division is about to occur. They stain very darkly with certain dyes and their presence is thus rendered more manifest. For the individuals of a given species, the number of chromosomes present in the nuclei of the cells is constant. But the number of chromosomes in the body or somatic cells is, however, twice that in the germ-cells or gametes of any individual. Consequently, when at fertilisation two germ-cells, paternal and maternal, unite, the resulting cell, out of which a new individual will arise by cell division, contains the higher or somatic number of chromosomes. Thence during all the cell divisions which occur as the new individual developes from this fertilised cell, this higher or body number is retained in all the somatic cells which result. But, at a certain stage, the individual forms its germ-cells, and during the process the number of chromosomes becomes reduced to one half.

Now there is clear evidence, derived from the study of these chromosomes in certain insects, phylloxerans, and aphids, to show that in some way sex is directly determined, or its determination is correlated with, the presence of an accessory chromosome in certain of the paternal cells. That is, if the egg-cells contain five chromosomes

then one half of the sperm-cells or spermatozoa will also contain five, but the other half will contain four only. There are thus two kinds of spermatozoa developed. We owe this interesting observation to the investigations of Professor Wilson, Professor Morgan, and Miss Stevens. Between them they have examined about a hundred species of insects and other animals, and in all of them they have found this double form of spermatozoon present.

Now it is very clear that if there are two kinds of spermatozoa in certain animals, the one kind approximately equal in number to the other, and there exists only one kind of egg-cell, in random fertilisations one half of the eggs will be fertilised by one kind of spermatozoa and the other half by the other kind. And from the fertilised eggs, fertilised by two different kinds of spermatozoa, it is to be expected that there will arise two kinds of individuals. Let us illustrate this by reference to the following diagram (Fig. 1). In this diagram A represents the spermatozoon carrying only four chromosomes, while B represents the other form of spermatozoon, that which carries five chromosomes. We may speak of the fifth or odd chromosome as the accessory one. The egg-cells are all of one type and carry five chromosomes; they are represented in the diagram by C. Now when A and C unite it is clear that the zygote or fertilised cell D^1 will contain only nine chromosomes, or one less than the zygote D^2. This latter, which is formed when B and C unite, contains ten chromosomes. The

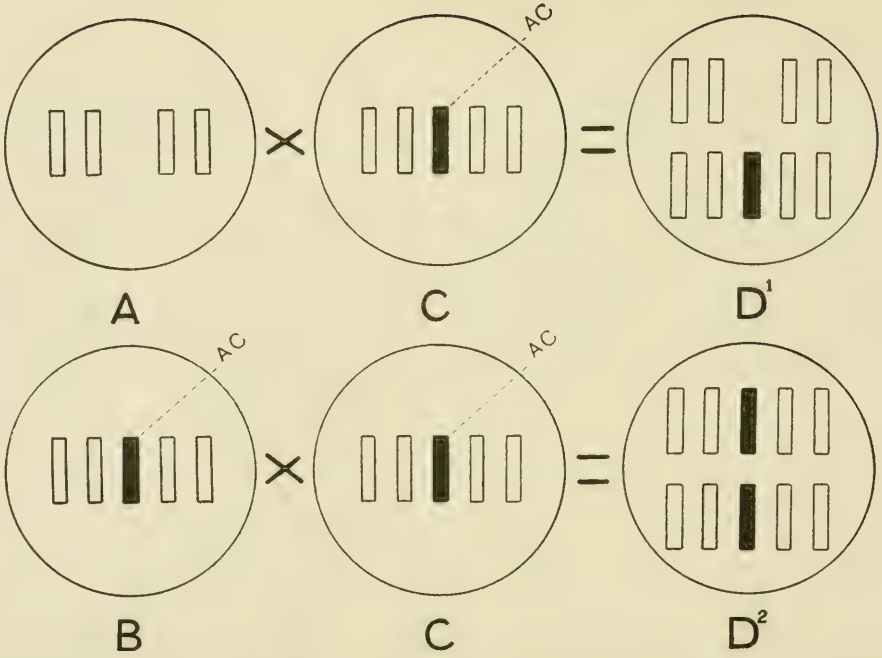

FIG. 1. Diagram (after Wilson) to illustrate the difference in the somatic composition of the male (D^1) and the female (D^2) zygotes or individuals in the insect *Anasa*. In *Anasa* there is one kind of egg-cell (C), but two kinds of spermatozoa (A and B). Male individuals (D^1) result from the union of a paternal germ-cell (A), which is devoid of an accessory chromosome with a maternal germ-cell (C), which contains one (A c). Female individuals (D^2) result from the union of a paternal (B) and maternal germ-cell (C), both of which contain an accessory chromosome (A c).

The male individual (D^1) thus contains one chromosome less than the female (D^2).

former zygote will be a male, for the observations of the three investigators just mentioned have shown that the somatic or body cells of the males investigated by them contain one chromosome less than the body cells of the females. The latter zygote will be a female, since it contains the full complement of chromosomes.

If these observations prove to be of general application, then it would appear that sex is determined by the presence or absence of the paired condition of the accessory chromosome. If it is paired in the zygote, femaleness results; if it is unpaired, then maleness results. But, whether the zygote shall contain the paired or unpaired accessory chromosome depends upon the kind of spermatozoon by which fertilisation is effected. The male is the arbiter which decides the result.

These interesting discoveries in connexion with the chromosomes of cells enable us to frame explanations of several things which were previously incapable of any satisfactory or consistent statement. We may therefore next turn our attention to the new interpretation of old facts which these discoveries enable us to frame.

As is well known, the eggs laid by the queen bee are some of them fertilised and some not. But both sorts of eggs develop. The unfertilised ones, however, give rise to the male drones, while the fertilised eggs produce the workers, which are immature or imperfect females. In order to understand

this result we must first consider another fact in connexion with the chromosomes of germ-cells. We have already seen that in certain insects there are two kinds of sperm-cells or spermatozoa ; one of these carries an accessory chromosome and its union with the egg-cell produces a female, while the other kind is devoid of the accessory chromosome and the egg fertilised by it developes into a male. Now, if we suppose that something happened by which all the latter kind of sperm-cells became destroyed, it is obvious that male offspring would never be produced. The product of fertilisation would be females always, and this we know is the case, not only with the fertilised eggs of the bee, but also of the green-fly (*Aphis*), of a water flea (*Daphnia*), and of a certain genus of plant-lice (*Phylloxera*). Is there then any evidence that this seemingly improbable selective destruction of a particular type of sperm-cell does actually occur ? Such a hypothesis appears to be exceedingly fanciful. None the less, it expresses an actual fact which has been ascertained by careful observation. For Professor Morgan has observed that in a *Phylloxera* one half of the sperm-cells are small in size and do not contain the accessory chromosome, and ultimately degenerate ; they therefore can take no part in fertilisation. But these are the cells which determine maleness in the offspring which arise from the eggs fertilised by them. Therefore, fertilisation being effected only by the other kind of cell, females alone are produced. Miss

Stevens has observed a similar condition of degeneration of those sperm-cells which are devoid of the accessory chromosome in another genus of plant-lice, *Aphis*. And in bees the degeneration of one half of the sperm-cells has also been observed by Meves.

In this way our knowledge of the behaviour of the accessory chromosome, while rendering it certain that sex is a gametic differentiation, that it is a quality not depending upon environmental influences, also enables us to explain the phenomenon that in bees, plant-lice, and water-fleas, fertilised eggs develop only into females. For femaleness, as has been shown by Professor E. B. Wilson in the insect *Protenor*, and by Professor Morgan in certain species of *Phylloxera*, is characterised by the presence in the body cells of the full complement of chromosomes, while maleness is characterised by the absence of one or two of these.

It may therefore be accepted as a demonstrated fact that in certain insects and in certain phylloxerans and aphids the body cells of the females possess one—or in some cases two—chromosomes more than those of the male. It is also a fact that in some insects, phylloxerans, and aphids there are two kinds of spermatozoa, one kind, which is smaller than the other, containing one or two chromosomes less than the larger kind. The smaller ones, which are the carriers of maleness, degenerate, and leave, therefore, only the larger ones, which are carriers of

femaleness, to effect fertilisation. It therefore follows as a natural sequence that from fertilised eggs only females are produced.

These being the facts then with regard to insects, phylloxerans, and aphids, and, further, it being known as an observed fact that in the bee one half of the spermatozoa undergo degeneration, we may be justified in believing that the same explanation is applicable there. We may feel fairly certain that when the case of the bee has been adequately investigated we shall find that the reason why all its fertilised eggs produce females only is because the male-bearing spermatozoa are the ones which degenerate.

We pass next to consider why it is that in the bee the unfertilised eggs—that is, the virgin or parthenogenetic eggs—always give rise to drones, which are male individuals. Here again we must seek our interpretation by appealing to facts which have been ascertained in phylloxerans and aphids. We have already seen that Professor Wilson and Professor Morgan have observed in certain insects (Wilson) and in phylloxerans and aphids (Morgan) a certain difference in the number of chromosomes in the body cells of the females and males. In the males of insects and aphids and in some phylloxerans there is one chromosome less than in females ; but in other phylloxerans there are two less. Morgan has quite recently shown that there are two kinds of parthenogenetic eggs, one containing a pair of accessory chromosomes and the other not ; that is, one kind

contains two more chromosomes than the other kind. And, moreover, he has shown that in *Phylloxera fallax* this internal difference is correlated with a difference in size, the egg which contains the accessory pair of chromosomes being larger than the one without. And he has further observed that the large parthenogenetic egg produces females and the small one gives rise to males. Now this difference between the two kinds of eggs arises in the course of maturation, for previous to this they both contain the higher number of chromosomes. But at the formation of the polar body, twelve chromosomes remain in the large egg, but only ten in the small one. It is clear then, that in the former case there has been no extrusion of the pair of accessory chromosomes during the formation of the polar body, while in the latter case they have been extruded into the polar cell. In other phylloxera and in aphids only one and not two chromosomes is thus extruded.

We thus see that those parthenogenetic eggs which are destined to give rise to males have one or more chromosomes eliminated from them. We may suppose this is also the explanation why the parthenogenetic eggs of the bee produce males alone. They apparently are all of one kind, and have had their accessory chromosome extruded during their maturation. They therefore carry the reduced number of chromosomes characteristic of the male somatic tissues.

The next case of sex-inheritance we may consider is that which results when the currant moth (*Abraxas*

grossulariata) is crossed with its lighter coloured variety *lacticolor*. Before the series of experiments which we are about to describe were made by Mr. Doncaster, *lacticolor* was known to occur only in the female form, for males were unrecorded. When *lacticolor* female is crossed with *grossulariata* male, the offspring consist of the *grossulariata* form alone, there being males and females in approximately equal numbers. It is clear from this result that *lacticolor* is recessive, since it does not appear in the F_1 generation. With regard to the dominance of one sex or the other, we may for simplicity regard the female as being dominant and as carrying maleness recessive, as we did for *Bryonia* (supra p. 134). The appearance of both males and females in the F_1 generation is then quite consistent with that assumption. Let us now see how the other possible crosses fall into line with such a scheme.

If this representation is right, then the F_1 *grossulariata* males will be all $G\,L\,\delta\,\delta$* and all the females will be $G\,L\,♀\,\delta$. The *grossulariata* character being dominant, both these forms are visibly *grossulariata*, but the male character being recessive can only manifest itself when femaleness is absent; hence the first form is a pure male and the second is visibly female, but carries male recessive. It is necessary before we go farther to make another working assumption. It is, however, not only adequately supported by the facts of these particular

* *Where G = grossulariata, L = lacticolor, ♂ = male and ♀ = female.*

experiments, but by other experiments in sweet peas. The assumption which the experimental facts compel us to make is, that between the *grossulariata* character and femaleness there is repulsion, as a consequence of which the two are never carried in the same germ-cell.

Let us summarise the position before going farther. The *grossulariata* character is dominant over the *lacticolor* one; femaleness is dominant and carries maleness recessive, and as a correlative outcome of this maleness is therefore recessive and pure; between the *grossulariata* character and femaleness there exists repulsion. Accepting these premises, we expect, on the basis of Mendelian principles, the following results when the crosses indicated below are made:—

Visible Nature of Cross.	Visible Character of Offspring.
$L♀ \times G♂$	$= G♀ + G♂$
$G(L)♀ \times G(L)♂$	$= G♀ + G♂ + L♀$
$L♀ \times G(L)♂$	$= G♀ + G♂ + L♀ + L♂$
$L♂ \times G(L)♀$	$= G♂ + L♀$

Now these expectations are fulfilled by the experimental results, for the above table is but an

* It should be borne in mind that the *lacticolor* character is not manifested when *grossulariata* is also present, and the recessiveness of the former is indicated here by enclosing it in brackets. It should also be remembered that all females are carrying maleness recessive and therefore not manifesting it.

epitomised statement of them, as well as being a tabulated expression of Mendelian predictions.

For the information of those who desire a full statement of the zygotic and gametic composition of the parents, and the zygotic composition of the offspring, the following table is appended. The four crosses here represented are the same as those given in the table above, and they are placed in the same relative order.

Zygotic Composition of Parents.	Gametic Composition of Parents.	Zygotic Composition of Offspring.
L L ♀ ♂ × *G G* ♂ ♂	*L* ♀ + *L* ♂ *G* ♂	= *G L* ♀ ♂ + *G L* ♂ ♂ *G* ♀ *G* ♂ †
G L̊ ♀ ♂ × *G L* ♂ ♂	*G*̊ ♂ + *L*̊ ♀ *G* ♂ + *L* ♂	= *G G* ♂ ♂ + *G L* ♂ ♂ + *G* ♂ *G L* ♀ ♂ + *L L* ♀ ♂ *G* ♀ *L* ♀
L L ♀ ♂ × *G L* ♂ ♂	*L* ♀ + *L* ♂ *G* ♂ + *L* ♂	= *G L* ♀ ♂ + *G L* ♂ ♂ + *G* ♀ *G* ♂ *L L* ♀ ♂ + *L L* ♂ ♂ *L* ♀ *L* ♂
G L̊ ♀ ♂ × *L L* ♂ ♂	*G*̊ ♂ + *L*̊ ♀ *L* ♂	= *G L* ♂ ♂ + *L L* ♀ ♂ *G* ♂ *L* ♀

(In the table, ° marks the asterisk * printed above the letter.)

* It is one of the working hypotheses that this individual in forming its germ-cells cannot carry the grossulariata character and femaleness in the same germ-cell or gamete, since these two characters are assumed to repel each other. But they can of course be carried in the same individual or zygote.

† The symbols in smaller type represent the *visible* character of the offspring. Those in larger type represent their estimated composition.

The scheme by means of which we have thus endeavoured to give a consistent statement of the seemingly remarkable and apparently incoherent experimental facts is one which we owe to Professor Bateson and Mr. R. C. Punnett.

One feature of a general but very important nature remains to be considered. The *grossulariata* individuals used in the experiments were taken wild. And in the cross of the male of this with the female *lacticolor* variety, all the offspring, as we have seen, were *grossulariata*. This fact indicates that the wild *male grossulariata* is pure with regard to that character. But quite recently, Doncaster has made the reciprocal cross, namely, wild female *grossulariata* with certain male *lacticolors* bred in the course of the experiments. The offspring consists of males *grossulariata* and females *lacticolor*. As Professor Bateson points out* this is a striking result, and is not only a confirmation of the validity of his scheme of interpretation, which was framed before this fact was known, but it leads to a most important and far-reaching conclusion. Because clearly it means that the wild *female grossulariata* moths living in districts where the *lacticolor* variety is unknown, are in reality hybrids of *lacticolor*, carrying that character recessive. The males, as we have seen, are pure *grossulariata*. For hundreds of generations, possibly, the *lacticolor* variety has been in existence, hidden recessive in the females of the

* *Mendel's Principles of Heredity—Cambridge.*

wild *grossulariata*. And it has not been manifested because the males are pure and the *grossulariata* character is dominant. Some light is thus thrown upon the nature and origin of variations. We may be justified in believing that the *lacticolor* variety became a manifest one by a germinal change in one or more of the eggs of a wild female *grossulariata*, resulting in the elimination of the factor which produces the larger spotted condition that chiefly distinguishes *grossulariata* from its variety *lacticolor*.

The first individuals, like the later ones of the variety, would be all females, and these would necessarily have to cross with the wild male *grossulariata* in order to perpetuate their race.

Fundamentally similar to the sex-inheritance of the characters we have just considered in the currant moth, is that of the black-eye in the "green" or the yellow canaries and the pink-eye of the cinnamon canaries. Miss Durham has shown that the pink-eye condition of these latter birds applies only to the early days after hatching, for, as they grow older, pigment of a chocolate colour appears. Although the pigment in the eyes of adult cinnamon canaries is really chocolate, yet it appears to be black, owing to the degree of concentration in which it occurs. The pigment in the eyes of the "green" and yellow canaries is really, and not apparently, black.

The chief feature of general interest which is manifested by these experiments is of the same

order as that we have just considered in the currant moth. For Miss Durham's experiments seem to show that while the *male* black-eyed canary* is pure with regard to that character, the *female* is hybrid and carries the pink-eye (or, in reality, the cinnamon character)† recessive. It will be remembered (*supra page* 147) that among moths the *male grossulariata* was similarly pure with regard to the *lacticolor* character, while the *female* was hybrid.

In canaries the existence of this remarkable condition is shown by the different nature of the offspring in the reciprocal crosses. For when male "black eye" is crossed with female "pink eye," all the offspring are black-eyed, both the males and the females. But when male "pink-eye" is crossed with female "black-eye," then among the offspring all the males are black-eyed and all the pink-eyed are females.‡ The result is thus one which follows from a mating of the ordinary Mendelian kind, namely, $D\,R \times R$ giving $1\,D\,R + 1\,R\,R$ (see table of Mendelian matings, page 69). In this case the female black-eyed bird is the $D\,R$, and the pink-eyed male is the $R\,R$. The black colour of the eye is dominant = D, and the pink-eye, or absence of blackness, is the recessive = R.

The new fact which these experiments reveal is

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* It does not matter whether the black-eyed canary is a "green" or a yellow one. The result is the same

† In addition to the eye characters the cinnamon further differs from green canaries in having light brown (cinnamon) markings instead of black ones. This colour is due to chocolate pigment.

‡ Four exceptions occurred in which the hens were black-eyed.

what in past days we should have called the correlation or coupling of the pink-eyed condition with femaleness. Now we regard it not as a coupling of femaleness and pink-eyedness, but as a *gametic* repulsion existing between femaleness and black-eyedness. It is important to bear in mind that this repulsion exists in the gametes (germ-cells) only, and that the female is a dominant and a hybrid with regard to sex. That being so, hen birds may be black-eyed; but it must be remembered that while the zygote (the individual) manifests femaleness and black-eyedness, all its germ-cells which carry femaleness will only carry pink-eyedness. Its visible femaleness combined with black-eyedness is due to the fact that both these qualities, as well as maleness and pinkness, which are both recessive, were brought in at the fertilisation of the egg-cell from which it has developed.

We may tabulate these facts and their associated hypotheses as follows:—

Zygotic Composition of Parents.	Gametic Composition of Parents.	Zygotic Composition of Offspring.
$P\,P\,♀\,♂$ × $B\,B\,♂\,♂$	$P♀ + P♂$ / $B♂$	= $B\,P\,♀\,♂$ * $B♀$ + $B\,P♂\,♂$ $B♂$
$P\,P\,♂\,♂$ × $B\,P\,♀\,♂$	$P♂$ / $B♂ + P♀$	= $B\,P♂\,♂$ $B♂$ + $P\,P\,♀\,♂$ $P♀$

* See Second note, page 146.

Since the pink-eyed condition is really due to the absence of black pigment, we may regard the alternative factors as being blackness = *B* and absence of blackness = *b*. In such case, we should substitute the symbol *b* for that of *P* in the table above. Similarly we may regard (*infra page* 156) maleness as simply a condition which is left when femaleness is withdrawn. In other words, maleness is simply absence of femaleness.

The two cases, namely, the currant moth and the canary, which we have last considered, are instances of what may be described as sex-limited inheritance: that is, a certain character in its inheritance is transmitted by the germ-cells which are carrying either maleness or femaleness, but not by both. There is thus conceivably a process of repulsion between this particular character and one of the sexes, when the factors which determine both are brought into association, in the ripening germ-cells. In both the cases we have just considered it is femaleness which exerts this repelling influence upon the particular character.

There is one further case apparently similar which came under my notice in the course of some experiments with poultry. Certain recessive white fowls carry the hidden factor which, meeting colour, produces grey-white feathers barred with blue-black. This " barred " character is spoken of in the " fancy " as the "cuckoo" character. When recessive white hens carrying "cuckoo" are crossed with black cocks, the offspring consist simply of "cuckoo" cocks and black hens.

And the facts of the experiments may be interpreted by assuming that the "cuckoo" factor is repelled by that for femaleness, so that the two can never occur in the same germ-cell. Hence all germ-cells which carry femaleness will be devoid of this barring-factor. The case, indeed, seems to be similar to those which we have already more fully discussed.

We pass on now to consider sex in Man. From the general fact which has been obtained from statistics derived from various sources, that the number of males and females are approximately equal at birth, we may say that in Man, too, the inheritance of sex is probably Mendelian, and follows the scheme $DR \times RR$. We have no reliable evidence at present which will enable us to say whether maleness or femaleness in Man is dominant. We may, however, tentatively infer that in Man, as in moths, canaries, and fowls, the female character is dominant, and therefore hybrid with regard to sex, since occasionally masculine secondary characters may appear in women. The appearance of such characters is, presumably, indicative that they are carried by woman, though usually not manifested.

Quite recently a book on Sex in Man, written by Dr. Rumley Dawson, has appeared.* It does not deal with the subject from the Mendelian standpoint, —indeed, the author is apparently not acquainted with Mendelism. But he formulates a remarkable

* A review appears on page 212 of this Journal.

and striking hypothesis, in support of which he describes a number of interesting facts.

To put them briefly, the principal points in his theory are these: The male exercises no influence at all in the causation of sex; this influence is wholly exercised by the female. From various clinical data he arrives at the conclusion that the female produces both male and female ova. This conclusion is interesting, since it is one which is quite Mendelian, but is arrived at by different methods from those employed by the Mendelian, and by one who is not apparently acquainted with Mendelism. But the feature of greatest interest in his theory is his assumption that the right ovary produces male ova and the left ovary female ova. He then further supposes that only one ovary is active and ovulates each month, the other not discharging any eggs. They thus alternately ovulate every other month, so that one month the right ovary is discharging male ova and the next month the left ovary will discharge female ova.

From these premises he proceeds to show that in normal cases, if we know the sex and date of birth of the first child, the sex of the following children can be predicted for any particular month, and, therefore, a boy or a girl can be begotten at will. The author produces some good evidence in favour of his theory, but it cannot yet be regarded as fully tested.

He tests his theory by considering the families of some eminent people. If ovulation occurs every

twenty-eight days, there will be thirteen periods of ovulation in each year. Consequently, in every succeeding twelfth month, the sex of the offspring will be the opposite of that in the first month. If in any October a baby girl was born, then if a birth should occur at the following October, the expectation is that it will be a boy. The author considered the date of the births of the children of various Royal personages. Taking Queen Victoria's family, we find that the Empress Frederick (Princess Victoria) was born in November, 1840, and King Edward VII. in November, 1841. In the case of the Duke of Edinburgh's family, a son was born in October, 1874, and a daughter in October, 1875. With the Duke of Connaught's family, a daughter was born in January, 1882, and a son in January, 1883. The family of the Tsar of Russia is interesting, since there are five children. Princess Olga was born in November, 1895, Princess Tatiana in June, 1897, Princess Maria in June, 1899, Princess Anastasia in June, 1901, and Prince Alexis in August, 1904.

So far, these instances and many others that might be quoted do support the theory, since prediction is fulfilled by fact. But Mr. Mudge has recently sent me the case of an Irish family in which two out of six predictions are falsified. I give it in a tabulated form on the next page.

The symbols with a cross appended to them indicate the cases where the prediction is not fulfilled, since two daughters were born where sons were expected.

Prediction.	Sex of Children.	Date of Birth.	Year.
	Daughter	May 31st.	1880
× ♂*	,,	November 20th.	1881
× ♂	,,	March 27th.	1883
♀	,,	June 4th.	1884
♀	,,	November 29th.	1885
♂	Son	December 28th.	1889

* ♀ = *female*, and ♂ = *male*.

A theory must not be judged too severely upon the basis of a few exceptions. Ovulation may have been suspended or irregular, or other disturbing causes may temporarily have disturbed the normal sequence. And if a large number of families, taken at random, in general confirm the theory, the exceptions to it must be regarded as exceptional. But still there are certain theoretical objections to the theory, into which we cannot now enter. Further it appears that the author's assumption, that the male does not influence the determination of sex, is inconsistent with a number of facts. For, as we have already seen (supra page 139), in some cases the male is the arbiter of sex; and, in all cases, by virtue of his definite gametic composition, he may be said to exercise as much influence as the female in the determination of the sex of his offspring.

If we now briefly summarise the facts and conceptions which we have considered in detail, we may say the evidence is clear that sex is predestined in the germ-cells, and is determined at the moment of fertilisation. It is highly probable, too,

that sex is due to a germinal factor, and there seems to be but little doubt that it is inherited in accordance with Mendel's law.

We have however to recognise the possibility that the females and the males of different organisms in the animal and vegetable kingdoms may be differently constituted. The evidence shows that in Man, canaries, fowls, possibly in rabbits, and in some insects, the female is apparently a Mendelian dominant hybrid, carrying the character of maleness as a recessive, while the male is a Mendelian recessive homozygote, and is therefore pure for maleness. On the other hand, in many insects, and possibly in lobsters and crabs, it is the male which is apparently the Mendelian dominant hybrid, carrying femaleness as a recessive.

The existence of these two types of sex leads us to suppose that in sex-heredity we have one more case in support of the " presence and absence " theory of Mendelian characters. In this newer scheme by which experimental results are symbolically represented, we still retain the original terms of dominant and recessive used by Mendel, but they are applied in a different way. When a pure grey rabbit is crossed with a black one, all the offspring are grey. Mendel would have spoken of the grey colour as being dominant to black which would be regarded as recessive. In other words, he would have looked upon the pair of alternative characters as being grey *versus* black. But we do not now so regard them. We believe the facts are

better represented by constituting the alternative characters as presence of greyness *versus* absence of greyness which gives blackness.* The presence of the character is thus dominant to its absence. So that presence of greyness is not said to be dominant to the presence of blackness, but to the absence of greyness. In the sense that the terms dominant and recessive are applied to the manifestation or non-manifestation in the zygote of the two unit-characters of an alternative pair, the terms are still used in the way in which Mendel employed them. But, as we have just seen, the conception of the nature of alternative characters has been modified. Dwarfness, for instance, is not now regarded as another quality to tallness, but simply as that condition which results from absence of tallness. Add the factor which determines tallness to dwarfness, and tallness is manifested. And similarly in respect to sex. We may conceive that femaleness is due to a germinal factor which determines the manifestation of that sex. It is something which is added to maleness, and in its absence maleness is manifested. The two alternative unit-characters of sex are therefore presence and absence of femaleness.

This conception of sex seems to be applicable to all cases which have yet been investigated, and it enables us to give a consistent interpretation of the facts in the two types of sex-inheritance which we have studied. Thus, if we let F represent femaleness and f its absence, then in man, since the female

* Grey being epistatic to black which is hypostatic.

seems to be a dominant hybrid, the female zygotic composition may be represented as *F f*. But man, the male, is pure with regard to maleness, and since this is simply absence of femaleness, his zygotic composition may be symbolically represented as *f f*.

And with regard to the other type of sexual inheritance, that represented in certain insects where the male seems to be the dominant hybrid, we have already seen (compare Fig. 1) that the male somatic cells carry one chromosome less than the female. In other words, femaleness is due to the presence of a chromosome absent in the male. In both types of sexual inheritance, therefore, femaleness may be said to be due to the presence of a factor absent in the male, as recently pointed out by Professor Castle, of Harvard University.

We may, therefore, regard the female as of more complex organisation than the male. And, in that sense, the female may be said to be physiologically the superior sex. We may thus further conceive that either the female is an extra-developed male, and has arisen by the addition of a new factor to maleness, or, perhaps more probably, that the male has arisen as a defective variation from the female.

METHODS AND RESULTS.*

BY "ARDENT MENDELIAN."

(1) The Present Position of the Mendelians and Biometricians.

THERE exists a Guild of very active and strenuous students which is known to science and to others as the Biometrical School. Its devotees and exponents are noted for the number and diversity of their pilgrimages and expositions, for they are prepared to apply mathematical methods to any problem, ranging from the infinitely little in the realms of Biology and Pathology to the infinitely great in the stellar domains of Astronomy.

It is true that when at last, after a weary journey over thorny paths, they reach the temple of their respective pilgrimages, the reception extended to them is not always gracious. For the gods of Anatomy, Biology, Medicine, Astronomy, and, we regret to say, even some of those of Mathematics, do not always anoint the pilgrims with unctuous and fragrant ointments, for too often that which is expected by them to be balm is rendered escharotic by the gods.

With regard to the organisation of this Guild we are led to infer, on the analogy of the maxim of "your corn in my bushel,"† that the Biometrical School is organised on the lines of a field army, and that its constitution comprises at least a supreme "field-marshal," a "staff-corps," and a "rank and file." We believe at the mobilisation of this scientific army martial law was proclaimed, and that it has not yet been reclaimed. We may

*Under this heading the various questions of discussion that may arise, or which have arisen, between the Mendelians and Biometricians will be considered from time to time.

†"British Medical Journal." March 13th, 1909. Letter by Professor Pearson.

further infer, therefore, that the discipline of the army is very severe, and perhaps this may throw some light upon the constant reappearance of the figure 0·5 in relation to the size of some of its artillery equipment. We believe further, from certain information which the dispatches of the "Field-marshal" reveal,* that the army has also its ambulance corps, consisting of "higher consultants" and "general practitioners." We have not the slightest doubt that such a militant organisation has urgent need for an ambulance branch, and that its duties must be incessant. And, when we review the many battles with the gods in which it has been engaged, and we recall their disastrous results, we find an explanation of the anomaly, that whereas other armies are content with "general practitioners," the biometrical one finds it necessary to retain "higher consultants."

In some respects it is a very fine army, and it is certainly an imposing one upon parade. It is led, officered, and manned by men of transcendent intellect, of whom any country may be proud. It is an army which in some domains may have achieved some eminent victories for truth; but in other domains we are afraid our judgement compels us to say it has but obscured the topography and geography of the country of its invasion by the smoke of battle, produced by the burning of its "correlation" gunpowder, and that it has failed to capture the Temple of Truth by the errors of its strategy and the ineffectiveness of some of its weapons of attack.

Opposed to the Biometrical army is the Mendelian. More recent in origin, less martial in organisation, but very vigorous, the Mendelian army has already turned the flanks and pierced the centre of the older one opposed to it. For signs of surrender on one wing, and of retreat, very skilfully covered, on the other, are visible in the biometrical ranks. The broken centre, encouraged by the boldness and coolness of its eminent Field-marshal —who like the kings of old personally fights on the battle-field—is making a rally on the high grounds to the rear. These hills are marked on the Mendelian

* "Biometrika," Vol. VI., p. 348.

map as very rugged and difficult of ascent, not to be rushed by brilliant cavalry charges, but not impregnable before the persistent, slow, and methodical onslaught of a courageous and patient infantry: they are named the hills of "Masked Segregation." On the biometrical map they are marked as impregnable, when once occupied and entrenched, and are named "Continuous or Fluctuating Variations," or, in their more recent maps, as "Intermediates."

The great battle of the future is that which will be fought along this rugged range of the "Intermediates." The task of the Mendelian army is to take it. And, already in the plains below its brigades are beginning to deploy, and are making those initial dispositions which indicate that the assault is being prepared. At the same time, far away on the enemy's flank, in the valleys of Copenhagen, a great turning movement is being developed, and the brigades of the "pure lines" are preparing for their march along the dip-slope of the range, in order to strike the Biometrical army in its rear at the moment when the main Mendelian army unfolds its frontal attack up the rugged face of the escarpment.

Meanwhile, along the crest of the range, the concentration and entrenchment of the shaken centre of the Biometrical army is apparent. On the right, its wing which defended the village wherein the long tradition that evolution was almost wholly a matter of continuous variation, and that segregation of discontinuous variations played but little part or none at all, has been hopelessly shattered. For everywhere that advancing and ardent left Mendelian wing has shown the evidence of such discontinuous variations and their segregation in the kingdoms of plants, animals, and Man. On its left, the Biometrical wing has been rolled back, and the position which defended the propositions that problems of inheritance can be solved by reference to one only of the two parents, that the characters of the offspring are determined by the summation in a regular series of ancestral increments, and that an advance in knowledge of heredity can be gained by an indiscriminate massing together of zygotic characteristics, has been carried by

assault. The right and left are in possession of the Mendelians, the centre has been reconnoitred and the attack is beginning. But it is here that the conflict must be necessarily prolonged, on account of the difficult nature of the country to be conquered. For it is here that the great challenge must be made and accepted, as to whether evolution is wholly a matter of that organic advance which would result from the blending in successive generations of minute, barely perceptible variations, existing simultaneously in large masses of individuals; or whether it is wholly a matter of more or less irregular advance, sometimes small, sometimes great, due to the spontaneous appearance in a single individual of a mutational character or "sport," and which in its inheritance, through succeeding generations, segregates cleanly and definitely, from its opposite or allelomorphic factor, already present in the race; or whether evolution is partly due to the one and partly to the other. The existence of selection and elimination, without which there could be no evolution, by either method, is defended by both armies.

The battle of the future which is to be fought between these two armies therefore turns upon the nature of "Intermediates." And it is along this range of biological hills that the Biometrical centre is concentrating. Do these intermediate stages between the two extremes of a character necessarily manifest the existence of gametic blending, or do they represent simply a series of segregable grades? That there do exist phenomena which at a first examination and in the absence of any extended experimental knowledge would justify us in the belief that they indicate the existence of blended characters is not denied. But the Mendelians assert that the appearances are false, that they need re-investigation by experimental methods, and that our present knowledge renders it easy for us to conceive of the existence of segregation without there being any obvious manifestation of its existence. The Biometricians impliedly maintain that the existence of intermediates between any two extremes of a character is inconsistent with the segregation of those two extremes, and also with the segregation of the intermediates themselves. And,

wherever they find the existence—or the alleged existence —of these intermediates, there they raise their standard and issue their challenge. They have done so, for instance, with regard to the colour of the Shirley poppy petals, of the colour and markings of mice, of the grades of single combs in fowls, and of the grades of skin colour in crossed races of Mankind. It will be our pleasant duty from time to time to comment upon their efforts in the pages of this Journal. And we proceed to do so at once by dealing with their recent publications on the two latter subjects, in the notes which follow.

(2) Skin Colour in Human Hybrids.

A Mendelian Reply, including a New Mendelian Hypothesis.

In a recent number* of "Biometrika," Professor Karl Pearson has a "Note on the Skin Colour of the Crosses between Negro and White." Those of us who are familiar with the published works of Professor Pearson will be impressed by a significant absence of that militant note which characterised his earlier attacks upon Mendelism. We no longer read the uncompromising assurance "that nothing corresponding to Mendel's principles appears in these characters for Man."† In its place we are glad to note a chastened tone, and we are informed, not that the evidence which he has adduced in this note is destructive of Mendelism, but simply that "In view of the opinions I have cited above, I think the suggestion that skin colour 'Mendelises' should not be vaguely made until some very definite evidence in its favour is forthcoming." And, so much has the uncompromising attitude been modified, that Professor Pearson further thinks it conceivable that such qualities as the negroid lip, the crimped hair, the characteristic *alæ nasi*, and the peculiar negro temper, would fit the Mendelian theory closer than skin colour.‡ And he thinks

* Part IV., Vol. VI., March, 1909.

† "Biometrika," Vol. II., pp. 214 and 215.

‡ *Ibid*, Part IV., Vol. VI., p. 352.

also that these characters "offer better material for a possible 'Mendelising' than skin colour." For it to be admitted that negro qualities may conceivably fit a Mendelian theory, and that possibly "Mendelising" may yet be found, is indeed a great admission from one who but recently proclaimed "that it is too early to assert that Mendelism holds for man or even for any plant or animal."* These sentences from Professor Pearson's note almost read as though the study of his own material had, in a prescient mood, warned him that the forthcoming of "favourable evidence" for Mendelism is but a matter of time, if, indeed, it is not already contained in his present note.

We will proceed to analyse this note with Mendelian eyes. The information in it has been obtained, Professor Pearson informs us, by communication "with a medical man who has spent his whole life in the West Indies and knows its people and their ways very intimately." The method adopted by Professor Pearson in obtaining this information was to put certain questions to his medical correspondent. Now, as every barrister knows, questions are of two kinds, those which lead and those which do not. In some instances, Professor Pearson's questions seem to us to be of the essential nature of leading questions of a subtle kind. We do not, of course, for one moment suggest that Professor Pearson desires to be unfair, or that the nature of the question has in the smallest degree influenced the answer. We accept the evidence quite unreservedly. But we do wish to protest against the imputation to Mendelians, direct or implied, of assumptions and statements which they have never uttered. For instance, in the second question to his medical correspondent, Professor Pearson frames his query thus: "(2) *Mulatto white* gives a quadroon. Is or is not the quadroon a blend? Theory says that the quadroon class should consist of half whites and half mulattos." And, again, question 3 is put in a similar way, thus: "(3) *Mulatto negro*. Is this a blend rather darker than a mulatto or not? Theory would say that 50 per cent. of the offspring were mulattoes and 50 per

* Discussion at Royal Society of Medicine. Heredity and Disease. London, 1909, p. 57.

cent. negroes in skin colour." And question 4 is, from the Mendelian standpoint, even more objectionably framed, for here we pass from an abstraction called a "theory" to concrete persons called "theorists." It reads thus: "(4) *Mulatto × mulatto.* Does or does not this cross usually give a mulatto in colour? The theorists say that 25 per cent. should be pure white skins, 25 per cent. pure black skins, and only 50 per cent. mulattoes." The unconscious prejudice which such a mode of questioning tends to create is manifested in the correspondent's fourth answer: "This statement of those whom you call the theorists is the most ridiculously incorrect of the lot." The imputed "theorists" are thus at once condemned as "ridiculous people." They are not even heard in their own defence. Fortunately, as we shall see presently, the real "theorists"—"the most ridiculous persons"—are not Mendelians but Biometricians in a hurry to frame too simple formulæ. Between Professor Pearson in "Biometrika" and popular writers in daily newspapers, the poor Mendelian, upon whose shoulders the burden of unwarranted assumptions has been placed, must be already cursed with a reputation in the West Indies that will ensure his due execution without trial when he makes his appearance in that country.

Now, who are the "theorists"? If by this term is implied the Mendelians, Professor Pearson, as a man of science, will know there is a canon extant in science, that is as the "law of the Medes and Persians," which requires the full reference to the authority who has been quoted. Will he be good enough to quote the actual words of any Mendelian who uttered such statements as those impliedly attributed to him in the questions above? We may say at once, for the information of our readers and for the future guidance of Biometricians, that no Mendelian has ever made any such predictions. In effect, what the Mendelians have said is this: (1) That we have not at present before us evidence which has been sufficiently analysed to justify a statement one way or the other. (2) That since in human eye-colour, in white hair tufts, and in some pathological or abnormal traits, Mendelian principles have been clearly demonstrated

to be operating in Man, there is no reason to believe that when the offspring of mixed races are adequately analysed, it will be found that Mendelian principles are not also operating among them, whether in respect to skin colour or other characters. (3) That we cannot formulate a prediction of the results which will follow a crossing of an European and a negro, and of the subsequent matings between the hybrids in their different degrees, until we have first ascertained the nature and the number of the gametic factors which determine skin colour in the negro and the European. This last consideration is the essential basis of all Mendelian predictions, and these factors have not yet been ascertained in regard to the transmission of skin colour in mixed races of Man. And, therefore, no Mendelian—whatever a Biometrician may be inclined to do—will be so "ridiculous" as to make predictions until he has first ascertained his essential facts. If it is necessary for the purpose of what we have been told is a "most exact and purely descriptive science of Biometry" to call in the aid of working hypotheses—just like any mere Biologist—then please let the Biometrician inscribe them on his own banner and not impute them to us.

We may pass now to consider the evidence of Professor Pearson's correspondent, and see whether it does not indicate some of those signs that we associate with segregation. Perhaps it will be best if we give in a summarised form the actual results of the matings, as far as they are described in Professor Pearson's note. We may then subsequently proceed to ask ourselves what results we should expect if the hereditary transmission of skin colour in mixed races is a process of blending and not one of segregation. Having arrived at some decision on that point, we can then compare our expectation with Professor Pearson's evidence, and note to what degree they coincide or diverge. If the comparison is indefinite and inconclusive, we may further proceed to consider certain possibilities which at once suggest themselves to the Mendelian, but which appear to have escaped the Biometrician. Having stated these possibilities we shall then consider

whether their influence is likely to mask the manifestation of segregation, and if so, to what degree. We may then compare these purely tentative Mendelian expectations* with the existing evidence, and consider whether they do not better fit the facts than those which logically flow from the hypothesis of "blending."

To come then to the facts as they are described in Professor Pearson's note. It appears that the offspring from a marriage of an European with a negress† is a "definite blend," producing a coloured type known as the mulatto. Two classes of mulattoes are known. There is a "brown mulatto," having his skin the "colour of mahogany," and a "yellow mulatto," whose skin colour is that of a "well-cleaned, nearly new brown boot." In the West Indies, the mulattoes are comprised of about 85 per cent. of the brown type and about 15 per cent. of the yellow type; this statement of proportional numbers is only an approximate one, and is based upon the impressions of general experience. The existence of two such apparently widely divergent coloured types of mulatto is an exceedingly interesting fact, and it has a Mendelian significance. It is a fact which is worth emphasizing now, but we defer its consideration until later (infra page 172).

We may next deal with the nature of the offspring which results from a marriage of mulatto with mulatto. As far as we can understand the answer of Professor Pearson's correspondent, it appears that when two brown mulattoes marry, the offspring is a brown mulatto like the parents. When two yellow mulattoes marry, the offspring are similarly like the parents and are yellow. The correspondent does not state this definitely. But comparing the answer with the question (which is the No. 4 alluded to above), we feel that is what it is intended we shall infer. But we are quite definitely told that "no pure black skins nor pure white skins come from

* We are led to advance those purely tentative speculations in order to show how different some Mendelian expectations may be from those which have been imputed to Mendelians by persons anxious to disprove Mendelism.

†Apparently the unions are nearly always a male European with a negress. The reciprocal union between a white woman and a negro is much rarer.

mulattoes married to mulattoes." We are informed that this "can be stated quite dogmatically." There is, however, one important qualification which we may notice. It is the statement that among the offspring of mulatto parents "there are now and then slight variations from the usual mulatto brown or mulatto yellow." Then, in another part of his correspondence, where the correspondent is dealing with the question of reversions to the negro type, he writes: "Of course, in families of the mixed breed you will often see a difference in *colour* [the italics are his] pure and simple; this is not at all *uncommon* [the italics are ours], and I would make a marked distinction between this phenomenon and that of a throwback to the negro." And Professor Pearson adds to it by remarking that: "I take it my correspondent is here referring to the continuous variability within the family." Now we will not further, at this point, comment upon this fact further than to emphasize the existence of variations, apparently not only in depth or intensity of tint, but also in colour, and that these variations are not at all uncommon. And, we will at once dissent from Professor Pearson's assumption that these variations are manifestations of continuous variability. There is no evidence for that assumption; and his correspondent's own emphasizing of the important fact, by italicising the word "colour," that there exist actual differences, not merely of tint, but of colour, among mulattoes, directly points to the conclusion that we are dealing with discontinuous variations and with segregation. In other words, with a Mendelian phenomenon.

We come next to the offspring resulting from a cross of mulatto with an European. It appears that the quadroon which thus results is "invariably lighter in colour than the brown mulatto, and in 90 per cent. of the cases is whiter than the yellow mulatto. Pure white skins do not occur in quadroons. This statement is dogmatic and true." We need make one comment only now. There appears to exist the same sort of variation among quadroons that there does among mulattoes, as is shown by the implication that about 10 per cent. are darker than the rest.

The offspring of the mulatto (which type is not stated, but we infer it is the common or brown one) and the negro is a "sambo," "having a deep mahogany brown skin colour." "The sambo type is very distinct, and there is no reversion either to the white or black races." There is appended to this description of the "sambo" a pedigree of a sambo family. Now this pedigree appears to us to be the most important statement of fact in Professor Pearson's note, and seems to be the key to the whole problem. Let us consider it. The father is a mulatto and the mother a negress. There are four daughters, one son, and a grandson. The colour of two of the daughters is described as "paler mahogany," another one as "dark mahogany," while the remaining daughter and the son are described as "very dark, but plainly not a negress or negro respectively." Now here we have apparently three degrees of colour in the one family from the same parents; there are (1) pale mahogany, (2) dark mahogany, (3) very dark mahogany. If this be not segregation, will Professor Pearson tell us what it is? He, himself, is apparently surprised at the remarkably wide range of colour manifested, for he remarks that: "In this case the range of colour is fairly wide, and it is open to those whom it pleases to divide this or any other family into two halves, containing the lighter and darker members respectively. The difficulty of such a classification is that the dark mahogany members are quite distinct from negroes and the paler mahogany from mulattoes." Now this difficulty is really not one at all, except to the biometrical exponent of the hypothesis of blending, and to him it is fatal. It is, however, well to remember that the question of distinctness between *some* negroes and *some* samboes and between these and *some* mulattoes has not yet been investigated by those familiar with the details of Mendelian phenomena.

Before we turn the first lever in the problem with the Mendelian key, let us see first how well or badly the biometrical key will fit it. This key is the hypothesis of blending. All the questions set by Professor Pearson to his correspondent strike that note; almost, indeed, pray for that consummation. What result then shall

we expect when "black" and "white" are crossed if gametic blending is the process at work? The result so far as colour is concerned may be anything. In the absence of actual information, and basing our expectation only upon analogy, we should equally expect the colour of the mulatto to be green, or blue, or violet, as that which it actually is. For in a combination of two bodies by blending, the colour of the separate factors may not count. When they blend, the properties which each possessed separately, may in the combination be profoundly altered, or even replaced by new qualities altogether. We have an instance of this in the action of silver nitrate upon living tissues. Both of these are colourless, but a person who has been drugged with this substance for a certain period will develope a livid-blue or lead-coloured skin. The question of skin colour is essentially a chemical one, and chemical combinations produce substances having characters which have no relation to those possessed by the constituent substances which enter into the combination before they are combined. Hence the fact that the negro's colour is black and the European's is white, by itself gives us no basis of expectation as to the colour of the offspring.

But there is one result which we should expect if these two qualities are permanently combined or blended in the sex-cells. Within narrow limits of variation, we should expect an uniform result. For instance, we should expect that all the germ-cells of the father and all those of the mother would in each case carry one condition only of skin-colour. That is, the father and the mother would in respect to this character each carry only one kind of sex-cell. And, consequently, since all their children will develope from fertilised germ-cells of the same kind, we shall expect that their skin colours will be the same. Such expectation is not in accord with the wide range of variation revealed by Professor Pearson's evidence. The evidence of variation, not only in tint, but in colour, appears to be fatal to the hypothesis of blending. In answer to this, Professor Pearson cannot urge that his general evidence does not relate to families but to a community. Such a plea only carries the question one stage farther back. For the blending

which occurs in a family is but a repetition in a small way of a much larger blending which antecedently has occurred in the community as a whole if the blending hypothesis has any meaning at all. Professor Pearson has himself endeavoured to convince us by his biometrical instruments that a man's ancestry ten generations back is merely a sample of his race or community.* If this biometrical abstraction is true, then Professor Pearson's general evidence as to the community of mulattoes, quadroons, octoroons, and samboes holds equally cogently for single families of such classes or *vice versa*. In a few alternative words, we may state the problem thus : On the hypothesis of blending we should expect not only the individuals of separate families to manifest considerable uniformity in skin colour, but under certain conditions we should not expect any large variation in skin colour in the community of mulattoes and the other hybrids as a whole. The chief of these conditions would be, of course, that the Europeans who originally married to the negroes in a given island should have been members of the same community. For even on the blending hypothesis, it is conceivable that the factors determining French skin colour may be different to those determining English or Danish, or Spanish or German colour. But even that condition does not carry us far. For the breeding together of the mulattoes within a circumscribed islandic area would soon produce an uniform result if blending be true. Professor Pearson does not tell us which particular island of the West Indies his evidence relates to. But we are probably not far wrong in assuming that so far as the nature of the population is concerned, it has remained approximately stationary for the past 250 years. That will give us something like ten to twelve generations of inter-breeding of mulattoes. This is as long a time as is required to give us an uniform race, on Professor Pearson's own calculation. And, since that racial uniformity has not resulted, it is, as far as it can be legitimately carried, adverse to the hypothesis of blending.

But apart from any considerations of this sort, there remains the clear and definite family of samboes which we have already described (supra page 169). Such large

* Grammar of Science. Second edition, p. 456.

variation as is shown among its individuals, all five having the same father and mother, can find no explanation on the hypothesis of blending.

Another fact which it is difficult, if not impossible, for this hypothesis to explain, is the existence of two colour classes of mulattoes, the " mahogany " and the " yellow." It is admitted at once, that this problem has yet to be investigated, and the factors producing the difference yet to be ascertained. We cannot be quite sure from Professor Pearson's note what the yellow mulatto is. Is he only produced when an European marries a negro, or does he occasionally come when a brown mulatto marries a brown mulatto, or when a yellow marries a brown one ? The first answer (supra page 167) of the correspondent leads us to infer the first of these alternatives, but his fourth answer (supra page 167) further leads us to suppose that the other alternatives are also possible. It is most desirable that we should definitely know what is the nature of the offspring when one parent is a brown and the other is a yellow mulatto. And it is also very much to be hoped that some information concerning the proportion of the yellow mulattoes to the brown ones, in the past, will some day be forthcoming. Is the " yellow " race a disappearing one or a stationary one ?

If it should happen to be the case that yellow mulattoes are sometimes the offspring of brown mulatto parents, then it is evidence of segregation, and is inconsistent with the conception of blending. Any such variation is necessarily incompatible with the existence of gametic blending. Unless the yellow mulattoes represent only the offspring of certain European marriages with negroes, or the marriages of yellow mulattoes *inter se*, their existence is fatal to the blending hypothesis. For when we consider the very large number of illegitimate births, which in the British portions of the Islands range from 53·9 to 79·2 per cent. of the general births, and that in the past it has been much higher, and we try to form some idea of the extent of the promiscuity which this represents, we find it difficult to believe that in 250 years the yellow and the brown mulattoes, if they have intermarried, have

not become completely merged, if the blending hypothesis expresses a truth, and Professor Pearson's calculations can be accepted. But it is all quite conceivable on the basis of gametic segregation. Indeed, such variation would be a manifestation of segregation.

If we come now to the results of crossing negroes with mulattoes, we at once acquire a singularly interesting fact. It is one which throws a significant light upon the difference in the methods employed by the Biometrician and the Mendelian. The former deals in masses, and in essence he seeks for the mean of the colour of the offspring and of the parents ; for the coefficient of correlation of the negro fathers *or* mulatto mothers, massed in classes, with the sambo offspring, also massed in classes ; and for the deviations of some or all the members of the negro, mulatto, and European ancestors from the type of their respective classes, and of the members of the sambo offspring from the type of their class. And, when all this has been ascertained, at the cost of very great labour, what reward is gained, but descriptive statements of the most general and widest kind, and which, while interesting in themselves, fail to throw any clear light upon the processes of inheritance ? If we are prepared to rest upon these statements, it is possible to believe that inheritance is merely a matter of blending, for we have placed the whole problem into the biometrical melting pot. But it so happens that in this case the influence of the Mendelians' past work has been felt, even in the biometrical camp, for on page 351 of Professor Pearson's note we recognise a typical Mendelian method, *i.e.*, a definite and single pedigree to examine. It is the one we have already described, and it indicates the nature of the sambo offspring derived from a male mulatto married to a negress. Now whereas, by the biometrical methods, we learn nothing which is inconsistent with the supposition of a blending process in inheritance, yet the moment we examine the solitary pedigree, and note the specific details—that is, adopt a Mendelian method—we begin to see some evidence of segregation. We have already commented upon this pedigree and pointed out that the existence of three colour groups of samboes, in the offspring

of the same parents, is quite inconsistent with the conception of blending, but is intelligibly explained on the conception of segregation. Thus while the biometrical method masks the truth and may even lead us to erroneous conclusions, the method of the Mendelian leads us to the clearer understanding of the nature of the problem, and renders apparent that which exists.

With regard to the offspring when both parents are mulattoes, there is nothing in the evidence provided by Professor Pearson that is inconsistent with the process of gametic blending. And it must be admitted there is at first sight, and for the present, very little that is indicative of segregation. But the evidence is far from complete, as Professor Pearson himself intimates, and on some points it is very indefinite. For instance, to which type of mulatto do the two parents belong? Since, according to the correspondent, the brown type constitutes about 85 per cent. of the mulattoes in the West Indies, we infer that the statements as to the offspring apply to that type alone. But it is necessary to know something more about the matter. As the "Mendel Journal" will be distributed to the West Indies, and it is possible that it may fall into the hands of persons who feel an interest in advancing the knowledge of the subject, perhaps we may take the opportunity of briefly indicating some of the observations it would be of advantage to make and record. The editor of this Journal will gladly publish at its expense, any authentic observations and photographs relating to the subject which may be sent to him. It is very desirable that some information as to the details of the offspring of separate families born of mulatto parents should be forthcoming. Particularly it is desired that the degree of difference in tint or in colour should be indicated. If one son is light mulatto, a daughter is dark mulatto, and another son medium mulatto, it should be stated, after careful comparison. The slightest difference in colour especially, as well as in tint or intensity, should be noticed. Another observation which is very much desired, is the nature of the offspring when the father is a yellow mulatto and the mother a brown one, or *vice versa*. In all cases, differences between the children of the same parents should be

looked for and carefully noted. Particularly should it be noted whether the colour of this offspring is a yellow-brown, intermediate between the two parents, or inclining more to one parent than another, or whether it is a new colour, unlike that of either parent. A further feature of some considerable interest is whether any sambo, known from his parentage to be a sambo, and not merely judged as such by his colour, ever resembles some mulattoes, or *vice versa*, whether some mulattoes ever resemble some samboes. There is some reason to believe that this may, now and then, be the case. In general, what is desired, is a very careful and very detailed observation of separate families of all kinds, and not of classes of hybrids. The fuller the pedigree and the farther back it goes, the more valuable it is. But even fragments of pedigrees are of value. All kinds of pedigrees are required, and not only those relating to mulattoes, but to all the various crosses between the pure forms and the hybrids, which are possible.

A review then of Professor Pearson's evidence shakes our time honoured belief in the existence of the process of blending in the hereditary transmission of the skin colour of the various hybrids resulting from European and negro crosses. The facts are not altogether in accord with what we should expect if gametic blending was operating in these cases. We may therefore next consider whether expectations based on segregation any better fit the facts.

Now there are two possibilities arising from segregation. If the blackness of the negro is an elemental matter, if it is simply blackness and nothing more, and if the colourlessness of the European is merely absence of negro colour, and if the colour factors, whatever they may be, are of the same genetic nature in Europeans and negroes, then the expectations will be those ascribed to the "theorists" by Professor Pearson. But there is no Mendelian who believes that this is probably the case. Not only does our knowledge of colour inheritance in animals and plants forbid us to believe that it is in Man of any such elementary nature, but anthropological knowledge of quite long standing would similarly prevent us from falling into such a pitfall. Almost every coloured

race of Man manifests more or less considerable range in its skin colour. It is markedly so in the case of the North American Indians, in which it varies from pale olive yellow to dark brownish yellow. And with the negro the variation is even more marked still, for in the Mozambique region alone, Froberville distinguished the presence of as many as thirty-one different shades ranging from dusky or yellow brown to sooty black. Similarly, among the Arabs, as is well known, considerable variation is apparent, and the existence of even jet-black races* have been recorded.

We are not justified in the absence of chemical evidence in believing that these various shades are simply expressions of greater or less dilution or concentration of one pigment colour. Analogy would rather lead us to believe that each shade is a definite entity, due possibly to different ferments or chromogens, or even to different pairs of these in association with each other. And when we bear in mind Froberville's observation mentioned above, and assuming that each of the thirty-one shades is a definite chemical entity, how are we to know, in the absence of genetic investigation and chemical analysis, how many of these shades are carried by any one negro of the darker hue of colour? If his colour is not an elementary matter of one simple pigment, but is due to a mixture of three, five, ten, fifteen, twenty, or thirty different pigments, obviously the problem is much more complex than the Biometrician imagines. Unto him the injunction might be uttered to remember that: "There is more in heaven and earth than is dreamt of in his philosophy," even though it is salted with a great desire to formulate simple and easy predictions for the Mendelian. And, similarly, if the European is equally complex in his colour constitution, and is carrying a series of ferments and one chromogen, or a series of chromogens and one ferment, of such a nature, that while he is "colourless" himself, he none the less possesses the power of influencing the colour of his offspring when he is

* The shegya Arabs, south of Dongola, on the White Nile. On the authority of Waddington.

mated with a coloured person, or if it is even more complex, clearly then, Nature will have denied those rash Biometricians, who imagined it was all so simple.

There is yet another feature which introduces a further complexity, and is indeed, the key to the whole problem. Let us try to make it as clear as possible. There is good evidence to show that colour in animals and plants is due to the interaction of two bodies, both colourless. There is reason to believe that one of these may be a ferment and the other a substance upon which the ferment acts. It is possible that the ferment determines the production of actual colour, and the other body—a chromogen—determines whether the colour shall be black or yellow, or some other hue. Colour cannot be produced unless these two substances are simultaneously present in the tissues of the individual, and the nature of the colour and its intensity will depend upon the nature of these bodies. An albino, for instance, may be carrying the chromogens, but no ferment or ferments. Consequently, in the absence of these latter, the skin will be colourless. Now the European skin is not of the nature of the albino skin, and it may possibly carry both chromogens and ferments, and the light coloured skin of the European may be due to the nature of these, or to the presence of colour-reducing or colour-inhibiting factors. In the negro we may also assume that both chromogens and ferments are present, and the nature of these is such that his skin is darkly coloured. Now if the chromogens and ferments of the European and the negro can be brought, by hereditary processes, into chemical contact with each other, they will modify the negro and European colour, and determine some other sort of colour, that of the mulatto.

Now the crucial point is this. If **C** stands for the presence of a colour producer (ferment), and **c** stands for its absence, and if $\mathbf{D}^1$ and $\mathbf{D}^2$ stand for the presence of two colour determiners (chromogens), and $\mathbf{d}^1$ and $\mathbf{d}^2$ stand for their absence in the negro, then **C** and **c**, $\mathbf{D}^1$ and $\mathbf{d}^1$ and $\mathbf{D}^2$ and $\mathbf{d}^2$ are three allelomorphic pairs, so that $\mathbf{D}^1$ and $\mathbf{d}^1$, and **C** and **c**, and $\mathbf{D}^2$ and $\mathbf{d}^2$, cannot occur as pairs respectively in the same gamete. But we have postulated that the European is also carrying

colour determiners and a colour producer. Let D^1 d^1, D^2 d^2 and C c stand for the presence and absence of these bodies respectively. Now if the pigmentary factors (determiners and producers) of the European are allelomorphic with those of the negro, so that C and **c**, or **D**^{1} and d^1 cannot occur in the same gamete, then the expectations attributed to the "theorists" by Professor Pearson are rational enough, however numerous these allelomorphic pairs may be. But if they are not, then that expectation is wholly irrational.

This consideration brings us to the question whether we are justified in believing that the pigmentary factors of the negro are *genetically* of the same nature as those of the European? Ought we to regard the one series as allelomorphic to the other? Let us take an extreme case. Should we expect the absence of blue pigment = **b** in a sweet pea to be allelomorphic to the presence of black = **B** in a mouse? Of course, we cannot make such a mating, but we desire to illustrate by an extreme case the point which seems to us to be one of the possible pivots round which the problem of human hybrids may turn. To take then the actual case, can we expect the absence of the colour producer = c of the European to be allelomorphic to its presence **C** of the negro? In other words, is **C** of the European and **C** of the negro genetically the same thing? And is c of the European genetically the same as, or is it different from, **c** of the negro? This last question compels us to consider what we mean by the "absence" of a thing. Must we regard **c** as a mere symbolical negation, or rather should it not be regarded as a negative quality associated with a positive base? That is, to take an illustration, the positive base may be regarded as an ultra-microscopic chloroplastid, and the negative quality as the absence of chlorophyll from the positive base or plastid. In a generalised way we may state it thus: the hereditary characters of organisms are carried in a physical base. When a character, let us say **C**, is present, it is carried by this base. When it is absent = **c**, then the base is present, but its associated character is not. The base, however, when without its character, behaves as an allelomorph to the base which is carrying

its appropriate character. In other words, the base with its character is allelomorphic to that without it. In mixed races, such as the mulatto, we may be justified in believing that **C** introduced by the negro, and *c* introduced by the European, may not be allelomorphic, and can therefore be carried in the same gamete. If we accept this assumption, then the apparent breeding true of the mulattoes receives an intelligible and consistent explanation on purely Mendelian principles. And what is true of **C** and *c*, will be so of $\mathbf{D}^1$ of the negro and of d^1 of the European.

Let us work out a hypothetical case. We will symbolise the presence and absence of a colour producer (= ferment ?) in a negro by **C** and **c** respectively. Let *C* and *c* stand for the presence and absence respectively of a colour producer in an European. Let $\mathbf{D}^1$, $\mathbf{D}^2$, $\mathbf{D}^3$ represent three grades of colour determiners in a negro, and $\mathbf{d}^1$, $\mathbf{d}^2$, $\mathbf{d}^3$ their respective absences. In the European, let D^1, D^2, D^3 similarly represent the presence of three grades of colour determiners, and d^1, d^2, and d^3 their absences respectively.

Let us take the constitution of a particular negro to be **C** $\mathbf{D}^1$ $\mathbf{d}^2$ $\mathbf{D}^3$, and that of a particular European to be *C c* D^1 d^1 d^2 D^3. When such a negro is married to such an European, the mulatto offspring will necessarily be constituted as follows : **C** *C c* $\mathbf{D}^1$ D^1 d^1 $\mathbf{d}^2$ d^2 $\mathbf{D}^3$ D^3. These mulattoes will then individually form the following kinds of gametes :—

(1)	**C**	*C*	$\mathbf{D}^1$	D^1	$\mathbf{d}^2$	d^2	$\mathbf{D}^3$	D^3
(2)	**C**	*C*	$\mathbf{D}^1$	d^1	$\mathbf{d}^2$	d^2	$\mathbf{D}^3$	D^3
(3)	**C**	*c*	$\mathbf{D}^1$	D^1	$\mathbf{d}^2$	d^2	$\mathbf{D}^3$	D^3
(4)	**C**	*c*	$\mathbf{D}^1$	d^1	$\mathbf{d}^2$	d^2	$\mathbf{D}^3$	D^3

Now, there are sixteen possible combinations between these four gametes, and neither of them will produce either a negro or an European. They will produce nothing but mulattoes of varying tints. If we analyse the composition of the mulattoes produced by these sixteen possible fertilisations, we shall find there are no less than eight kinds of mulattoes. One of these will be identical with the two parents, assuming them to be the same as

each other. Notwithstanding that there may thus exist eight kinds of mulattoes in the offspring of a single pair of parents, yet, owing to the fact that the differences in their composition may be but slight, they will all appear to be, within certain limits of variation, of one kind, namely, that which has been called the mulatto. That is, mulattoes may appear to breed true within these limits of variation, and yet in reality there is no breeding true, but gametic segregation.

There is, therefore, nothing at all in the evidence of Professor Pearson's mulattoes which is inconsistent with the operation of Mendelian principles of inheritance in the hereditary transmission of skin colour in human hybrids. Indeed, the evidence of some degree of variation in the mulattoes' colour is clearly not only consistent with Mendelian principles, but actually is precisely what these principles require. A glance at the constitution of the four kinds of gametes formed by the particular type of mulatto we are considering, shows that differences in the tint and colour of mulattoes are to be expected, and that these will be in some cases of a very minor kind, and in others greater. And this is precisely what the evidence adduced by Professor Pearson instructs us is the case.

While this hypothesis which we have very tentatively advanced gives a satisfactory interpretation of the absence of European and negro skin colours among the offspring of mulattoes, it apparently fails, in the light of our present limited evidence, as adduced by Professor Pearson's correspondent, to explain why the sambo, the quadroon, and the octoroon are all different in colour or tint from mulattoes. But if future precise investigation, directed specially to the elucidation of this point, shall show that there is a wider range in colour or tint variation among mulattoes than we at present believe, and that at one end of the scale some mulattoes resemble some samboes, and at the other resemble some quadroons, then this hypothesis will cover a wider field of fact.

Our own limited observations compel us to believe that the variation among all classes of European-negro hybrids, is wider than is commonly believed. We have several times seen in Wimbledon, a family of three brothers

whom we judged to be quadroons. There were remarkable differences of colour in the three. The eldest, probably fourteen years of age, was quite dark and of a brownish-black hue ; the youngest one, about ten years of age, was much paler, and the difference was sharp and at once obvious ; while the middle one, about thirteen years of age, was perceptibly a shade paler still, and his tint was almost that of a sallow European. The jaws and the hair in all three were quite European, as far as a cursory examination allowed one to judge. Their nurse, on the other hand, while having only a sallow European skin, had hair, eyes, and jaws fully negroid. But the point we desire to emphasize is, that in some cases at least, there is a considerable range of discontinuous variation in the hybrid members of the same fraternity. That is well shown in this case, and in that of Professor Pearson's sambo family. Upon this point more complete and more precise information is wanted.

But for the present even Professor Pearson's evidence points to the existence of segregation, and not to blending, as the hereditary process at work in the transmission of the skin colour of human hybrids. That is, Mendelian principles are in operation.

But there exists more evidence than that adduced by Professor Pearson. And it is of a different order. We have, in the remarks we have made, accepted Professor Pearson's evidence that mulattoes never beget individuals with European skins. The tentative hypothesis we have framed is based on that accepted evidence. But can we regard this evidence as complete or final ? It applies to the mulattoes of the West Indies, but does it necessarily apply to mulattoes elsewhere ? Professor Pearson himself does not regard " the views of his correspondent as conclusive." He says " they deserve great weight," and with that we agree, but we do not think this evidence can be accepted as final.

Our reasons for this attitude are based upon two facts. We will state them *seriatim*. On September the 24th of this year, we were passing a common boarding house in the south-east of London, when we observed a somewhat big, broad woman standing in the open doorway. She had unmistakably the crimped negro

hair in profusion, the negro nose and negro jaw, and thus far was a typical negress. But the skin of her face, neck, and the whole of her forearm, was almost that of an European. We have since examined her more minutely, and she would certainly pass, at a distance, as an European of the darker complexion. The skin of her forearm was almost quite white, but in contrast with an European it is possible to detect a trace of colour in her face and especially on the back of her neck. Now we feel little doubt of the negroid ancestry of this woman. One of her two parents may have been a mulatto, a quadroon, or an octoroon. And yet in this woman we have clearly a segregation of skin colour, still associated with typical negroid characters, which Professor Pearson's correspondent asserts "dogmatically" does not occur.

For our second fact we are indebted to Colonel H. de H. Haig, R.E., who spent many years in an island where mulattoes and other degrees of hybrids were abundant.* On page 24 of this Journal, under the heading of "The Mendelian Collection of Human Pedigrees," we have published a pedigree of the offspring derived from a marriage of a mulatto gentleman with an English lady. And in the family of seven children, two possess undoubted European skins, and are described as very beautiful European women. Here we have a very perfect and clean segregation of the European skin colour. And this case is a perfectly authentic one, inasmuch as the family is well known.

When we combine the observation of Colonel H. de H. Haig with the South-east London one, there can be no doubt at all that European skin colour does segregate from a coloured ancestry, and that it may be quite clean.

The divergency between the evidence produced by Professor Pearson and that produced in this Journal, raises further questions. It suggests that the problem is very complex. It is not impossible there may exist several races of negroes in regard to the genetic behaviour of their pigment factors. Or, more probably, there may exist a large number of negroes having different zygotic

* We are not, for obvious reasons, permitted to mention the island. It is not one of the West Indies.

pigmentary constitutions. Thus, a negro having a composition of **C c D^1 d^1 d^2 d^3**, when crossed with any European, would give a mulatto offspring, the members of which, married to an European, would produce Europeans as well as mulattoes. The divergency, therefore, between the two sets of evidence is probably apparent and not real. The mulattoes of the West Indies, it is conceivable, may have one zygotic composition, perhaps similar to that which we gave on page 179, while some or all of those of the other island have another, similar to that just given. In marriages with Europeans, the one kind will not be expected to beget Europeans among its offspring and the other will.

(3) Variation in the Single Combs of Fowls.

Some Mendelian Comments.

In a recent number of "Biometrika"* there appears a paper under the joint authorship of Dr. Raymond Pearl and Miss Maud Dewitt Pearl, entitled "Data on Variation in the Comb of the Domestic Fowl." The authors have propounded a number of questions, but the central thesis turns upon the one question as to whether single combs or their various grades breed true. And, if they do not, to what extent do they vary. As the authors point out, "nothing is more certain than that all single combs are not alike in respect to any feature whatsoever, even including their singleness." While we are not disposed to question the statements in this sentence as a whole, the last clause of it calls for comment. It implies that there are forms of combs, which are neither single, nor pea, nor rose, but a sort of blend of single with pea, or single with rose. The occasional existence of such apparently transitional forms, is well known to the Mendelian, and the explanation of them is given later in this article (infra p. 193).

* Vol. IV., Part IV.

As is generally recognised, there are many different kinds of single combs, and some of them are illustrated in the authors' paper. Some are high, others low; some are erect and others lop over to one side; some are prolonged backwards in the form of a spur beyond their attachment to the skull and others are not, but end, more or less abruptly, at the limit of their attachment; some single combs have many "points" and others few; in some the "points" are broadly triangular in form (Fig. 3), in others the triangle is longer and narrower (Fig. 4), and in others they are almost rectangular (Fig. 2).

The authors seek to know whether these different varieties of single comb may be hereditarily transmitted in accordance with Mendelian principles when individuals bearing them are bred together. They also desire to learn, "how far from the normal in any direction the single comb may be expected to depart in pure-bred birds?"

If these represent the objects which the authors had in view when they commenced their investigation, we feel forced to confess that we are a little sceptical as to the utility or validity of the methods employed. They are, of course, typically biometrical methods and are orthodox to the last letter of biometrical law. But, unfortunately, that does not make them adequate or valid instruments of biological research. As biological instruments they are now quite antiquated, for biologists have a more fruitful and modern method, that of the genetic analysis of individuals by breeding experiments. But if these objects are after-thoughts, inscribed when the investigation was completed, and perhaps traced out by the omnipresent sword-point of the biometrical "Field Marshal," it will explain the existing incongruity between the objects and the methods.

The apparently real object of the investigators, that with which they commenced their investigation, seems to be contained in the following statement: "In this paper we have endeavoured to give a clear and, as far as possible, quantitative description of the nature and amount of variation normally occurring in a homogeneous pure-bred strain of Barred Plymouth Rock hens in respect to the form and size of the comb." Now if this is the precise object of the authors, then the methods adopted

by them to achieve it demonstrates at once the wide and inseparable gulf between the methods of the Biometricians and those of the Mendelians. The object is to ascertain the nature and amount of variation of the single comb. Now to do this we must have perfectly homogeneous material in respect of the *character* which we are investigating. We must be sure that the combs we measure belong to one gametic entity ; they must be all members of a definite, single gametic type. Otherwise, we shall not be dealing with true variation at all, but merely with an eidoscopic display of aggregated types.

Now Dr. Raymond Pearl has investigated the problem with biometrical methods, and the appearance of his paper in "Biometrika" must be accepted as evidence that from the standpoint of the hierarchy of Biometricians it is accurate in method and sound in conclusion. We are not, therefore, commenting upon Dr. Pearl's work in any other sense than that we desire to examine the validity or scientific utility of the biometrical instruments which he has employed in his investigation. Could we assent to his methods we might commend his results ; we can admire his skill as a workman, while lamenting his tools. We shall, therefore, consider his paper as a ripened product of Biometry, and as one written by, shall we say, "an ardent" Biometrician.

The object then before us is a study of the range and nature of the variation in the single comb of a certain race of hens. Now it must be confessed that were we as Mendelians about to undertake this investigation the first thing we should endeavour to obtain would be a homogeneous race of combs and not of hens. It is in this matter that we think the Biometricians have invalidated at the outset the value of any conclusions they may reach. They have endeavoured to obtain what they call "a homogeneous pure-bred strain of Barred Plymouth Rock hens." Now is it possible to obtain such a thing as a homogeneous race of individuals ? In pre-Mendelian days—that is, previous to 1900—the term pure bred had quite a different significance to that now attached to it by Mendelians. And, in its older sense, it is still employed by fanciers. We know now that a bird

can be pure bred in the old sense, can be bred for successive generations from a particular race, and yet not be homogeneous; indeed, may be most markedly heterogeneous. Blue Andalusian fowls, for instance, may be bred for indefinite generations from blue Andalusians and yet not a single homogeneous blue Andalusian bird will be obtained by the process. The Andalusian race thus bred will, in fact, remain a most heterogeneous one, throwing both black and splashed white individuals in each generation. Such instances can be multiplied, but this particular one will suffice.

The mere fact then that the Barred Plymouth Rock hens which form the material of Dr. Pearl's biometrical investigation "had been carefully and closely selected in their breeding for more than 25 years" is, by itself, no guarantee of the homogeneity of the race, and still less is it any guarantee of the homogeneity of the comb characters. Neither is there any greater degree of guarantee introduced by the fact that "during the last nine years they have been 'line bred'—that is, no fresh blood has been imported." The statement therefore that: "It would be difficult to find anywhere material for the study of variation more homogeneous than that dealt with here," has really no scientific meaning at all. It has a fancier's meaning, but nothing more. There is no proof forthcoming of the homogeneity of this material in the scientific sense. On the other hand, the three plates containing 96 figures of very diverse single combs, which illustrate the authors' paper, is an eloquent testimony to the heterogeneity of the material. We think these plates constitute such clear evidence of this that there remains little doubt in our minds "that it would be difficult to find anywhere" more heterogeneous material.

Now what is it the Biometricians want us to understand by the term "a homogeneous pure-bred strain of hens"? What is a homogeneous race? We doubt whether any such thing exists. Are we asked to believe that this race of Barred Plymouth Rock hens is homogeneous in all the multifarious characters of each individual? Or that it is so only in respect of those characters

which have been selected by the fanciers ? If it is this latter alternative, then we imagine that the selection of plumage, of size, of habit, of form, and other characters, has played an equally important part, probably a more important one, than that of the selection of comb characters. And, if this is so, the Biometricians are not justified in believing that the single combs of this race—and it is with this character that they are alone dealing—is anything else than markedly heterogeneous. Even though a special selection of comb characters on the basis of old conceptions* and by old methods had been made, there would, even then, have been no reason to regard the material as homogeneous. But even this has not been done ; for Dr. Pearl himself informs us on page 428 : " The drawings and figures here presented show that in the *absence of special selection*† in regard to comb size, the character shows a range of variation all the way from the condition shown in large combed types in Leghorns to the very smallest of single combs." It is clear then, on the admission of Dr. Pearl himself, there has been no special selection exercised in regard to one of the characters of the comb, namely, size, the variation of which he set himself the problem to measure and describe. How, in the face of this admission, his material can be described as " so homogeneous that it would be difficult to find any material more so," appears to us to be an aberration of enthusiasm not perhaps difficult to comprehend. We are therefore compelled to dismiss as untenable the claim that this biometrically chosen material is homogeneous in any but the fanciers' sense of the term.

Let us pass to a further consideration of the problem. We will take one only of the many characters exhibited by the single comb, namely, its size. Now what is size ? A

* The old conceptions alluded to here are those which flow from the belief that the visible body characters are a true and trustworthy criterion of gametic constitution. We now know that in the majority of cases this is not so. The old methods alluded to are those which are based on this old belief, and which consist in selecting individuals because of their external or visible body characters. The Mendelian or newer biological method is to select them because of their gametic constitution, which is judged of by their genetic behaviour, manifested in the course of experiment.

† The italics are ours.

thing which has size possesses length, height, and width (= thickness); it has three dimensions. What evidence has Dr. Pearl that each one of these dimensions is not a biologically independent variable? In other words, the length, height and width of a single comb may be separate gametic entities, each segregable from the other. Or, on the other hand, they may of course be coupled together. Before we can be assured that this material is the "most homogeneous possible," we must not only consider the comb, but each one of its separate characters. Points like these have to be considered and the homogeneity of the material proved, not by massing heterogeneities together, but by genetic experiment, before we have any real basis upon which our judgement as to the homogeneity or otherwise of the material can be formed.

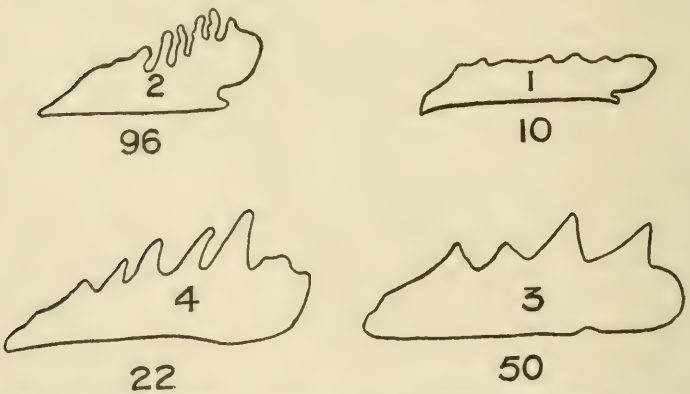
We must now consider another source of error in this biometrical treatment of the subject. It is the implication that the ordinary or older method of selection by inspection of the visible or somatic characters will enable us to fix a homogeneous race. This older method of selection is based upon an old conception. In pre-Mendelian days it was generally believed that the external or visible body characters were a true and trustworthy criterion of the gametic constitution of an individual. We now know that in the majority of cases which have been investigated this may not be so. We know, for instance, that two races of white sweet pea, which so long as each race is bred within itself will produce only white-flowered individuals, but yet when the two races are crossed purple-flowered individuals may result. It is clear that the visible and somatic white character of the flowers of these two races is not a reliable criterion of the hidden factors which each race carries. Selection of these two races because of their visible white character does not produce a race homogeneous for whiteness. Again, we know it is not possible to fix the jonquil character of canaries by selecting the best jonquil canaries to breed from. And we know also that we cannot fix the blue Andalusian character of the Andalusian hen by selecting the best blues and rejecting the black and splashed-white individuals. For many years blue Andalusians have been selected by such a mode of selection, namely, that based

on the inspection of visible somatic characters, but it has not resulted, and cannot result, in evolving a homogeneous race of blue Andalusians. On the contrary, the race maintained with steady persistency a heterogeneous gametic constitution. It was not the biometrical treatment of a few thousands of blue Andalusian hens which advanced our knowledge of the matter, but that precise, specific, individual gametic analysis that is the diagnostic feature of Mendelian treatment which finally enabled us within a couple of years to show that the blue Andalusian was a heterozygote, and could be made at any time by simply mating a black hen with a "splashed" cock or *vice versa*.

And the same considerations apply to the comb characters of hens. We cannot by such somatic selection, however long it may be continued, obtain a homogeneous single comb in respect to any one of its characters, except as the result of a happy and exceedingly rare accident. We cannot, for instance, know by mere inspection that an individual hen which bears a single comb prolonged backwards beyond its attachment to the head into a large spur is not forming gametes which carry another type or types of single comb. To endeavour to fix a race of hens which shall be homogeneous in one particular character by breeding from a bird chosen by mere external inspection is waste of time and money. And to endeavour to investigate the question of variation of the characters of single combs by indiscriminately massing together a hundred or a thousand, or even ten thousand hens which have been bred upon this principle of selection, and to obtain the arithmetic mean, the standard deviation and the coefficient of variation of their combs, appears to us to be of the nature of "love's labour lost." And that which is true in this respect of the combs of hens is true of all those multifarious questions of inheritance and evolution which relate to horticulture, to agriculture, and to Man. To believe that an inherently black body is white because it is externally painted that colour seems to us to be the highroad of delusion, the "straight, broad road" of biological science. And yet it is this road which the Biometricians are traversing, notwithstanding that nine years ago Professor Bateson called

their attention to its nature and to the goal of academic destruction towards which it led by facile stages.

Now let us examine Dr. Pearl's material with Mendelian eyes. Even a cursory examination of the ninety-six figures which illustrate his paper leads us to believe it possible that within this so-called "homogeneous and continuously variable material" there conceivably exists at least four types of single comb, so far as teeth characters alone are concerned. And, it is also conceivable that they represent gametic types. We will endeavour to enumerate and describe these groups. In so doing we would desire it to be understood that we are only indicating in a general way some of the considerations a Mendelian would formulate who contemplated undertaking an investigation into the variation and transmission of the



Possible gametic types of single comb. Selected from the 96 figures illustrating Dr. Raymond Pearl's paper in "Biometrika," Vol. VI.

Fig. 1 to 4 indicate the types of our four hypothetical groups.

Figs. 10, 22, 50, and 96 indicate the number of the figure in Dr. Pearl's paper, from which those here figured are copied.

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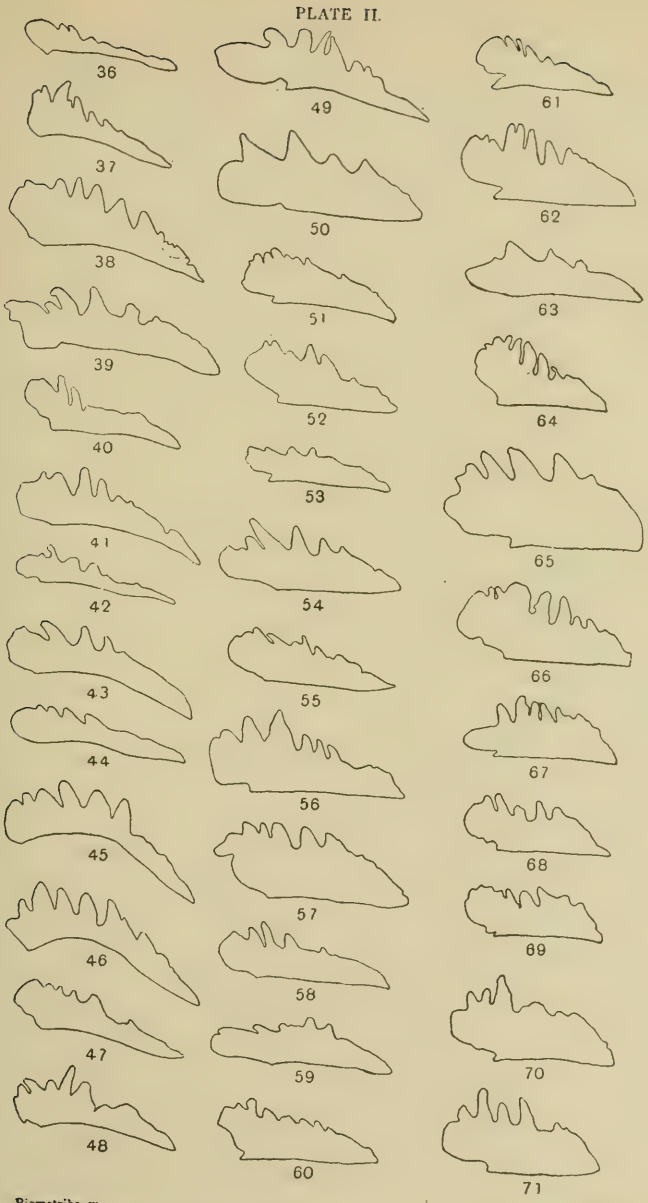
characters of single combs. The type comb of our Group 1 is illustrated in Fig. 10 of Dr. Pearl's paper, and in our Fig. 1. Around it the combs illustrated in his Figs. 1, 35, 36, 42, and 44 fluctuate in one direction or the other. This group is characterised



This Plate has been reproduced with the permission of the Editor and publishers of "Biometrika," from the three plates of 96 figures which accompany Dr. Pearl's paper. For explanation, see note appended to this article.

by the low form of comb; the serration is not pronounced, the teeth tend to run together, and the apices of the teeth are acute. In our Group 2 we find the type illustrated in Dr. Pearl's Figs. 88 and 96, and in our Fig. 2, and around them those shown in his Figs. 11, 64, 67, 76, 77, 78, 86, and 90 fluctuate in one direction or the other. This group is characterised by having the teeth somewhat in the general form of a parallelogram, they tend to be indented or spurred, and their apices are sub-acute. In our Group 3 the type is illustrated in Dr. Pearl's Figs. 50 and 92, and in our Fig. 3, while a lower form of comb of the same type is illustrated in his Figs. 24 and 63. The group is characterised by its well defined, triangular large teeth with acute apices. Our Group 4 is shown in its typical condition in Dr. Pearl's Figs. 13 and 22, and in our Fig. 4, with his Figs. 7, 71, and 73, and others fluctuating around them. This group is characterised by its well defined, somewhat long-narrow, triangular-shaped teeth with sub-acute or obtuse apices. A form of comb like that figured in Dr. Pearl's Figs. 69 and 85 and some others are probably to be regarded as heterozygous ones, and carrying some two or more of the above four groups.

We must once more intimate that we do not commit ourselves to the positive statement that these four groups are actual gametic entities; experimental breeding alone can decide that. But we do assert that Dr. Pearl's figures appear to us not to form a continuous series without some perceptible gaps, and that it is possible to believe at least four distinct gametic types exist within the class of single comb in the Barred Plymouth Rock hen. In the investigation of such problems the only method that can be of any avail is that of the Mendelian. We must, for instance, take the two most diverse types, such as our Group 1 and Group 3, and breed them together. In other words, we should endeavour to see whether such a comb as that shown in Dr. Pearl's Fig. 10 or 36 clearly segregates from such a type as that shown in his Fig. 50 or 92. When that has been adequately attempted, we shall then be able to claim a

real advance in knowledge. It is not a difficult experiment, and it is one which any breeder of fowls who is willing to do the work carefully and conscientiously could quite easily undertake.

We have felt bound, though reluctantly, to comment upon the conclusions and the methods of Biometricians. We have felt impelled to do so, because from the Mendelian School—after years of experimental breeding, during which more than 12,500 hens were bred and recorded by Professor Bateson and Mr. R. C. Punnett, and to which must be added the valuable experimental work of Mr. C. C. Hurst and of Professor Davenport, there has issued the conclusion that the characters of the combs of fowls are hereditarily transmitted in accordance with Mendelian principles; and that the experimental results can be adequately explained by supposing that the various characters are determined by the existence of definite gametic factors. The paper of Dr. Pearl which has appeared in "Biometrika" is published ostensibly with the purpose of casting doubt upon the Mendelian conclusions. It is perhaps unnecessary to further contrast the difference in the method or the extent of the work of the two schools. We need only compare the 96 combs gathered indiscriminately to the biometrical mill, and remember that the parentages of the individuals which bore them were unknown or ignored, and the relation of one generation to the next therefore was unconsidered, with that precise and definite gametic analysis of each of the many individuals chosen to constitute the breeding stock from the total of the 12,500 hens which were bred in the course of the investigation, and which is the essential attribute of Mendelian methods, to enable us to arrive at a conclusion as to which of the two Schools has its methods most based on a lasting foundation.

There is one further point that requires some comment here. On page 432 of their paper the authors lay great stress upon the existence of "intermediates" between typical pea combs and single combs. That fact has long been known to Mendelians, and was described in the first report to the Evolution Committee of the Royal Society, in 1901, page 94. It would have been more in consonance with

scientific impartiality had some reference to the later Mendelian work (Rep. Evol. Commt. iv. 1908, page 21) been made either by the authors or by the Editor of "Biometrika." This later Mendelian evidence suggests that these "intermediate" pea combs are not due to imperfect dominance or to continuous variation, but to a specific difference in the genetic nature of the single-comb base upon which the pea constitutent of it rests.

To ignore deductions derived from experimental evidence and to persist in maintaining conclusions based on observations that are eight years old, and which have been superseded by more recent and extended work, cannot we think help the advance of knowledge. The Biometricians still persist in believing that all single-combs are genetically alike, in spite of their own standard deviations and coefficients of variation, whereas the Mendelian has already good evidence to show that there exist several distinct gametic types. And the existence of these explains the intermediate pea-single type of comb.

NOTE.—After this article was in press, we sought the permission of the Publishers and the Editor of "Biometrika," to the reproduction in our pages, of four selected figures from Dr. Pearl's three plates. The Editor, while unable to see his way to grant permission for the reproduction of the four figures selected by us, was willing to allow us to reproduce any one of the three plates as a whole. At our further request the Editor of "Biometrika" very kindly permitted us to reproduce one of Dr. Pearl's plates together with the four figures. Hence the appearance of one whole plate, to which there is no reference in the text of our article. Doubtless, it is the desire of the Editor of "Biometrika," in fairness to Dr. Pearl's conclusions, that one series, at least, of his figures should be seen by our readers, side by side, with the four selected by us. We very gladly publish this plate. It is intended to show the existence of continuous variation. To us there appear to be perceptible gaps. But even though the blends or grades formed a perfect series, the fact still remains, that the only reliable way to test whether *gametic* blending exists or not, is to breed the extremes of a reputed blended character, and note the results.

MISCELLANEA.

Science and Democracy.

In an article in a daily paper of recent date there appeared a statement which is as remarkable from the standpoint of imagination as it is erroneous from the standpoint of fact. After passing certain comments upon a letter written by Sir Oliver Lodge, in which the writer manifested a misunderstanding of the nature and use of scientific hypotheses, he proceeded to pronounce the following obiter dictum :—

" It is not the case that the public is incredulous with regard to scientific assertions. The tendency is all the other way ; the plain man accepts too unhesitatingly what he is told by the professors. And there is danger when anything that claims to have the authority of science behind it is too readily accepted. There are physiologists and sociologists, for example, who would apply the laws which speculation has formulated in other fields to human life, though these laws may be nothing more than untested hypotheses or may have no application to man. Thus Mendel's famous law is valuable enough in dealing with plants, but in the case of man it appears to break down. If it applied to the human race, the progeny of every negro married to a white woman should be in definite proportions pure white, pure black, and mulattoes, and this, as is well known, has never occurred."

We are not now concerned with considering the credulity or incredulity of the public nor in asking whether it is true that the " plain man accepts too readily what the professors tell him." For we have first to learn that the plain man, or the legislator, or the politician, ever reads the scientific works of men of science. Neither are we concerned in holding a brief for the so-called sociologists. For, unfortunately, the great majority of those who pass as such, are neither men of science by training nor by instinct. They are perhaps not incorrectly described as dilettante or vicarious philanthropists desirous of spending somebody else's money in an attempt to secure the salvation of those whose salvation, there is too much reason to believe, is beyond both prayer and hope. With regard to the statement respecting the physiologists,

we can only express our surprise that a responsible paper should have framed such an accusation without producing a particle of evidence to substantiate it.

It is impossible to conceive of statements more calculated to inspire contempt and hatred of science in the popular mind, or to lead that mind astray, than those which appeared in this article. The "professors" of science are by implication, but perhaps not intentionally, denounced as charlatans, and the mature conclusions of academic physiologists, arrived at after laborious and prolonged investigations, are lightly dismissed as "untested hypotheses having no application to Man." In the earlier days of scientific history in this country, when she was fighting and making headway against the forces of superstition and bigotry, the Canon Wilberforces of the time were the open and avowed enemies of science. It can be said that they were honest men for they drew forth their sword in its unsheathed nakedness. But to-day, the locus of this enmity —for anything which misrepresents is a virtual enmity—appears to have extended and its form to have varied, for it is now no longer open and avowed, but assumes the invidious guise of putative criticism, uttered in an ostensibly friendly manner. It is akin to the kind of attitude that our lukewarm friends adopt towards us; it "damns with faint praise."

When we recall the fact, as the late Professor Huxley reminded us, that it was not an Autocracy, nor an Oligarchy, nor even a Theocracy, but a Democracy which condemned Socrates to death, we may well begin to wonder, when we read articles of this harmful nature—no doubt unintentionally so—addressed to the people, whether history in repeating itself has not brought us back to the days when rhetorical sophists swayed the civic multitude of Athens, and by the institution of such a form of government thereby inevitably wrought the national destruction. It compels us to remember that there are not wanting the signs that in the English life of to-day, mere orators, minor poets, and tanners of the type of Lycon, Meletus, and Anytus occupy the platforms in the market places, where sophistical, erroneous, and inflammatory utterances find an appropriate response. And, while this is so, the important conquests of science are either passed by unheeded and her methods ignored, or they are misrepresented and consciously or unconsciously held up to contumely, as in the case now before us.

We pass next to consider the particular paragraph in this article with which the *Mendel Journal* is more directly concerned. We are told by the writer "that if Mendel's law applied to the human race the progeny of every negro married to a white woman should be in definite proportions pure white, pure black, and mulattoes, and this, as is well known, has never occurred."

Now this is a remarkable statement, and it should not for one moment be supposed that any Mendelian ever uttered it. We should certainly be interested to hear the name of the authority and the chapter and verse of the book where such an expected progeny from such a mating is stated to be a Mendelian prediction.

The moment is perhaps opportune for a brief statement of the real facts of the subject. Now one of the most significant of these facts is that no Mendelian has ever made any prediction as to the nature of the expected offspring between mulattoes bred *inter se*, or between mulattoes and quadroons, or indeed between any of the crosses that are possible between European-negro hybrids. These alleged statements have been imputed to Mendelians either by those who imagine such predictions are orthodox, or by those who have got into a hopeless confusion as to the real nature of " intermediates " or grades of characters. It is unfortunate, as Mr. R. P. Gregory has pointed out in a reply to the article, that people do exist who are sometimes too ready to make unwarranted assumptions, which are then attributed to Mendelians. If the public desires to know Mendelism, it should of course, itself pass through the plains of Cirrha and climb the hills of Cithæron, and accept it from the Mendelian Apollo alone.

The Mendelian attitude in the matter of European-negro hybrids is the same as that in all other questions of genetics. It is, in effect, that we must first ascertain by breeding experiments the nature and number of the primary gametic factors which are concerned before we can make any prediction at all. In this particular case the Mendelian requires first to carefully ascertain the gametic factors which determine the blackness of the negro's skin and those which operate to produce the European colour of skin. We need not pursue the question in detail here, because we have already considered it (supra p. 163) in commenting upon Professor Karl Pearson's contribution to " Biometrika."

But we desire to pass one more comment upon the remarkable statement of the problem as it is given in the article we are considering. To a Mendelian the error of the statement is of course obvious at once. Assuming that Mendelian principles are operating in negro crosses with Europeans, and further assuming that the negro blackness is an elementary character, not resolvable into a series of factors, and that European " whiteness " is simply an elementary absence of negro colour, it is even then erroneous to say that we should expect " the progeny of every negro married to a white woman would be in definite proportions pure white, pure black, and mulattoes." For, as Mrs. Haig Thomas pointed out in a letter to the paper in which this article was published, the first generation cannot manifest the segregation of characters which occurs in the invisible gametes of that generation. It is not

until the second generation of individuals have been born that segregation can be seen.

We would like to add that if newspaper readers desire some knowledge of biological matters, and they have neither the time nor the desire to read scientific books, the best course available to them is either to read articles which are written by men of science and are signed by their authors, like that excellent series by Sir E. Ray Lankester in the Saturday morning's issue of the *Daily Telegraph*, or be content to leave science alone. For it is better for any nation to abide in ignorance than to be led in error.

How pernicious such errors as these may be is well exemplified by a subsequent letter which appeared in the newspaper in which the article was published. This letter adverted to that article and was signed by "Jamaican." He appears to have lived in Jamaica for fifty years, and he expresses "astonishment that such an idea as that a white child could be born of a black mother could be maintained." And well might "Jamaican" be astonished! But the seriousness of the matter from the aspect of science is that this error, having obtained a start, may never be completely overtaken. It has gone forth to the public that science has made a pronouncement, which a part of that public, from its experience, knows is erroneous. It has been promulgated that from a black mother science has said white, mulatto, and black children shall be begotten. And every person of the public who may be in a position to judge knows quite well that it is an erroneous statement. And the public which seldom or never discriminates, will now associate men of science with palpable errors, while the real transgressor may very possibly be regarded as a god who has destroyed false idols. Such neglect of scientific accuracy in any widely circulated journal, is a matter to be deplored.

It is only fair, however, to say that the newspaper in which this article appeared, inserted in three different issues a correction of the error which the article contained. And we have reason to believe that had not the pressure upon its space, owing to the political situation, precluded the possibility, a full statement of the Mendelian attitude towards the question would have been published in its columns.

Mendelism cannot long remain a wholly academic subject. Already the influence which it is to exert in horticulture, in agriculture, and in human affairs is manifest. Inevitably it must come into more or less public view on that account. And, we would like to utter an appeal for a fair treatment to be accorded it. It is becoming an increasingly complex subject,

and already needs a specialist's knowledge to accurately present it to any audience.

The time has passed when science was solely an affair of "cloistered halls" and men of science lived in "academic groves." Science has entered into human affairs, and is now an essential part of them. Men of science are called upon by circumstances to keep pace with the march of events. The old attitude of exclusion from participation in these wider events is no longer possible, if science is to be rightly understood and properly appreciated by Englishmen as a whole. The danger arising from that exclusion is manifested in the event which we are now discussing. To be forewarned should be to be forearmed.

The Nature of Scientific Hypotheses.

The discussion on "Speculation in Science," which took place in certain newspapers quite recently cannot fail to arouse widespread interest in the subject which has been described as the "scrap-heaps of science."

The full consideration of the matter at once raises the question of the nature and use of hypotheses in science, and, it is doubtless possible, by means of various analogies, to convey some concrete idea of their nature and of the part they play in the march of science to those who are not special students of scientific problems.

The various hypotheses which have been formulated from time to time, in order to attempt some sort of a description of the nature of the æther, have been mentioned in the course of the discussion alluded to above. And, the fact that many of these have been discarded, or, as it was there stated, "thrown on the scrap-heap," is impliedly held to be a rebuke to science. Now far from it being a matter for rebuke, this "scrapping" of defective hypotheses is in reality a great virtue. It is a sign of progress. Holding fast to dogmas when they are no longer tenable is not progression, but either retrogression or stagnation. The greatest victory that science has won for the intellectual freedom of mankind is this very right to relinquish exploded conceptions as soon as they are no longer tenable, and to substitute for them others more in accord with the knowledge of the day.

The discussion which we are now considering, may perhaps have tended to create the impression that this destruction of

exploded hypotheses is something which men of science regret and only reluctantly tolerate. But it is only just to remember that scientific men themselves look upon this relegation of untenable hypotheses to the scrap-heap as a quite normal event in the affairs of science. And, similarly, they regard the birth of new hypotheses even from a frail parentage, so long as it be legitimate, as a normal event. There is no danger in the existence of these hypotheses in the world of science, because no delusion is there associated with them; men of science understand them and appreciate their nature. It is only when they pass into the hands of men who are not scientific and who endeavour to use them to support their own prejudices or desires that they become dangerous, because they then become misapplied.

These so-called speculations in science are never intended to be infallible dogmas; no scientific man ever makes that pretence. They are simply working hypotheses or necessary implements of work, without the use of which it is impossible to advance. They are not Laws, but merely instruments of scientific research. When the work which they are intended to fashion and to mould is accomplished, these instruments of achievements—the working hypotheses—are cast aside. They have fulfilled their function, and science cannot be retarded by retaining implements which are no longer capable of useful work. In Biology we see one such derelict scientific tool in the now almost generally discarded hypothesis of the hereditary transmission of acquired characters. This hypothesis in its time served a very useful purpose. Had it never been called into use, biological science in certain of her problems would have stood sixty years behind the position she now occupies. In a tentative way and to a limited degree, Darwin utilised it as a working instrument, and his use of it ultimately led other biologists, and especially Weismann, to subject it to a searching inductive and deductive analysis, based partly upon experiment and partly upon verified observation. And as a consequence of such work—the outcome, let it be emphasized, of the critical use of a scientific hypothesis—the large majority of biologists to-day feel justified in relegating this hypothesis to the "scrap-heap." We need no longer encumber our path and retard our progress by futile discussions and unrewarded experiments on the "yea" or "nay" of the transmission of acquired characters. And when we look into the realm of Medicine—which is but applied Biology—we can conceive what an immense impetus to progress it must be for medical investigators and pioneers of knowledge, scouting out into the unknown regions, to know that this particular tool can no longer accomplish any useful work for

them. It is a great thing to know that there is one less of the useless tools for them to waste their efforts upon in a fruitless handling. The regions of the Unknown and even the frontiers of the Known, are strewn with such deceptive and futile tools, so full of promise, but so unavailing when they are tested, and so impossible to detect at sight from the tools of truer metal, that it is imperative all within our range must be tried before a real step in advance can be accomplished. Biology and Medicine to-day owe a forward advance of at least sixty years to the fact that Lamarck formulated this working hypothesis of the transmission of acquired characters, that Darwin tentatively used it, and that Weismann and others, using it yet more fully, tested it thoroughly and found it wanting. Had this hypothesis never been formulated, we should to-day on matters of the most profound importance—questions for instance that came before the Departmental Committee on Physical Deterioration and before the Royal Commission on the Care of the Feeble-Minded—have been sixty years to the rear of the vanguard of the knowledge of to-day. And this important advance, so intimately affecting our national worth, has alone been possible because a working hypothesis " has been put upon the scrap-heap."

We may speak of these exploded derelicts as scrap-heaps if we like. In doing so, we are really describing their real nature. There is a similarity between these discarded hypotheses of men of science and the pile of broken, rusted, and antiquated machinery of the manufacturer. And it is not only important but just to remember that the materials of a scrap-heap, historically or economically, represent a value almost infinitely greater than that of " old iron." The value of an engine consigned to the scrap-heap is not measured merely by its weight of metal at current prices, but in the history of mankind it will be valued by the nature and quantity of the work it accomplished in the days when it was the " latest thing " and was being worked at its highest expedient pressure. And, similarly, in the history of mankind we shall not value sailing ships by their worth to-day, and think that because they have been superseded by " Lusitanias " and other steam leviathans, they have been useless in the service of man, or valueless in determining the present polity of nations or the present distribution of human races. The value of sailing craft will not be historically measured by what they are worth to-day as a means of transit between Europe and America, but rather by the historical consequences that have arisen because they were the craft which conveyed Columbus and others to America and opened that vast region to Europeans. In old days the corn was cut solely with the scythe, but as an efficient instrument of the present it is now to a large

extent upon the scrap-heap; more efficient implements are taking its place. Yet assuredly, in the days when it was solely used, it was an incalculably valuable tool. So it is with the working hypotheses of men of science; like the scythes and the sailing ships, they are the efficient instruments for the work of the day. But men of science know that to-morrow and at successive periods they will be supplanted by a better instrument, until one is found which does its work ideally. And when in Science such a working hypothesis has been found—one which does its work ideally—it is called a "Theory," or if very ideal a "Natural Law."

These "Natural Laws" will probably never be surpassed or replaced. They differ from hypotheses in their permanence. In Science there are very few of them. The conditions of their existence are rigid, severe, implacable, and onerous. They are tested by facts unlimited and time illimitable. We may count the number of them upon the fingers of our hands. It will not be amiss, by way of illustration, to mention some of them. In the Physical World there is the "Law of Gravitation," the "Law of the Conservation of Energy and Matter," and in the Realm of Biology there is the "Law of Evolution," and the "Law of the Survival of the Fittest."

Now what is it that a man of science understands by the term "Law"? He does not understand by it the same thing that a legislator does. It is not a written code asserting what shall or shall not be, or stipulating what we may do or may not do. To him it is merely a statement of the consequences or results which arise as the outcome of natural processes at work. It is a generalised expression which accurately describes all the detailed and particular phenomena to which the "Law" applies. In other words, it is only a general way of describing the relationship of a large number of particular events or facts, which are more or less remotely related to each other. The "Law of Gravitation," for instance, enables us to describe in uniform terms the falling of an apple and of a feather; but it also explains the rising of a balloon, the floating of a ship, the swinging of a pendulum, the parabolic path of a bullet, the motion of meteors, and the orbits travelled by the moon, earth, and sun. The "Law of the Conservation of Energy" similarly enables us to describe in consistent and uniform language the production of electricity by the burning of coal; the formation of coal forests from the energy of the sun that shone millions of years ago; the production of heat and motion in the human body by the consumption of oxygen and other elements; the conversion of a sugary solution into an alcoholic one by the activities of the yeast plant; the growth of a man or a bean plant from a microscopic egg under the influence of the

energies liberated from the sun, and a great many other apparently diverse phenomena. The " Law " is but the description of the fact that when coal is burnt and has apparently disappeared, it is not really destroyed, but the energy of heat which has been liberated by its oxidation or burning is merely changed into some other form of energy, that of electricity or of motion, or of both. And similarly, the sun's warmth and light which falls upon the growing animal and plant is not lost, but is merely changed into that molecular unrest which constitutes life.

These two " Laws " will stand as long as the present order of the Universe endures, for they simply describe in generalised language the whole complex and ever-shifting phenomena to which they relate. And while they thus describe in accurate language the phenomena of Nature, they are not inconsistent with—that is, they are not contradicted by—a single fact. And yet, for fifty years or more, this " Law of the Conservation of Energy" has been tested by multitudinous, detailed, and varied experiments conducted in every civilised country throughout that period. And as for the " Law of Gravitation," who is there who doubts that its duration is coincident with that of the Universe itself ?

And similarly with " Evolution " and " the Survival of the Fittest." They too describe the phenomena of Nature in the domain of living organisms. They are consistent with all that is known of organic life, including Man, and fifty years of hostile criticism has left them strengthened, not weakened, impregnable, not shaken.

But we should never confound in our thoughts or in our conversations, the fleeting " hypotheses " which but represent the accomplishing tools, with the permanent " Laws," or, as they are better called, the formulated statement of the " Processes of Nature," which represent the finished product of the accumulated labours of, it may be, many generations of men of science.

REVIEWS.

Heredity.—*J. Arthur Thomson, M.A., Regius Professor of Natural History in the University of Aberdeen. The Progressive Science Series. John Murray, London. Price 9s. net. Pp.* 605.

Professor J. Arthur Thomson is the Ruskin of Biology. His writings partake alike of the mellowness of Autumn and the freshening vigour of the Spring. They are full of the hopeful joys that arise perennially from the vernal freshness, and they spread a golden hue over the pessimism that is eternally born of all experience. As one reads his book, one begins unconsciously to paint life as a resplendent far northern sunset, a scene of purple hills and placid waters, lying bathed in their entirety beneath a rose-red splendour that emanates from a silver sun. This psychological effect is due not to the matter of the statements but to the poetry of their utterance, for Professor Thomson is far too accurate to lead us by his statements into that Utopia which is born of dreamers but is demolished by Biology.

Most of us have to be content with the prosaic utterance that "the unfit among organisms are eliminated while the fit survive." But in Professor Thomson's book it is given to us in the language of imagery: "Rotten twigs are always falling off the tree of life. There is a continual irrecoverable precipitation of incapables, who thus cease to muddy the stream." In this way he paints at once, on the canvas of the mind, a picture of the process of Natural Selection. There can be none so uninstructed that they do not at once grasp the meaning of this destruction from the mental image thus called forth by analogy.

Now and then, Professor Thomson allows the golden sunset to sink, and in the colder, if clearer light, of advanced dawn, the ruggedness of life and its pitiless inexorableness, is allowed to break upon our vision. Equally well timed and written is his warning that: "Besides the advance of preventive medicine, the spreading enthusiasm for health, the awakening of a eugenic conscience, the suggestions as to 'marriage-licenses' and other forms of social selection, all making for the greater healthfulness of the

human breed, we have, of course, to remember that our race has not got beyond the scope of natural selection, much as we try to evade it." It is well that the large audience to which Professor Thomson's book will appeal should be reminded that the Stygian gulf of life cannot be crossed except by such who can pay Charon his ferry fee. It might not have been amiss, perhaps, if our author had pointed out that had we been brave enough in the past to have left Natural Selection unfettered, we need not to-day have burdened ourselves with "eugenic consciences," with amended "marriage licenses," with costly schemes of "preventive medicine," and a "spreading enthusiasm for health." To us, it appears preferable to see a community that does not care whether the *bacillus communis coli* is in its corporate intestines or not, to one composed of individuals always counting their pulses and swallowing lactic-acid tablets! Picture the two types: the one man spontaneously happy, vigorous, indifferent to germs, while the other wastes the hours of life standing by the brink of its waters shivering from fear and incapable of plunging! And yet it is this latter type that our "enthusiasm for health" and schemes of "preventive medicine" are rearing fast. Professor Thomson's warning is in season, and it is to be solemnly hoped that they who feel the glow of his golden sunsets will not forget that upon the other horizon, under cover of the rising darkness, a devastating storm is following the sun.

While, here and there, the author thus expresses a salutary warning, not infrequently his hopes appear to be too much of the ethereal order. When, for instance, he tries to persuade his readers that it is possible to escape, in some measure, the inexorable hereditary persistence of congenital defects, by the same mechanism that sometimes causes a desirable or favourable variation to be lost, he is approaching dangerously near to "building castles in the air." That such a thing may be possible is not of course disputed. But such negative variations are, as far as our present evidence instructs us, exceedingly rare, relatively to the number of times in which such characters persist. We have only to recall such a pedigree as that of Mr. Nettleship's stationary night blindness, where a defect persisted through ten generations for a period of 270 years, and still exists, to perceive that it is not along that line of Nature's processes we can hope for any amelioration of human suffering. There is danger in arousing such expectations and in building above earth. Careless readers —and how many there are—will believe that it is possible to stop the sun of heredity at noon-day, and to reverse its inevitable onward, westward march. Alas! it is harsh, perhaps, but better to emphasize the lesson that while there may be occasionally a tumultuous sun-spot on the sun, the relative motion of which may

be for a little while east, the sun itself goes on to the westward, with given pace to its unalterable goal. Again, Professor Thomson's allusion to Job's utterance, " Who can bring a clean thing out of an unclean ? " in relation to the fact that out of a generation of wheat plants susceptible to the " rust," some immune offspring may be obtained, is apt to lead careless or emotional readers to the contemplation of false beliefs and futile hopes. For Professor Thomson asks the question, " If it is possible among plants to get a pure thing out of an impure, it may be that for domestic animals and for man himself the purification of a tainted stock is not a chimera." It would have been more charitable to have warned his readers that before the clean can be obtained from the unclean, cleanness must first be put in. Professor Biffen, during his experiments with wheat, did not waft a magician's wand over an unclean " rusted " generation, and lo and behold, there came forth the clean ! Not in this way are such things accomplished. The immune—the symbolically cleanly—was first put in to the " rusted " generation, by the act of fertilisation. Of course, Professor Thomson does not say that such wand-waving feats are possible. But our experience of students and of general readers—especially of social reformers seeking a justification for their codes—leads us to believe it possible the passage may be thus translated. All Professor Thomson desires to state is that, given a stock which has become tainted, it may be possible to breed out its taint. But this of course supposes that in addition to the taint there is some goodness. If the stock is all taint, and we breed it away, nothing of the stock will be left. And, in social affairs, we may be apt at the end, after we have paid for the breeding away, to ask what we have obtained for our money !

We strongly recommend this work to all who are interested in the momentous and interesting biological questions with which it deals. Whether we are students, doctors, philanthropists, Eugenists, social reformers, Salvationists, or politicians desirous of the country's welfare, we should read it, mark it, and inwardly digest it, from cover to cover. It is not too much to say that he who does that will have gained a knowledge of biological and medical problems and facts wider in their range than it is possible to obtain in any other single book.

Many of the older conceptions, such as telegony and the transmission of acquired characters, and the phenomena of reversion, are considered and discussed. There is a chapter on the history of " Theories of Inheritance," another on " Heredity and Disease," one on " Common Modes of Inheritance," and a fourth on the " Physical Basis of Inheritance." The more modern aspect of the subject is described in a chapter on " Statistical

Study of Inheritance," and in one entitled the " Experimental Study of Inheritance." In this latter chapter an account of Mendel's discoveries, of the theoretical interpretations flowing from them, of the recent elaborations of Mendelism and the practical bearing of Mendelian results, are given and discussed. The problem of " Sex " has a chapter to itself, and so also has " Heredity and Development." This last chapter is essentially a statement and discussion of Weismann's theory of the Germ-Plasm. The final chapter deals with " Social Aspects of Biological Results."

There is appended a Bibliography, a Subject-Index to Bibliography, and an Index. There are forty-nine exceedingly good illustrations and diagrams, and many of them are coloured. They should help to render difficult conceptions clear and easy.

G.P.M.

Recent Progress in the Study of Variation, Heredity, and Evolution. *R. H. Lock, M.A. John Murray, London. 5s. net. Second Edition. Pp. 334.*

The object of Mr. Lock's cleverly written book is to give an account of the modern progress of knowledge relating to heredity, and to attempt to render the account intelligible to the general reader as well as to the more scientific public. In this endeavour Mr. Lock has admirably succeeded. His statement of some of the many questions with which he deals, are the clearest of many that have been written. Some problems are of such a nature that they may be described in a most laborious fashion and rendered more complex than they really are, by the treatment they receive. Professor Johannsen's theory of the " pure lines," is one of this nature. An unskilful pen would render a statement of it intolerably complex and hopelessly confusing. Mr. Lock's treatment of it recalls our nursery days when we lived in fairy-land, and all our difficulties vanished by the touch of the fairy-queen. We read his description and we grasp its meaning while we do so. And it is the same throughout the book. Indeed, the criticism we should feel most inclined to make, is that it reads too easily. Readers are apt to imagine that they know more than they really do, if they have gathered that knowledge with too little effort.

The book gives an account of the evidences of Evolution and of Natural Selection. And, although it is in large measure, a " Mendelian book," the author has written an interesting account

of some of the biometrical methods and conclusions, in relation to variation, evolution and inheritance. A chapter is devoted to the later aspects of the theory of Mutations and an account of de Vries' experiments with evening primroses and with ever-sporting varieties is given. There is also an interesting chapter on the work of the older hybridists, Kölreuter, Knight, Herbert, Gaertner, Naudin and Millardet. These are names that Darwin's "Origin of Species" made familiar to the reading public. The latter part of Mr. Lock's book deals with Mendelism, to which two chapters are devoted, and to the bearing of modern cytology upon Mendelian problems, which occupies a third chapter.

The last part of the book considers the application of the conclusions derived from biological investigations, to human society. And, in this part the author gives an account of the origin and meaning of the term "Eugenics." In the chapter under the title of this term, Mr. Lock has done good service in bringing before a wider audience Sir Francis Galton's work on "Human Twins." This work was originally published in the Journal of the Anthropological Institute in 1875, under the title of "The History of Twins, as a Criterion of the Relative Powers of Nature and Nurture." It has unfortunately been completely ignored in all discussions upon social problems where it should have formed their base. It is difficult to believe that the Utopian ideas, reforms, panaceas, social transformation scenes, or what other name they may be known by, could have been seriously preached in the columns of the medical press, in newspapers, in pamphlets, before Royal Commissions, and before Parliament, had Sir Francis Galton's "History of Twins" been known to the promulgators and its full significance grasped. Mr. Lock has rendered a service of the greatest value in thus calling attention to the existence of what we believe should be regarded as among some of the most important facts relating to human life. Sir Francis Galton himself propounded the question whether in view of the facts revealed by this history of twins, we can believe that nurture plays any part at all in the determination of the qualities and conduct of men. For it is but too true that "The steady and pitiless march of the hidden weaknesses in our constitutions, through illness to death, is painfully revealed by these histories of twins." And, as Mr. Lock points out, "From this evidence (the history of twins) it seems right to conclude that the hereditary nature of a man is more important than his training and circumstances in determining his adult mental and physical equipment. You may educate generation after generation, and yet the starting-point from which each individual has to begin his struggle upwards may remain the same."

Mr. Lock concludes this chapter and the book, by quoting a paragraph from a paper by Professor Karl Pearson, which we recommend all to read. For, whatever may be the differences between the Biometricians and the Mendelians, we rejoice in the fact, that all are agreed, it is impossible by nurture to evolve "golden conduct out of inherent leaden instincts," which are born of nature.

Mr. Lock's book is well illustrated and it contains reproductions from the portraits of Darwin, de Vries, Galton, Kölreuter and Mendel.

The Heredity of Acquired Characters in Plants.—*By the Rev. Professor George Henslow, M.A., F.L.S. John Murray, London. Price 6s. Pp.* 107.

Professor Henslow is one of the remaining few of a resolute rearguard. While the large majority of English biologists have followed Weismann, and have rejected the theory of the transmission of acquired characters either as untenable or unproven, there yet remains a small band of biologists who still maintain that this theory is worthy of acceptance. Among these, Professor Henslow expounds the theory from the botanical side, and Professor Gadow and Mr. J. T. Cunningham from the zoological side; and among botanists there are others who occupy a neutral territory.

"The object of the present book is to prove that Evolution—so far as plants are concerned—depends upon the inheritance of acquired characters." Professor Henslow describes a large number of examples, which he believes demonstrate "that new and changed structures in plants do arise as acquired characters, and that they can be hereditarily transmitted, and so become fixed as varietal or specific characters." He also contends that these examples prove the power of plants to adapt themselves during their development by responding to new conditions.

We are afraid not many biologists will accept Professor Henslow's conclusions, but before these are commented upon, it is only fair to consider the merits of the book apart from the particular attitude it assumes towards a once much debated question. It is a work which can be well recommended to every student of nature. It is full of most interesting facts relating to the many

adaptations and responses of plants to a changeful environment. The illustrative examples—which Professor Henslow of course regards as " illustrative proofs "—of direct adaptation are culled from a wide range of plants and plant organs. The structure of stems, spines, and aquatic leaves ; the habits and structure of climbing plants, of succulent plants, of Alpine and Arctic plants ; of epiphytes, parasites, and saprophytes ; the habits and form of swollen roots and tuberous organs ; the phenomena of degeneration, and the various considerations relating to the origin of monocotyledons, are all called upon to produce their evidence. There are many very excellent illustrations showing the differences produced in plants as responses to varying environments. The type is large and clear and the paper good.

Having thus recommended the book as a statement of an interesting series of facts, perhaps we may claim the privilege of criticising some of the deductions. We will first consider Professor Henslow's main deduction. He asks us to believe that species have arisen under " the joint action of the two great factors of evolution—variability and environment—without the aid of natural selection." He contends that plants directly respond to changes in the environment and that in time these changed structures or activities are hereditarily transmitted. He denies in a quite uncompromising manner that evolution depends at all upon the elimination of " unfit " organisms and the maintenance or persistence of the " fit." Granting for the moment, that plants may become permanently modified by responding to the direct action of the environment, and that this change may be hereditarily transmitted, has Professor Henslow ever inquired how this capacity to thus respond came into being ? The wide plasticity of plants is an obvious thing. But how did that plasticity arise and how has it been maintained in the vast majority of plants ? Are all plants equally plastic ? Plasticity is as much an inherent quality of a plant as its chlorophyll, its scent, and its form. And is not this plasticity—the capacity of responding to a changing environment—itself the product of evolution by Natural Selection, a matter of the survival of the fittest ? Let us imagine two hypothetical plants, both at the seedling stage, placed in a nearly sterile soil, and in a diffuse light. One of these plants can respond to this harsh environment, and adjusts its metabolism to the conditions. The other cannot, and its katabolism goes on at a pace which is consonant with a richer food supply than that available. Clearly the latter must perish, while the former, stunted in growth, survives. The stunted growth is not an effect of the direct action of the impoverished soil, it is merely the manifestation of the inherent capacity of the plant to respond to external conditions. And this particular

innate capacity, the evidence seems to us to show, is the product of evolution by the action of Natural Selection. Professor Henslow, of course, may reply that there does not exist any such unresponsive plant. But we think if he will appeal to known facts, he will find plenty of evidence demonstrating the existence of unresponsive plants.

Let us illustrate the argument by an appeal to these facts. The Sweet Woodruff is a British plant which lives in woods and shady places. It is not uncommon, as an undergrowth, in some beech forests. In such forests when thickly crowded, there is a deep gloom or shade that would be fatal to other green plants. But in this semi-darkness the Woodruff thrives quite well. That which is a harsh or fatal environment to other plants is to it a necessity of its existence. Now, when for commercial reasons, some of the beech trees are felled, and the forest is cleared in order to let in more light and air, what happens to the woodruff? If Professor Henslow's contention is true that it is a universal property of living protoplasm to respond to direct influences of a changed environment, then the woodruff should adapt itself to the new environment of a stronger light and more abundant air, and survive under the changed conditions. But it does not. It perishes. It is inherently unfit to survive these new conditions, and it suffers elimination in consequence. It is the same with the flax. It can live in the valleys, but if taken to the mountains, where the light is stronger, it perishes. The plants now growing in the desert of Kaits, in Ceylon, illustrate the same thing, but more cogently. They have been living under desert conditions for thousands of years, but yet they have acquired none of the characters that belong to desert plants, except a compactness of habit. They do not show sunken stomato, thick cuticle, and succulent parenchyma. And the only desert peculiarity which they possess, namely, their dwarf stature, as shown by Holtermann's observations, is lost when they are removed from the desert and planted in garden soil. The influence of the desert conditions has not changed the characters of these plants, for they do not possess that combination of desert characters which is the diagnostic quality of desert plants.

These phenomena and the whole range of responses which Professor Henslow adduces in his book, including M. Ph. Eberhart's experiments with woody stems, and Dr. Reid's statement concerning the Kauri pines of New Zealand, seem to us to be but different manifestations of the one thing, the inherent capacity which most plants have of responding to their changing environment. But all plants do not possess this capacity in the same degree or in the same way. It is a character which has been

evolved by Natural selection operating through the medium of spontaneous germinal variations. Those plants which have mutated in the direction of acquiring this plasticity can live under diverse conditions, because they can respond to them; those which have not thus acquired it, like the woodruff and the flax, can only live under certain limited environments, because they cannot respond to wider changes in their surroundings.

In one part of his book, Professor Henslow asks us for the evidence of elimination of the unfit. He asserts "that Nature does not produce seedling plnats possessing structural and functional defects." We have not far to go to find it. Any seed of *Pinus* will provide it. In this seed, at a certain stage of development, there are from sixteen to twenty embryos. At the germination of the seed only one comes forth as a seedling. The others have been eliminated; their metabolism fell behind the pace set by the victor, they were unfit, and they perished. Such illustrations can be multiplied and can be culled from the Animal Kingdom too.

Those who are interested in these problems should of course read the case for both sides. And, from the botanical standpoint of the transmission of acquired characters, the reader cannot do better than to carefully consider the facts described by Professor Henslow in his book.

The Causation of Sex.—*By E. Rumley Dawson, L.R.C.P., Lond., M.R.C.S., England. H. K. Lewis, London*; 6*s. net, pp. XII.* + 196.

The problem of Sex has ever been one of the most fascinating of biological problems. It has attracted to itself the attention of both eminent zoologists and botanists, in this and other countries, during the current and in past centuries. Around the relatively few facts known concerning it, many hypotheses—too often incorrectly called theories—have been woven. There are so many of these that at the beginning of the last century it was calculated more than five hundred of them had been framed at one time or another. In the face of such an army of hypotheses, one feels that Dr. Rumley Dawson's claim to have framed a new one is courageous indeed. It is certain that it departs from the great majority of the older hypotheses in that it excludes the influence of environmental factors in determining Sex, and falls back wholly upon the inherent structure of the right and left ovaries of the female.

The older hypotheses were based upon various assumptions, or upon limited evidence, and were born of a preconceived belief that the influence of the environment was practically all-powerful in the determination of Sex, as well as of other characters. The influence of warmth, of geographical elevation, of quality and quantity of food, of the relative age of parents, the freshness or staleness of the germ-cells at the time of fertilisation, the social rank and habits of the parents, the relative constitutional vigour or sexual ardentcy of the father and mother, the number of spermatozoa taking part in conception, and other factors, have all been pressed into service in the attempt to explain the determination of Sex. But in this, as in other biological problems, the day of the environmental dominancy is coming to its end. It is becoming more clearly recognised that an organism is predestined in its structure and its habits, for these latter are but the manifestations of the hidden factors contained in the fertilised egg-cell out of which the organism arises.

Dr. Rumley Dawson's theory is in harmony with modern investigation in so far as he ascribes the determination of Sex to the inherent nature and structure of the egg-cells. But he departs from our modern conceptions in that he affirms the impotence of the male sex-cell to determine Sex at all. He says: "It will come as a serious blow to the vanity of man to know that this question must be answered with a decided negative. Man, or the male, has nothing to do with the causation of the sex of the future child." He supports this conclusion by adducing evidence which shows that certain women have had *only* sons or daughters by different husbands, and that certain female animals have had offspring all of one sex by several different males. This evidence is certainly suggestive, but it is hardly comprehensive enough to render it conclusive. On the other hand, recent Mendelian experiments* tend to show that the male does influence the determination of sex. It may be possible that in some cases his influence is uniformly in one direction, and it may be the peculiar structure of the female egg which differentiates in the determination of Sex. But that does not justify us in asserting "that the male has nothing to do with the causation of Sex." To take a familiar analogy from domestic life. It may be true that the mere man has no influence at all in determining whether his wife's new dress shall be pink or green, or striped purple, green, and white. That may be decided wholly by the peculiar temper of the lady. But we imagine that the cheque book of the husband does influence the possibility of the lady having the dress at all. In the absence of a cheque,

* See Mr. C. C. Hurst's paper on "Mendelism and Sex," page 125 of this Journal.

there is no dress of any colour whatsoever; and, similarly, without a male there will be no offspring, either daughters or sons. This part of the theory asks us to accept too much. It asks us to believe that there is a fundamental divergence in the nature of male and female sex-cells. We do not think that the evidence as a whole lends any support to that assumption.

The central part of Dr. Rumley Dawson's theory is that the right ovary of the woman forms egg-cells which carry the male characters, while the left one forms egg-cells which carry the female characters. Consequently, an individual who developes from a right ovum must be a male, and one who developes from a left ovum will be a female. He then further postulates that ovulation occurs from each ovary on alternate months, so that if conception occurs on one month a boy may be born, while if it occurs the month after or the month before a girl will result. In support of his theory he adduces a great deal of very interesting facts, mainly of a clinical nature. And it can be said that he has produced a strong *prima facie* case in its support.

But there are certain general considerations which tell against it. The theory implies that there is a fundamental difference in the structure of the right and left ovaries. It implies a physiological and structural asymmetry of a very primary nature. It may be true that such an asymmetry can be demonstrated, because the right ovary is slightly larger than the left. But an asymmetry of mere size does not necessarily imply the same thing as one of diverse qualities. This latter asymmetry can only be due to some early asymmetrical cell division in the development of the individual. Are we justified in believing that such an asymmetry is a normal event in the structure of woman? The assumed existence of alternative ovulation is again another hypothesis which involves this asymmetric physiological activity. The evidence derived from lower mammalia does not to any great degree support it. Dr. Rumley Dawson, however, raises woman to a pedestal, and will not admit that her physiology is akin to that of lower animals. Of course, the objection to the theory on the ground of asymmetry is, it must be remembered, wholly an *a priori* one. And very marked asymmetrical arrangements are known, as quite normal affairs, in both Man and lower forms. In birds and dogfishes there is only one ovary, and in the former only one oviduct also; and, in Man, the functional speech centre in the brain is situated in the left side.

The reader must, however, read the book himself, and consider the evidence brought forward on behalf of the theory. The author also considers various objections which have been brought forward against his theory. He has collected a large amount of clinical information, and has brought it together in

the form of an interesting volume. Those who are anxious for the advent of a son or a daughter should certainly read the book, and see whether Dr. Dawson's prophecies always come true. For if his theory be true, it is possible to forecast the sex of the expected child, and, therefore, by instituting adequate measures to determine events in accordance with our wishes. If his theory will pass successfully through the crucible of experience and careful analysis, it is not too much to assert that it will be a theory of very great importance in relation to some affairs of human life.

The Problem of Evolution.—*Erich Wasmann, S.J. Authorised translation from the German. Kegan, Paul, Trench, Trübner, & Co., London. 6s. net. Pp. 266.*

This is a book dealing with scientific problems and written by a Jesuit Father. It is true that Father Wasmann claims also to be a zoologist, and an authority on the parasites of ants and termites. But it is clear that the theologian looms larger than the zoologist; the former stands in the background as far as possible, and this is of the nature of a mountain range, the foreground being the atmosphere, pervaded with a mist. The book has been written with a purpose, one which is often designated by the title "The reconciliation of theology and science." The book appears to be a publication of a series of addresses delivered in Berlin, before an audience containing a number of scientific men. At the completion of the series a discussion took place. The report of this discussion forms the second part of the book.

The attitude of the author towards the doctrine of Evolution is indicated very early in the book. When dealing with the various modifications of different parasites infesting different species of ants and termites he comes to the conclusion as the result of his personal studies that "The principle of the theory of evolution is the only one which supplies us with a natural explanation of these phenomena, and therefore we accept it. But to what extent are we to accept it? Just as far as its application is supported by actual proofs." Then the author proceeds to tell us just how far, in his opinion, the evidence does extend. We will indicate this extent in his own words: "In the case of the species of the same genus, the genera of the same family, and often for the families of the same order—even for orders of the same class, the probabilities are in support of evolution." "But the higher we ascend and the more closely we approach the great

chief types of the animal world, the scantier becomes the evidence." It is needless for us to indicate after this that Father Wasmann raises Man above nature and refuses to regard him as a product of evolution. He even denies that Zoology has any claim to express a conclusion as to the origin of Man. Psychology alone among the natural sciences has that right. And, beyond Psychology, Theology only has a right to pronounce a judgement " as to the way man came into being." We are informed Psychology teaches us that " the soul of man is not only essentially different from the soul of an animal, but is a simple spiritual being. Such a being can come into existence only by way of creation. Therefore the soul of man cannot owe its origin to evolution." We are living in the rationalism and philosophy of the 20th century, but this recalls to us the methods of the Scholastics of mediæval times.

Father Wasmann, who acknowledges the evolution of lower forms, has overlooked the fact that the conclusions relating to Man's evolution are based upon evidence identical in its nature with that which he admits is conclusive for these lower forms. If this evidence is true for one it is as true for the other. The introduction of an obvious prejudice, which is born of an unjustifiable pride, into the problem of Man's origin may be good Theology but it is bad Science.

The book will, of course, appeal to many; it will appeal to those who seek for consolation and to whom Truth is only acceptable when it is sweet. Those who desire solace in the contemplation that " Man is a fallen Angel," rather than in believing he is passing upwards to a higher state and to nobler realms of intelligence and morality, will find in this book the comfort which they seek. But, for our part, we feel a nobler pride in believing that the evolutionary Angel is to be the goal of our nobler future, rather than that it has been the lamented loss of a wicked past.

In all countries and in all times, the Church—under whatever denomination she may manifest herself—has high functions and noble purposes to fulfil. And surely she must recognise that it is better for her and for the populations she should guide, to consider more exclusively these functions, rather than to intrude prejudice or dogma into realms that do not concern her, unless it be to give her light and guidance.

We should like to discuss this book more fully were this a fitting occasion. But the reader will find an interesting discussion contributed to by distinguished members of Berlin Society in the second part of the book. The reader will also find that it is freely annotated by Father Wasmann.

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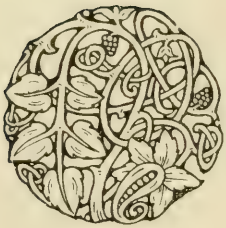

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THE MENDEL MONUMENT.

The
Mendel Journal
No. 2 February, 1911

GREGOR MENDEL.

AN ARTICLE
On the eve of the unveiling of the statue erected to his memory.

By T. v. WIESNER,
Member of the House of Peers, and the Academy of Science, Vienna.

(From the *Neue Freie Presse, Vienna.*)

Translation by the Countess Bertha de Scheler.

In front of the Königskloster (King's Monastery), a monument will be unveiled to morrow, the statue of Gregor Mendel, surrounded by a garden, which is intended to perpetuate the memory of a unique and deserving man, and to adorn the town. This beautiful statue was executed in Vienna by Theodor Charlemont, and more than one reader of these lines may have seen it in the Master's Studio, and been charmed with the pleasing plastic production. The artist represents Mendel in his best years, in the clerical dress of the "Altbrunner Augustine Monks." The position of the statue and the monastic attire of its subject will impress all those who look upon it with the idea that this must be a man who had done great work for his Institute, or, as a priest had particularly earned the love of the populace. For there are many who are ignorant of Mendel's epoch-making achievements, and his name as yet is far from having become

popular. But the erection of the imposing Mendel Monument was decided upon for other reasons.

It is true that Mendel, first as priest and finally (1868-1884) as Prelate of the Brünner Augustine foundation, did good work, and that as possessor of a vote (Virilstimme) in the Moravian Parliament, he was successfully active ; in his younger years also as teacher at the newly-founded State School (Staatsrealschule) he instructed and trained grateful pupils in Natural History ; but the greatest successes of his work, which he himself with his natural modesty and unselfishness hardly suspected, were of a very different kind. At his death, about a quarter of a century ago, many obituaries of him were published in Moravia, where he worked, and in Silesia, where his cradle stood. But they only spoke of the honoured Master, of the good priest, and of his work in the Moravian Parliament ; that in him a great naturalist was carried to the grave, nobody who knew him even suspected, and in the whole world all were ignorant of the fact. And now not only Brünn but the biologists and botanists of the civilised world do homage to him. This is shown by the history of the monument, which to-morrow will be unveiled. Even though the Königskloster contributed largely to the erection of the monument, and also the land of Moravia, the Ministry of Public Instruction, and several large-hearted inhabitants of Brünn, yet the contributions poured in from the whole world, not only from all parts of Europe, but also from America

and Japan, as soon as the intended scheme of honouring Mendel was made known.

Mendel's fate as an investigator shaped itself very strangely. We see in him a man who in his thoughts and works in important biological subjects was far in advance of his times, and a noble, modest, retiring personality, who did not in the least try to attain scientific celebrity. So his fate ordained that late, and long after his death, he should be recognised. The great question of heredity he studied by means of producing hybrids in plants. The question had long been raised, especially since Darwin. But the answer to this question had been on the one side crudely empirical, and on the other too largely speculative, so that in reality it was very unsatisfactory and of a very hypothetical character. Mendel took the only right way which could solve the question, for unlike his predecessors he did not experiment in a vague or restricted fashion and try to clear up the tangled events by hypothesis, but he systematically made the simplest trials, interpreting their results by the clearest logic. His observations were based on numbers, and he pervaded all with a mathematical precision. He was dominated by the same spirit as the great Julius Robert Meyer, the discoverer of the law of the Conservation of Energy, who once said: "A single number is of more value than a whole library of hypotheses."

Mendel had, as I have already said, a mathematical head, and I dwell on this again, so as to put Mendel's position as biologist in the right light. It

is becoming clearer that the methods of investigation for Biology are the same as those proved correct for other branches of Natural Science, namely, that great problems must be examined with mathematical accuracy, and be solved with mathematical clearness : the more the method of working recedes from mathematical accuracy, the more uncertain, doubtful, and phantastical the results will be. Mendel is a luminary of the first rank in his conception of biological problems, who, through discovering definite laws of Heredity, has given the greatest impulse to the study of the subject. Numberless men of research, zoologists and botanists, have been encouraged by Mendel's teaching. Many volumes could be filled with work based on his results. Herewith the realm of his achievements is not exhausted, rather there remains the best part to be told of his work. By the simplification of the method of attacking problems, by the mathematical spirit with which these are handled, he has become a guide for exact biological research in the subject of Inheritance, although the latter is still in its infancy.

The Mendel Monument is of white marble. Mendel stands upright, looking from a hedge of flowers into the distance. The plants which climb and are intertwined are somewhat conventional, but one recognises their species—they are peas and beans, which the artist chose fittingly, because the fundamental trials on which Mendel built up his laws of Heredity were especially made with these plants.

Even if I cannot now enter more deeply into his researches, yet I will indicate the essence of his observations as simply as I can. In the beds of the Monastery garden at Altbrünn he cultivated white-flowering and purple-flowering peas: he then pollinated them by artificial crossing, and the seed produced he used for a fresh culture. Now, what happened through this crossing? According to the then prevailing opinion, one expected in these hybrid peas pinkish (light purple) flowers whose colour would be a mixture or blend of the original colours of the two flowers. But the trial turned out quite differently. The flowers of the first generation were all purple, and in this generation the white-flowering character seems to have entirely disappeared. As a matter of fact, this character had not disappeared but remained latent in this generation. In the second one it reappeared, for both purple and white flowering peas were present. In respect to this, the following marvellous behaviour showed itself. In all following generations when the plants were self-pollinated, from white-flowering peas only white-flowering peas appeared, but the purple-flowering peas divided themselves through their seed into white and purple flowering peas and exactly in the proportion of one to three respectively.

This example shows the simplest case of crossing, as only one character, the colour of the flower, is different in the two parent forms. Naturally, however, in other crosses two or more characters were different—for example, colour of the flower, shape of

the seed, colour of the seed, &c. Mendel, in tracing these relations, has got as far as it is possible to get, as he solved the question quite generally, so that now every single case, complicated as it may be, is already decided by this general law, namely, can be calculated in advance.* Mendel's general law of Heredity is as mathematically and accurately precise as, for example, Newton's law of gravitation. One can calculate in advance with the help of this general law of Heredity and from the number of characters shown by the crossing, the number of possible hybrid forms, and the number of the descendants which will remain constant. The discovery of laws expressed with such mathematical exactness means the highest step that can be reached in the investigation of Nature. Mendel published the results of his long years of patient researches in 1865, and yet almost through a whole generation his works remained entirely ignored. One must ask oneself how it is possible that Mendel's discoveries, which to-day are declared classical and epoch-making, could remain without having received immediate recognition.

We must in large measure ascribe it to the modesty and retiring nature of Mendel. He published his essays on " Heredity " in the Journal of the Society of Natural Research of Brünn, which, with all deference to this Society, let it be said, was a poor

* It is to be supposed that what Professor T. v. Wiesner desired to express here, was not that the study of inheritance was finally settled, as the sentence might lead one to believe, but that simply in all cases where it can be shown that the Mendelian law is operating, the results can be certainly predicted.—EDITOR.

choice. It is said, had he published his works at one of the big academies, his law would have been recognised quickly. This explanation is, however, not sufficient. We know to-day something about the correspondence of Mendel, and that he communicated to the great botanist and biologist Karl von Nägeli all the details of his discoveries. But Nägeli, who was through his acuteness rightly much esteemed as a naturalist, not only did not recognise the value and far-reaching importance of Mendel's law, but he even apparently ignored it. Science, however, had to progress, and it was not until some fragments of Mendel's treasures of knowledge had been discovered by others that attention was attracted to the great Master himself. Then enquiries into past writings on the matter, instituted to see if some predecessor had not previously got hold of the thread of these phenomena, as yet but fragmentarily known, resulted after long research in the discovery of Mendel's work. It was soon apparent how deep and embracing, and far in advance of his time, or even of the generation which succeeded him, were his studies on Heredity.

As the result of this discovery a wholly unknown man became suddenly a famous celebrity. Mendel's law was enunciated and discussed everywhere, especially in England, where principally his experiments brought forth fruit and were most fully appreciated. Few serious refutations met the law. On the contrary, a universal enthusiasm lifted the man, who was in the deepest sense modest, to a pinnacle of fame which only few attain.

Not long after the publication of Mendel's experiments in the modestly planned and unobtrusively executed essays which explained the law of Heredity, and which slept so long among the journals of a society little known in the world, so that in scientific circles absolutely no notice was taken of them, they were accepted in the celebrated collection which is edited by Östwald under the name of "Classical Authors on the exact Natural Sciences."

Here Mendel's name shines beside those of the greatest scientists who have worked in the field of Natural Science: beside Malpighi, Knight, Brücke, Theodor de Saussure, and Pasteur. Now the capital of the land of Moravia can rejoice in possessing within its walls the statue of a man who laboured here many years, and created a work which has brought appreciation and even fame to Austrian Natural History Science Research, as few others have done, and whom we can with every right place beside such men as Rokitansky, Brücke, Endlicher, and Franz Unger.

To conclude, I would mention that Mendel's law has not alone a theoretical interest. In our time, in which all the technical and practical sciences, aided by Natural Science, progress with giant strides, theory is quickly transformed into practical application. See how Bacteriology, one of the most modern sciences, seized hold of Medicine, Hygiene, and the technique of fermentation, and how quickly the discoveries of Uertz led to wireless telegraphy, and how rapidly the Röntgen rays became useful to

the science of healing. Like these, the laws of Heredity determined by the hybridisation of plants find their practical application in the cultivation of plants and breeding of animals. In the place of empiricism a rational breeding of plants and animals can now be based on science, thanks to Mendel's discoveries, in all those cases at present in which the crossing of races is desired.

Brünn, 1st Oct., 1910.

The capital of Moravia is already decorated to celebrate the memory of Gregor Mendel, and to greet the illustrious guests who will arrive from Austria and foreign countries. The following scientists are expected: Professor Bateson, Cambridge; Dr. Lotsy, Haarlem; Professor A. von Tschermak, Vienna; Professor Cieslar, Vienna; Professor Baur, Berlin; Professor Schindler, Brünn; Professor Punnett, Cambridge; Professors Grobben, Hatschek, and von Wettstein, Vienna; Professor Kunsker, Breslau; Professor Fruwirth, Vienna; Professor Molisch and Professor Pinter, Vienna; Professor Dr. Cuboni, Rome; Professor Dr. Mikocsh, Brünn; Professor Hönig, Brünn; Philippe de Vilmorin, Paris; Professor R. Muller, Tetschar, Liebwert; Professors Walleschek, Drs. Porsch and Brunnthaler, Vienna, and many others. The head of the local committee, Dr. Stefan, Baron v. Haupt-Buckenrode, Professors Tschermak and Iltis, have prepared everything which could make the Mendel celebration successful and

international. The statue is on the Altbrünner Monastery Place, which after to-morrow will be called Gregor Mendel Place. The figure looks towards the Augustiner Foundation, where he lived and worked for several years. The background is enclosed as a garden. The well-known sculptor, Theodor Charlemont, received, after a competition, the order to execute the monument, in 1907. On a simple base of granite is set the pedestal, which like the statue is made of Laasar marble. Gregor Mendel is standing in clerical dress, over life size, the right foot stands rather forward, the fine head slightly inclined to the right ; the head is uncovered. With both hands the scientist touches common beans and peas, which creep up a little wall to the side. The favourite subjects of Gregor Mendel are conventionalised and are raised to a clearly visible part of the whole monument. Flowers and pods fall over the wall. The artist only had a photograph of the scientist at his disposal, notwithstanding which the work is full of life ; upon the face there is an expression of amiability and kind-heartedness. On the front side of the base is a shield with the inscription :—

" To the Natural Scientist and Investigator."

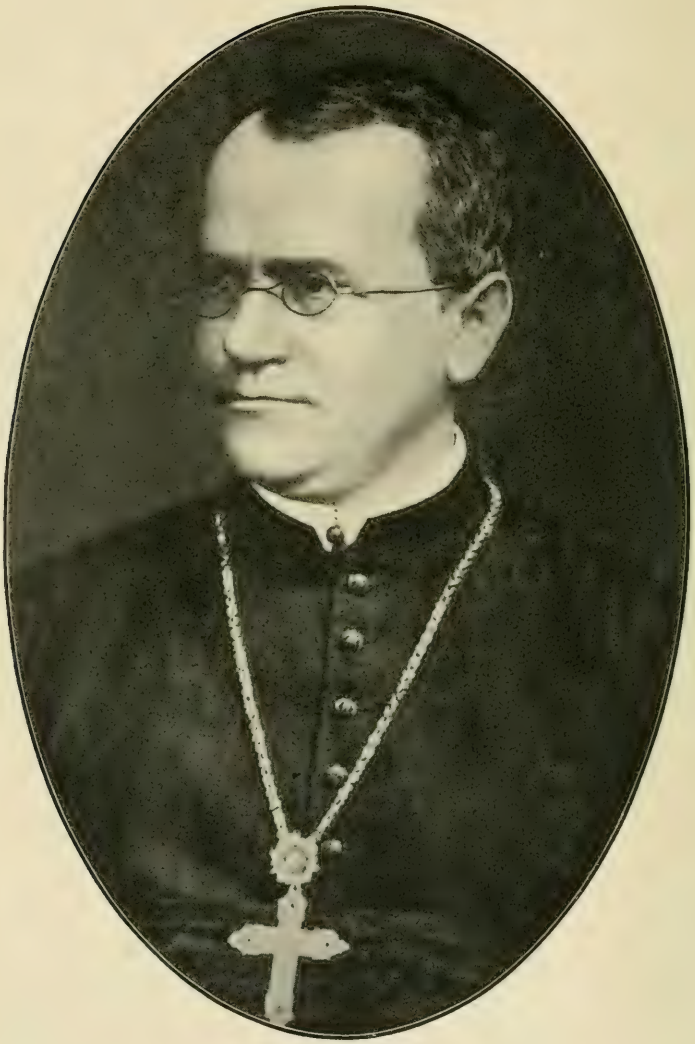
P. Gregor Mendel.

To the left of the shield kneels a nude boy, in slight relief ; to the right, a girl ; they clasp each other's right hands at the feet of Gregor Mendel. This decoration of the base is intended to show delicately the influence of Mendel's theories on human life

also. Herr Charlemont has achieved in this work, which was especially difficult on account of the symbolism required, a noteworthy work of art, and warm thanks are due to him from all men of science.

Greeting of the guests in the Brünner "German House," 1st October, 1910.

This evening the reception of the guests took place in the German House. Dr. v. Haupt-Buckenrode, Professor v. Tschermak, and Professor Iltis received the numerous ladies and gentlemen. From the birthplace of Mendel, Heintzendorf, appeared the only male representative of the family left, Mayor Mendel; also several nephews and nieces of the celebrated man came.



ABBOT GREGOR MENDEL.

PAPERS READ TO THE MENDEL SOCIETY.

A SKETCH OF MENDEL'S LIFE AND WORK.

An Address delivered to the Mendel Society on June 6th, 1910.

By D. J. SCOURFIELD.

1. Introduction.

Almost exactly ten years ago the well-known experimental botanists de Vries, Correns and Tschermak independently made a most important discovery. It was not the discovery of a new scientific fact, however, but the unearthing of a little paper by one Gregor Mendel, which had been published as far back as the year 1866. We can imagine their astonishment as they read that old paper to find that it actually contained the clue, set out with almost mathematical precision, to many of the puzzles they themselves had been struggling with in the course of their work. They lost no time in making known their discovery, and thus was inaugurated what, from the point of view of the study of heredity, may justly be termed the Mendelian era.

At the present day, as we all know by experience, the name of Mendel and such words and phrases as "Mendelism," "Mendel's Law," "Mendelian principles," "Mendelian ratios," "Mendelian facts," are continually

forcing themselves upon our notice, sometimes in the most unlikely places. And yet, in spite of all that has been said and done around the name of Mendel during the past decade, it is doubtful whether very many even of those who take a real interest in biological matters have a clear idea of who Mendel was and what he did. It is in the hope that another brief account of his life and work may be useful to some who have not hitherto had an opportunity of going into the subject that this paper has been written.*

2. A Short Account of Mendel's Life.

Mendel was born at the village of Heinzendorf, near Odrau, in Austrian Silesia, on the 22nd July, 1822. His parents and grandparents were all natives of the same place, and it is known that the Mendel family had been established there from at least the seventeenth century. Young Gregor, or rather Johann, as he was then called, Gregor being only an adopted and not his baptismal name, at first attended the local school at Heinzendorf, but at eleven years of age he was sent to school at Leipnik, and subsequently to the gymnasia at Troppau and Olmütz. It is a sure sign of the exceptional ability shown by Mendel in his early student days that he should have been enabled by his parents to continue his studies so long, for his family was by no means

* Those who wish for further information about Mendel and his work, together with the principal results of recent research on Mendelian lines, should consult Bateson's "Mendel's Principles of Heredity" (*Cambridge University Press*, 1909), a work to which the author of the present sketch desires to express his great indebtedness.

wealthy, his father being but a small peasant proprietor.

In 1843, Mendel, being then twenty-one, obtained admission to the Augustine monastery, known as the Königskloster, at Brünn, the capital of Moravia. For the next eight years he was largely engaged in taking part in the educational work which in those days formed an important part of the functions of the "Kloster." During this period he was ordained a priest (1847), and he also commenced to make experiments on plants in the garden of the monastery, a line of work which was to prove so fruitful a few years later.

From 1851 to 1853 Mendel went through a further course of study in mathematics, physics, and the natural sciences at the Vienna University, being aided in this case by the Königskloster, another proof, if one were needed, that he was a man of marked ability. On his return to Brünn he again took up his educational duties, acting as a teacher in the "Realschule," a sphere of work in which he is said to have been very successful. At the same time he devoted himself with extraordinary strenuousness to various lines of scientific investigation, especially to experimental work on the hybridisation of plants. It was during this period, 1854-1868, that all his most important scientific work was done.

In 1868 Mendel became Abbot or Prälat of the Königskloster, and, owing to the increased responsibilities of his new position, and especially to the

worries of a protracted resistance to what he considered an unjust law imposing special taxes upon the property of religious houses, he seems to have found but little time for further scientific investigations. He died on the 6th January, 1884, at the age of sixty-one.

3. Mendel's Scientific Work.

Mendel's scientific interests were very varied, but may be grouped mainly under the two heads of experimental Botany and Meteorology. As regards the latter he made systematic observations for many years on the meteorological conditions of Brünn, which were communicated to and published by the Naturforschender Verein (Natural History Society) in Brünn in their "Verhandlungen" from the year 1863 onwards. He also published in the same journal a special paper in connection with a cyclone which swept over the district in October, 1870. It is further known that he made observations on sun-spots, especially from the point of view of their possible connection with weather conditions, and that he also gave some attention to the systematic measurement of underground water.

It was, however, to the experimental study of the effects of hybridisation in plants that his energies were mainly directed. He seems to have been attracted to this subject by observing the great regularity in the appearance of identical hybrid forms whenever crosses were made between the same species, and he must have conceived the idea

of elucidating the constitution of the hybrids by observations on their progeny at a very early date. It is quite clear at any rate that when he commenced his great series of experiments with peas (1856 or 1857) he must have clearly recognised the importance of extended investigation of the progeny of the hybrids. This series of experiments with peas undoubtedly constitutes Mendel's chief claim to fame. It extended over a period of no less than eight years and involved careful records of more than 10,000 individual plants. As Mendel himself says, it required some courage to undertake a labour of such far-reaching extent. Nevertheless, he not only brought this particular piece of work to a successful termination, but he was, as we learn from incidental references in his most important paper, "Versuche über Pflanzenhybriden" (read 1865, published 1866*) and from his letters to Nägeli, at the same time experimenting in a similar way with many other kinds of plants—*Phaseolus*, *Verbascum*, *Campanula*, *Lathyrus*, *Dianthus*, &c.

More remarkable still, and this is a fact which is not often associated with Mendel, who is usually regarded as a botanist pure and simple, he was also conducting at this time an investigation on heredity in bees. It is said that he had fifty hives under observation and that he effected crosses between various European, Egyptian, and American races. Nothing, however, was published about his results in this connection, and unfortunately no

* In Verhandlungen des Naturforschenden Vereines in Brünn. Band IV.

trace of his notes on this subject has so far been discovered. The only other paper published giving results of work on heredity was a little one on *Hieracium* hybrids ("Ueber einige aus künstlicher Befruchtung gewonnene Hieraciumbastarde"), read in 1868 and published in 1869. Considering the great amount of unfinished work which he must have had on hand in 1868 it must ever be a matter for regret by all biologists that Mendel became charged with administrative responsibilities which practically ended his career as a scientific investigator.

4. The Special Features of Mendel's Method of work on Heredity.

Coming now to a closer consideration of Mendel's work on the crossing of different races of plants, we have first of all to notice that Mendel introduced into the plan of his experiments three ideas which have proved the most powerful means of research in heredity that have ever been brought forward. All three are very simple ideas and appear almost self-evident necessities of any experimental work on heredity, but they seem to have been quite novel in Mendel's day, and I believe it is very largely due to a want of a proper appreciation of these three leading features of the Mendelian method of investigation that so many wholly incorrect notions have been, and, unfortunately, are still being disseminated about Mendelism and even about heredity in general.

The first idea is that of fixing the attention in each experiment upon a single pair, or at most upon a few

pairs of contrasted characters, without being distracted by any other changes which may result as the effect of the cross. This may seem a very obvious method of procedure—something, in fact, very like taking the old advice of breaking the sticks one by one instead of trying to break the whole bundle at once—but in reality it involves an assumption which is even yet regarded by many as somewhat of a heresy, namely, that an organism *can* be regarded in any degree as a bundle of separable unit characters. Luckily, in the case of Mendel's peas, the selected characters proved to be simple and completely separable, and so the problem of their transmission was reduced to its simplest terms. Mendel himself, however, seems to have realised that it might be very difficult, in certain cases, to say offhand what were the characters which were treated in heredity as units, for he says "the uniformity of behaviour shown by the whole of the characters submitted to experiment permits, and fully justifies, the acceptance of the principle that a similar relation exists in the other characters which appear less sharply defined in plants, and therefore could not be included in the separate experiments." Later experiments have abundantly shown that it is practically impossible to say at first whether any particular character that can be visually recognised is or is not a unit character, and it therefore becomes an essential part of any investigation on heredity to ascertain what are the fundamental characters involved.

Disregard of this has led to all sorts of misconceptions, and even to actual misinterpretation of results.

The second idea running through Mendel's experiments was that of following the results of a cross through several (at least three) generations. This is an extremely important point, for, in conjunction with the next idea of separate records of all individuals, it permits clear evidence to be obtained from the progeny, of the kind of factors present in the germ-plasm of the parents, whether those factors are visibly manifested in the latter or not. The possibility of carrying out this idea evidently depends upon the fertility of the hybrids, and it is not the least of Mendel's incidental contributions to the methodical study of heredity that he so clearly saw the importance of experimenting with forms which produce fertile hybrids. Most investigators before his time, and for long afterwards, were content to make crosses between different species and races and to note the results in the first generation, without attempting to carry the experiment any further. But this only led to confusion, for, as we now know, the hybrids may be in appearance exactly like one parent or exactly like the other, or partly like one and partly like the other, or again like neither parent but with a character of their own, while all the time perhaps the factors determining the original characters of *both* parents are being treated by the cell-divisions leading up to the formation of the germ-cells of the hybrids as perfectly distinct entities, ready to show themselves

again without the slightest change in the individuals of the succeeding generations.

The third idea introduced by Mendel, and about the originality of which there cannot be the slightest doubt, was that of keeping separate records of the results obtained from every individual seed produced. So important is this, that it is quite certain Mendel would never have formulated the law which now bears his name if he had not adopted this method of work. The number of forms which the progeny of the hybrids between different races can assume and the numerical ratios existing between them can be ascertained only by this laborious method, and without a knowledge of these ratios there can be no certainty about the hereditary factors involved in any particular case.

5. The Great Result of Mendel's Work on Heredity—Mendel's Law.

In order to show quite clearly the outstanding result of Mendel's work, namely, the formulation of what is now known as Mendel's Law, it will be well to refer very briefly to a few facts connected with his experiments with peas.

After some preliminary trials, devoted mainly to testing the purity of the varieties of peas with which he proposed to work, Mendel selected seven pairs of differentiating characters for observation. As examples of these may be cited :—

{ Ripe seeds round and smooth, or nearly so. }
{ „ „ angular and deeply wrinkled. }

Seed albumen (cotyledons) yellow.
,, ,, ,, green.
Plants tall —stem 6 to 7 feet.
,, dwarf— ,, ½ to ¾ foot.

Crosses were made between plants possessing one character of one or more of the pairs and those possessing the contrasted character or characters. In each case the hybrids produced exhibited one character only of each pair, and this character, which always came out to the exclusion of the other, Mendel called the "dominant" character. The other he called the "recessive" character, because it was not destroyed or altered, but simply receded from view, so to speak, when the dominant character was present in the same plant. It is worth noting at this point, however, that the phenomenon of dominance of certain characters, although found in all seven of the pairs of characters used by Mendel, is by no means of universal occurrence, and moreover has nothing to do with Mendel's Law. When dominance occurs, it adds a complication to the results obtained in breeding from the hybrids, causing an apparent simplification which is misleading without further analysis, as will now be seen.

By allowing the hybrids to be self-fertilised Mendel next obtained for each pair of characters results of the following nature :—

5,474 round and 1,850 wrinkled seeds,
6,022 yellow and 2,001 green cotyledons,
787 tall and 277 dwarf plants,

giving a ratio of almost exactly 3 dominant to 1 recessive in every case. When two or more pairs of characters were present in the same cross the results were exactly the same for each pair of characters, but the characters themselves were associated in every possible way and not merely as in the parent plants, while the ratios for the different types of combination approximated with great accuracy to a multiplication of the 3 : 1 ratio by itself as many times as there were additional pairs of characters associated with the first pair. Thus when two pairs of characters were associated in the cross, four types of plants were produced in the following proportions :—9 : 3 : 3 : 1, which is evidently the result of $3 + 1 \times 3 + 1$. Some actual figures obtained by Mendel in the case of crosses between round yellow and wrinkled green seeded forms were :—

315 R Y : 101 W Y : 108 R G : 32 W G.

If Mendel had stopped at this point in his investigation it is very unlikely that he would have been led to his great discovery. But by continuing his work into the next and still later generations he was able to show that the apparently simple ratio of 3 : 1 for each pair of characters was really a ratio of 1 : 2 : 1, for whereas the one recessive in every four individuals was pure for its character, of the other three, *i.e.*, the dominants, only one was found to breed true, the other two proving to be hybrids exactly the same as their hybrid parents, and

giving, upon self-fertilisation, progeny in the ratio of 3 dominants to 1 recessive.

The foregoing facts elucidated by Mendel may be tabulated as follows :—

Generation.	Description.	Representation of character-factors present.
Parental (P).	Parents, one exhibiting the dominant and the other the recessive character of a pair of differentiating characters.	D × R
1st Filial (F_1).	Hybrids, all of the dominant type, but all containing the recessive character-factor also.	D (R)
2nd Filial (F_2).	Progeny resulting from the self-fertilisation of the hybrids, namely, 3 dominants to each recessive. Two of the apparent dominants, however, as shown by later generations, contain the recessive character-factor, and are therefore hybrids, like their parents. The other dominant, and also the recessive, are pure and remain constant indefinitely.	D, D (R), R, in the ratio of 1 : 2 : 1 or as regards appearance only 3D : 1R.

We are now in a position to consider the problem which presented itself to Mendel when he had made out this constant ratio of 1 Pure Dominant : 2 Hybrids : 1 Pure Recessive for each pair of characters among the progeny raised from the hybrids produced by the original cross. That hybrids when bred together should produce hybrids was what might have been expected, but that each of the two original characters should come out again in a perfectly pure form, without any tendency to produce in any subsequent generation

the slightest approach to the opposite character with which it had been so intimately associated, was a fact of fundamental importance, and Mendel seized upon it with characteristic acumen as the key to the whole position. He asked himself the question, "How is it possible for the original characters to come out again in the progeny of hybrids in an absolutely pure form?" His answer was, since we know from experience that to keep a race constant it is essential that only individuals exhibiting the same character should be mated together, so when constant forms arise from hybrids it must be due to the fact that they are produced by the meeting of germ-cells containing the factor for the character in question and that alone.

Having arrived at the conclusion that in the formation of some at any rate of the germ-cells of hybrids a complete segregation of character-factors takes place, Mendel next sought for an explanation of the fact that, in regard to each pair of characters, the number of pure forms arising from the hybrids was exactly one-half of the total number produced ($\frac{1}{4}$D + $\frac{1}{4}$R). With his mathematical training he had no difficulty in seeing that the only assumptions necessary to account for this are that the segregation of character-factors should take place in such a way that half the germ-cells (whether male or female) carry the factor for one character and half for the other, and that the matings of the two types

of male and female germ-cells are entirely a matter of chance. For under these conditions the only possible unions among the germ-cells are :—

* ♀ Dominant × ♂ Dominant
♀ ,, × ♂ Recessive
♀ Recessive × ♂ Dominant
♀ ,, × ♂ Recessive

As the numbers of each type of both male and female germ-cells are equal, it follows that, on the average, each ♀ D will have an equal chance of being fertilised by either a ♂ D or a ♂ R, and each ♀ R an exactly similar chance. As the chances are equal therefore in all cases, the numbers of matings of each of the four types will be equal also, and the result must be on the average :—

1 D : 2 D (R)† : 1 R.

So Mendel's Law amounts to this, that when a pair, or any number of pairs, of contrasted characters are combined in one individual (*i.e.*, when that individual is hybrid with regard to those characters) the two factors for each pair are separated from one another during the formation of the germ-cells, so that half the latter contain the factor for one character and half the factor for the other character, the various pairs of characters behaving in an absolutely independent manner and giving rise, therefore, to germ-cells containing

* The sign ♀ represents female and ♂ indicates male.
† It was ascertained by reciprocal crossing that there was no difference between ♀ D × ♂ R and ♀ R × ♂ D.

every possible combination of simple character-factors in equal numbers. In other words, the germ-cells of the hybrids are never hybrid, but exactly correspond to the original pure stocks, except that, when two or more pairs of characters are involved, the character-factors are associated in every possible way in equal numbers.*

The reasoning by which Mendel arrived at the foregoing generalisation may seem very simple and natural to us now, but it was nevertheless a very bold step in the fifties of last century to apply to germ-cells, about which scarcely anything was known, facts which had been obtained by the observation of pure and hybrid races. At the present time it is very easy to believe that segregation of character-factors, each pair into two equal groups, may take place; in fact, most of the phenomena of cell-division and the maturation of the germ-cells lead us to suppose that such is actually the case.

6. Incidental Results of Mendel's Work on Heredity.

As in the case of all really important pieces of work, the effect of Mendel's study of hybridisation phenomena was by no means limited to the most evident result. In the elucidation of questions of heredity, Mendel's Law has of course been of

*Mendel's own words are that "Die Erbsenhybriden Keim- und Pollenzellen bilden, welche ihre Beschaffenheit nach in gleicher Anzahl allen constanten Formen entsprechen, welche aus der Combination der durch Befruchtung vereinigten Merkmale hervorgehen."

supreme value in recent years, but it is difficult even at the present day to fully realise the far-reaching influence of a thorough appreciation of the Law and of the facts brought to light in connection with its establishment and verification. There are, however, two important fields in which they have had very striking consequences, and a few words on these may fittingly serve as a conclusion to this little sketch.

Let us take first the effect which a knowledge of Mendel's Law and the associated facts have had upon the subject of variation in animals and plants. It is apparent at once that the fact that certain proportions of the progeny of hybrids can be perfectly constant, although exhibiting quite new combinations of characters as compared with the original parents, gives the clue to one important section of the very various kinds of phenomena grouped under the general term of variation. For instance, seven pairs of characters can be combined theoretically in one hundred and twenty-eight different ways (*i.e.*, 2^7), each combination being possibly constant, and Mendel was fortunate enough to obtain the full number of forms in the course of his experiments with peas. Similar recombinations have been obtained in many other plants and also in animals, and it is not too much to say that the special peculiarities of a considerable number of the constant races of domesticated animals and cultivated plants are nothing but recombinations of characters previously existing in earlier races.

Another group of variation phenomena which has been very much illuminated by the work done on Mendelian lines is that due to the influence of the character-factors upon one another when combined in the same individual. The simplest case is that where one character of a pair is completely dominant to the other, as in all of Mendel's pea experiments. But it often happens that the two characters associated in a hybrid act upon one another in various degrees, so that either a more or less intermediate type is produced, or an entirely novel character appears. But even more important than this action of one character of each pair upon the other in the body of the hybrid has been the discovery that the characters belonging to *different* pairs may profoundly affect one another when occurring in the same organism, and that what are apparently quite new forms or, it may be, reversions to old forms arise in this way. One general outcome of Mendel's Law, and of the work done in connection therewith, has thus been the demonstration of the subordinate and accidental character of many of the phenomena of variation, the importance of the really fundamental variations in the germ-plasm being consequently brought into even greater prominence than before.

The other direction in which Mendel's work on Heredity has had a most important though indirect influence, is in connection with the problem of Evolution. The Darwinian conception of Natural

Selection undoubtedly decided in the affirmative the general question of organic evolution, for there can be no reasonable doubt either as to the fact of selection taking place in nature, or as to the occurrence of favourable variations to be selected. But it was more or less tacitly assumed that all kinds of variations could be inherited and thus serve as material for the production of new forms by selection. Variation phenomena, however, as is now well known, are of many different kinds, and, apart from the variations due to the recombination and interaction of characters alluded to above, there has been much uncertainty as to the rôle, in the process of evolution, of continuous variations (*i.e.*, variations which are connected with one another by a continuous series of intermediate forms) on the one hand, and discontinuous variations on the other. It is here that the evidence from the Mendelian researches seems to come to the support of the view that it is the discontinuous variations which really count in evolution. For if the factors for the differentiating characters between races and species are treated as separable entities by the cell-divisions concerned in the production of the germ-cells, it is extremely improbable that these characters could have been evolved from one another by the selection of imperceptibly small variations. In other words, segregation of the character-factors involves an antithesis between the factors which is inconsistent with the idea of continuous variation. So far, therefore, as Mendel's Law holds good, and it is

becoming more and more probable that it applies to a very large number of the characters which separate closely allied races, it decidedly strengthens the theory that evolution has to a great extent, if not altogether, proceeded by the selection of discontinuous variations. This conception of evolution taking place by distinct steps, some very small no doubt, but still discontinuous, is of great importance, and, if firmly established, may have theoretical and practical results which can only be dimly discerned at the present day. This, however, is a subject beyond the scope of this paper. It only remains to be pointed out that Mendel himself clearly saw the bearing of his work upon the evolution problem, for, after referring to the great labour involved in the method of work he adopted, he adds significantly that "This appears, however, to be the only right way by which we can finally reach the solution of a question the importance of which cannot be over-estimated in connection with the history of the evolution of organic forms."*

*Translation in Bateson's "Mendel's Principles of Heredity," p. 318.

Pedigree of the Short-fingered Family.

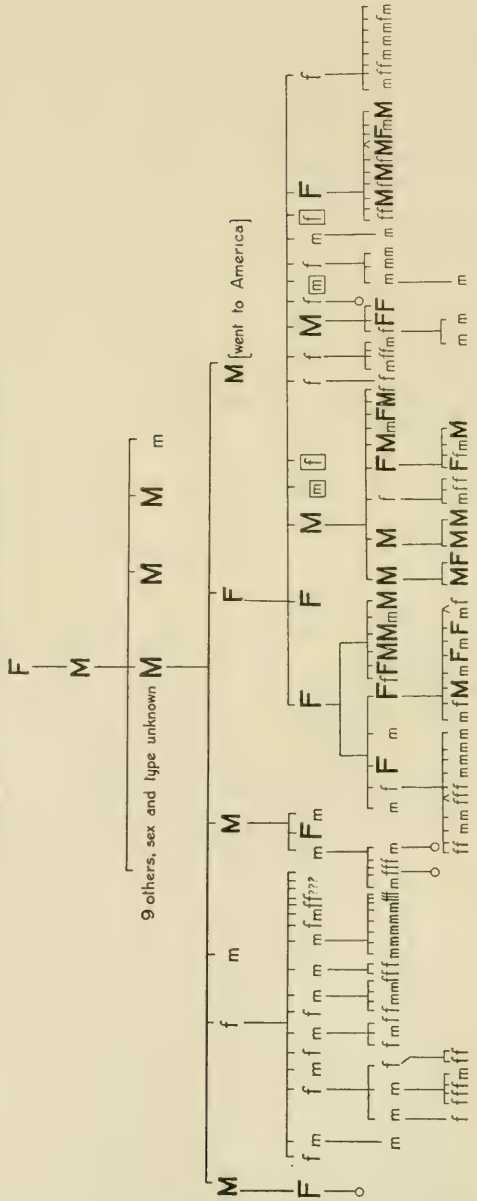


PLATE 1.

The Inheritance of "Thumb-fingerness" in a Short-fingered Family.

A Case of Mendelian Inheritance in Man.

An Address delivered to the Mendel Society on June 6th, 1910.

By H. DRINKWATER, M.D., M.R.C.S., F.R.S. Edin.

The family which will be described in this paper presents a rare and easily-recognised deformity which has affected at least seven generations. It is interesting in many ways. Its hereditary character is particularly striking, for it is practically identical in every affected individual, and the student of Mendelism will be impressed by the close conformity of the numbers affected to theoretical expectations.

I shall avoid all technical terms as far as possible, so that the essential features may be understood by the readers who are unfamiliar with anatomy.

The genealogical chart (Plate 1) shows all the individuals, as far as they can now be traced, through the last seven generations. Whether the abnormality began with the woman at the head of this chart, or whether she inherited it from one of her parents, is not now known. There is every reason to believe that its first appearance was as we now see it, and that it did not appear or develop by a gradual process. In

other words, it first occurred as a "*sport*" or "mutation," and in each succeeding generation some of the offspring have exhibited the deformity.

The chart shows 174 individuals, of whom 107 were alive in 1907 when the chart was constructed. The last four generations are complete so far as numbers and sex are concerned. The four members enclosed in squares in the fifth generation all died in infancy, and whether they were affected with the abnormality or not cannot now be ascertained.

The capital letters indicate the abnormal members, the small letters the normal ones. M and m stand for males, F and f for females. Thus it is seen that the first member (now traceable) who showed the deformity was a woman.

I could not ascertain whether she had any brothers or sisters. The line passing down to M indicates that this woman had a son, and it is seen that he was abnormal. Nothing further can be ascertained about this second generation. This man had three abnormal sons, and a normal one, in addition to nine other children whose sex and type are now unknown. One of these males of this third generation had four sons, three of whom were affected—one normal daughter and one abnormal. The youngest son went to America and settled there. This is interesting from the fact that a family showing an apparently identical peculiarity has been described by an American observer (Mr. Farabee), and it is possible that they have descended from this member of the English family, but this point is not yet settled.

The chart is thus complete as regards *numbers* for the last *five* generations.

The deformity which this family manifests affects chiefly the fingers and toes, but before describing it in detail, it will be well to get a clear idea of the normal anatomy of these parts.

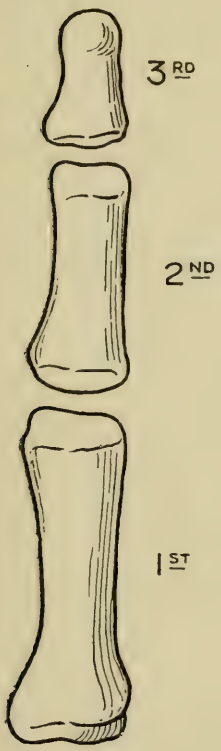


FIG. 1.

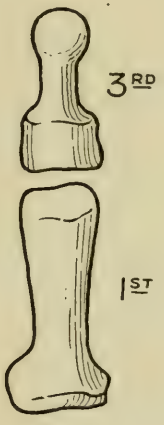


FIG. 2.

FIG. 3.

Each finger and toe (except the thumb and big toe) contains three bones, those of a finger being longer than those of a toe, but otherwise similar. **Fig. 1** shows the three bones of a normal middle finger.

They are numbered 1st (the nearest), 2nd (the middle), and 3rd (the terminal one which supports the nail). The first is the longest bone, and the third is the shortest.

Each finger or toe has two joints, one between the first and second bone, and one between the second and third. Each joint in a finger causes a crease on the palmar surface, and a knuckle on the back. A normal finger shows these two palmar creases very clearly.

The thumb and big toe have each only *two* bones, one joint and one crease.

Now, how do the hands and feet of the abnormals differ from the common type ?

Abnormalities ot the fingers and toes are not by any means rare occurrences, and many people are familiar with them ; but the precise peculiarity is seldom accurately reproduced in successive generations. One member of a family may have four fingers, another three, another two.

Now, the remarkable fact about this family is the accurate reproduction of the deformity in all the abnormal members.

Whenever the deformity appears at all, it affects every finger and every toe of *both* hands and *both* feet. The deformity consists in the absence (or apparent absence) of the middle bone, so that each finger and toe has only *two* bones instead of three, and *one* joint and *one* crease instead of two. The fingers are thus reduced to the condition of thumbs (Plates 2 and 3).

PLATE 2.
Photograph of a Short-fingered Hand.

Mr. Farabee says that the middle bone is absent, but this does not appear to be the case in my family, for though there are only two separate bones in the adult, the middle one, as will be shown presently, is not really absent but has undergone a remarkable change, which might cause its presence to be overlooked. At any rate, this is the case in the English family, and from Mr. Farabee's illustrations I feel sure that the American people are exactly like the English family.

Fig. 2 shows the shape of the two bones in one of these short fingers. The first bone differs from the normal type (Fig. 1) in being shorter, but otherwise it closely resembles it. The second appears to be altogether missing. If, however, the terminal one (Fig. 3) is carefully compared with the normal third, a marked difference will be observed.

At the base there is seen a cubical mass which is not present in the normal bone (Fig. 1). This cubical mass represents the *middle* bone, which has become joined by bony union to the terminal one, the two forming one bone in the adult (see 2. 2. 2. 2. Plate 4).

A radiograph of the hand of an abnormal child shows this middle bone as a distinct and separate structure, but much shorter than the normal (2. 2. 2. 2. Plate 5). It is imperfect also in other respects. Each bone during childhood is only partially "ossified," for there is a piece of gristle lying at the end of each bony portion, and this is well seen in the "first row" of Plate 5. This gristle during early adult life becomes transformed into bone and fuses with the joint.

PLATE 3.
Hand of Normal and Abnormal Brothers.

Now, in the abnormal child this middle bone (the second) consists of *one* bony piece only instead of two, and this one is much smaller than normal (Plate 5). Instead of remaining separate, it unites later on with the third bone, and thus we eventually have two bones in the finger instead of three. Though all the bones of the finger are shorter than normal, the marked shortening of the fingers is due chiefly to the imperfect development of the middle (second) one.

Union of the third bone with the imperfectly-developed second is the *rule*, but there are a few exceptions to it.

This union invariably occurs in the *first* and *fourth* fingers, but sometimes in the middle and ring finger it does not do so, and the middle bone can then be seen even in the adult as a separate though very short bone (middle finger, Plate 4). Thus *the second bone is never absent, but it is always imperfect and generally joined to the terminal one.* When present as a *separate* bone, it has only about one-quarter the normal length.

The first bone of the *thumb* is very much shortened (Plates 4 and 5), sometimes so much so that its length is exceeded by its breadth, but it is never united to the terminal one. It is imperfectly developed similarly to the middle bone of the fingers.

This description of the fingers and thumb applies equally well to the toes (Plate 6).

It is thus clear that the fingers of these people are reduced to the condition of thumbs, for, as a rule,

PLATE 4.
Radiograph of Hand of Abnormal Adult.

each finger has (like the thumb) only two bones and one joint, and the same is true of the toes.

The bones of the palm show certain peculiarities, but as they are of no special interest I shall not describe them here.

The *external aspect* of the hand is very characteristic (Plates 2 and 3). It is short, and looks unduly broad. Each finger shows only one crease, corresponding to the single joint. These points are well seen in Plate 3, where the abnormal hand of a man is shown above that of his normal brother.

Length of Hands.

The average length of the hands was as follows :—

Normal males ..	..	$7\frac{1}{4}$ inches.
Abnormal males	..	$5\frac{1}{2}$ „
Difference..	..	$1\frac{3}{4}$ „

Middle finger :—

Normals ..	..	$3\frac{6}{16}$ inches.
Abnormals ..	..	$1\frac{15}{16}$ „
Difference	..	$1\frac{7}{16}$ „

Symmetry.

In every individual the two hands are symmetrical, the peculiarities of one hand being accurately duplicated in the other, as shown both by photography and by radiography. The same is probably true of the feet, though in most cases only one foot was examined.

The shortening of the hands is thus seen to be almost entirely due to the abnormality of the fingers,

PLATE 5.
Radiograph of Hand of Abnormal Child.

which are nearly an inch and a half shorter than those of the normal members of this family; in fact, the fingers are only a trifle more than *half* the normal length (Plate 3.)

This shortening of the fingers handicaps these people very considerably and prevents them following any trade or profession requiring fingers of the normal length. None of them seems able to play any musical instrument. They are all engaged in occupations where there is no call for great manual dexterity.

Stature.

Another peculiarity of these people is their short stature. The men are, on an average, $8\frac{1}{2}$ inches shorter than their male normal relatives (the tallest being only 5 feet $3\frac{1}{2}$ inches), and the women are $4\frac{3}{4}$ inches less than the normal women.

This shortness of stature is not present in the children; indeed, at two years of age they are actually $\frac{3}{4}$ inch taller than the long-fingered children. In fact, it is not until after the fourteenth year that retardation of growth occurs. This is true of both sexes. Farabee noted that the American family are like the English as regards the short stature of the adults.

Several comparative measurements are shown in Fig. 4.

The *conspicuous* peculiarities of these people are the shortness of the hands and toes, and the shortness of stature; but the abnormals differ in several other respects from their normal relatives.

I shall only refer to a few of these differences.

PLATE 6.
Radiograph of Foot of Abnormal Adult.

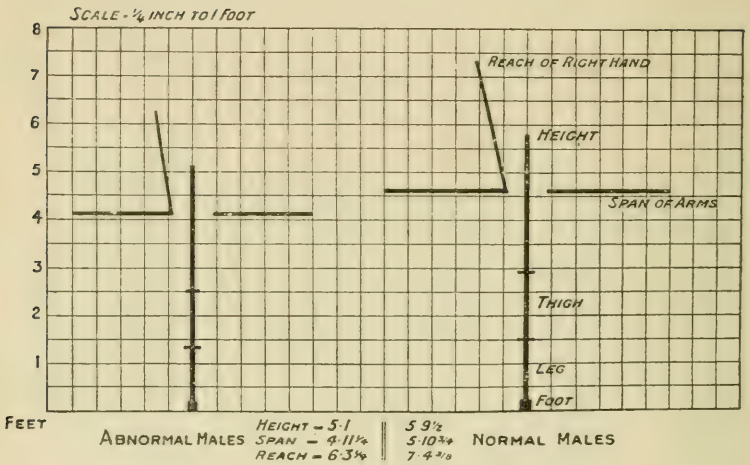


FIG. 4.

GENERAL HEALTH.

In the first place, they are apparently more vigorous and healthy; indeed, they seem to be remarkably free from sickness.

MARRIAGE.

They have not only married in greater percentage but at an earlier age. "The short-fingered members of the family always get married first." Why this is so I cannot tell, for they are certainly not so good-looking, but, as before remarked, they are more robust.

There are fourteen marriageable normals still single; there is only one such amongst the abnormals, and she has been "engaged" for some considerable time. It is her fiancé's fault that she is not yet married.

Fecundity.

The abnormals are the more fertile members of the family in the ratio of 6 to 4, and the women are more so than the men in the ratio of 8 to $4\frac{1}{2}$.

Hence it follows that the abnormality does not show any sign of dying out; on the contrary, it is increasing in numbers.

In the	4th	generation	there are	4	cases.
,, ,,	5th	,,	,, ,,	7	,,
,, ,,	6th	,,	,, ,,	19	,,
,, ,,	7th	,,	,, ,,	9	,,

This last figure (9) will probably be considerably increased, for there are three parents of the sixth generation still in the child-bearing age, and eight who, not having yet reached it, may marry and have children.

Summary.

The short-fingered people have short, symmetrical hands, short toes—the middle bone being very imperfectly developed; and they are short of stature. They are robust, marry early, and are prolific, and are increasing in numbers.

Their occupation is not mentioned, as it is not desirable to do anything which will lead to their identification.

Mendelian Inheritance.

My investigation of these people was undertaken without any bias as to theories of heredity, but a careful consideration of all the data cannot fail to convince

one that they furnish a striking example of some of the laws propounded by Gregor Mendel, as I shall now attempt to prove.

Mendel selected a tall pea (the offspring of tall parents) and crossed it with a dwarf pea (the offspring of dwarf parents). The hybrids resulting from this cross were all tall, thus

Tall × Dwarf.
|
Tall (Hybrid).

These hybrids crossed with dwarfs produced talls and dwarfs in equal numbers.

He also experimented with other characters besides stature, such as colour of flowers (purple and white), shape of seeds (smooth and wrinkled), colour of seeds (yellow and green), and found the same laws to hold good. The first hybrids exactly resembled *one* of the parents in respect of the chosen character, and when these were again crossed with a plant presenting the alternative character of the *other* parent, he got in the next generation *both kinds in equal numbers.*

Thus :—

Tall × Dwarf.
|
Tall × Dwarf.
|
Tall and Dwarf in equal numbers.

The abnormality of these short-fingered people is a dominant character,* and each individual may be compared to the tall hybrid plant in the above

* See Mr. Scourfield's paper for an explanation of this term, p. 24.

experiment, for since there has been no intermarrying each one has had one parent abnormal and the other parent normal.

It follows that when one of these abnormals (comparable to the hybrid plant) marries a normal, their children should be of both kinds; one half should show the abnormality and the other half not.

As a matter of fact, 52 per cent. of the offspring of abnormals have been normals, and as there is always in such cases an element of chance, this percentage is held to be a sufficiently close approximation to theoretical requirements.

Mendel found that the dwarf plants when self-fertilised, or bred amongst themselves, always breed true to the dwarf character, and not only so, but that the dwarfs of the third generation (one of whose parents was tall) invariably breed true and produce nothing but dwarf plants. Further than this, if the first hybrids (tall) are bred amongst themselves or are self-fertilised, they produce a certain proportion of dwarfs (25 per cent.), and even these dwarfs breed true to dwarfness.

These dwarf plants, from a hereditary point of view, correspond to the *normal* members of the short-fingered family. Such being the case, a normal person (one of whose parents is short-fingered) ought, when married to another normal, to produce normal children only, and this has been the invariable result.

If the chart is consulted, it will be seen that the normals (represented by small letters) have not in a single instance had short-fingered children. The

normals, in fact, breed true to the normal character, and the abnormality can only be transmitted by one or other abnormal parent.

According to Professor Bateson and Professor Punnett, a dominant character is due to the *presence* of some factor which determines its manifestation, and its non-appearance is due to the *absence* of the same factor. If, then, the peculiarities of the short-fingered people are due to the presence in them of some factor or element which is absent from the normals, it follows that the normals can never reproduce the abnormality which requires the presence of this factor for its manifestation.

It is obvious that a purely short-fingered race could be established by breeding from them alone, and it is equally clear that this abnormality can only be eradicated by preventing the marriage of the abnormals. The same observation applies to any disease, or predisposition to disease, and to any defect or peculiarity which is hereditary and plays the part of a dominant characteristic; but this is not the place to discuss the subject in further detail.

There is little doubt that in a wild state any individuals who presented the physical peculiarities described in this paper would, owing to the struggle for existence, have little chance of surviving long enough to perpetuate their kind: but the conditions of modern civilisation are such, that vast numbers of people, handicapped at birth, are not only able to survive, but to perpetuate their kind, even though it tends to the deterioration of the race.

Plate 1 and Fig. 4 are reproduced by kind permission of the Royal Society of Edinburgh, from their "Proceedings," and the others are new.

ON TABBY CATS.

And some Features in the Inheritance of their Coat Patterns and Colour.*

An Address delivered to the Mendel Society on June 6th, 1910.

By R. I. POCOCK, F.L.S., F.Z.S.,
Superintendent of the Zoological Society's Gardens.

According to its derivation from the Arabic *atabi*, meaning a particular kind of watered silk, the epithet "tabby" is applicable, strictly speaking, to any thing with a wavy pattern, like that of the material in question; but for no very obvious reason it seems by common consent to be restricted in use to domestic cats marked with a definite brindled or striped pattern.

Quite superficial scrutiny will show that tabby cats may be readily sorted by their patterns into two distinct kinds which differ so greatly from one another that no zoologist would hesitate to regard them as representing different species if they existed in a natural state. In one kind, which may be called the "Striped Tabby," the sides of the body are marked with narrow, wavy, vertical stripes stretching from the back towards the belly, and commonly showing a tendency, especially on the hindquarters, to break up

* The question of the origin and relationship of the breeds of domestic cats has been already dealt with by the author in Proceedings Zoological Society, 1907, pp. 143-168, pts. viii.-x.

Plate 1.

into spots ; the middle line of the back is confusedly marked, but shows no broad longitudinal stripe on each side of the spine (Plate 1). In the other kind, which may be called the " Blotched Tabby," the stripes are much broader and fewer, and form on the sides of the body a somewhat spiral or circular arrangement which fanciers speak of as the " horse-shoe " mark ; while on the back there are three longitudinal stripes —a thinner one in the middle line and a broader one on each side of it (Plate 2).

There is considerable individual variation in both these types. In the Striped Tabby the stripes may be broken up into larger or smaller spots, so that the original pattern is more or less obscured ; and in the Blotched Tabby the dark marks may widen and extend over and more or less obliterate the paler intervening areas ; but whatever be the nature of the variation, it never seems to take the direction of convergence of one type of pattern towards the other. The two do not intergrade, and when once the differences between them have been seen and understood, there is no difficulty in assigning every tabby cat to the one type or the other.

This is a very remarkable fact ; yet only within the last few years has its importance been appreciated. Breeders and exhibitors of domestic cats have long been acquainted with it ; but it rarely happens that fanciers take a scientific interest in their pets, and apparently no cat fancier of either sex has troubled about the real meaning of tabby markings. From this ignorance of the significance of pattern, it

Plate 2.

comes about that the National Cat Club gives a quite subsidiary importance to markings as a basis for distinguishing domestic breeds, preferring length of coat, shortness of tail, tint of hair, and other comparatively trivial features.

Apart from Siamese, Manx, and one or two other less important breeds, domestic cats are classified under two headings, namely, "long-haired," otherwise called "Persians" or "Angoras," and "short-haired" or ordinary cats, both long-haired and short-haired being further subdivided by colour into blacks, whites, blues, smokes, reds, creams, tabbies, and the like. Thus the pattern is merely regarded as equivalent to colour and of less value than length of coat or absence of tail; nor does it appear that any stress is laid upon the difference above alluded to between the two types of tabby.

The real importance of pattern is shown by a study of wild species of Cats which zoologists comprehensively group together as *Felis*. Tigers, for example, are always striped, and leopards always spotted. The stripes in the one and the spots in the other may vary in number, but the pattern in each species remains substantially the same. Not so the hair. Mongolian tigers and North Chinese leopards differ strikingly from their tropical kin in the length and thickness of the Winter coat. The difference is not so marked as between a thoroughbred so-called Persian Cat and an ordinary Cat. Nevertheless, the fact proves that the length of the coat is a much more variable feature in Cats than the pattern.

So, too, with colour. Black varieties or " mutations " of wild species of *Felis* occur in a state of nature. Black leopards and jaguars are not uncommon ; black servals have been shot on several occasions, and black tigers have been recorded. Leopards of tropical Asia are richly tinted ; those of Persia are markedly greyer ; while those of tropical West Africa usually have a characteristic dusky hue, as compared with Asiatic specimens. Again, some species of wild Cats are dichroic ; that is to say, reddish or blackish specimens occur side by side in the same locality. This phenomenon is known in the South American Jaguarondi Cat (*F. jaguarondi*) ; the West African Tiger Cat (*F. aurata*), and in the east Asiatic Temminck's Golden Cat (*F. temmincki*). What is perhaps more remarkable still is that an example of theW est African Tiger Cat that lived in the Zoological Gardens changed its colour from dark brown to blackish grey during growth.*

All these facts point to the conclusion that colour is a somewhat variable feature in Cats, and since the pattern, when pattern exists, in the above-mentioned species remains the same whatever be the tint of the ground colour, it is evident that pattern is a more stable characteristic than tint. The spots of black leopards are always, it seems, visible in certain lights. Similarly the tabby pattern always appears to be detectable in black kittens of domestic Cats. I have often seen it also in white kittens, the visibleness

* Proceedings Zoological Society, 1907, p. 659.

of the pattern in both black and white kittens being due to the greater gloss of the hair that forms the pattern as compared with that of the intervening spaces.

It might at first be considered somewhat curious that although domestic Cats are commonly white, white examples of wild species are exceedingly rare. This, however, is not in reality a surprising thing, because, except in the Winter in Arctic and Antarctic latitudes, white is a very conspicuous colour. Hence a white wild Cat would be severely handicapped in the struggle for existence by inability to conceal itself both from its enemies and its prey. It is, moreover, a matter of common knowledge that white Cats are frequently deaf, and if, as is not unlikely, this or some other organic defect, such as want of stamina, accompanies whiteness in wild species, another reason for the rarity of white wild Cats becomes apparent.

I have only seen white skins of two wild species of *Felis*, both of which are in the British Museum. One is a tiger, the other a cheetah or hunting leopard. In both it is noticeable that the pattern is not white, like the ground colour, but yellowish or "tan." This proves two things: First, that the animals were not pure albinos; and, second, that in these partial albinos the yellow or tan ground colour has turned white and the black pattern has turned "tan"—that is to say, "tan" in albescent individuals is an intermediate colour phase between black and white. Similarly, "tan" is an intermediate colour

phase between white and black in nigrescent individuals or partial melanos. True melanos are wholly black, just as pure albinos are wholly white; but partial melanos are quite common, black-and-tan dogs being a familiar instance. If a black-and-tan dog be compared with a pale-coloured wolf, it is obvious that the tan round the mouth, over the eyes, on the paws, and other parts of the dog correspond exactly with light areas on the wolf, and that the black parts of the dog correspond in the same way with the greyish-black pigmented parts of the wolf.* Just as black-and-tan dogs are partial melanos, so are black leopards, as a rule, if not always, partial melanos; that is to say, they are seldom if ever uniformly black. The intermediate spaces are a little paler than the spots; and the underside of the body, which in the normal animal has a pure white ground colour, is very dark tan. Black domestic Cats, on the contrary, are generally complete melanos when adult, the pattern being invisible because the hair is everywhere as black as the pattern.†

Black and white are thus two extremes of colour mutation, yellowish-red or tan being the intermediate phase. These three phases are usually called melanistic, albinistic, and erythristic. All three are common in domestic Cats. The majority of Cats seen roaming at large and practically uncared

* R. I. Pocock, Annals Magazine Natural History (7), xix., pp. 192-194.

† Black cats commonly have a white speck on the chest at the anterior end of the sternum. This white speck is frequently present in Scotch wild cats. The persistence of the whiteness of this spot in black cats is a puzzling fact.

for are normal tabbies, red tabbies or blacks ; of less common occurrence are normal tabbies, red tabbies or blacks varied to a greater or less extent with white ; rarest of all are pure whites and so-called "tortoise-shells," the latter being Cats in which both the pattern and the ground colour show a confused mixture of tan and black, with white commonly pervading more or less the legs and lower parts of the body.*

The question of "red" and "tortoise-shell" Cats cannot be dismissed without reference to the as yet quite unexplained fact that the former are nearly always males and the latter nearly always females. The frequency of this phenomenon has given rise to the saying that the "red" is the male of the "tortoise-shell" variety. † Mr. L. Doncaster, by carefully analysing the evidence contained in the literature of fanciers, was able to show that female tortoise-shell Cats were the mongrel forms of black Cats crossed by red ones. On the other hand the "red" males were similarly mongrels of the same kind of cross. In males, therefore, red is wholly dominant, but in females only partially so, some of the black manifesting itself.

One final word about white Cats. I have already said that the white tiger and cheetah skins above referred to were not the skins of pure albinos, because the stripes in the one case and the spots in the other were pale tan instead of white. I have never seen a

* White is considered a defect in "show" tortoise-shells.

† The only species of mammals known to me which are naturally "tortoise-shell" are the African hunting dog (*Lycaon pictus*), and the Lemming (*Myodes lemmus*).

white Cat with tan pattern; but I have also never seen what would be regarded as a pure albino Cat; that is to say, a white Cat with pink eyes. White Cats often, however, have blue eyes. This is a sign apparently of incomplete albinism, if we may judge from the fact that blue or bluish eyes occur not uncommonly in white or partly white horses and dogs, especially sheep-dogs, and typically accompany pale complexion and blonde hair in mankind. A very distinct and remarkable albinistic breed of Cats is the Siamese. The newly-born kittens are pure white, and the eyes are frequently blue; but as age advances the Cat gradually turns brown, the head, legs, and tail becoming first of all suffused with dark pigment, and this later in life extends more or less over the body as well. This change from the albinistic to the melanistic phase is comparable to the change from the reddish to the blackish-grey phase in the West African tiger cat above mentioned. Faint spots, however, may often be seen on Siamese Cats, betraying the descent of this breed from a "tabby." It may here be explained that the eyes of Cats differ in colour, but not to the same extent as the coat. Green is the normal tint; but just as albinistic cats resemble blonde human beings in often having blue eyes, so do red Cats often resemble red-haired men in having red, or as fanciers call it "amber," eyes. Amber eyes are also considered by fanciers a sign of good breeding in Cats of other colours.

In addition to the three principal mutations, black, red, and white, other colours may be seen in

domestic cats, but much more commonly at cat shows than in the streets. Such are dark greys or "blues," pale greys or "chinchillas," "creams," and others; but whatever these rarer colours may be, they are all intermediate, in one direction or the other, between black, red, and white; and the evidence derived from mice and flowers confirms the suggestion that "greys" or "blues" are simply dilute forms of black, and cream a dilute form of red.

A marked feature about the cats at cat shows is the number of self-coloured animals that are exhibited. It happens to be the fashion to dislike "pattern." Hence the efforts of fanciers are perseveringly directed towards its elimination. All breeders, however, know what an obstinate and persistent feature it is. In a natural state pattern may disappear in several ways; either by the toning down of the markings to match or almost to match the ground colour, as in desert-living representatives of the African cat (*F. ocreata*), and probably also in adult lions and pumas; or by the darkening of the ground colour and the concomitant lightening of the pattern till the two reach the same tint; or, more rarely, by the breaking up of the pattern into a multitude of small spots or specks which become universally diffused. An approach to this is seen in the so-called Servaline Cat, which is flecked all over with small black specks instead of being blotched with large black spots as in the parent form, the Serval (*F. capensis*). Another good instance of this is seen in a variety of the leopard occurring at

Grahamstown, South Africa, where the multiplication and the fusion of small spots has produced a black animal by a totally different process from that which gives rise to ordinary black leopards. The uniformity in colour of self-coloured cats has probably in most cases been produced by the toning down of the pattern to match the ground colour, or by the mutual alteration of both to the same tint. But one breed known as the "ticked" seems, like the Grahamstown leopard, to owe its origin to the disintegration and general diffusion of the pattern of the Striped Tabby. "Ticked" domestic Cats, which may be met with amongst London Cats, are practically indistinguishable from so-called "Abyssinians." Both appear to be mutations of the Striped, not of the Blotched Tabby.

All the Cats above mentioned, be they "blacks," "blues," "smokes," "reds," "creams," "whites," "skewbalds," or "piebalds," are probably all to be regarded as mutations either of the one kind of tabby or the other. This remark applies both to long-haired and short-haired Cats.

It is possible, but I think not probable, that all English long-haired Cats trace their descent from specimens imported from Persia or Asia Minor. At all events, there is a tradition to that effect; but the truth of it can neither be proved nor disproved. It is quite possible that a long-haired breed was in ancient times fostered in the countries lying near the eastern end of the Mediterranean, just as a special breed has for generations been fancied and rigorously

preserved in Siam. But there appears to be not a particle of evidence that either Persian or Siamese Cats have been derived from species differing from those to which our own English Cats owe their origin. As for so-called "Manx" Cats, there is no doubt whatever that they are tailless varieties of the same type or types. No other pattern than the Striped or Blotched Tabby is found amongst them. When and where the breed arose is quite unknown. Some of those who believe in the specific distinction of this Cat claim Cornwall as its home; others the Isle of Man. Probably both suggestions are as little supported by evidence as the tradition that "Tortoise-shell" Cats came originally from Spain.

What, then, was the origin of our domestic Cats? This has been a question much debated by zoologists; but it cannot be admitted that the debaters had any intimate acquaintance either with domestic breeds or with the two wild species from which they sought to derive them. No author, for instance, except perhaps Blyth, realised that there were two widely different types of pattern in domestic Cats to be accounted for; or perceived that neither of the wild species claimed as the agriotype resembled even remotely the blotched tabby in markings. And none apparently was aware that these two wild species are closely related forms, differing probably less from one another than Chinese differ from west African leopards and hardly more than Mongolian differ from Sumatran tigers.

The two species in question are the European Wild Cat (*Felis sylvestris*) and the African Wild Cat (*Felis ocreata*).*

It is needless to describe these two species in detail and to point out the comparatively small differences that exist between them. The European Cat extends at the present time from Scotland and Spain in western Europe, through central Europe as far as Asia Minor, but does not occur in Scandinavia. The African Cat ranges along the southern shores of the Mediterranean, and is said to occur in Sardinia. The European species is a northern mountain form, with a thick coat and bushy tail; whereas the African Cat is a southern low-country or desert form, with a short coat and a thin tail. In structure, proportions, size, and pattern, these two species closely resemble domestic Tabbies of the Striped type (Plate 1); and since, with the exception of the Jungle Cat (*Felis chaus*), a larger, longer-legged, shorter-tailed species than either of the others and than any domestic cat, there are no other wild species of *Felis* at all like our Striped Tabby occurring in the countries bordering the Mediterranean, the cradle of European and north African civilisations, it is

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* *Felis sylvestris* is almost always called *Felis catus* in works on Natural History; but the original description of *Felis catus* shows clearly that the name was applied by Linnæus to the domestic blotched tabby, and cannot therefore be used for the European wild cat. *Felis ocreata* is better known as the Egyptian or fettered cat, or the booted lynx (*F. caligata*, or *maniculata*), or the Caffre cat (*F. caffer*), the latter being the south African as *F. maniculata* is the north African representative of a species widely distributed on the African continent.

needless to look beyond these two wild species for the ancestor of the domestic Cat in question.

There are, it is true, no records of the taming in past times of the European species, but the African species is known to have been domesticated in Egypt many thousands of years ago. From Egypt it was no doubt introduced eastwards into India and Siam, and gave rise to the striped or nearly self-coloured domestic Cats of the former country and to the peculiar albinistic breed of the latter country. Hence there is no reason to suppose that these Oriental domestic Cats have any infusion of European Wild Cat in their veins. It may be that this accounts for the differences that commonly but not invariably exist between Oriental and European domestic Cats of the Striped Tabby type. From Egypt also the African Cat was no doubt introduced as a tame animal into the countries of southern Europe, where it would come into contact with the European species. However that may be, the evidence that *F. sylvestris* has contributed to the formation of the Striped domestic Tabby is that the latter is seldom exactly like either of its supposed ancestral forms, resembling the European species in some characters and the African species in others. This, however, is a matter of no very great moment. The important point to remember is that the African and European Wild Cats and the Striped Tabby resemble each other in all essential characters and differ from all other species of *Felis* known in the world.

Although historical records show that the Striped Tabby has been a domesticated form in Europe at least from the 16th century—and no one knows how much earlier—and is abundant everywhere both in town and country places, at least in England, many English writers in modern times seem to have been unaware of its existence. Examples introduced into the tropics and run wild have been more than once described and named as new varieties or species allied in the opinion of some authors to the European Wild Cat, in that of others to the African Wild Cat.

Although the origin of the Striped Tabby may be claimed with some assurance as definitely established, that of the Blotched or Marbled Tabby (Plate 2) is at present not only quite unknown, but seems likely to remain for ever a mystery. Several hypotheses may be held with respect to it. It may have arisen *per saltum* as an abrupt mutation from the Striped Tabby and have been capable of preservation by owners and breeders who admired the type, because of its segregability in inheritance. The only reason that can be alleged against the idea of its being a mutation is the fact that no such variation has ever been known to occur in any species of *Felis*. Nevertheless it is a possible explanation of the phenomenon. If it be the true one, the Blotched Tabby must be described as a highly interesting instance of abnormal dimorphism of pattern. It seems quite certain, however, that this pattern has not been preserved by the art of selective breeding, because, with the possible exception of the

tailless so-called Manx breed and of the peculiarly coloured Siamese breed, there seems to be no reason to suppose that selective breeding of Cats was started before the latter half of the 19th century; and we know from Linnæus's description of *Felis catus* that the Blotched Tabby existed as a perfect type in Sweden as early as the middle of the 18th century. This fact disposes of another hypothesis that might otherwise be entertained, namely, that the blotched pattern was developed step by step from the striped pattern by the slow and gradual process of preserving and breeding from slight varieties tending in the fancied direction.

It has also been suggested by an author unaware of the closeness of the relationship between the European and African Wild Cats and of their fundamental similarity in pattern that the Blotched Tabby was the result of crossing individuals of these two species. When two distinct species of striped or spotted mammals are crossed the offspring sometimes resembles neither of its parents in pattern. Professor Ewart,* for example, found that when he paired a male Chapman's zebra with a bay female pony, the foal, while resembling the dam in colour, showed a pattern of stripes not the least like those of its sire, but rather closely resembling those of a totally distinct species of zebra, namely, Grévy's zebra; and believing that the pattern of Grévy's zebra is a more primitive type than that of Chapman's zebra, he considered the pattern of the hybrid foal to be an

* The Penycuick Experiments. A. and C. Black. London, 1899.

instance of reversion to an ancestral type resulting from crossing the two species.* However that may be, the point to be noticed is that the foal differed from both parents. Hence the possibility of the origin of the Blotched Tabby from crossing the European and African Wild Cats. When tested by experiment, however, this suggestion broke down. In the Zoological Gardens I crossed a pure-bred male Scotch Wild Cat with a female African Wild Cat from Uganda, which had never previously kittened. The result was, as I expected, a litter of three kittens exactly resembling the Striped Tabby. Only one of these kittens lived to be half grown. By that time she had almost lost the very distinct pattern of kittenhood, and was becoming daily more like her mother, in which the pattern was evanescent.†

* Mendelian experiments throw a new light on this phenomenon of reversion. A "reversionary character" is a compound one, and is only made manifest when two or more factors react upon each other. If the factors are separated, and one is carried in one individual and the other in another, then the "reversionary character" may remain unrevealed for generations, and will only become patent as soon as two parents which carry between them the complementary factors are mated together. Black mice, rats, and rabbits may be mated to certain albinoes without begetting any grey (brown) individuals among their offspring. Yet, when mated with other albinoes, young ones having the grey or reversionary colour of the wild type will appear. This is due to the fact that colour is a compound character, and that grey is due to the meeting of two complementary factors, one of which is carried by the black parent and the other, in the cases we are considering, by certain albinoes, which carry the factor determining greyness. Other albinoes lack this factor, but carry that which determines some other colour, such as blackness for instance. The reversionary colour greyness can therefore only be produced when an albino carrying the grey determiner is mated to some other coloured partner, such for instance as a black one. But if such a partner be mated with albinoes carrying the black complementary factor, the only colour that will appear among the offspring will be black.—EDITOR.

† P. Z. S., 1908, pp. 749-750, text figs. 194-195.

Another view that may be held is that the Blotched Tabby resulted from crossing some introduced exotic species with the Striped Tabby. But there is no reason to think that, apart from menagerie-kept specimens, representatives of any exotic species, except tamed African Wild Cats, have ever been introduced into Europe. It is also absolutely certain that the Blotched Tabby is not the pure-bred descendant of any species of existing Cat, since its pattern is quite unlike that of any known species of the genus *Felis*.

Finally, it may be held that the Blotched Tabby is the survivor of some species of European Cat which is now extinct as a wild animal. In this connection it may be remembered that complete extinction as wild species has followed the domestication by man of the agriotype or wild form of European cattle, of eastern humped cattle, of the ass, of more than one species or race of horse, according to modern views, certainly of one perhaps of both species of camel, and perhaps of our long-tailed breed of sheep. This extinction of the wild forms of some of our domesticated animals points to the possibility of a like fate having overtaken the ancestor of the Blotched Tabby. However that may be, it seems to me that the origin of this Cat must be explained on the hypothesis either that it is a mutation of the Striped Tabby or that it is the survivor of some species now extinct in a wild state.

From the Mendelian standpoint these two Cats are of the greatest interest, for the following reasons :

They have been living side by side, and freely interbreeding for many generations without ever producing intermediate types, so far as is at present known. At all events, I myself have never seen a tabby Cat that could not be at once assigned to either the Blotched or the Striped type ; and for several years I have carefully noted the pattern of every Cat that has come under my eyes, both in towns and in the country, and I have visited Cats' homes and Cat shows, for the purpose of verifying or disproving the conclusion that intermediates do not exist. Another fact that I can vouch for of my own knowledge is that both types may occur in the same litter of kittens.

So far as I am aware, no attempt at experimental breeding of Cats on Mendelian lines has ever been undertaken ; but from what has been said as to the apparent permanence of the segregation of the two kinds of tabby, it appears that much useful work could be done in this direction. It could be carried out, too, at comparatively small trouble and cost anywhere in the country. Cats are naturally hardy, and require no artificial heat. They should be kept in grass runs, the larger the better, covered in with wire netting. This enclosure should be fitted with wooden shelves and branches, and with a well-made warm wooden covered-in shelter as a protection against cold winds and rain in the winter. The animals should be given water to drink and raw fowls' heads, varied with rabbits, mice and sparrows or other birds to eat ; that is to say, their diet should be made as

nearly natural as possible. Under these conditions the Cats would thrive and breed.

It was for the purpose of drawing attention to Tabby Cats and of thus suggesting the great possibilities of achieving interesting results by breeding them, that the editor of this Journal asked me to contribute an article on these animals. But I have intentionally discussed at some length the other characters, especially colour, by which domestic breeds of Cats are distinguished, and have mentioned one or two curious facts, such as the correlation of of " red " and " tortoise-shell " with male and female sex respectively ; so that these features, as well as pattern, may be considered in any experimental breeding that is undertaken in the future.

CONTRIBUTED ARTICLES.

INHERITANCE IN RACE HORSES.

Coat Colour.

By ROBERT BUNSOW.

PREVIOUS publications* have shown that bay[1] coloured thoroughbred horses may be either pure with regard to the hereditary transmission of this colour, and will therefore transmit no other colour to their offspring, or they may be impure and will transmit some other alternative colour, such as chestnut. Since bay horses may thus carry in their hereditary mechanism some colour which is not visibly manifested in the presence of the bay colour, we speak of this latter, in Mendelian language, as a dominant colour, while chestnut is spoken of as a recessive.

I have just now used the term "hereditary mechanism." What is meant by that? I will endeavour to explain. The cells that form the tissues of an animal or plant which reproduces itself by sexual processes may be divided into the body or somatic, and the reproductive, sex or gametic cells. It is these latter which, in sexual reproduction, constitute the hereditary mechanism. They

* C. C. Hurst. Proceedings Royal Society, Vol. 77B, 1906.

[1] For the purposes of this article the author has used the word bay to include browns. There is no absolute distinction between these colours. Some horses in winter are bay and brown in summer, and some are *vice-versâ*. Others have a colour which cannot be designated by either bay or brown, and in the Stud Book are described as "bay or brown."

are the carriers of the qualities which one generation transmits to another. Now the most essential feature of the Mendelian conception of inheritance, is that of any pair of alternative characters, such as bay and chestnut colours, each sex-cell can carry only one of the pair.

It does not carry both of them blended together, like wine and water, nor mixed like oil and water. The two characters are regarded as being carried in different sex-cells. If, therefore, a given horse carries only chestnut colour, all its sex-cells will carry that colour—or rather the factors which when present in the body cells produce that colour. But if it carries both bay and chestnut, then one-half of the sex-cells of the individual will carry the one colour, and the other half will carry the other colour.

If we remember that every individual is the product of the fusion of two sex-cells, one being derived from a mother and the other from a father, we must regard each of them as a sort of double mechanism with regard to every one of the characters that make up its body. It is, for instance, a bay colour because its bayness has come from the sex-cells of both father and mother, or because it came from one only, the sex-cell of the other parent contributing chestnutness, which is not manifested if bay is already present in the body cells.

Now, we may symbolise these facts and conceptions by calling the bay colour a dominant character

over chestnut colour, which we call a recessive character. If we symbolise the dominant character by D and the recessive one by R, then each letter will represent a group of sex-cells. A chestnut coloured horse since it carries nothing but chestnut, will, therefore, have only one kind of sex-cell with respect to this character, and its gametic constitution will be represented by R R. Similarly, a pure bay will be represented by D D. But a hybrid or impure bay will contain two classes of sex-cells in equal numbers, namely, those carrying bay and those carrying chestnut. Such an individual will be gametically represented by the symbol D R.

Now if a pure D D bay stallion be mated with a pure R R chestnut mare, all the foals will be impure D R bays, because the D sex-cells of the stallion can in reproduction meet nothing but the R sex-cells of the mare. But if an impure D R stallion is mated with an R R chestnut, then the foals will in the long run consist of both bays and chestnuts in approximately equal numbers. But the bays will be impure or hybrid $D R^{s}$. In this case the two kinds of sex-cells of the stallion have two possibilities, for its D sex-cells may meet the R sex-cells of the mare, and so also may its R sex-cells. There thus result two unions, a D R or hybrid, and a R R or pure recessive.

One further feature is of interest. It will be observed that a chestnut horse is one which manifests a recessive colour, and must be therefore always pure with regard to this colour; that is,

always R R in gametic constitution. When chestnuts are therefore bred together, their foals should be always chestnut. If bred with bays their foals may be either all bays, or half chestnuts (R R) and half bays (D R), as we have just seen. In the former case, the result is due to the fact that the bays mated with them are D D and in the latter case because they are D R.

To the (D D) class of bays and browns belong "St. Simon," "St. Serf," "Galopin," "Ladas," "Merry Hampton," and "Cabin Boy"; to the (D R) bay and brown class "Royal Hampton," "Donovan," "St. Angelo," "Isinglass," "Orvieto," "Ayrshire," "Florizel II.," "Pioneer," "Isonomy," "Melton," "Wisdom," "Rose Window," and "St. Maclou."

In the records of the "General Stud Book of Race Horses," are found some exceptions to the rule that chestnuts crossed with chestnuts give chestnuts only, and that (D D) bays always breed true. These apparent exceptions it is worth while to consider. In reality they do not exist, but are either misprints or errors of entry. It is of course not only difficult, but sometimes impossible, to satisfactorily clear up erroneous entries. But in most cases inquiry reveals them.

If we turn to Volume XXI. of the Stud Book, which was published six months ago, we find it contains fifteen so-called exceptions to the rule that chestnut parents have only chestnut foals, namely—

Page.	Year.			
12 ...	1904 ...	bay filly	out of	The Alabama
25 ...	1904 ...	,,	,,	Anavene
33 ...	1900 ...	,,	,,	Arcadia
40 ...	1899 ...	,,	,,	Ashtwig
65 ...	1898 ...	,,	,,	Bella Valley
106 ...	1904 ...	,,	,,	Burganilda
121 ...	1904 ...	,,	,,	Cassimere
247 ...	1903 ...	bay colt	,,	Evening Flight
327 ...	1902 ...	,,	,,	Good Day II.
327 ...	1903 ...	,,	,,	Good Day II.
381 ...	1904 ...	bay filly	,,	Inquisitive (Stale News)
552 ...	1898 ...	,,	,,	Merry Lass
583 ...	1901 ...	bay colt	,,	Mrs. Candle
611 ...	1902 ...	bay filly	,,	Nenemoosha (Cyanean)
615 ...	1899 ...	bay colt	,,	Nimblefoot

In addition to the English Stud Book, the German Stud Book contains only one exception. It is there stated that from the mating of the chestnut stallion "Botschafter I.," out of the chestnut mare "Legality," there resulted the bay foal "Longobarde," which died when it was six months old. As soon as I drew attention in the German sporting newspaper *Sportwelt* to this erroneous statement, pointing out that the entry certainly must be a mistake, the breeder of "Longobarde," namely, the Prussian Government's official at their Stud, at Graditz, came forward with a published statement that "Longobarde" never was a bay, and that neither the veterinary surgeon who assisted the

mare when foaling nor any of the Studwarders had the slightest doubt that this colt had always been a chestnut and that nobody could explain how this misstatement came into the Stud Book.

Now, looking into the fifteen exceptions given above, in the first place I would point out that it is very often an impossibility to state exactly what colour a recently born foal is, for it may look exactly like a bay—with the exception that it has no black tail and black mane—and yet after having shed its first woolly coat it may develope into a real chestnut. There is another fact worthy of notice. The entry of a foal's colour into the Stud Book may be made before it sheds its first woolly coat, and if its coat changes at its first or later moult, this entry obviously becomes erroneous in as far as the real or definitive coat-colour is concerned. It is only in very few cases, and never if the foal happens to die before it is put into training, that anybody troubles to alter the erroneous entry in the Stud Book. It remains there as it was originally entered. But even after a foal has been put into training, there are still some cases in which the original entry of the first or foal colour remains as it was. Even the Racing Calendars, which give in their index the colours of the horses which have performed on a racecourse, still preserve the erroneous entry. The reason for this is largely a clerical matter. The man who compiles the index is a member of the office staff, he never comes to the racecourse, and has only one book from which he can obtain the

record of the colour of different horses, namely, "The General Stud Book." So he transfers the old error on to the new print, and if somebody is not personally interested in the colour of the special horse the error remains and becomes a pitfall in the hands of persons who are not practical racing men. In this way some supposed exceptions to Mendelian inheritance of coat-colour in horses have been used in an effort to disprove the operation of that law in this character of horses. This has been done in spite of the fact that these supposed exceptions do not constitute more than two per cent. of the whole, and in some cases even less.

For years I have endeavoured to investigate and correct such erroneous statements, but have had to relinquish it as hopeless in all cases where the foal was born dead, because very often nobody was to be found who remembered the colour of such a foal. In many cases I received an answer to this effect: "I took it for a bay, but of course it might as well have been a chestnut, for you cannot tell at this age, and there was no reason to trouble about it because the foal was dead."

But in all cases, when the foal was still alive and older than half a year, it was easy to show that a mistake had been made, and the easier of course if it had been raced. From the fifteen so-called exceptions only two appeared on a race-course. One of them is "Cyanean," bred by Lord Londonderry in 1902. In the Stud Book of 1905 (Vol. XX.) it is given as a bay, and even in the

Stud Book of 1909 (Vol. XXI.) the same colour is given, in spite of the fact that it ran always as a chestnut and never was anything else but a chestnut. Even the indexes of the various Racing Calendars give it as a chestnut.

The second case is "Stale News," alleged to be a bay coming from a chestnut stallion out of a chestnut mare. In spite of all inquiries I could get no explanation, and it appeared to remain a fact that "Stale News" was really a bay. But a few days ago the enigma was solved in a very simple way: The dam of "Stale News" is no chestnut at all, but a *bay* mare, and so there is nothing extraordinary in her daughter's bay coat. These errors never happen in Stud Books which are specially kept for the breeding of chestnuts only. For instance, we shall find none in the Suffolk horses, which are chestnut without exception; nor in the Prussian half-bred Stud Trakehnen, where a chestnut herd is kept, has such a case ever happened. I am informed by Herr von Oettingen, the Governor of the Stud, that a stud book for the chestnut herd has been kept for over one-hundred-and-fifty years.

So it is, I think, quite proven that chestnuts crossed by chestnuts always give chestnuts, and that (D D) bays crossed by chestnuts always produce bays.

One of the best known (D D) bays was the famous thoroughbred stallion "St. Simon," who died two years ago. All his progeny were bays or browns, with the exception of the very last one,

which was born after his death and was called "Postumus" (Plate 1), and is now in training at Kingsclere. This colt was and is grey.

For breeders of race-horses "Postumus" is a specially interesting horse. First of all, it was thought by some that "Postumus" did not fall into line with the Mendelian principles, and that even a pure (D D) bay parent did not breed true. But they overlooked the very important fact, that the pureness of the (D D) bay character only holds with regard to bayness and to colours recessive to it, and that it can be best tested by mating it with a chestnut. The question at stake is simply this: Is the bay colour of this stallion dominant to the chestnut colour? It is answered by the fact that "St. Simon's" offspring out of chestnut mares were invariably bays. So it is proved that "St. Simon" was (D D) for bay, since were he a (D R), that is carrying the dominant (D) bay colour and also the recessive (R) chestnut colour, he would have produced, as some other bays which are ($D R^s$) have done, chestnut foals as well as bays.

If "St. Simon" is thus a pure bay, the greyness of his last foal "Postumus" is of some interest. Among horse breeders, it has been attempted to frame an explanation of the appearance of this grey foal upon the fanciers' old conceptions of inheritance. We will first deal with these attempted explanations, and then view the matter from the Mendelian standpoint. We shall see how well the Mendelian interpretation harmonises with the facts in the

pedigrees, while the other interpretations leave them without coherency or relationship.

We will deal with Mr. B. Robertson's view first. He is an authority upon race-horses, and he thinks that the grey colour of "Postumus" came through his sire "St. Simon" from the distant ancestry of the mare "Grey Wilkes." He believes that there is fair proof for this statement in the fact that other horses not coming from "St. Simon," but having "Grey Wilkes" in their pedigree, show grey hairs in their coat, or at the root of the tail. In my opinion this view is not tenable, because "Grey Wilkes" lived in 1707, two hundred years before "Postumus," and her name appears only in the fourteenth remove of "St. Simon's" pedigree. It is possible, of course, that some posterity of "Grey Wilkes" may show some grey hairs, but "Postumus" is another case altogether, for it is a good and complete grey colt, and not a brown or bay, with a patch of grey.

A very prevalent idea among horse breeders with regard to cases like "Postumus" is that its grey coat colour is the result of the "nicking" or meeting in his sire "St. Simon" of two grey stems, which can be traced down on either side of its pedigree. Upon the basis of this hypothesis, the greyness of "Postumus" will be traced (Plate 1) on the dam's side through "Pontillon," "Maid of Wye," "Vedette," and "Mrs. Ridgway" to "Nan Darell,"* and also upon the sire's side through "Vedette" and "Mrs Ridgway" to "Nan Darell."

* Nan Darell is a grey mare and is outside the pedigree on the dam's side of it.

But after a careful study of the matter I am convinced that the hypothesis of "nicking," as we generally understand it, cannot adequately explain the facts of the pedigree. I will proceed to explain why.

A general consideration of the parentage of grey horses shows that a grey horse can never be obtained unless one of the parents is a grey. No exception to this statement is known. Another fact throwing considerable light upon the general principles of colour inheritance in horses is that two grey parents need not necessarily have all grey offspring, and that frequently they do produce other coloured foals. These two facts thus point to the conclusion that grey is dominant to all other coat colours, because if it were recessive a grey dam and sire could not produce other coat colours among their offspring. Also a dam and sire of some other colour would beget grey foals, but this is not the case.

If grey be thus a dominant colour, it is clear that "St. Simon" could not have received any grey strain from "Galopin," or "Vedette," or "Mrs. Ridgway." The grey colour of "Nan Darell" was in fact lost in the roan "Mrs. Ridgway," and it does not re-appear in "Vedette," "Galopin," or "St. Simon." Clearly, then, "Postumus" does not derive his grey from the sire's side of the pedigree. The hypothesis of "nicking" is therefore untenable.

Turning next to consider where "Postumus" got his grey colour from, and why it was that "St.

Plate 1.

Postumus (*c*) (1908)	Pontoon (D R) (*m*)	Pontillon (*m*)	Maid of Wye (*m*)	Euxine (*m*)	Varna. *m.*
					King Tom. *s.* (*bay*)
				Vedette (*s*) (*brown*)	Mrs. Ridgway. *m.* (*b. roan*)
					Voltigeur. *s.* (*brown*)
			Fernandez (*s*) (*bay*)	Isola Bella (*m*) (*bay*)	Isoline. *m.* (*bay*)
					Stockwell. *s.* (*chestnut*)
				Sterling (*s*) (*bay*)	Whisper. *m.* (*bay*)
					Oxford. *s.* (*chestnut*)
		Orvieto (*s*) (*bay*)	Napoli (*m*) (*bay*)	Sunshine (*m*) (*bay*)	Sunbeam. *m.* (*bay*)
					Thormanby. *s.* (*chestnut*)
				Macaroni (*s*) (*bay*)	Jocose. *m.* (*bay*)
					Sweetmeat. *s.* (*brown*)
			Bend Or (*s*) (*chestnut*)	Rouge Rose (*m*) (*chestnut*)	Elleen Horne. *m.* (*chestn.*)
					Thormanby. *s.* (*chestnut*)
				Doncaster (*s*) (*chestnut*)	Marigold. *m.* (*chestnut*)
					Stockwell. *s.* (*chestnut*)
	St. Simon (D D) (*s*) (*bay*)	St. Angela (*m*) (*bay*)	Adeline (*m*) (*bay*)	Little Fairie (*m*) (*chestnut*)	Lacerta. *m.* (*bay*)
					Hornsea. *s.* (*chestnut*)
				Ion (*s*) (*bay*)	Margaret. *m.* (*bay*)
					Cain. *s.* (*bay*)
			King Tom (*s*) (*bay*)	Pocahontas (*m*) (*bay*)	Marpessa. *m.* (*bay*)
					Glencoë. *s.* (*chestnut*)
				Harkaway (*s*) (*chestnut*)	Fanny Dawson filly. *m.* (*chestnut*)
					Economist. *s.* (*bay*)
		Galopin (*s*) (*bay*)	Flying Duchess (*m*) (*bay*)	Merope (*m*) (*bay*)	Velocipede's dam. *m.* (*bay*)
					Voltaire. *s.* (*brown*)
				Flying Dutchman (*s*) (*bay*)	Barbelle. *m.* (*bay*)
					Bay Middleton. *s.* (*bay*)
			Vedette (*s*) (*brown*)	Mrs. Ridgway (*m*) (*bay-roan*)	Nan Darell. *m.*
					Birdcatcher. *s.* (*chestnut*)
				Voltigeur (*s*) (*brown*)	Martha Lynn. *m.* (*brown*)
					Voltaire. *s.* (*brown*)

All grey horses in this pedigree are coloured: ☐

c = Colt. *m* = Mare. *s* = Stallion.

Simon," who had sired hundreds of foals, but all of them bays or browns, and never begot a grey until "Postumus," his last foal, came. We notice that along the whole of the maternal line of "Pontoon's" pedigree (the dam of "Postumus"), there is an unbroken stem of grey (Plate 1). We further notice throughout this stem that every grey mare—"Euxine," "Maid of Wye," "Pontillon," and "Pontoon"—is the offspring of a grey crossed with a bay. That is, every grey horse must have one parent grey.

Now these and other indications are clearly Mendelian phenomena. "Pontoon," the dam of "Postumus," is a Mendelian heterozygote, or hybrid, carrying dominant grey and recessive brown or bay, and possibly recessive chestnut too. The sire of "Postumus," namely, "St. Simon," has shown by his numerous offspring that he is pure for bay.

The mating out of which the unexpected grey colt, "Postumus," was produced, therefore resolves itself into one of "Pontoon," which is D R for grey (D) and bay (R), with "St. Simon" (D D) for bay. Now if grey is dominant, the Mendelian expectation in such a mating is an equal number of heterozygote or hybrid greys (D R) and of bays (D D). And it so happens that a grey has come first. Unfortunately "St. Simon" is dead, and the same pairing cannot be repeated. But it is to be hoped that "Pontoon" may be mated on several occasions to a sire known to be homozygous

for brown or bay. That will afford a further opportunity to test the Mendelian interpretation of this case.

We need not, however, wait for this future mating, because Mr. Huby, the stud groom to the Duke of Portland's stud at Welbeck, tells me that "Pontoon" had a foal previous to "Postumus," by "Gold," and that it was brown. Now for a grey horse like "Pontoon" to have a brown foal by any kind of mating, shows that it is not a pure grey horse (in Mendelian language is not homozygous for greyness) but is carrying in its sex-cells brownness as well as greyness. The assumption, therefore, which the appearance of "Postumus" out of "Pontoon" by "St. Simon" suggested, that "Pontoon" is a D R, where D stands for dominant greyness and R for a colour recessive to it, such as brown or bay, is thus demonstrably proved by the nature of her matings and of her resulting offspring.

"Top Hane," another interesting grey horse, appears to have the same gametic composition as "Pontoon." She is a foal of the grey stallion "Le Sancy," out of the bay mare, "Distingué." Consequently she will be a D R for greyness, and half of her sex-cells will in that event carry the factors for the grey character, and the other half will carry them for the brown or bay character. Both brown and bay, in addition to grey offspring, may therefore be expected among her offspring if she is mated with a sire like "St. Simon," who is

D D for brownness or bayness. "Top Hane" was so served by "St. Simon," and produced a filly foal called "Tsu Shima," which was born a grey, and is so entered in the Stud Book. But she changed at the first moult and became a brown, and has so remained. It will be interesting to note next the colour of "Top Hane's" other offspring, when crossed with other D D browns or bays. As taken from the Stud Book, they are as follows :—

In 1904, a grey filly, "Banzai," by "St. Frusquin," a D R brown.
In 1905, a grey or brown filly "Tsu Shima," by "St. Simon," a D D bay.
In 1906, a grey filly "Topaz," by "St. Frusquin," a D R brown.
In 1907 and 1908, barren.
In 1909, a grey or brown colt by "Misselthrush," a D D brown.

The colt of 1909, by "Misselthrush," which is entered as a grey *or* brown, is evidently like "Tsu Shima." Both were born grey, but turned brown at the first moult.

We have thus good reason to regard the two grey horses, "Pontoon" and "Top Hane," as what we may for the moment call impure greys. They are externally grey, but we are led to regard them as carrying, in addition to greyness, some recessive colour such as brownness or bayness.

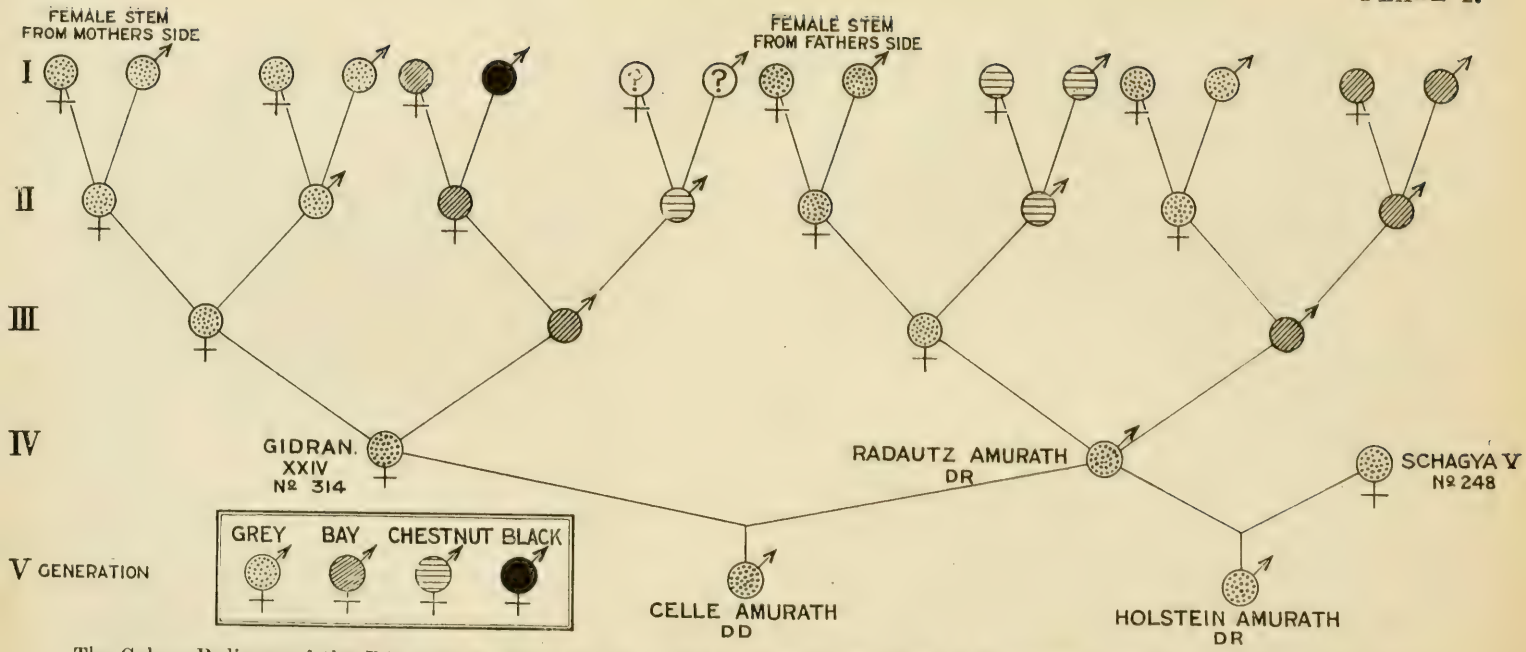
In connection with grey horses the next question which we naturally ask is: "Do pure greys exist?" By this I mean are there grey horses which transmit

no other colour than the dominant greyness, so that whatever kind of mating they may be a partner in, their offspring, without exception, will be grey. In other words, are there grey horses whose sex-cells *all* carry the factors that produce greyness in the coat? Greyness being, as we have reason to believe, a dominant character over black, chestnut, brown, and bay, it follows that in any mating of grey horses, with those of either of these other colours, the sex-cells of the pure grey parent, being carriers of grey alone, must necessarily introduce the dominant colour grey into one side of all such matings, and grey will result. I think I can produce such a case from one of the most scientifically kept Studs in the world, namely, that of Celle belonging to the Prussian Government. In this Stud every detail is accurately recorded by properly trained experts.

The case I have in view is that of a ten year old white Arabian stallion, the "Celle Amurath." This horse, in England, would be called grey, but white in Germany. Its coat is white, but its eyes, mane, tail, and feet are black.

"Celle Amurath" has covered about six hundred mares of *all* colours, and his numerous offspring, without exception, have been greys and whites. There can therefore, I think, be little doubt that he is a pure white (grey) and is D D for whiteness (greyness), carrying only this colour character.

When we examine his parentage and more remote ancestry, our conclusion as to his Mendelian



The Colour Pedigree of the D D grey Arabian stallion, "Celle-Amurath" and of his D R brother, "Holstein-Amurath."

"Celle-Amurath" has covered about six hundred mares of every colour, and his offspring have been invariably white. His half-brother, "Holstein-Amurath," has sired both whites and bays among his offspring.

This difference in the nature of the offspring of the two horses indicates very clearly that the one ("Celle-Amurath") is a pure or D D white, and the other ("Holstein-Amurath") an impure or hybrid D R white.

The generations are numbered I. to V. in the reverse direction to that usually adopted by horse breeders, who speak of the youngest generation as the first, the parental generation as the second, and so on.

gametic constitution is fully corroborated (Plate 2). Following the maternal line we find that grand, great-grand, and great-great-grand maternal ancestors were all grey. On the paternal side, seven of the ancestors and the sire, "Radautz Amurath," were whites (greys). Now, from the Mendelian standpoint, the nature of the maternal parentage was such, that the mother herself must have been an impure grey, that is, was D R for greyness, carrying in one-half of her sex cells the factors for greyness, and in the other half the factors for bayness probably, while those for chestnut are not necessarily excluded (Plate 2). On the paternal side, the father was also an impure grey, because his parents were a grey mare and a bay stallion. He was, therefore, carrying in one-half of his sex-cells the factors for greyness, and in the other half those for bayness.

The parents of "Celle Amurath" are probably thus both gametically similar, in that both are carrying sex-cells some of which bear greyness and others bayness. "Celle Amurath," from the Mendelian standpoint, is thus the product of the meeting of a grey-bearing egg-cell of the grey mare with a grey-bearing sperm-cell of the grey stallion. His gametic or sex-cell constitution is thus G G if we symbolise the actual colour, or D D if we symbolise the fact that greyness is the dominant colour.

That this Mendelian interpretation of the gametic constitution of "Celle Amurath" and of his parents may be regarded as correct is further corroborated

by the nature of the offspring of one of his half brothers now serving in another Prussian Stud, at Holstein. This brother is named "Holstein Amurath." (Plate 2.)

Now we desire to ascertain the gametic constitution of this stallion by a reference to his parents' origin, and from the gametic behaviour* of his brother, "Celle Amurath." We have already seen that the brother is a pure grey, because all his offspring (six hundred) have been grey. We also saw reason (p. 91) to regard "Radautz Amurath," the sire of "Celle Amurath," as an impure grey, bearing in his sex-cells greyness in some and bayness in the others. That being the case, the Mendelian expectation for his offspring, when he is mated with another impure (D R) grey, such as we saw the dam of "Celle Amurath" is, will be one-half pure greys and the other half impure greys.

Now impure greys, carrying greyness and bayness, can be tested by mating with bays. In that event, both whites (greys) and bays are expected in the offspring. "Celle Amurath's" half brother, "Holstein Amurath," was thus mated and his offspring are some whites and some bays.† The Mendelian prediction is thus fulfilled in the actual results in so far as both expected types appear.

In conclusion, I would like to add that I do not treat the study of Mendelism as a hobby, but try to get some deductions out of Mendel's laws which

* Measured by the nature of his offspring.

† The number of each is being ascertained from Germany, and will be published in the next number. (EDITOR.)

we can apply in horse-breeding, where we are sadly in need of correct laws, because all we have done up till now is simply to wander about in utter darkness. None of the existing so-called laws in horse-breeding can stand a trial before modern biological researches. They are all based on statistics got from the results on the racecourse, or from the degree of inbreeding in pedigrees, and from old conceptions largely based on fancies. But, by statistics, one might conceivably prove that a horse cannot win a Derby if his tail is not at least two feet long, or if his jockey has not fair hair, for statistical coincidences are not necessarily biological analyses. It is not my object now to go further into this question, but it will suffice for me to point to the fact, that about three thousand blood-horses are born year by year in great Britain, and that two thousand seven hundred of them are quite useless for racing purposes. I believe that English breeders are very much interested in Mendelism, and I can say the same of continental breeders, because when I published last November a series of articles in German sporting papers about the Mendelian laws and their application to horse-breeding, I was simply inundated with letters from all parts of German-speaking countries. These letters did not only come from horse-breeders, but from Professors of Universities, Gynækologists, and Biologists. So the interest exists, it only requires developing, and I look to the *Mendel Journal* as one of the best means to further this development.

Editorial Note to Mr. Robert Bunsow's Article on Inheritance in Race Horses.

It has been our good fortune to have had several long interviews with Mr. Robert Bunsow. In the racing world Mr. Bunsow is a well-known person. In this country he represents many foreign horse-breeders and the interests of several continental sporting papers.

His article presents several interesting matters for consideration. In order that we may observe them in their proper perspective two points need to be remembered. In the first place, Mr. Bunsow has, during the greater part of his career, been in practical and every-day contact with all matters relating to the breeding of race horses. His statement, therefore, that a certain percentage of errors creep into the Stud Books and Racing Calendars, and the reasons which he assigns for this, are therefore of the greatest value.

The most serious error, it seems, that is liable to arise in the keeping of the Stud Books is an insufficient attention to the fact that horses in many cases change their coat colour as they get older. In this way the divergence of description between the Stud Books and the Racing Calendars, whenever they occur, can be accounted for. Let

us take the case of "Tsu Shima," mentioned in Mr. Bunsow's article. She is entered in the Stud Book as a grey, but Mr. Huby, the stud groom of the Duke of Portland's stud, at Welbeck, in a letter to Mr. Bunsow, says: "When I saw her last she was not grey at all, although she had a tuft of grey hair at the root of her tail. She is now a brown mare." The case of "Postumus" is interesting in the same way. He was born a brown colour, and Mr. Huby says: "It was only in the Autumn of the year he was foaled that he became grey. He is a good grey now, and every time he sheds his coat he will become whiter still; by the time he is five or six years old he will be white. His dam is now white, and previously always had been a good grey." This statement of Mr. Huby's is interesting because it appears to express, in the form of a prediction, the result of his experiences. It suggests that in his experience grey horses in some cases turn whiter with each moult, until they become what are called white horses. It is further of still greater interest, because it suggests that this capacity of *changing* colour is itself hereditarily transmitted. That suggestion is intimated in the statement that "his dam was previously a good grey and is now white," and in that relating to her son, "that he is a good grey now and will become whiter with each moult." It is a matter of some importance to note whether this change in "Postumus" will actually occur.

The question of colour-change with age is one worthy of a fuller investigation. It appears not to

be a very simple matter; at least, not at present. But Mendelian investigations tend to considerably simplify these problems. Mr. Bunsow leads us to understand that grey horses may be born all colours, though most of them are born black with sparsely scattered white hairs. It seems that it is very seldom they are born actually grey in colour. Another fact which we have learned from Mr. Bunsow is that some grey foals may become white in their second year, while others do not become so until later years.

This colour change in animals and in the flowers of some plants with advance in age* is a familiar fact. It is so much so, that its significance seems not to have received that degree of study which it deserves. It is, we may suppose, conformable to the appearance of the antlers in a stag, and to the beard in man, both of which appear relatively late in life. Grey rats are born colourless, with the exception of the eyes; their skin then turns black and the hairs when very small are black, subsequently passing through grey-black to greyness (brown). The same phenomenon, we understand from Mr. Hurst, is seen in rabbits.

This change of colour in grey horses, and the difference in time at which the complete change occurs, suggests that grey horses are not all alike. Mr. Bunsow has already dealt very fully with that, and has shown that some grey horses are pure for

* The term age is here used wholly in a relative sense. A period of a day or a few hours in flowers may constitute the transition from full bloom to faded and withered petals.

greyness and will beget none but grey offspring, while others are impure and may beget both greys and bays among their foals. But we think there may possibly be a difference more than can be expressed in the unqualified symbols D D and D R. It is conceivable that a D R grey horse carrying greyness and bayness may be foaled of a different colour to a horse carrying greyness and chestnutness, or greyness and roanness. Moreover, some factor, which may be independent of any colour factor, may exist, which determines not the colour but the precise stage at which the colour-change will occur.

There is one statement in Mr. Bunsow's article which calls for some comment, from the national standpoint. It is that relating to the scientific accuracy with which the details of the Prussian Government's Stud Books are kept. The matter of horse-breeding in Germany is one which is dealt with on as sound and as scientific a basis as possible. We should hesitate to say that it was perfect in all respects ; but there is no question, we think, that their system is more precise and accurate in its records than ours. The details of their work appear to be done with the characteristic Teutonic thoroughness. There is little doubt either that as soon as the significance of the Mendelian principles has been fully grasped and their application to the breeding of coat colours in horses has been demonstrated—as we hold to be the case—those who are officially responsible for the conduct of the Prussian Government's

Studs will proceed to investigate the Mendelian transmission of other qualities, such as nerve strength, staying power, and capacity for speed. One problem already awaits investigation either by the Prussian Government or by English private breeders. It is the question of "nervousness" in horses. There is current in racing circles a belief that much inbreeding results in "nervousness." We believe it may be possible to closely inbreed and yet by judicious matings, based upon Mendelian principles, to rear horses that are not "nervous." We cannot say dogmatically it will be so. There is apparently very little data available, and the question is one of careful scrutiny of such facts as we already possess and of future observation, definitely directed to the particular question. But there is no real reason to doubt that it is possible to interbreed and yet to avoid, by an early elimination, any undesirable character that may have made its appearance. But in this matter, as in others, an ounce of experiment is worth a ton of essay-writing.

"Pontoon," from the Mendelian standpoint, is an interesting horse. So far as Mendelian investigations have gone, we know that chestnut is recessive to bay, brown, and black. Any horse of these latter colours may therefore carry chestnut recessive. But a point which has not yet been investigated in horses is the relationship of these colours to each other in reference to their relative dominance. We do not know whether brown is

dominant to bay. There are some facts, Mr. Bunsow tells me, which suggest that a brown horse can carry brownness, bayness, and chestnutness. This possibility leads us to consider the gametic possibilities of a grey horse. If grey is dominant to all other colours, as it appears from Mr. Bunsow's pedigrees to be, then grey horses may be of many gametic classes Some may carry blackness and brownness recessive, others blackness and bayness, others brownness and chestnutness, others roanness and brownness, and so on. Or, they may even carry three or four different recessive colours in their sex-cells. We may therefore expect that when finally the offspring of all the different possible matings of grey horses with those of the other colours have been adequately noted, they will present a polychromatic display.

The fact described by Mr. Bunsow on p. 88, of two horses which were born grey, a dominant colour, and later turned brown, a colour recessive to grey, is one which it is hoped, in the light of Mendelian knowledge, will be more fully examined. These two horses were "Tsu Shima" and the colt of 1909 by "Misselthrush."

At first sight this fact appears to be quite inconsistent with any idea of dominance or of the conception of the purity of gametes carrying recessive characters. The phenomenon, however, is not altogether new in Mendelian experiments. In poultry two sorts of "whites" have been detected. One of these is a recessive white and the

other a dominant. The former is a true recessive, and is manifested as a white because no colour is present. But the latter is, as it were, a false white. It is so, not because colour is absent, but because another factor is present which inhibits the manifestation of the colour in the body tissues.*

It is therefore conceivable that two sorts of bays and browns among horses may exist. First, those which are true recessives to greys because they carry nothing but bayness or brownness. Second, those which carry greyness in addition ; but in these cases, to the greyness there is superadded a third factor, which, meeting it in the body tissues, inhibits its developement and allows the recessive or underlying bayness to manifest itself.

If we let B stand for the factor producing bay colour, G for that producing greyness and g for its absence, and B^1 for that producing inhibition of G and b^1 for its absence, then we can symbolically represent the relationship of grey to the two postulated types of bay horses as follows :—

Bay colour - - = B g b^1
Grey colour - - = B G b^1
Bay colour following grey = B G B^1

There is yet a third possible type of bay or brown horse. Its early greyness may be simply due to a late developement of its normal colour.

The bay horse of the composition B g b^1 may be regarded as a true recessive bay and is bay because it

* For fuller information about these and for a general discussion of colour phenomena see "Mendel's Principles of Heredity," by Prof. W. Bateson, p. 102, Cambridge University Press.

carries no grey factor. But the bay horse of the composition B G B^1 is not a true bay because it also carries the dominant colour grey. It is bay in colour only because another factor B^1 is present which inhibits the developement of the grey colour. The horse of the composition B G b^1 will be a grey colour because it carries the grey factor which is dominant over the bay one, and the inhibiting factor B^1 of grey being absent, grey can accordingly manifest itself.

One feature of some interest and of great practical importance remains for consideration. The evidence is quite clear that the transmission of coat colour in horses, so far as it has been investigated, is in accordance with Mendelian principles. Mr. Hurst's analysis of the Stud Books and Racing Calendars, and the pedigrees, which are given and described in Mr. Bunsow's article in this Journal, all tend to that conclusion. But as a very practical nation—perhaps too exclusively so—we naturally enquire as to whether these principles of Mendelism apply to other and more important qualities. Do they, for instance, hold equally true for capacity for speed, for nerve and muscle endurance, for great strength, and for some of the various diseases to which horses are liable? It is quite within the bounds of probability that they will be found to so apply. These qualities are of a physiological and pathological nature, and there are already cases known where such qualities appear to segregate as distinctly as structural ones

do. Indeed, coat colour itself is rather more a physiological attribute than a structural one.

Mr. Robertson has made some attempt in this direction, but his cases, though very suggestive, appear to require more critical analysis than he has at present given them. But as far as they go, they do seemingly point towards a Mendelian segregation of pathological traits.

A matter of a few years would be sufficient to decide the mode of inheritance of some of the physiological and pathological characters of the horse, provided that the investigation was adequately wide and conducted with scientific accuracy.

Biological Iconoclasm, Mendelian Inheritance and Human Society

A PLEA FOR THE OPERATION OF A MORE VIRILE SENTIMENT IN HUMAN AFFAIRS.

A Criticism

By LOUIS COBBETT, M.D.
(Lecturer in Pathology, Cambridge University.)

In his very vigorous address printed in the opening number of this journal Mr. George Percival Mudge attacks the modern sentiment which is guiding our community, and argues that it tends to "preserve and procreate the unfit citizens, and hamper and discourage the fit." "The whole influence of this modern sentiment," he says, "is tending in the wrong direction." By our hospitals, asylums, and workhouses we are interfering with the law of the destruction of the unfittest. "A continuation and extension of this sentiment will lead the nation to its destruction."

All this is based upon a study of the biological laws of inheritance, and particularly on Weismann's doctrine that acquired characters cannot be transmitted. Consequently, do what we will to improve the health of the mind or the body, we can but influence the individual; when he dies the results of our efforts will perish also, for they cannot be transmitted to his progeny. Hence it is of no

permanent use to educate a gutter child, or to cure an epileptic, because the children will have just the same propensities as though their parents had been utterly neglected. Moreover, such conduct does positive harm, because if the potential parents had not been cared for, the children might not have been born.

I do not think I overstate Mr. Mudge's case. He says himself: "The whole influence of this modern sentiment is tending in the wrong direction. It sets out with the belief that it can effect the salvation of the unfit by improving their environment. It will end by achieving the destruction of the fit, without having even gained the salvation of the unfit." Again: "It is time we learned how little environment can do, and how much the inborn qualities determine all for us."

Among much which is admirable in Mr. Mudge's able address I cannot help feeling that he has kept out of sight a very important factor, namely, the possibility of creating a permanent environment; for even if we admit to the full Weismann's doctrine that there is no biological inheritance of acquired characters, nevertheless there may be a sociological inheritance of the new environment which becomes associated with those acquired characters; and the inheritance of this new environment may be, for all practical purposes, as good as the inheritance of the acquired characters themselves. Let me explain what I mean. If we take children from a slum

environment, train and educate them to become well-conducted, self-supporting citizens (the possibility of which I think Mr. Mudge will be willing to grant) their children, though biologically they may inherit nothing from our efforts, and will be born with the same inherent bad characters as their parents, nevertheless will be brought up under entirely different conditions from those which they would have experienced if nothing had been done for their parents; and their bad characteristics may consequently never have a chance of developing. For example, what does it matter if I, in England inherit cannibal instincts from remote ancestors? Here in a civilized country those instincts have no chance of developing. They may not perhaps be eradicated by the civilization of centuries, and remain latent in my character, yet a permanent change of environment has been brought about, transmitted by inheritance, which is just as effective as if the cannibal instinct itself were abolished. Take another example. We do not inherit speech; but because our parents have learnt to talk we grow up in an environment such that speech comes to us without conscious effort, almost as easily indeed as if we really, in the biological sense, inherited language. Or, again, take the hypothetical case of an inherent predisposition to tuberculosis in a given family. Of what permanent use to society is it, Mr. Mudge seems to ask, to cure the affected members? Their children will suffer all the same. Not so; if the affected members are let alone the children will grow

up in an environment which includes the tubercle bacillus, and they will become consumptive. If the already affected members of the family be cured or removed, the children will no doubt, grow up with their predisposition unaffected, but they will stand a much better chance of not meeting the tubercle bacillus in effective numbers, and will therefore, for the most part, escape. It is in this way that tuberculosis is being eradicated.

Enough has been said to show that by the conscious efforts of the community, changes of environment may be produced which will permanently affect society. I need not labour the point. I am defending the popular cause. Perhaps Mr. Mudge must not be taken too seriously when attacking the efforts of philanthropists and others; his business is to show, not so much that we are doing wrong in curing the sick, etc., as that we are not doing enough to check the multiplication of our criminal and worthless classes. And in this I am entirely at one with him.

A Rejoinder to Dr. Louis Cobbett's Criticism.

By GEO. P. MUDGE.

Dr. Cobbett appears to accept Weismann's doctrine that characters acquired during the life of an individual as the result of external influences are not transmitted to the next generation. Here then we stand upon common ground. But Dr. Cobbett contends that if certain characters need a particular external agent to call them into activity, they will not be rendered manifest in the absence of this agent, even though they exist. Therefore, if civilisation can be made to produce certain conditions, unfavourable, for instance, to cannibalism and tuberculosis, these qualities and others, though inherently present in individuals, will not be manifested. Similarly, though the capacity for speech is inherent in us, yet it cannot be manifested as speech unless the appropriate stimuli of education and example are present.

Now I cannot help thinking that the argument which rests on cannibalism is exceedingly unfortunate. First, Dr. Cobbett cannot show that every individual in this country possesses the cannibalistic instinct. If it be not present, then of course it cannot be manifested. How many of us are there who want to eat in grim earnest the flesh of our fellows? Who among us feels an irresistible impulse to first

kill our friend and then to eat him, but are only deterred from doing so by the conditions of society ? I think Dr. Cobbett may rest assured that it would " matter very much, if, in England, he and others had an inherent impulse to eat human flesh." For, in spite of civilisation, he would find means of satisfying that innate hunger, much as the congenital drunkard gratifies his irresistible craving in opposition to those external influences which operate against his deeds.

We cannot look the ordinary facts of life in the face and feel that there is any comfort in Dr. Cobbett's contention. The popular memory is proverbially short, and those who plead popular causes are apt to be forgetful of facts which are ugly and immovable. Let us therefore recall one. It is, I suppose, only some fifteen years ago, when civilisation was horrified by a series of revolting murders characterised by unspeakable mutilations in the East End of London. The mutilations were all marked by certain features in common, which pointed to the conclusion that they were the work of one criminal. The conditions which attended the murders suggested there could be no particular or personal motive inspiring their perpetration. The facts indeed led people to suppose they were done by some man who was impelled by a ghoulish delight in such gruesome work. The mutilations were performed with almost anatomical precision, and must have been carried out with perfect calmness and collection of thought and action, and yet with

rapidity. The criminal was apparently never found, notwithstanding that these murders were committed at different periods.* This being one of the facts of our mundane life, I would like to ask Dr. Cobbett how far it is consistent with his hypothesis that cannibalistic instincts may be suppressed under the softening influences of civilisation? I would like to ask him where in that civilisation are those external promptings which encourage the perpetration of such foul deeds to be found? On the other hand, are not all the influences of society against them? The fear of the hangman's rope is there to keep the wild instincts of such cowardly beings in bounds; but, as is clear, no external influences can modify them or avert the consequences of their operations, if they be present. I need not multiply this particular type of example, for almost every day the newspapers contain some account of them.

I may, however, pass to some other matters which newspapers do not publish. Partly, perhaps, because there is no particular interest in them, and partly, because in these days of democratic sentiment, when the play is to the highly strung emotions of the gallery, it may be deemed impolitic from a financial point of view to point the moral of the facts. Such considerations and others akin to them need not deter us. We have no love at all of those things that are associated with the gallery,

*I am aware of Sir Robert Anderson's recent statement to the effect that these murders were the work of a Polish Jew, known to the police, and that the murderer is described as a "maniacal sexual pervert." But this does not in any way invalidate my argument.

notwithstanding the widely prevalent environment of our times. We may therefore proceed to look at some particular, though perhaps unpopular, facts. I am impelled to the consideration of these facts by the nature of Dr. Cobbett's central plea. He says, "*If we take children from a slum environment, train and educate them to become well-conducted, self-supporting citizens (the possibility of which I think Mr. Mudge will be willing to grant), their children, though biologically they may inherit nothing from our efforts, and will be born with the same inherent bad characters as their parents, nevertheless will be brought up under entirely different conditions from those which they would have experienced if nothing had been done for their parents; and their bad characteristics may consequently never have a chance of developing.*" Now I happen to be familiar with a social experiment which has been carried on for the past forty years, and which conforms in its main features with the conditions and suppositions described by Dr. Cobbett in the sentence which I have just quoted. We may therefore proceed from mere suppositions and possibilities to accomplished facts, and endeavour to see how far Dr. Cobbett and those who believe with him, are justified in their golden hopes and the aërial castles of their social dreams.

Travellers along the West Coast of Scotland are familiar with the charm, the honesty, and the natural gentlemanliness of its inhabitants. They have their faults, of course, but we may pass them over. For I am not concerned, except in an

indirect way, with the natives of this Western Coast, but with an inherently vicious class which has been thrust into their midst. This class has been thus thrust upon them by the stimulus of that peculiar sentiment which I considered in my original article, and which characterises the doings of modern civilisations generally. Glasgow has, like other cities, bred and reared a race of useless people, civic cripples, hooligans, criminals, and women powerless to protect themselves. Faced with the pressing problem as to what is to be done with this class of defective citizen, the Glasgow Parish Council cast their municipal eyes upon the beautiful and verdant Western Coast of Scotland and upon its honest and simple inhabitants. "Here," in effect, they said, " is a good environment and a trusty people. In the sea breezes of this beautiful and romantic coast we have conditions of health and scenic displays that will arouse the romance and imagination of our shipwrecked horde of civic outcasts. Among the native population, God-fearing and honest, we may find a human environment which will appeal to the better nature of our morally and mentally crippled citizens, which will induce them to become better and nobler men and women, and will rear them to service of work and to manly independence. We cannot," they proceeded to argue, "hope to do much with the parents. We may as well recognise the fact that they are hopeless and irreclaimable. But with the children it is otherwise ; they are plastic to our moulding and responsive to our better influences.

We will try our experiment with them. Willy-nilly, having conceived our well-intentioned idea, we will make no further enquiry, we will assume there exists nothing more in heaven or earth than exists in our philosophy of fatuity, and in the midst of these honest people we will plant our tares, the children of paupers, ne'er-do-wells, thieves, murderers, prostitutes, hooligans, drunkards, and those of vile and vicious language and of reprehensible and indescribably filthy conduct. From such an ancestry we shall, with ratepayers' money and good environment, rear a community of civic saints ! " These are noble intentions, no doubt, and they are based on the same sort of contention as that urged by Dr. Cobbett. But let us leave the intentions, and come to the results. They are the very reverse of what it was intended they should be. In the citation of the following facts I will confine myself to the results of personal knowledge and enquiry made in one of these Western islands. I do so, because here my enquiries and observations have been made throughout a sufficiently extended period. of eight months spread out over four years.

The pauper children from Glasgow are boarded out with the native crofters, who are paid from three and sixpence to five shillings weekly for the maintenance of each child. This money is paid by the Parish Council. The children are sent to the island when quite young—some little more than infants —and attend at the village school under the guardianship of the crofter with whom they are boarded

until they are fourteen years of age. When they leave school some of them remain upon the island and are employed in farm work by the crofter upon his croft. It was expected that under these conditions they would become healthy, useful, law-abiding citizens. Taken from the slums and from their parents' evil influences, and in many cases deprived of all knowledge of whom their parents were, and placed under those healthy conditions which it is supposed had produced the native population, it was optimistically believed that a great transformation was going to be effected. It was thought that the young and plastic minds and bodies of the offspring of congenital civic wrecks were to be turned into successful farmers and useful citizens. Doubtless in a few cases there have been ostensible successes. I say ostensible, because in some of the supposed successful cases with which I am acquainted, a sufficiently long period has not elapsed to enable one to judge of permanent success, and in a few cases it is possible that the ancestry has not been bad. But these cases are outside my present contention, which is, that environment cannot modify an *inherently bad stock*. What we require to know, in attempting to judge of the success or failure of these efforts, is not that a particular girl has been in service for a few years and is doing well, but we should seek to know what has been the nature of her career when her life is over and her children are old enough to show their worth or worthlessness. Brass may be polished and shine like gold for a little period, but it is time

which tarnishes and covers it with verdigris; it is gold only that remains untarnished. Such experience as I have gathered in other fields of these ostensible successes, does not lead me to believe that they are of lasting duration, not even with the individual, and not at all with his offspring. Leaving, then, these questionable, untested, and unanalysed successes alone, what are the results with regard to the remainder? They are precisely that which the old adage "What is bred in the bone will come out in the flesh" would lead us to anticipate. We may sum them up in a single sentence. It is that, in this beautiful island with its historic and even sacred associations, *a new slum area is being created* by the operation of the inherent slum instincts of the putatively rescued denizens of Glasgow's slums. Here we see in the making, not slum people by slum environments, but slums fathomed out of the depths of the slum instincts of a congenital race of slum producers. Here in this island, where at one time only ordinary human weaknesses prevailed, is now heard the obscene language and the suggestive songs of the slums; here at night time, the Glasgow rowdies congregate in bands and create noise and disturbance. They link their arms and rushing through the village street in a serried rank, shouting, whistling, and gesticulating, drive all others before them. Where there are maid servants, there they collect in groups and indulge in language which is not of the Highlands, but of Glasgow slums. No windows are safe from them, and many have been

broken by stones thrown at night time. Blinds are wrenched from their rollers and knockers from the doors. Slates are knocked off the roofs. Old women of eighty live in terror of these rowdies. The young native children are bullied and terrorised by them. They are viciously cruel to the cattle and dogs left in their charge. They insult the visitors, making personal remarks of an offensive character. They collect in groups at corners, and cough, and guffaw when a lady or a gentleman, or both, pass by. They stand outside the open windows of houses where visitors are staying and eavesdrop. They assemble at dusk and nightfall when children are going to bed and make as much noise as possible, by bursting paper bags and in other ways. They deliberately come up in groups where two visitors may be talking, and commence whistling and laughing in an obviously strained manner, while attempting to seem inoffensive and unobtrusive. They chase and worry sheep in lamb, causing premature birth and rendering the sheep valueless. They carry false and unauthorised messages from their guardians and obtain articles of food by false pretences from the shopkeepers, and then consume the articles among themselves. They steal various articles when cases of goods are being unpacked outside a shop, as soon as the shopman's back is turned. When taking messages to the natives, the moment an opportunity occurs they steal whatever is within reach. Some are such perpetual thieves that the crofters send them back. If they are rebuked by their masters for bad or

neglected work, they will surreptitiously destroy some of his property in revenge. The consequence is, that deeds which ought to be punished go unpunished.

Many of these youths are half-witted, or are mentally defective, and are quite irresponsible for their deeds. One such fellow in the Summer of a recent year was answerable for the death of two native children, and in addition of a fellow Glasgow boy, against whom, it is said, he entertained a grudge. I went into the circumstances of this case, and I feel inclined to agree with the impression existing among the villagers, that they contained some disquieting features. While, I suppose, it will never be possible to say with absolute certainty, one way or the other, whether the death of these three children, by drowning, at eleven o'clock at night, was a pure accident or not, for the natives themselves are doubtful, there is no question as to the absolute callousness of the dull-witted youth of eighteen or nineteen years of age, who was responsible for it. Neither at the time, nor since, has he shown any sign of regret of any kind, for the sad deed. He passed the stricken mothers and fathers whose children had been lost, without the least sign of shame or remorse. When those who came to the rescue had succeeded in saving one child, and had spent some time in a futile search for others, desired to ascertain how many were in the boat, and turned to question the youth who had upset it, they found he had disappeared. A search for him revealed the fact

that he had walked home, undressed, and got into bed without mentioning a word of what had happened to his master or anyone else. They found him sound asleep and difficult to awake. He was roused eventually, and told to come to the village. Arrived there, he was asked how many children went overboard. His reply was "to stand still and to burst out laughing!" That is all the reply they got from him. And greater compunction than that he has never manifested. Now, what is the general significance of this case?

Here, under an environment which in no way ministers to them, do the slum instincts come out. Those instincts which delight in roaming abroad at night indulging in horse play, and leading others to do wrong, are here manifested, not in the slums, but in a beautiful environment. From the evidence it is clear that this youth did not appear in the village until after 10 o'clock at night, and that he then by cajolery or threats inveigled smaller boys, including the one against whom it is said he had a grudge, to come down to the boats, instead of going home as they were at the moment doing. "He packed the four children all into the bows of a small boat, where they cowered under protestation, and thus weighed the bow low down into the water, and though he knew nothing of sculling, he then commenced to pull with one oar in a slot in the stern sheet of the boat. He swung the oar violently from side to side, and while the boat was lurching terribly, the oar slipped, and the bow being overladen and low in the water, the boat at

that part went under, filled and sank." All the occupants went under, except the Glasgow parish youth who was answerable for the accident. He apparently knew the art of floating, and remained on his back until a second rescue boat picked him up. Only one of the four other children was saved.

Now I would like to ask Dr. Cobbett a question. What influence is there in civilisation generally, or in the example of the very cautious natives of this island, that can be accounted answerable for the manifestation of the utter recklessness which I have just described, or for the exhibition of that equally utter callousness which this youth exhibited after the committal of this deed ? There is nothing whatever. Every external influence, every social idea, every convention of life is dead against them both. This youth did what he did because of his defective nature. And to rear him in health at the expense of better citizens, in the hope that because our society has given up or does not now entertain "cannibalistic desires," and, that therefore, though inherent cannibals be among us, they will fall smoothly into line with non-cannibalistic citizens, is simply to disregard the plainest facts of life, and to rear persons who, born with innate qualities opposed to the prevailing environment, will none the less manifest them.

But I will take Dr. Cobbett yet a stage farther into the consideration of what I cannot but help regarding as a remarkable plea. In this island there are two brothers of the fellow who caused the drowning disaster. Both of them are half-witted,

and one is a broad, burly fellow. This one is boarded out with a certain crofter, and he is now about sixteen years of age. He is entrusted by his master with a gun for shooting rabbits. If he is annoyed he becomes like a madman. One day a native child annoyed him over some trifle. This burly fellow lost his temper and endeavoured to catch the native child. He failed in that, but he threatened to shoot the boy when he got a chance. Is he likely to do it, it may be asked ? Will he commit a personal injury ? His previous deeds may answer for him. Some time ago a Glasgow parish girl annoyed this youth. He seized a " grape," a double-forked instrument with pointed prongs used for digging up potatoes, ran after her, endeavoured to stab her, missed her body but punctured her hand. The girl was in a serious condition for some time and nearly lost her hand. Does Dr. Cobbett contend the conditions of society must be so altered that we must not have " grapes " or other peaceful-occupation instruments, because they are capable of becoming lethal weapons when handled by persons of vicious instincts, or that the environment of our society must be so altered that tantalizing " Eves " shall be no more ? And all this is to be done—and those citizens who delight to have teasing " Eves " around them are to be rendered restless and unhappy —because, forsooth, we must not let the vicious instincts of a defective race have an opportunity of manifesting themselves ! Surely that is a remarkable

doctrine! I cannot think Dr. Cobbett quite appreciates what his own doctrine really means. Would he like to sweep civilisation clean of its "Eves," because some men have very bad tempers and little tact, and it is not desirable their environment should be such as to allow them to manifest their defects? Would it not be better to get rid of the relatively few imperfect men and to leave our Garden of Eden as we know it? I feel sure that Dr. Cobbett and I will agree upon the answer.

But I come once more to insist on the primal point, that the environment which reigns in the island makes for peace, and industry, and honesty. Yet in spite of that, this environment is degraded, the lives of people are endangered, slum attributes are manifested, because there are thrown into it persons of defective instincts, derived from defective parentages. But not only do they not respond to a good environment, they deliberately and persistently endeavour to destroy or nullify all the good efforts which kind and interested persons endeavour to make on their behalf.

There is a lady who lives upon the island throughout the year, and who feels a maternal interest in the young persons there, both native and the imported pauper element. She endeavoured for several years to hold a Bible Class for girls and boys over fourteen, in the schoolroom. But it was futile. The Glasgow parish youths, who had been sent to the island to be influenced by better surroundings, collected together outside the schoolroom windows and laughed, and

guffawed, and indulged in horse play, so that it was impossible for the class to proceed. But they even proceeded to worse acts of hooliganism than that. They opened the windows and threw in mud, snow, and stones.

Very much the same treatment awaited the altruistic efforts of one of the assistant schoolmistresses. On two evenings a week this lady endeavoured to hold a Sewing and Conversational Class for all the girls in the island, who were over fourteen years of age. At first she tried the schoolroom. But that was rendered intolerable by the Glasgow hooligans outside. The teacher had to break up the class, night after night, because these roughs threw things in at the window. When she went home in despair they jeered and laughed at her. Not willing to give up without a further struggle, she endeavoured to hold the class at her own rooms. It was of no avail. The roughs invaded her home and indulged in the same annoyance as before. The result is, as I am informed, there is neither Bible Class nor Sewing and Conversational Class in the island. In other words, utter lawlessness prevails. Yet here are the very circumstances which, if Dr. Cobbett's contention be true, should yield us the fruit he has led us to expect. Everything in the native environment of the island calls for good order. They who are there, be they native or visitor or boarded-out youths and girls, find nothing to incite them to such acts as I have described. Yet we do find precisely what biological considerations indicate we must inevitably find, namely, that

the transplantation of inherently vicious and criminal instincts from the slums of cities to the beautiful environment of the Western islands, is but re-creating there the slums which were also made in the city by them. Given bad instincts, and be the environment what it may, they will manifest themselves with the same certainty that the sun rises and sets alike on mountain and valley, on sea and land.

The operation of the sentiment which I criticised in my original article, but which Dr. Cobbett has endeavoured to defend, is one of grave menace to Society. In an island like the one with which I am now dealing we can see its menace at once. It is easy to take a bird's-eye view of the whole problem. There we can see a peaceful and not too energetic population, of honest and easy-going people, enduring the mob-rule and tyranny of a lawless section, rather than put themselves to the necessary trouble of crushing the mob, or running the risk of incurring stealthy and underhand reprisals. Indeed, some representations, I have been informed, have been made to the Glasgow Parish Council but without avail.

Among them I may mention that of a very respectable native holding a responsible position. There is only one school in the island, and to this both the native and Glasgow pauper children are sent. The consequences are undesirable. In ways which I cannot mention the pauper children are dirty and unwholesome. The bigger ones are brutal and rough. Their language is foul and offensive. This native, a man with a large family, quite naturally resents sending his children to associate with such companions. He

therefore wrote to the Clerk of the Glasgow Parish Council, and asked if he was to be compelled to send his children to the same institution as pauper children. The reply he received was that the school was not an institution, and that he must educate his children. That is all the care and sympathy which a respectable member of the community, desiring to preserve his children from contact with foul language, rough behaviour, and dirty habits, receives from a public official! Nowadays, it seems, we have only pity and help and money for the undesirables. And this remarkable sentiment, which believes in the influence of the environment, and which sends the scourings of the Glasgow slums to this island for improvement, swamps the place by an importation so great that seventy to eighty per cent. of the school population is made up of the imported element! In a word, the old and good environment is swamped by the new and imported vicious one! And this by the people who believe in the influence of the environment! Was there ever such a *reductio ad absurdum* of any doctrine before?

I will not now pursue this matter further, except to say, that the remarkable social sentiment which I have been criticising, in so far as it has been operating in the island, has resulted in sowing there the seeds of potential tragedies. I think it right to take this opportunity of uttering a serious warning. I should neglect an obvious duty if I did not, for the circumstances of the situation are fraught with the gravest danger. We are all aware of the power which mere suggestion can exercise over unbalanced and weakly minds;

minds which are swirled into tempestuous action, much in the same way that Autumn leaves are swept before the equinoctial gales. In this island, those capable of making violent suggestions, and those with weak and defective minds that are swayed by these suggestions, are both present. I am speaking on reliable information and from personal knowledge, when I say, that on a recent occasion a violent suggestion was made and the unstable minds responded. A mere accident of circumstance only averted a tragic act. There are imported youths in the island who are not mentally defective in the ordinary sense of the word, but on the contrary are exceedingly cunning, calm, and calculating. They have almost the voice of a woman, the plausibility of a consummate actor, but the instincts and habits of a criminal. These are they who, at a distance and under cover, throw out suggestions of mischief and evil. The mentally defective —some, as I have said, entrusted with guns or other dangerous instruments—are there with responsive and helpless wrecks of intellect. They are the instruments of the cunning minds.

Some day, unless the island be purged of its defective people, it will be the scene of another pathetic tragedy. All the elements for it, are there. Those who are responsible for the maintenance of this state of affairs are incurring a heavy responsibility. I trust, before it is too late, they will weigh the circumstances carefully.

I come back now to Dr. Cobbett's main plea (ante p. 105) that, though individuals may be defective in their character, yet if they are brought up under new

and better conditions, their bad characteristics will not have a chance of developing. This plea, it seems to me, falls hopelessly to the ground, in the light of the facts which I have described. It is not only in this island that these facts exist. They are everywhere around us. They are in our pauper institutions, in our reformatories, in our prisons, in our schools, in our daily life, and in many cultured homes, the good names of which have been disgraced by the deeds of an adopted son or daughter, chosen from an unhappy stock ; so that they who have been deceived and whose delusions have vanished, may say in the hour of their remorse to those who have disappointed them and have crushed the pearls of their benevolence and cherished hopes : "*Annon sicut lac mulsisti me, et sicut caseum coagulasti me?*"

I will next attempt to deal with Dr. Cobbett's contention from another aspect. We may consider the question of habitual alcoholism and drunkenness. Now, I think all will agree that a permanent change of environment, in the sense that social tradition and custom have altered, has been brought about and that it has been operating for at least two generations. Yet both these forms of vice are common, and to such an extent that in these latter days, optimistic Chancellors have turned into futile moralists. Every experience of our social life points the conclusion that a traditional inheritance of external concepts is incapable of exercising any useful influence, except upon those whose physiological inheritance is so constituted, that it *spontaneously* responds to the external agents.

The truth is, if the inherent desires and instincts are there, no environment which we can originate, or maintain, or conceive, will render it impossible for those instincts, or desires, or defects to manifest themselves. We may as well try to persuade ourselves that by removing food and water from a community, we can thereby still the pangs of hunger and thirst, as to believe that manifestations of other inborn qualities will not evidence themselves in the absence of that positive environment which we conceive to be necessary for their manifestation. There is some danger that two different things are being confused. We cannot, by any environment, call into activity qualities which do not exist. But neither can we suppress inherent qualities, or even the ultimate manifestation of their activities, merely because an appropriate environment is absent. To take Dr. Cobbett's own example of the supposed reduced mortality from consumption because of an improved sanitation. Even though we grant, for the sake of argument, that the reduced death-rate from consumption may be due to those improved conditions which have reduced or rendered more difficult of multiplication the number or virulence of the tubercle bacilli, are we sure we have also minimised the manifestation of that human defectiveness which results in this disease ? Pneumonia of recent years has shown a decided tendency to increase. What is that due to ? Is that, too, to be ascribed to improved sanitation ? Are we quite sure that, in spite of an environment which, we are told, is becoming more and more unfavourable to the spread of tuberculosis,

the defective quality which renders a subject liable to tuberculosis is not merely manifesting itself in another way ? We may, perhaps, escape the toll which Nature demands from us at one gate on the road of life, but she will have it from us at another.

Then there is the question of correlation. Does the tubercular diathesis accompany some other defective quality ? Here we have no certain answer. But we have a general experience, and there is no doubt that in some families this diathesis is merely the expression of a much wider constitutional defect. It is accompanied by a frail physique, a slender hold on life, and a lassitude of action. Now, any improvement in the environment which saves this type of tuberculous person, also propagates a race characterised by the possession of the correlative qualities. Is that desirable ? I imagine not. The truth is, in our desire to save life and reduce death, at all costs, we are being carried too far. We are running grave risks. Let us try to foresee one of them.

At a certain, ill-defined stage in the history of the world, it could have been said that the march of Civilisation and of Empire was Westward. There are not wanting the signs that it will again turn Eastward. Suppose it does. Before the West relinquishes its sceptre to the East, its retention will be fought for on many a bloody battlefield. But bullets and sabres are not the only instruments of destruction, even on a battlefield. Epidemic diseases play their part. While the West, according to Dr. Cobbett's environmental doctrine, has been saving

hordes of persons innately susceptible to microbic diseases, because of the improved sanitation, in the East, the teeming millions of China have been evolved in the unspeakable insanitation of its cities, and in many instances, under a tropical sun. The armies of the East and West would meet on unsanitary battlefields. That of the East is composed of men evolved under unsanitary conditions, and therefore more or less immune or only difficultly susceptible to microbic attacks. But the army of the West is full of soldiers reared under softer conditions, derived from ancestors who for generations have evolved under circumstances where the constitutionally weak as well as strong survived. That army must contain a large proportion, perhaps the major part, of persons susceptible to microbic attacks. Under such circumstances, the East has an advantage over the West. The one army will be decimated by epidemics, the other not. The sceptre will pass from the nations which feared death too much and loved life too well, to those who faced the struggle of existence with a hardier bravery and a more Spartan resignation. And, again in the history of Mankind, the "breath of the Angel" will decimate another army—that of the West, while it lies during the night outside the camp of the East—as in ancient days, before the sleeping army of the Jews, it withered the host of the Assyrians and destroyed them to a man.

This raises a further consideration which I am impelled to urge. Even though we grant, for the sake of discussion, that it is possible to prevent the

manifestation of tubercular disease in the absence of those environmental agents which call it forth, there still remains the very important question, Can we ensure that these particular agents will always be absent ? Is it tolerably certain that at no future period in our history circumstances may not arise which will render it impossible for this artificially imposed environment to be any longer maintained ? Let us look, while yet we have time, along one road which opens out to a vista, where danger is spelt at every point along its view.

I think the possibility, which I have just described, of a crucial struggle between the East and West, whose respective denizens are evolving under very different conditions of sanitation, suggests that before it is too late Western Civilisation should mark time, and take heed of the direction and velocity of its movement. For it should never be forgotten that every improvement in the sanitary conditions of life enables yet weaker and less resistant stocks to live and propagate. It is, therefore, possible to raise the quest of sanitation into a fetich, and under the guise of a false health to hide the fact, that we are by invidious and stealthy methods ensuring the ultimate destruction of our race as a dominating factor in the affairs of the world.

But improved sanitation of the co-operative and compulsory type does more than save the weak. It saves the innately dirty people from the consequences of their habits. I do not think anyone who has gone into the question will doubt that, with but few exceptions, dirtiness and untidiness of personal habit

is a matter deeper than convention or training. At any rate, I am prepared to discuss that question on a basis of fact, if I am challenged.

Now to save innately dirty people from themselves, is to rear in increasing numbers a race of an undesirable type. At present, apparently, in the plenitude of our modern wisdom, we are prepared to compel the dirty to live within certain limits of cleanliness. We have set aside officials for the purpose. Doubtless, the arrangement will work well, until the dirty people are so numerous, that it will be impossible to have sufficient officials to check the manifestation of their undesirable habits, unless we shall be prepared to make all the rest of the nation officials. Then will come the reckoning. In the latter part of the nineteenth century we did not pay the toll we should have done to Nature, but in the latter half of the twentieth century She will have it, with exorbitant interest, exacting it under "harsh and unconscionable" terms. For the inherently lazy, sluggish, and dirty people, grown into numbers beyond control, will destroy the clean environment, and will re-create unsanitary conditions, which, descending upon a race ripened by modern sentiment for the harvest, will garner its fruit in almost illimitable measure.

The noblest sentiment this nation can adopt is not that of cheating Nature, by presuming upon our imperfect knowledge and the arrogance of overweening aspirations, but that which is expressed in Wordsworth's lines :—

> " To the solid ground of Nature, trusts the Mind
> which builds for Aye."

Biological Iconoclasm, Mendelian Inheritance and Human Society

A PLEA FOR THE OPERATION OF A MORE VIRILE SENTIMENT IN HUMAN AFFAIRS.

A Criticism

By Miss H. M. WODEHOUSE.

(Lecturer in Philosophy in the University of Birmingham.

BEING myself the most unskilled of amateurs in natural science, I am filled with the deepest respect for those whose professional studies lie in this department; and I accept with an undoubting faith everything that these experts tell me of the objects which their studies bring before them. Only when a scientific expert crosses the line into my own department of "mental and moral" science does this paralysing reverence relax its hold. It is this overlapping of subjects which gives me courage to offer a few tentative comments on an interesting paper, by Mr. G. P. Mudge, in *The Mendel Journal* for October, 1909, "A Plea for the Operation of a more Virile Sentiment in Human Affairs."

The part of the paper which concerns me may be summed up as follows, probably in unscientific language, but I hope not unfairly. "The unit qualities which make an individual valuable or

valueless are determined by heredity. Environment cannot affect them, but it can select and encourage the individuals with the best qualities by means of getting rid of the unfit. Hence social reformers are grievously and dangerously wrong in attempting to alter the natural environment of any class in society." I propose to examine successively the three parts of this text, beginning at the end.*

1.—"Let Environment alone."

I have found much difficulty in understanding the author's conception of Nature on the one hand, and of interference with Nature on the other. "Those best fitted to survive," he tells us, "will do so by purely natural processes, without any artificial and compulsory interference or help. And we may be sure that these processes are benign; biological teaching is clear on that point. By these processes, throughout the whole realm of nature, the maximum amount of happiness and vigour is attained, with the minimum amount of misery or pain. Human interference but decreases the former, while increasing the latter. It is better, in the faith of the greater religions, to accept our destiny."

Metaphysicians who hold that this world, in popular language, is the best of all possible worlds,

* My criticism deals with only a small part of the paper. I do not touch, for instance, on the author's plea that the unfit ought to be killed off, not maintained in workhouses, asylums, etc. It is conceivable that in a more enlightened age we should deliberately kill more people. I only wish to urge that in such an age we shall not so trust the workings of Nature in a slum environment as to leave her to do the selection and the killing for us.

may perhaps be a little surprised that Biology should be undertaking in so explicit and courageous a way to teach Metaphysics; but they will be pleased to have its help in the defence of a Nature which includes earthquakes, butcher-birds, keas, the typhoid bacillus, and the tsetse fly. We accept as benign all processes in which these factors are concerned. The difficulty only enters when we have to distinguish the natural from the unnatural processes in the affairs of human society. Studying animal societies, we find that mortal combats between jealous wooers, and the careful nursing by ants of the larvæ of beetles which will presently devour the baby ants, and the extermination of cattle by the bite of an insect, are all natural and therefore admirable events. The moral for us would seem to be that we should make haste to remove such flagrant interferences with nature as are found in the main drainage system and the organisation of the metropolitan police. Acceptance of destiny is found purest in that mother who refuses to alter her child's environment to defeat scarlet fever or concussion of the brain.

Seriously, what is this Nature which includes and justifies the doings of all animals but man? which renders infallible all instincts and aspirations but his? Where is this benign and perfect force which we have only to make way for? and how is this making way, in practical politics, to be brought about? I can find no profit in this way of using words.

2.—"Environment Selects the Best Individuals."

I express this part of the text in an ambiguous form because the ambiguity seems really to be present in Mr. Mudge's treatment of the subject. The literal truth, one is taught to suppose, is that any set of conditions tends to select the individuals whose qualities give them an advantage under those conditions. Science, I take it, commits itself to no further meaning of "fittest" or "best." In fact, we often condemn an environment because by strict natural law it gives the advantage to qualities which we condemn; witness "the corruptions of a Royal Court"* where the time-server and flatterer succeed and survive. Given any environment, Nature may be trusted to select. Can we trust her to select such qualities as we shall approve of?

Further, no conceivable environment can take account of the whole of an individual's qualities when it gives its judgment for life or death. The Summer Savory in Mr. Mudge's instance can turn purple on the mountain-side, whilst the Flax from an "inherent defect" remains what it was in the valley, and dies. But in the valley, I imagine, the first would have found not the smallest advantage in its possession of this power, and the second not the smallest disadvantage in the lack of it. Their selection by the valley environment must take place without any regard to its presence or absence. Whether or not this is so with plants, it is certainly

* "A Plea," page 121.

so with any organism as complex as a human being. How wide is the range of human qualities which determine the survival of a Congo native to-day, or of a factory child a hundred years ago? One can hardly suppose that the inherent capacity for writing the plays of Shakespeare or for making the discoveries of Newton or Darwin, or any benevolence, or wit, or refinement, or gaiety, or unselfishness, would have very much to say in the matter. Given any environment, Nature will select, but the kind and the range of the qualities she favours will depend on the environment within which she works.

On these two points I cannot help thinking that "A Plea for Virile Sentiment" is lacking in explicitness, and that the author's constant use of "fittest" and "best" without qualification has a dangerous tendency to disguise the fact that they must mean no more than "having an advantage under the given conditions." This is all that Nature is charged with, and in this sense the fittest will survive whatever we do and whatever our aristocratic or democratic policy may be. Whether the best in any more ordinary sense survive will depend on the conditions, and these conditions, by action or abstention, we must partly fix. Is it Mr. Mudge's opinion that the present slum environment selects its survivors on the basis of qualities so valuable and so varied that we can no longer hope to improve it?

3.—"Unit Qualities, which determine value, are determined by Heredity and not by Environment."

I come now to the most serious of all my objections to "A Plea for Virile Sentiment." This objection rests on what seems to me the very remarkable nature of the author's assumptions as to the unit qualities of the mental world.

If I have not misunderstood Mendelism altogether, it appears that no application of it can be made with certainty until the experimentor has discovered what the transmissible unit qualities are. For instance, the tendency of a sweet pea to be a dwarf is such a quality, but the tendency of a petunia to be white is not a unit but results from a combination of factors; and the prophecies about inheritance must be quite different in these two cases. Further, the power of a muscle to do very hard work is not asserted to be transmissible if that power has been acquired by exercise. The capacity for developing such a power may be inherent, but the power itself is not inherent but acquired, and no prophecies about its inheritance can be made so far as Mendelism is concerned. If then we are to prophesy about the inheritance of mental characteristics, it will be all-important to find out the inherent units. Now what I urge from the side of psychology is this: that, except to a minute extent, nobody can say at present what the units are; that scarcely any of the qualities instanced by Mr. Mudge

seem at all likely to be units; and that we are almost certainly wrong if we choose as units any of the qualities which are markedly valuable or hurtful to the human race.

I yield at once to my opponent the obvious exception of such definite deformity in the mind or brain as shows itself in insanity or feeble-mindedness. There seems to be good ground for believing that these may be units and transmissible like deformities in the hand or foot. But, unless special evidence is produced, I do not yield those qualities which we praise or blame in sane men. My reason is that most of them seem to fall with far greater naturalness into other classes, in none of which inherent and simply hereditary units are to be found.

(*a*) For instance, a great part of the social value of a man depends not on anything that we can believe to be simple qualities, but on the combinations of these. An amount of caution which in a phlegmatic character without keen interests might almost paralyse the man's power of action, might in an energetic nature be just the tempering required to make a first-rate general or Prime Minister.

(*b*) Another great part of social value or disvalue depends on the fact that qualities, like muscles, increase in strength as they are used. Consider the way, familiar to educators, in which a wise training may increase the power of taking responsibility. No doubt the capacity for acquiring this power may

or must be inherent, but this in itself is quite insufficient to determine the value of the child and the desirability of keeping him alive. Consider again a boy with a strong enjoyment of bodily pleasures, whose desires if unwisely indulged may turn to devils within him, but who if temperately trained may keep the love of beautiful and pleasant things as a deep rich undertone to all his life, making that life delightful to himself and to all around him. If we as benevolent autocrats held power of life and death over such children, we should be bound to ask not only, What is in them? but, How are you going to bring them up? What are the opportunities and temptations of their environment to be? For on the answer might depend whether they would grow up to be the best supports or the worst burdens for the community.

(*c*) The existence of a third and by far the most important class of values depends upon the commonplace of ethics, that nothing in heaven or earth is good without qualification, except the good will.

Being interpreted, the truth in this classical doctrine shows itself as what common sense knows, that we can scarcely say whether a quality is good or bad until we know in the service of what purpose its owner is using it. The energy and inventiveness which make a small boy the plague of his school, may so be turned presently as to make that boy the leader of the school in all that is admirable.

The same courage and resolution and quick decisiveness that serve Garibaldi serve also the Tottenham murderers. Whether the civic value of a man or a child shall be positive or negative depends chiefly not on any simple qualities but on the direction of purpose and will. Now it is just this direction of will that is found nowhere short of humanity to a noticeable extent, which therefore it may be difficult for natural science to judge aright. From the point of view of moral science I urge with all possible emphasis that to treat such direction as a simple quality, transmissible as a deformity is transmissible, is impossible in every way; it is absurd; it is mythology. Criminality *in this sense* is no more innate than loyalty to Austria was innate in the Swiss Von Mechel, or loyalty to Italy in the Englishman Peard. Anyone can see a wise teacher winning a child to the side of authority, or a foolish one worrying him into rebellion against it; it will only be the foolish one who speaks of "innate naughtiness." The child, and still more the man, knows that he can make a choice; can decide for himself what object he will serve and pursue. Some choose when they are young to serve society as such; a few decide to fight society as such; and none of them ever manage to live consistently on those lines. Our lives are made up of a continuous series of decisions, and the civic value of most of us is positive and negative by turns. Whichever it is, we decide it continually as we go along. If our choices are inherent, it is in a metaphysical sense

which makes them just as much inherent in the environment as in us. What I do depends on my capacity for loving, and on the objects of love which are presented to me.

One may end here with the real and valuable political truth which lies behind the scorn thrown on environment. *Mere* environment is truly of very little account compared with the presentation to us of objects which we are prepared to love. If the instinct for bodily comfort is not yet so developed and refined as to find satisfaction in cleanliness, it may be of little use to provide the man with chances of cleanliness. On the other hand, it may at the right time be just the chance for which the development of the instinct was waiting. The difference between those improvements of environment which are thus worth while and those which are not worth while is a matter for the most careful, patient, and long-suffering study and experiment. As students of moral science we hope for the greatest help from students of natural science, only they must not encourage us to slur distinctions, to draw easy *a priori* conclusions, or to commit in any way those sins of rough and careless work from which in their own department they have so nobly forborne.

A Rejoinder to Miss Wodehouse's Criticism.

By GEO. P. MUDGE.

I WILL endeavour to reply to most of the criticisms which a metaphysical critic has passed upon my article in the last number of this Journal. It may also be permissible to attempt to show that Biology is transcendent to Metaphysics, if not in its aërial castles, at least in its mundane temples.

Though recognising that Miss Wodehouse has desired to be fair and has tried to understand my attitude, I am afraid that the metaphysical spectacles, through which she has surveyed it from a lofty height, has rendered her task a little difficult, and not yet can it be said that she has fully understood the nature of my plea. If we may judge from one of her statements, it is not certain whether we are to believe that metaphysicians regard Metaphysics as a science or simply look upon it as a relic of the Scholastic Period; an academic reminiscence of the early days of culture when it was the best that the era could produce, and the pronouncements of the Schoolmen were accepted as articles of faith. But Miss Wodehouse may rest assured that the days of "accepting with undoubting faith everything that experts tell us," even though we be "unskilled amateurs," have passed for ever. I have asked no

one to believe me, but simply and quite honestly have endeavoured to bring to the attention of my countrymen some biological considerations which, I am impelled to think, indicate the existence of a grave and imminent danger in the present conditions and the ruling sentiment of the body politic. It is open to them to enquire whether those considerations are valid or invalid; and whether they do or do not throw for them a new interpretation upon the origin and cause of the human misery and squalor which exists. In this discussion I may be allowed to claim an advantage over my critic; for I have passed through the phase that marks her present attitude to that which is indicated in my article. I am not oblivious to the arguments, nor to the interpretations of human life, which are opposed to my own. I have endeavoured to consider most of them.

Miss Wodehouse has in a very fair manner intimated in the beginning of her criticism that she has probably summed up my position in unscientific language, but she hopes not unfairly. There are one or two statements, however, in which certainly my position is not represented in language which quite accurately conveys its meaning. I should not, for instance, say that the environment "encouraged the fit by getting rid of the unfit." We may, in *social matters*, harass the fit in order to encourage the unfit; but that is a different thing. Neither is my attitude quite fully stated when I am credited with the statement that "social reformers are wrong in attempting to alter

the natural environment of any particular social class in Society." I hold this opinion, it is true. But the strongest objection to modern sentiment, is not so much that it is striving to alter the natural environment under which we have evolved, as that it is making an effort to substitute for it a wrong and dangerous artificial one. This new environment, I think every one will admit who has considered the question at all, is operating to destroy or to harass the better and self-supporting citizens in order to save an increasing number of altogether unworthy, helpless, and hopeless civic cripples, and is encouraging others who are capable of self-support, to throw themselves upon the charity of their country. What is most dangerous in the practical effects of this modern sentiment is that it is turning human society upside down, and is by artificial processes rendering the naturally fit artificially unfit, and the naturally unfit artificially fit. In other words, it is destroying a Shakespeare to save an itinerant preacher, and it is harassing out of existence a Reynolds in order to preserve a pavement colourist. It is conceivable that even Miss Wodehouse will not contend, in the light of modern knowledge, even with the aid of metaphysics, that however "careful the treatment and judicious the training," it is possible to convert the people who have mistaken aspiration for inspiration into Shakespeares or into Reynolds'. If she does so contend the question may be asked: How is it that in the period when Shakespeare and George Stephenson were living,

they accomplished what they did without any National Elementary Free Compulsory Education? And that after forty years' operation of this national fetish neither a Shakespeare nor a Stephenson have arisen from the people as the outcome of it? If all that she contends be true, can she vouchsafe to us why it is that, now we have Nature Study classes in the Elementary Schools, the Surrey County Council finds it incumbent to frame a regulation imposing a fine for the destruction of flowers in the fields? When that question has been answered, is it possible that we may be further informed from those heights of metaphysics accessible only to the elect, why it is that boys instructed in Nature Study classes and reared in the influence of the sentiments that appear to have become vicariously attached to them, should go into every byeway and field they can find, and wring the necks of nestlings just for the fun of the thing? Before compulsory education for the masses was instituted, cock-fighting was one of their amusements. We have striven by various means—including a costly national education—to suppress this form of recreation. While we have not altogether succeeded, wringing "nestlings' necks for fun" has appeared as a substitute for it. It may be possible to change the form in which cruelty or viciousness may be manifested, but we cannot by any means at our disposal convert the inherent instincts which lie at the root of these things. That is why the unmetaphysical prophets of old said: "Who can make straight that which the Lord has made

crooked ? " or sought an answer to the question : " Can the Ethiopian change his skin or the leopard his spots ? "

Another phrase of Miss Wodehouse's that is liable to give a misimpression of my attitude is that in her footnote on page 132. She says that I have pleaded the unfit ought to be killed off. It is strange how almost universally an author's passive attitude is converted into a positive one by his critics. The phrase " killed off " is not mine. It was urged that the multiplication of the unfit should cease. Essentially, the view I advocate in nearly all social questions is that, after justice, and the army and navy have been adequately provided for, the rest should be left to voluntary co-operation and to *laisser faire.* In this I simply follow Herbert Spencer, whose prophecies are being rapidly fulfilled. We cannot escape the obvious conclusion, that a fit nation can only be so if it is composed of self-reliant and self-supporting individuals. The nation must necessarily weaken as the number of its weak, helpless, and unreliant citizens increase, while those of antithetic qualities decrease. The France of to-day is a standing warning of the danger that faces the England of to-morrow. A strong nation will essentially be one which has adopted as a guiding principle the maxim of " letting those live who can," and those who cannot shall find an abode elsewhere. Of course, I urge nothing against the altruism of the family nor of that of friends. That is not likely, in the long run, to be

misapplied, and it may call into manifestation the nobler attributes of our kind. I should certainly be the first to regret the disappearance from human life of generous and noble deeds, and of altruistic efforts voluntarily rendered. But it is dangerous to indulge in compulsory altruism, especially when it is wrongly directed. It results in altruistic aids for those who do not deserve them, and in return they who give are met with the grossest and most revolting form of egoism. The obtrusive poor who have always been the perennial recipients of charity have ever afforded the best illustration of this statement.

Miss Wodehouse says she has much difficulty in understanding my conception of Nature. It is not, I suppose, difficult to believe that study-chair conceptions of it will largely depend upon temperament. To some people Nature is an abode of beauty, of song, of happiness, of peace, of arboreal pathways flowing with milk and honey, along which float the strains of melodious avian music, and disturbed alone by Man's advent. To others it is a ceaseless battlefield of the vulture's talons and the tiger's canines, and all is ceaseless misery and pain and death beneath a canopy of blood. But to others, those whose lives have been spent in the mountain, on the sea, in tempest and in calm, as well as in cities—and I rank myself among them—Nature is but the unceasing operation of irrevocable, inexorable, and eternal processes, which are sometimes called laws. In their operation there is inflicted some pain and misery, but there is also attained the maximum of happiness

and the evolution of races to which life is a gleeful struggle, its vigorous exercise a joy, and death merely a passing incident.

This restatement of my conception of Nature brings us next to Miss Wodehouse's gentle and subtle sarcasm, when she says "that this best possible of all worlds includes earthquakes, butcher-birds, keas, the typhoid bacillus, and the tsetse fly. We accept as benign all processes in which these factors are concerned. Studying animal societies, we find that mortal combats between jealous wooers, and the careful nursing by ants of the larvae of beetles which will presently devour the baby ants, and the extermination of cattle by the bite of an insect, are all natural and therefore admirable events. The moral for us would seem to be that we should make haste to remove such flagrant interferences with Nature as are found in the main drainage system and the organisation of the metropolitan police. Acceptance of destiny is found purest in that mother who refuses to alter her child's environment to defeat scarlet fever or concussion of the brain." This seems to be a terrible indictment. In reality it is only metaphysical war paint. A little biological washing and it comes off.

Let us take the earthquake first, and, uninvited by our critic, let us also add volcanoes. Miss Wodehouse appears to believe they are evil things, even though they be natural. Let us see how far they may not be also regarded as implements of good. Personally, as beneficent agents working through

the portals of a passing misery to the goal of a lasting happiness, I should rank them just as high as the plague bacillus. Now what are the facts? The volcanic and earthquake areas of the world are all well known. Where an earthquake occurs to-day it has occurred before. Where the destructive molten stream pours over a country and devastates its vineyards, it is but repeating to-day what it has done many times before in the history of Man. Yet no sooner has one eruption of Vesuvius calmed than upon its lava crust, still aglow beneath, new vineyards are planted. Is it beneficent or is it malign to harass and eliminate silly people who will deliberately return to the paths where they must know destruction lies in wait? When a great city has been destroyed by earthquakes in an earthquake region, and men yet build their hopes and lay down upon its rifted surface their material possessions, is it the fault of Nature or the folly of men if these hopes are disappointed and these possessions are destroyed in one short minute?

It is cruel, no doubt, to our sentimental ideas, that at the fiat of sentimentalists and to the tune of wailing voices earthquakes and volcanoes will not cease to hold possession of the areas where, by their permanence in the past, it has been indicated long ago that they have come to stay. But while they thus ruthlessly destroy the foolish races who are not wise enough to profit by experience, have they not in some small measure aided in the evolution of an intelligent and courageous race of

Japanese, who adapt the structure of their houses to the rigid requirements of earthquake mechanics ?

Cannot we say the same of that which, in the medical language of our peculiar and overwrought time, is described as " a great scourge," " a terrible curse," a " devourer of millions of people " ? I allude to the plague. It looks a malign disease upon the surface, but let us get deeper than these sanitary conceptions created by the growing army of sanitary officials, to the biological foundation upon which all these questions rest, and see if there is not something at least which we can call benign ? Plague is a disease essentially associated with dirt ; to-day it ravages those villages of India in which the subsoil is the product of generations of offal and domestic excrementa. In such a soil, the intermediate host of the plague bacillus, the black rat, lives. Clear away the soil, and with its removal or destruction that of the vermin which flourish upon it follow in its wake. Suppose the natives, whose inherently inartistic instincts allowed this subsoil to accumulate, will not or cannot be taught to voluntarily clear their villages of it and maintain a cleaner state of things, are protected against themselves by an organised staff, paid for by Government monies wrested from the cleaner citizens, are we thereby achieving good or evil ? Let us enquire what it is we are really doing. Here in these villages, races have existed and propagated every generation, for many centuries. Throughout that period dirt has been the condition which their

instincts have tolerated. In the light of our knowledge to-day we have no reason to believe that such defective instincts are not hereditarily transmitted, and much to show that they really are. No one who has carefully investigated family pedigrees can doubt the fact. I am quite certain from families which I know that the qualities of being dirty or clean, methodical or unmethodical, are hereditarily transmitted as such. I affirm nothing as yet regarding their Mendelian inheritance. I assert only their inheritance. If Miss Wodehouse will make unbiassed observations in the light of my statement I feel sure she will recognise its truth. The facts are familiar to everyone ; it is the interpretation which is lacking. Therefore if we set outside powers to work to save these people, so that their propagation may be long continued and prolific, we shall rear in increasing numbers vast hordes of inherently dirty people, who will have to be kept clean by an alarmingly increasing army of human wash-tub attendants, otherwise known as Government officials, paid for by the clean people. Why not at once let us be complete and logical ; and because the mouth is the main portal to the lungs and stomach, in which in dirty people all sorts of microbes may be lodged, ordain another army of tooth-scrubbers, who shall at a stated hour twice a day clean the teeth of all dirty people who will not voluntarily do it ? Ludicrous though it may seem to those old-fashioned but worthy people who believe that an individual should stand or fall by his own merits or at most by those

of his family, we are nearer than we imagine to the consummation of such a folly. The medical and dental inspection of the school children in our own country is but the prelude to further agitation by the sentimentalists; for even now they are calling for free medical and dental treatment in addition to inspection, and they will next demand the free supply of tooth-brushes and tooth-powders. Eventually finding that the tooth-brushes are consigned to the dust-bins, and the powder is forgotten, they will further demand a house-to-house visitation by County Council teeth-cleaners! In this way the social momentum of hysteria over the unwashed, unwashable, and hopeless increases until we fail to recognise in the current momentum the tiny mass and the slow pace we set in action only a generation back.

Thus the plague is benign to the race in the long run, though ruthless to the inherently undesirable individuals of the present. It evolves ethical beauty and destroys ethical ugliness. No doubt a few of the beautiful are also destroyed, but it is better that this should be so than thousands of those who are inherently ugly and dirty in their attributes shall be reared by the pamperings of civilisation. In this matter there are before us two alternatives. We have to choose between our overwrought humanity and the consequences which the manifestation of that humanity will beget.

Let us pass to the tsetse fly. Miss Wodehouse seems to regard it as a curse; so do medical men

and others. But are we quite sure we know the full extent of its operations in Nature ? It cannot yet be said just what link it makes in the chain of Nature's work. Suppose we shall succeed in eliminating it, have we yet thought out the possible consequences ? It is well not to forget the fact cited by Darwin, that the combs and nests of humble bees are destroyed by field-mice and these by cats. If, therefore, the people of a particular village, who are pestered by cats, destroyed every one they could, they would reap, as an unexpected and unpredictable consequence, the loss of their honey harvest. No one had ever imagined that there could be any relationship whatever between the presence of cats and the production of honey. But it is so. When the cats are destroyed, the mice will multiply, and these will raid the stores of honey carefully preserved by the bees. When the cats multiply the mice will be destroyed and the honey will be saved. There is more even in the concrete facts of Biology than is dreamed of in the abstractions of Metaphysics.

But let us grant that the tsetse fly is a malign agent. Is there nothing of good which it has achieved ? It appears to have evolved a race of wild cattle in Africa which is indifferent to its bite, since the individuals of that race have their blood swarming with the trypanosomic germs conveyed by the fly, and yet these cattle are healthy and vigorous. There has thus been evolved a race of cattle happy and strong in spite of the trypanosomes and the tsetse flies. Just as in Malta there has

been evolved a race of goats the individuals of which are quite indifferent to the presence in their tissues of the germs of Malta fever. In both cases the susceptible individuals have been eliminated and the immune have survived. What would Miss Wodehouse have ? The reverse condition ! A race of weak and continually ailing cattle and goats, and no vigorous ones. Truly then the metaphysicians would have problems to solve, beside which those of to-day would be easy and commonplace.

But still we will accept for the moment her conception of Nature as a place where the tsetse fly should not prevail, and that, although it is a quite natural thing, it is not an admirable one. Let us then banish the tsetse fly. Has it ever occurred to her that the severest struggle for existence in Nature is not between widely separated orders or genera, but between closely allied species ? If we banish this fly, are we quite sure that somewhere within the range of its geographical area there does not lurk such another species, whose numbers at present are by direct or indirect means kept down by the operations of this "fitter" tsetse fly ? Upon the destruction of the tsetse fly this partially suppressed species would become dominant and might conceivably introduce another trypanosome, against which the present race of cattle have not been evolved. Once more, therefore, through the processes of elimination of those which have by artificial interference been rendered unfit must a new race be reared, and the rash disturbance of

Nature's balance restored by a great indemnity, towards which even super-man may be called upon to contribute.*

It is vain to urge that because in our conception of things Nature appears brutal in her processes, therefore with regard to Man we should become artificial and ignore Nature. To endeavour to do so is to deliberately reject the fact, that Man is but a unit in Nature and is only one link in her long chain. Not even his possession of speech and of a long tradition raises him above it or removes him from it. If our resort to artificial methods is to result in increasing the sum total of human misery, then whether there be butcher-birds, earthquakes, tsetse flies, and typhoid germs, or whether there be none of these things, our clear duty is to go back nearer to that Great Mother upon whose breast we have been reared to our present greatness.

This much we can say of Her: that while even the butcher-birds dwell in our memories, we know the she-bear loves its cub, that the lioness will fight to her death so long as in so doing she protects her offspring. Day and night these wild instinctive mothers, born of Nature, hunt and care for their offspring. It is only under the artificial conditions where butcher's birds are not, but metaphysicians and others are, that parents are reared who spend their money in the public-houses, and whose children

* It is not difficult to conceive at least one process by which a given species of tsetze fly may keep another reduced in numbers or even induce its elimination.

are only saved from starvation because the more natural and fitter parents are rated and taxed to effect that purpose. It is also under such artificial conditions, where the biologist has not yet been adequately heard but the wailing of the sentimentalist has, that hereditary races of cruel and vicious and drunken parents are allowed to procreate and multiply their kind, while that of the nobler parents is threatened with a rapid extinction.

Miss Wodehouse seems to think (pages 132, 134) that in my treatment of the environment in relation to the individual I have left an ambiguous impression upon the reader's mind. I think if Miss Wodehouse will read the article again she will find no ambiguity manifested. But in this matter she pursues an old and futile line of argument, and reminds us that all which is implied by the conception expressed in the phrase, "survival of the fittest," is simply the survival of those "whose qualities give them an advantage" under the particular conditions which prevailed during their evolution. I hope she does not think I am such a careless thinker, that I have forgotten such an elementary fact. Any careful reader who will refer to what I said about the Tasmanians (page 54) will at once see that I dealt with that very point. It was there stated: "Some day we shall learn that the characters of men are relatively fixed and stable, and are the *products of evolution under set conditions*. As we find men, at any given period, in any given place, they are adapted to the *particular combination of conditions* under

which they have evolved, and *to no other*." Is there any ambiguity here ? Surely Miss Wodehouse could not have read this page. But in making this contention Miss Wodehouse not only shows she has not carefully read my article, but she delivers herself into the hands of her opponent. For if that statement be true, as we know it to be, then the masses who have evolved under the conditions in which we find them, are adapted to these conditions. Any alteration that is made in them therefore necessarily involves a new evolution. This means a new elimination. Does Miss Wodehouse plead that the masses shall undergo a new elimination ? Does she ask that we shall interfere with them, in order that death shall reap its harvest among them ? It is Miss Wodehouse and those with her, and not I, who ask that the "killing off" process shall proceed at greater pace ! I simply urge that consistent with the rights of others, people shall be left alone to enjoy the conditions and circumstances which belong to their line of evolution. I have protested against the sentimental busybodies, who under the cloak of a false protection carry the instrument of a real destruction. Miss Wodehouse then proceeds to say : " Given any environment Nature may be trusted to select." Then she puts the question which, if I have not misunderstood her, is the central thesis of her article, " Can we trust Nature to select such qualities as we shall approve of ? " Now I take two exceptions to such an attitude. I should not in the first place speak of " selection of individuals whose qualities give them an advantage

under the set conditions," but rather of the elimination of individuals, because they are not adapted to those conditions. The one implies a positive act which is not operating, and the other a negative one which is.

My second objection is to the assumed superiority of man over Nature implied in the sentence: "Can we trust Nature to select such qualities as *we shall approve?*" Now I am curious to know who are the "We." Miss Wodehouse quoted a sentence of mine containing the phrase "corruptions of a Royal Court," and added a comment of her own, "where the time-server and flatterer succeed and survive." I will not stop to remind her that this type of person is found elsewhere than in Royal Courts, and that elsewhere they are so unfit in their art that they are unmasked by their own crude craft. But I would like to ask whether the "We" means the uncultured and hormonic demagogue, who, incapable because of his inherent defects of acquiring either knowledge or property of his own inflames the passions of equally unculturable masses against citizens whose biological attributes have enabled them to acquire such things? Are the "We" those men who, envious of the honourable achievements, marked success, and noble family traditions of others, endeavour to hold them up to the ridicule of the "great unwashed" of democracy—which is still unwashed in spite of the fact that there is now no tax on soap and education is free—by calling them the "great rats"? Who cling to office at the cost of broken

pledges and who ascended to its platform up the steps of pyrophorus oratory that fills to the brim the gullibility and credulity of the masses? Or, are the "We" the sentimentalists who would give self-government to subject races, whose line of evolution demonstrates their incapacity to successfully perform the responsibilities attaching to it? Or, are they the people who are demanding that in the National Elementary Schools we shall have an army of "State nurses to bind up cut fingers"?* If these are the people involved in the "We," let us infinitely sooner go back to a Nature that would eliminate every Anglo-Saxon and Teuton to-morrow, than we should any longer disgrace our manhood by crying aloud for help because a few school children of the "People" have cut their fingers! Is this the type of selection which the "We" are going to adopt for the citizens of a nation whose forefathers won on bloody battlefields, ice-bound coasts and dense tropical jungles, at the cost of cleaved heads, battered limbs, and disentrailed bodies; who crossed the arid deserts and reached the verdant slopes and plains beyond; and who have made an Empire on which the sun never rises because it "never sets"?

* In view of the agitation for the enfranchisement of women, it may not be amiss to state that this demand was made by a woman. Also that at a Suffragist meeting on Wimbledon Common not long ago, another feminine advocate demanded that the State should provide flannel "which was expensive" for the children of the poor, because flannelette was inflammable, and would easily catch fire when mothers left their children alone in a room with a fire while they went out, presumably to the beer shop.

Poor and contemptible nation it will be which shall substitute such a "We" for a beneficent but merciless Nature which evolves a race of men to whom a cracked head in the service of their country is but a passing factor in their lives. Compare the product of such a Nature with that which the "We" seems desirous of evolving. "They run," is the message brought to the fatally wounded General; "Then I die happy," comes the simple answer from the lips upon which death has already set its seal, may represent the symbolic and actual product of the one, while a wailing crowd of sentimentalists quivering with emotion at the sight of a "cut finger" of one of the "People's" children is the product of the other!

Or, pursuing another line, are the "We" represented by those sentimental people who, sitting as Justices of the Peace, or as magistrates, or as juries, let free the inborn criminal who has pilfered and raided the property of others, that he may repeat his depredations, wasting alike the material possessions and the time of better citizens? That he may by the immunity which he enjoys subsequently encourage others and widen his own sphere of activity? The thrifty, capable, and acquisitive citizen, thus robbed illegally at one end of the scale and legally at the other, through rates and taxes imposed by the sentimental and, in very recent time, also by the vindictive "We": What line of evolution does all this indicate the nation

is following? Has Miss Wodehouse considered the question from this aspect?

This transcendent and omnipotent "We" appears also to be responsible for that remarkable product of Modern Sentiment called the "Boarding-Out System" of the children of paupers. This "We" believes that the children of congenital paupers and congenital criminals, whose grand-children, let it be remembered, will be also in the main paupers and criminals, can be converted into noble and self-supporting citizens if they have their dinners cooked in copper utensils, if they have more or less costly pictures placed in the passages, bedrooms, and lavatories of the residences they inhabit, and if their washstands are of marble. It also believes that if laws are framed which raise these children into a privileged class, and inflicts heavier fines for physical chastisement of their misdeeds than would be imposed upon anyone who assaulted the child of an ordinary respectable person, that straightway we are following the path that shall lead to their glory!

I will not dwell on this remarkable type of the "We" any farther, though one sorrowfully recognises that its remarkable peregrinations have by no means been traced in all their manifold ramifications. It will suffice for the present to say that Englishmen will one day awake, not to read an article in the Mendel Journal in which scepticism for the "We" is the dominant note, but to find the arms of that "We" stretching around and within their homes, ordering their every deed, determining their wishes,

spending for them their money, and generally directing their lives from early morn to night, and from the cradle to the grave. That is the inevitable goal of every artificial line of evolution determined by the "We."

Having dealt with the "We," let us turn next to another aspect of the question raised by Miss Wodehouse. If I have not misunderstood her, she implies that the conditions of human society are not ideal, and "that in fact we often condemn an environment because by strict natural law it gives the advantage to qualities which we condemn." Now in this matter Miss Wodehouse is dealing with the environment of human society. By implication she says the environment of the slums is rearing a slum race and one adapted to those conditions. Mr. Bernard Bosanquet, in his "Philosophy of the State" maintains the same attitude, if I have rightly understood him. I will not for the moment follow the logical extension of such a contention, but it suffices to point out again a fundamental fallacy involved in it. I have already in my article on "Virile Sentiment" alluded to it (pages 49 and 55). The fallacy consists in believing that it was the slum environment which made the slum people. The very converse, as I endeavoured to show, is true. It is the people of "slum" instincts who have created the slums. If they did not, we are bound to ask who did? Will Miss Wodehouse point out to me any Act of Parliament which ordained the creation of slums? Have the sentimentalists and social reformers for the past eighty years been

advocating abolition or formation of slums? Have the District Visitors created them? Did the Act of 1870 bring them into existence? Did the Oligarchy of England prior to 1830 or its Democracy subsequently call them into being? Is it the Republican Government of France, the Autocratic one of Germany, or the Oriental Despotism of the East that have made them in these respective countries? Or, finally, has it been the growing ethical codes of civilisation that has planted the slums—a disfigurement and a waste—in its beautiful and verdant valley? When a benevolent friend of mine, a Managing Director of a large works, situated three miles beyond a great city, built near them, for the ease and convenience of the workers and their families, a row of nicely arranged and well-equipped cottages, with gardens and bathrooms, and he found the workmen would not use them, but preferred to pay a daily train fare and a heavier rent, in order that they might live in the slums of the city, will my critic tell me who made these slums? Or, when as in the South Wales colliery districts, the houses of the colliers contain a bathroom, and the baths are turned into coal-cellars and dust bins, surely we are entitled to ask of Miss Wodehouse, if these are the people, who if "temperately trained may keep the love of beautiful and pleasant things as a deep rich undertone to their lives, making them delightful to themselves and to all around them." Would Miss Wodehouse care to undertake the "temperate training" of these people? I urge, with all humility, that if she really

believes what she writes, which I do not doubt, that an imperative duty lies upon her to undertake the task, and to demonstrate the reality or falsity of her belief.

If Miss Wodehouse will pass down a certain street near a certain great railway terminus in London on a Saturday evening in the Summer, until she reaches another street at its other end, she will be passing through a slum area. Yet in the upper part of the first street (the end nearer the station) she will find on either side of it, on the whole, a row of clean, well-kept, neat, and orderly houses, inhabited by a respectable artizan class. In the lower street and its environs the people are mere animals. The streets are crowded with children almost naked, whose bodies and limbs are freely exposed, who are swarming with vermin and infested with sores and boils, solely the result of dirt ; there, too, are habitual drinkers, and mothers careless in behaviour, uncouth and unkempt in appearance, and unsavoury and repugnant to every one of our five senses. Can Miss Wodehouse tell me why these two adjacent streets leading into each other are so different ?

Who made these two environments, situated under the same sun, the same government, the same rate collector, served by the same school and public-houses, infested by the same dust-laden atmosphere, with the same examples and influences operating upon both ? The answer of Biology is that it is the inherent, congenital nature of the people living there. The answer of the older Theology is

"Character." What is the answer of Metaphysics? Is it the omnipotent "We"?

The considerations and facts which I have put forward above must form my answer to Miss Wodehouse's question to me on page 135 of her criticism: "Is it Mr. Mudge's opinion that the present slum environment selects its survivors on the basis of qualities so valuable and so varied that we can no longer hope to improve it?"

I once more assert that the whole evidence before us, when rightly analysed, leaves no alternative conclusion but that the slums are made by the slum people, and that there does not exist among them as a class any good qualities which can be "selected" either by man or Nature. They are there *as slum people* because of the absence of any such good qualities which can make them otherwise. I will not further labour the point, but refer my readers to the considerations and facts which I advanced in support of this conclusion on pages 49-57 and 94-96 of my original article.

Next let us turn to the remarkable moral which Miss Wodehouse extracts from my article. We are told that the moral of the teaching that all natural processes are benign "would seem to be that we should make haste to remove such flagrant interferences with Nature as are found in the main drainage system and the organisation of the metropolitan police." I should be sorry if my critic could point to a single utterance in the article which she criticises which could legitimately justify anyone in inferring

that any such moral could be drawn. Conversely, the very opposite doctrine is impliedly taught. I have advocated that the fit should enjoy the benefits of their fitness. Now under civilisation what is fitness ? It is the possession of that degree of capacity, honour, and morality which makes a man a useful member of his community. No man who becomes a burden upon others, except upon his own family, can fall within such a category. Now who made the main drainage system possible ? Assuredly not a Government ; equally assuredly not sentimental considerations ; nor free education ; nor medical inspection of elementary national school children ; nor free feeding of the children whose parents spend the money which should have provided their dinners upon beer and tobacco for themselves ; nor the workhouses and infirmaries. None of these things, all of which are institutions for the weak and defective, and the civic lame and halt, built the main drainage system. That was essentially the product of the fittest intelligences and the highest organising capacities of a people left free to enjoy the fruits of their labours. It was work undertaken by the fit for the benefit of the fit. That such work should continue and such capable citizens be encouraged to work, and not discouraged through the legal robbery of the products of their fitness, which they are now experiencing, is precisely what I have been urging.

With regard to the Metropolitan Police, very much the same considerations apply. This is an institution organised by the fit for the protection

of the fit. Fifty years ago it did its work effectually, and the thief when caught was hanged. But now, as I have already pointed out, under the baneful influence of a sentiment gone mad, the work of the police is being progressively nullified and undone. The police may catch the thief, the magistrate may agree that a drastic punishment is required, but outside sentiment foolishly sets the criminal at large.* So far has this pernicious sentiment gone, that after a judge and jury have condemned to death a cowardly murderer, who has walked into an old and defenceless man's office, and shot him dead, there can be found a Home Secretary who will yield up justice to the clamours of servant girls and of small suburban tradesmen! Most assuredly I am for the police, as an organisation of the fit, which, when efficiently and justly worked, will remind the "unfit" of their place in the scheme of things.

Miss Wodehouse, not content with fancifully distorting the logical outcome of my position, goes even further, and commenting upon my plea that it is best for the congenitally defective to accept their destiny, as much greater citizens have done, proceeds to assure us "That acceptance of destiny is found purest

* See the comments of Mr. Lane, K.C., magistrate at the West London Police Court, in a case of embezzlement, where he remarked that he held the view that such offences should be rather severely punished, but in deference to public sentiment ended by merely binding the defendant to come up for judgement if called upon. Or, where, as at the Wimbledon Bench of Justices, hooligans caught red-handed in the act of thieving are set free merely with a paternal warning not to do it again. A warning which is so effectual that two out of the three thus dismissed are charged a few weeks later by the police for a similar offence. In the meantime, it may be left to the imagination as to the amount of annoyance and loss which the thieves had caused to the honest citizens.

in that mother who refuses to alter her child's environment to defeat scarlet fever or concussion of the brain." Were I a Carlyle what thundering sentences I should write in reply to this. But I cannot; so I will simply assure my critic she is quite mistaken. It would not be the purest destiny but crassest folly for a mother to do anything of the sort. What I said in effect was this: When we find mothers so inherently defective and so devoid of parental love and foresight that they cannot or will not save their children from preventable misery and injury, it is better such a mother should accept her destiny than that we should help her to multiply and propagate such a foolish and defective race. It is better she should accept her natural destiny than that capable and loving mothers should have the destiny of the wicked artificially cast upon them and their children by the act of sentimentalists. If we spend a sovereign through the rates upon the vicious mothers, inevitably we take it from the pockets of the good and careful mothers. Sovereigns are not made in heaven by metaphysicians and showered upon the earth at the cry of the worthless; they are the product of the labours and capacities of the biologically fit.

Seeking for further illustrations that natural events are not, according to the standard of Miss Wodehouse, necessarily admirable events, she cites the "mortal combats in animal societies between jealous wooers." Well, are these not admirable? I think they are. So long as it is a fact of Nature that some

wooers are strong and others less strong, I shall continue to hope, in the interests alike of animal welfare and of the truest ethics, that mortal combats shall continue, and that the strongest shall win. Would my critic desire to see the weakest win and multiply its feeble race and spread it wide upon the surface of the earth? Is she quite sure that these mortal combats are distasteful to the animals which indulge in them? Has she not rather jumped to a conclusion and measured their psychology in the bushel of some fearsome and timid person? I have my doubts; from what I have seen and read I should imagine they rush to combat as a hungry man does to a meal. Miss Wodehouse cannot prove that the motor stimulus and the motor gratification of the combat are not enjoyable things and are of the nature of a normal physiological activity in the absence of which normal life would be impossible. She will surely recall those savage races of men who seek a gratification of their heightened motor impulses by laceration of their bodies. To such men this is pleasure, and not pain. It is one of the most fatal mistakes to imagine that because to fight and be lacerated would be to some people a painful and fearful thing, that therefore it is so throughout the whole animal kingdom. It is not, I believe, even true for man. On the steppes of Tartary the women are not happy unless their husbands beat them periodically; they imagine they are not loved if they do not receive this attention. There can be but little doubt that prize-fighters and others are

incapable of feeling pain to the extent that more normal people do. Why then does Miss Wodehouse think that mortal combats are not desirable? They result in the elimination of the weak and the survival of the strong, who fear not strife and relatively feel not pain. Surely that is a desirable state of affairs. It is better to have a strong stock than a weak one; one that cannot feel pain, or, feeling it, can endure it with Spartan silence, rather than one which shrinks away at the sight of any implement capable of inflicting pain. If Nature has evolved that race, surely it is superior to that which the "We" is endeavouring to bring to a fearsome and miserable existence.

Very much the same considerations hold for that other supposed horrible state of things, which I am accused of commending as desirable, where a certain race of "ants carefully nurse the larvæ of beetles, which will presently eat the baby ants." But is there any evidence at all that this is a painful process? Even if we suppose it to be so, is it more painful than would be the death of the beetle larvæ, which would perish from starvation if they had not the baby ants to eat? If Miss Wodehouse shudders at the fate of the one, does she also shudder at the fate awaiting the other? These things are unavoidable; they are an integral part of the operations of Nature; and the Kea Parrot which swoops down upon the back of sheep and is said to extract their kidneys, and the butcher-bird which impales beetles and frogs upon thorns to a tree trunk, are doing no

more than Man is doing. We must in reviewing such events depart from particular aspects and come to the general ones. Man and the butcher-bird and every other organism are impelled by the instinct of self-preservation. To preserve itself the butcher-bird does one thing and Man another. No artificial alteration of the environment can modify or destroy this instinct. We may as well lament that the earth is not Mars, as to bewail the existence of this instinct in animal life. We have to recognise that it exists, and that every battlefield and every revolution brings it to the surface in all its nakedness and ethical horribleness, as a quality deep seated and inherent in Man. We have also to recognise a further fact. The moment a race loses such an instinct, its end is near. It will be destroyed by one whose instinct of self-preservation is strong.

Miss Wodehouse asks, "Seriously, what is this Nature which includes and justifies the doings of all animals but man? Which render infallible all instincts and aspirations but his?" "I can find no profit," she says, "in this way of using words." Neither can I, and that is why I did not use them in that way. I should not, for instance, ascribe aspiration to the lower animals. But I pass this by, for I desire to come to the implied and supposed infallibility of the instincts and aspirations of some men, perhaps those for whom Miss Wodehouse may conceive some hope and place them among the "We." Let me recite the facts. A girl of eighteen was in a London workhouse. A male friend of hers and a ne'er-do-well

was outside of it, for the moment. By some mutation of chance he had come into possession of half-a-crown. He proceeded to the workhouse and asked if the girl could come out of it. When asked why he desired her to come out, he replied "because he had half-a-crown, and as he did not know when he should earn another, he would like to marry her now!" Perhaps from the heights of metaphysics we may be informed whether this is instinct or aspiration, and whether it is fallible or infallible?* In another London workhouse there is a woman inmate with numerous illegitimate children by several different paramours. Such events are quite common. Is this too an "infallible aspiration" of man's? The sentiment which at the cost of thrifty and respectable people maintains such persons, may also be regarded, I presume, as another human infallible instinct, or may it be an aspiration! I will not multiply these instances, though I have records of hundreds of them. Truly "no profit can be found" in such metaphysical language as the "infallible instincts and aspirations of Man," but, as Huxley would have said, only in the "veracious facing of the ugly facts of life."

We will come finally to section three of Miss Wodehouse's criticism. She calls this the most serious of all her objections to my plea for the operation of a more virile sentiment in human affairs.

* While this article is passing through the Press, the *Evening News* of July 9th, 1910, publishes the application of a pauper who had an income of sixpence weekly, to the Gravesend Guardians, to help him pay the cost of a marriage certificate. His "young *lady*" was in the workhouse. Is this, too, one of the infallible instincts of Man which are so superior to those of that Nature which Miss Wodehouse impliedly condemns?

She attributes to me certain "assumptions of a very remarkable nature as to the unit qualities of the mental world." I at once disclaim having made anywhere in my article a single assumption as to the nature of mental unit-characters. Nowhere in Miss Wodehouse's article can I find the least reference to any specific case, which in fairness she should have quoted, word for word. That they exist and will be one day formulated, I have little doubt. I will presently give some tentative evidence suggesting that such unit-characters may exist. But if my present readers will read again pages 57-59 of the article on "Virile Sentiment," they will be able to see what I actually said in the matter. In a few words, the substance of my remarks may be thus summarised: It was contended that in regard to some of the qualities of man, the evidence is pretty clear that there is segregation in their hereditary transmission from one generation to another; that there was no reason to believe that the mental and moral qualities of man are transmitted in any way different from the physical qualities. As examples of the transmission of mental qualities, I cited the historical cases of the eloquence of Marcus Antonious and of the high statesmanlike qualities that were known for many generations to descend in the family of Brutus. Since then I have been working at human pedigrees, and it seems clear, that whatever may be the nature and the number of the unit-characters, if any, involved in such qualities, that the mathematical faculty and inventive genius are hereditarily

transmitted as such. If Miss Wodehouse will read Dr. Adams Woods' book on "Inheritance in Royalty,"* she will find abundant historical instances of the transmission of normal mental and moral qualities, and of high intellectual capacities. But the actual existence, nature, and number of the possible unit-characters involved is a matter for future investigation. More than this I have nowhere implied.

In the passages which I have already indicated I spoke of the hereditary transmission of those qualities that make the "loafer, the wastrel, the congenital drunkard, the habitual criminal, the congenitally tuberculous, the mentally deficient, the thriftless and generally incapable persons." Does Miss Wodehouse deny the inheritance of these defects? I gather not, for she says "I yield at once to my opponent the obvious exception of such definite deformity in the mind or brain as shows itself in insanity or feeble-mindedness"; and she admits "there is good ground for believing that these may be units." Having made that admission, I must confess I fail to see the justification of her attitude towards mental traits which are normal. Does she wish us to believe that normal and abnormal qualities of mind or body constitute two separate and distinct kingdoms of characters? If so, what explanation is forthcoming of the fact that feeble-minded children are born of the same parents who produce normal children; that in families where

* Reviewed in this number of the Journal.

insanity has been known in each generation for several generations, some sons and daughters are sane and others are insane; that sane parents may produce insane offspring; that in families where "hooliganism" exists and can be traced at least in two generations, there can be found in the same generation of brothers and sisters some who are hooligans and others who are respectable and hard-working persons? I am speaking from personal knowledge derived from investigations not yet complete, and I find it impossible to fall in line with Miss Wodehouse's attitude. If these things occur in the same parentage, and an abnormal feature may be regarded as "a unit-character," why not the other, which appears to be its alternative character?

Miss Wodehouse has attributed to me an attitude in regard to mental unit-characters, which as a cautious man of science, I did not adopt, and which I was very careful to avoid. But since she has raised the question, I am not at all averse to discussing it on general grounds. I would like, however, to say, for the interest of those who are not conversant with Mendelian methods, that a general discussion is not Mendelism, but is of a converse nature. I am now merely concerned in dealing with certain theoretical considerations.

Miss Wodehouse says that she cannot regard those qualities which we praise or blame in sane men as being due to simple hereditary units. Now, how is it possible to test this question of the existence of unit-characters in human mental life? By

precisely the same tests that we apply to it in all our experimental cases. We have two antithetic or alternative qualities to deal with. If they are transmitted as segregable characters and they appear in the descendants in certain ratios, we interpret the collective phenomena on the basis of unit-characters. No cases of this kind have, as far as I am aware, been investigated by rigorous Mendelian methods. But still I think there is some evidence which points strongly in the direction of the segregation as unit-characters of these subtle mental differences that distinguish sane men from each other. Let us imagine what would happen to a normal person if it were possible to extract from his nerve cells that which we can all conceive as possibly being a unit-character, namely, the Nissl granules.* What would be the phenomena resulting from such a simple change in the constitution of the nerve cells? As far as we can gather, from the slight physiological evidence we have before us as to the part played by these granules as sources from which the nerve cells derive their stores of energy, it would not be a simple or single phenomenon, expressed in a single word. They would be a whole group of phenomena indicating profound organic mental and physical disturbances. If this be the case, what validity is there in Miss Wodehouse's contention that the mental qualities which we admire or blame in sane

* There is no need in this article to deal with the nature of these granules. That they represent a something from which nerve cells appear to derive their energy, is sufficient and accurate enough for my present purpose.

men appear to group themselves into groups? Assume that it may appear to be the case, is it really so? May not a whole group of qualities—inherited as a whole—be simply due to some simple hereditary factor, such as the Nissl granules, or to a few factors hereditarily coupled together? When such a contention as that maintained by my opponent is accepted by her, is she not merely making an assumption? She is assuming that appearances are realities. If we must make assumptions at all, at least let us do so in accord with our available knowledge, and not in discord with it.

There is yet more to be said against her contention. Physiological knowledge leads us to the conclusion that our spontaneous and reflex actions depend upon the existence of certain groups of nerve cells, collectively spoken of as nerve centres, which may act either singly or in combination with others. The nerve centre that controls the beating of the heart is lodged in a certain area of the hind-brain; that of speech, which appears to be asymmetrical, in another area; that of audition in another, and so on. Now, in the course of individual developement, whence came these groups of cells? Recent embryological investigations of the experimental order show us clearly that, not only groups of cells, but whole organs, may take their origin from a single cell or blastomere, in the early stages of the segmenting embryo. If that cell is destroyed the organ which arises from it will be absent in the definitive (adult) stage. Such a cell may be regarded

as the carrier of the unit-characters which determine the number and the qualities of the cells of a particular group in the adult. What does it matter if such an embryonic cell carries one, two, three, or more unit-characters? So long as it is that cell which has dropped out from the hereditary mechanism, the whole group of unit-characters carried by it behave as a single-whole, and the adult organism will have lost not one but a number of qualities. It is conceivable that the differences which distinguish some hooligans from some respectable people, which mark the brilliant person from one a little less brilliant, which contrast the lethargic with the active, and which stamps the difference between the orator of Celtic fire and overflowing language with the unimpassioned person of few and adequate words, may be due to the presence or absence of such embryonic cells carrying one or more unit-characters. Or, the absence of such cells may be rather potential than actual, and due to the presence of some inhibiting factor, which, although the early embryonic cell or cells were present, may have inhibited their subsequent multiplication by division.

These contentions I am quite aware are at present hypothetical, but they arise from the knowledge of our day. If we must go forward in advance of the knowledge of the present, then let us follow the road which that knowledge best indicates, rather than the one which runs in an opposite direction.

But we can look at the question in another way. An individual may be conceived to be made up of a

bundle of characters not separable from each other and transmitted as a whole. That is the old view of inheritance. Under such a conception we look to the transmission, not of characters, but of individuals. But this conception is being rudely shaken, and in many instances clearly does not express the truth. In human life, and in mental traits, it obviously does not hold in certain cases. For if it did, how do we explain the fact that the mother's mental and the father's physical capacities may pass to a particular child ?. . Clearly in such cases there has been a separation of definite characters from individuals, and their recombination in another. It is in this way, among others, that unit-characters behave in hereditary transmission. As far as it goes, therefore, such a fact indicates the existence of unit-characters in Man.

In further discussing the question along this line, I may cite some instances of what may possibly be examples of a unit-character, the presence of which produces a characteristic trait, which have come under my personal notice. The people who are concerned in these cases I have known intimately throughout the whole period involved in each. Imperfect, as scientific demonstrations, though they are, I am the more tempted to describe them, since by so doing it may be possible that the attention of others will be called to the nature of what must be quite familiar observations. I assume that Miss Wodehouse will grant that speech is related to mind ; and that any characteristic intonation which may be manifested may be regarded either as a unit-character or as a group of such

characters which are hereditarily transmitted as a whole. Now, I know an instance where a young girl child when three years old had the same characteristic intonation in her voice that marked the speech of a feminine cousin of her father's. The young child had never seen this cousin, and the father's speech was quite devoid of the intonation. It could not therefore be due to imitation or instruction. The young child's brother did not manifest it, neither the father nor his two brothers; neither the two grandparents, nor any of their brothers. Neither did the only sister of the cousin, nor any of her brothers, or either of her parents. Though characteristic it was essentially a feminine intonation. It must therefore have been carried as a recessive character in the gametes of the child's father and paternal grandfather, and the cousin must have received it through the gametes of the child's granduncle. I do not see how we are to interpret this case, except on the basis of segregable unit-characters. Of course, it is only right I should say that in comparing the intonation of the voice in the two people concerned I am trusting entirely to ear and memory, and that the comparison has not been made with the two people side by side, nor under other conditions that scientific precision would demand. But I do not think I am mistaken, and I recognised the intonation some years ago, as an ordinary fact of life. It is only now that Miss Wodehouse has raised the question that I recall the matter.

This instance recalls to me another event of a very similar kind. Some years ago I knew a lady under her married name only. Her maiden name I did not know until the event occurred which I am about to describe. She was, among other things, interested in Celtic folk-lore and symbolism. I have often conversed with her, and I had noticed when speaking to anyone she had a very characteristic style of expressing herself, not in her choice of words, but again as what I cannot better and yet hardly correctly describe than as an intonation. Two years ago I was in Iona, and was introduced to a certain gentleman from Edinburgh. Upon walking one day through the ruined Nunnery and Cathedral, and discoursing with him upon the meaning of the various symbols sculptured out of the stone ornaments, I imagined that I must have heard him before. But no events which I could recall brought me in any way into relation with him, and I dismissed the matter. During one of the subsequent sentences which he uttered in describing a certain symbol, so exactly did his intonation resemble that of the lady who had discoursed on a very similar topic several years previously, that I could have no doubt he must be a relation of hers. Accordingly I asked him if he were not a brother of Mrs. ——, and he answered that he was. I also know the sister of this lady, and her mode of speech has nothing in common with either her brother or her sister. They were reared together as children, and we cannot, I think, attribute this resemblance of

speech to mere unconscious imitation, otherwise the other sister should have manifested it. The resemblance in the intonation was so close that had I not known it to be a gentleman before me, I should certainly have thought it was the lady.

Miss Wodehouse says "that a great part of the social value of a man depends not on anything that we can believe to be simple qualities, but on combinations of these." This is precisely what I endeavoured to say in my article when I wrote:* "An organism does not manifest a single quality alone, but is made up of a complex combination of many qualities. Some of these qualities, be they structural or psychical, are themselves not simple, but complex. Some of them are independent of others in the hereditary transmission, but others are correlated and always go together. . . . These facts render all questions of social interest extremely difficult to consider, and are the best justification for the *laisser faire* attitude." Because matters are so complex, does Miss Wodehouse think that her transcendent "We" is more qualified to deal with them than Nature? She condemns her own attitude with her own words. It is because these things are so seemingly complex that I have urged we should leave them alone.

With regard to the influences of education and to Miss Wodehouse's contention "that some part of the social value of individuals depends upon the fact that organs increase their capacity by use;

* Page 13.

that a wise training may increase the power of taking responsibility; and that a boy with strong enjoyment of bodily pleasures, whose desires if unwisely indulged may turn to devils within him, but who if temperately trained may keep the love of beautiful and pleasant things as a deep rich undertone to all his life, making that life delightful to himself and to all around him." All these conceptions and possibilities we may recognise, but they have nothing to do with my article. Whether the boy with strong bodily pleasures will be a good or bad citizen, depends not on his nurture but his nature. If with these strong pleasures he is yet capable of responding to good influences, he will do so. But if his inherent characters render him indifferent to such influences, he will be swept along the path which leads to the gratification of those strong pleasures. All through her criticism my opponent has been dealing with the good, capable, and responsive citizens; while in mine I dealt with the bad, incapable, and irresponsive ones. I have never said that a good citizen will not be a good one, nor a responsive one irresponsive. But what I did urge was the folly of believing that inherently and congenitally bad and irresponsive citizens could be turned by education into good and responsive ones. Miss Wodehouse has not endeavoured to controvert that plea, but has taken a line of argument entirely foreign to the purpose of my article, and has implied that I disbelieve or oppose precisely those things which I do as a matter of fact accept as obvious

truths. If Miss Wodehouse contends that she can by "temperate training instil a deep rich undertone into the lives" of *all* boys, I am afraid, if she attempts it, she will experience many disappointments. I have in mind just now two young boys of a hooligan family, and if Miss Wodehouse would care to undertake such an instillation, and if those philanthropists who believe that "golden conduct can be wrought from leaden instincts" will test the courage of their convictions by defraying the cost of the process of instillation and of the keep of these boys from now until they are twenty-five or thirty, and will give them the ordinary liberties of citizens, we shall have come down from metaphysical platitudes to mundane biological facts, and have ascended to the experimental verification or destruction of our beliefs.

These considerations hold, too, for the common-place of ethics cited by her, namely, "that nothing in heaven or earth is good without qualification, except the good-will." But if there be no good-will the "common-place of ethics" has no meaning; while the biological fact that the relatively good and bad are hereditarily transmitted is full of pregnant significance.

Miss Wodehouse has urged that the civic value of a quality may be rendered negative or positive by the "direction of purpose and will." She thinks it is difficult or even not possible for natural science to judge the nature of this direction and this purpose and will aright. She says "that from the standpoint

of moral science, it is to be urged with all possible emphasis that to treat such direction as a simple quality, transmissible as a deformity is transmissible, is impossible in every way; it is absurd; it is mythology." Has Miss Wodehouse ever read of the Jonathan Edwards' and Jukes' families? If not I commend them, not only to her attention, but to a reverent study. She will then find that mythology becomes fact and absurdity becomes reason. She will find that while metaphysics has been soaring in the Empyrean regions of speculation and abstraction, incontrovertible fact has been walking on the earth.

Let me make one disclaimer of my opponent's statement. I have not said that "purpose and will" may be regarded as a simple quality. I did not deal with the subject at all. But I am prepared to say and to prove that the capacity of forming wrong purpose, the capacity of determining will in the wrong direction, are both hereditarily transmitted. I am prepared to state it in another way, and to say that there are some people who can never form a right purpose, nor develope a right will, nor say the right thing at the right time, nor do the right thing at the time it should be done; and that these incapacities are hereditarily transmitted and are not cured by education. I do not know whether this is a "common-place of ethics," but I think everyone will recognise it as a common-place of life.

Miss Wodehouse throughout her criticism has misunderstood the standpoint from which my article

was written. Consequently she has been criticising many matters which she herself has raised, and which find no place in the plea I urged for a more virile treatment of the affairs of human life. When she urges "that from the side of psychology, nobody can say at present, except to a minute extent, what the inherent unit-characters of mental qualities are," she is dealing with one of these self-created subjects of easy criticism. To play at nine-pins by putting up the pins of your opponent in the most favourable position for bowling over, is, of course, a facile way of demonstrating his stupidity. But I confess I prefer to arrange my pins myself. The question which concerns us for the present, as a body of cautious citizens, is not whether it is yet demonstrated that mental traits are transmitted as Mendelian units or not, but whether they are hereditarily transmitted at all. Upon that matter there is no question. It is undeniable. Any intelligent and observant parent can answer it. Children think and play in some things just as their parents thought and played at the corresponding age. They may manifest little tricks that were the peculiarity of one of their grandparents, whom they have not seen. To endeavour to turn my plea from its sociological bearing and from those facts which are sufficient and legitimate for practical affairs into an academic groove, is but to cloud the issue which my plea raises, and to beg the question with which it is concerned.

Similarly, that vague and unsubstantiated accusation impliedly brought against me in the closing

paragraph of Miss Wodehouse's criticism "of encouraging metaphysicians to slur distinctions, to draw easy *a priori* conclusions, and to commit sins of rough and careless work," may serve well as a peroration; but, unsupported by adequate citations from the article which she criticises, cannot help students of natural science to strengthen their belief in the existence of the scientific spirit in the methods of metaphysicians. But if it is possible that metaphysicians are susceptible to the wicked encouragements of erring biologists, is it not time they resigned their position alike as intellectual speculators and critics?

If metaphysicians are swayed by extraneous and irrelevant influences, what value can we attach to Miss Wodehouse's criticism? Are we justified in accepting her *obiter dictum* concerning "the real and valuable political truth which lies behind the scorn thrown on environment"? It may be that we are impelled to think that political convictions have influenced the judgement more than those harsh and ugly facts which none of us like. We may be driven to believe that when once Miss Wodehouse has admitted the crucial fact "that mere environment is truly of very little account compared with the presentation to us of objects which we are prepared to love," to endeavour to tone it down by the doubtful hope that "a clean environment *may* be just the chance for which the developement of a questionably existing instinct of bodily comfort was waiting,"

suggests an attitude of mind which persists in indulging in futile hopes at any price. "To live in hope though we die in despair" may be good counsel to those for whom life has in store nothing more solid than aspirations which remain unfulfilled, but it is neither science nor sound counsel to a nation that has forgotten all that was good in its religion and has ignored the best of its traditions. We cannot rest a nation's destiny upon a metaphysician's "may"; but only upon Nature's "is."

But though, for the sake of argument, we may grant the possibility of her hope, and suppose that on a given day in the life of a youth born of hooligan parentage with a similar grandparentage behind it, there may arise a spontaneous and burning desire to become washed with hyssop and to become changed into a saintly citizen, at somebody else's expense, and that he only awaits the coming of the right environment created around him by the national grandmother and her retinue of State officials, how futile is this metaphysical abstraction compared to the mundane and concrete biological fact that given an organism with right inherent desires, the right thing will be done, even under the most adverse environment. Is it not better to have the noble man unscathed and unscathable by the fires of temptation than to endeavour to render indestructible that which is incapable of transformation? I commend to Miss Wodehouse two names that must be ever honoured by those who admire dauntless and brave characters—George Stephenson and Captain Cook. They achieved their

greatness without national doles or the expenditure upon them of metaphysical hopes and "mays." I would fain recommend the biography of these two men to the study of metaphysicians. It is possible that if Metaphysics began and ended with a complete study of these two lives, it would avoid those pitfalls and erroneous conclusions into which a wider if more superficial study appears to have led it. I think all will agree that it is better to have a nation of Stephensons' and Captain Cooks', wrought into great citizens by their inherent worth, than it is to have one of incompetent clerks created by County Council Scholarships out of persons who would have been alike more useful and ornamental as manual workers. Scholarships of this sort are themselves but the outcome of unsound metaphysical abstractions and social hebetations.*

* For further elaboration of the plea in regard to the incidence of Plague and to the relative insensibility of some animals and men to pain see Postscript, page 208.

METHODS AND RESULTS.

A Note Regarding Variation in the Single Combs of Fowls.

By RAYMOND PEARL.

In the first number of the Mendel Journal, a copy of which has just reached me, I find a number of pages under the heading of "Variation in the Single Combs of Fowls. Some Mendelian Comments," devoted to a rather violent and caustic criticism of a recent paper* by my wife and myself on this subject. Inasmuch as "Ardent Mendelian," whoever that person may be, in these "Mendelian Comments" gives an entirely and absolutely incorrect statement of (*a*) the purpose with which the paper he criticises was written, and the problem with which it deals, and (*b*) my general standpoint with reference to the problems of inheritance, I feel bound to make the corrections which stand in the present note. A failure to reply would, I fear, be taken by the readers of this Journal to mean that I really hold the opinions attributed to me by "Ardent Mendelian." In this reply I must state at the outstart that it is not my purpose to attempt to embellish what I have to say with such a wealth of rhetorical ornament as is displayed in the writing of "Ardent Mendelian." If, as "Ardent Mendelian" would I suppose contend, Mendelians are all right and biometricians are all wrong in their views about inheritance, I venture to think that the interested biological public is rather more likely to be convinced of this fact by a plain and clear statement of the reasons why it is true than it is by the methods of sarcasm and ridicule which he adopts.

* Data on Variation in the Comb of the Domestic Fowl. Biometrika. Vol. VI. (*not* IV.) pp. 420-432. 1909.

The points which I wish to make in this note are :—

(1) A clear and correct statement of the purpose with which our study of variation in single combs was undertaken.

(2) An attempt to show that this purpose was a scientifically justifiable one.

(3) A definite statement of my position in regard to the problems of inheritance in general.

(1) "Ardent Mendelian" states that the objects of the investigation reported in the paper criticised were (1) to answer the "one question as to whether single combs or their various grades breed true" (Mendel Journal, p. 183); (2) to determine "whether these different varieties of single comb may be hereditarily transmitted in accordance with Mendelian principles" (*loc. cit.* p. 184); and (3) to "cast doubt upon Mendelian conclusions" (*loc. cit.* p. 193). Each one of these statements is absolutely and entirely incorrect, and I hereby challenge "Ardent Mendelian" to bring forward any evidence from the paper he criticises to justify them.

As to what were the objects of the paper I cannot do better than quote from the original: "All studies of this kind take their point of departure in an attempt to analyze a broad Mendelian category. In Mendelian discussion 'single comb' is a 'unit character.' All 'single' combs are put together in one category,* all 'pea' combs in another. But nothing is more certain than that all single combs are not alike in respect to any feature whatsoever, even including their 'singleness.' How much and in what ways do they vary? Do the variants *within* the category mendelize? Are all variants exactly equivalent in crossing with other categories? An answer to these and other easily suggested questions could not fail, it seems to us, to throw light on the problem of the constitution and physiology of the gametic determinants of 'unit characters,' assuming that such determinants exist.

"In *this paper* we have endeavoured to give a clear and, so far as possible, quantitative *description* of the

* This, I may say, was written before I had seen Rep. Evol. Comm. for 1908.

nature and amount of variation normally occurring in a homogeneous pure-bred strain of Barred Plymouth Rock hens in respect to the form and size of the comb. *The aim of the paper is purely descriptive, and it is regarded by the authors as preliminary to the analytical investigation of comb inheritance.*"

The portion of this quotation which is here italicized one would suppose to be a sufficiently clear statement of the fact that the writers were studying or about to undertake the study of the inheritance of comb characters by the application of Mendelian methods and that the present investigation was simply preliminary. By "an analytical investigation of comb inheritance," I meant and still mean a Mendelian investigation, in so far as that I know of no way whereby one can determine how a particular type of comb is inherited other than by breeding birds which possess this type of comb amongst themselves and with other breeds of fowls and studying the *individual* pedigree records so obtained. Such investigations I have been engaged upon for nearly three years* now, and while I have not counted up the number of pedigreed chicks that have passed through my hands in this time, I fancy that it is certainly not so far from the "12,500" with which "Ardent Mendelian" endeavours to confound and overwhelm me. I protest most vigorously against the intimation that I am so stupid as to undertake a purely descriptive biometrical investigation of *variation*, and *only* that kind of an investigation, for the purpose of determining whether the domestic fowl "breeds true" with reference to comb characters!

(2) The next point which I wish to consider is as to whether the objects which we had in mind, as set forth above, in undertaking the investigation and publishing the paper under discussion, were biologically valid. I think they were, though I freely admit that there is room here for a difference of opinion (which is not the case in regard to the matter of fact raised under 1). While I grant unreservedly (and have, indeed, in the

* It should be said that the Biometrika paper under discussion was written in the early fall of 1908, though it did not appear until well on in 1909.

papers* from my laboratory tried to emphasize) that the somatic condition of a character is, generally speaking, an unreliable criterion of its behaviour in inheritance, yet I am very strongly convinced that the careful and thorough study of somatic characters and their variation, quite apart from the study of their gametic determinants, is a very important, though distinct, branch of biology, and one which at every turn may be of help to the student of heredity. I am most heartily in sympathy with that point of view in biology lately so ably championed by Prof. Wm. E. Ritter, which aims to investigate " things as they are " in biology, not as they " might be " or " ought to be " on this, that, or the other theory.

It seemed to me that within the broad category of " single comb " there might exist more than one gametic type. Fully understanding (contrary to the intimations of " Ardent Mendelian ") that the only way ever to settle whether or not this was the case was by means of the breeding pen and the individual pedigree record, nevertheless I thought and still think it a useful and valid piece of preliminary research to determine what different kinds of combs actually occur in a " pure bred "† flock of birds, particularly since I was using the birds from this very flock in Mendelian experiments. It seems to me that " Ardent Mendelian " virtually admits this himself, since he picks out on the basis of our figures alone four " possible gametic types " of single comb. Hitherto in Mendelian writings I have never seen any reference to more than one gametic type of single comb, excepting for the suggestion as to the different types of single comb base made by Bateson and Punnett in Rep. Evol. Comm. 1908. To have so successfully gained recognition for the idea that there

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* Cf. for example. Ztschr. f. Abst. u. Vererb.-Lehre, Bd. II., pp. 257-275. 1909.

† " Ardent Mendelian " objects to our use of the terms " pure bred " and " homogeneous." To set his mind at rest I will at once say that I did use those terms in the much maligned " fanciers' " sense. On a strict interpretation of the case such usage is, I admit, open to criticism, but just how I was to describe the actual facts regarding the past history of the birds at this Station without either the use of these terms or a long dissertation on the subject I do not know. It would convey an utterly wrong impression regarding the true breeding history of these birds to say that they were heterogeneous or not " pure bred."

might be more than one such genotype in regard to other characters than the base of the single comb, is, it seems to me, no small point to the credit of our paper.

The piece of work here under discussion was, as has been said, undertaken and carried out as a contribution to the descriptive anatomy of the domestic fowl. It is no more justly to be criticised for not being a Mendelian investigation than is any text book of human anatomy. It is my conviction that in spite of the lucubrations of "Ardent Mendelian" there still remains in biology room for the ideals and methods of careful and accurate descriptive anatomy.

(3) Finally I wish to state as clearly as possible, so that there may be no room for misunderstanding, my position regarding the general problems of inheritance. I should not think of doing this, since my personal point of view is of no particular interest to anybody but myself, were it not for the fact that views are attributed to me by "Ardent Mendelian" which I do not hold, and because he singles me out as an example of a school of thought *respecting inheritance* with which, as a matter of fact, I am not at all in agreement. My position is simply this: I am thoroughly convinced of the great usefulness in helping to solve biological problems of the application of appropriate mathematical methods. Being of this mind, I have endeavoured so far as possible to perfect myself in the use of such methods, inasmuch as it would seem the part of wisdom to learn to use correctly any scientific tool which one is to use at all. But the fact that I have used such mathematical methods in my work should not imply in the slightest degree, so far as I can see, that I agree with any person's theoretical views regarding heredity. There would seem to be as much justification to assume that a person subscribed to the doctrine of the quadrille of the centrosomes, for example, because he used a microtome!* As a matter of fact my own study of

* In the writer's opinion it is extremely unfortunate that the impression should have become so prevalent among biologists that the use of biometric methods implies *per se* a belief in any particular view of inheritance. This idea, one feels sure, has kept biometric methods from taking the place in the biological armamentarium which they justly deserve to occupy.

inheritance has led me to the opinion that the so-called "law of ancestral inheritance" probably has very little relation to the actually important and essential facts of inheritance in plants and animals. On the contrary, I am led, primarily as a result of the investigations carried on in this laboratory during the past three years regarding the inheritance of, and effect of selection upon a character exhibiting fluctuating variation (egg production or fecundity in fowls)* to believe that inheritance is in general in accordance with Johannsen's "pure line" scheme. The fact of "line" inheritance for a character showing such wide fluctuations (= phaenotypic variation, Johannsen) as does egg production is made very obscure and difficult of analysis as compared with the characters of self-fertilising plants, by the circumstance of sexual reproduction. But of the fact there can, in my judgment, be no doubt. Furthermore, I may say that it is my opinion that the general viewpoint respecting inheritance comprised under the idea of "pure lines," which is so clearly and forcibly set forth by Johannsen in his recent book, is the most fundamental, accurate, and comprehensive view of heredity yet proposed. The relation of Mendelism to this view is evident.

"Ardent Mendelian" calls me an "ardent biometrician." If by this he means merely (which he obviously does *not*) that I am an ardent advocate of the proper use of mathematical methods in biology, *including* Mendelian studies, I accept the designation. If, on the contrary, he means (which he obviously *does*) that I "ardently" uphold the views regarding inheritance which he attributes to me he is in most complete error.

The "ardent" espousal of schools of biological thought seems to me to be somewhat absurd. Unless I quite mistake the import of the scientific method it

* Cf. in particular the following papers: Pearl, R., and Surface, F. M. A Biometrical Study of Egg Production. Part I. Variation in Annual Egg Production. U.S. Dept. Agr. Bur. An. Ind. Bulletin 110, pp. 1-80, 1909. Studies of the Physiology of Reproduction in the Domestic Fowl. II. Data on the Inheritance of Fecundity Obtained from the Records of Egg Production of the Daughters of "200-egg" Hens. Me. Agr. Expt. Stat. Bulletin 166, pp. 48-84, 1909. Is There a Cumulative Effect of Selection? Data from the Study of Fecundity in the Domestic Fowl. Ztschr. f. Abst. u. Vererb.-Lehre, *loc. cit.*

implies above everything else the maintenance of a critical spirit, both in regard to observations and to theories. Dogma, whether it be Mendelian or "biometric," has, it seems to me, no place in science. It has appeared to many biologists that the standpoint of some of the workers along Mendelian lines has been that a critical attitude towards Mendelian results or theories was necessarily a hostile one. Such a position is, of course, in the long run not tenable. The present writer, and he feels tolerably sure that the majority of conservative biological opinion is with him on this point, proposes to continue in his study of inheritance, to maintain a critical attitude towards all methods and ideas, whether Mendelian or otherwise, trying all and holding fast to those which are good. Nobody is going to be argued, whipped, or ridiculed into the adoption of any view of heredity. If the *basic and fundamental* Mendelian data and ideas regarding inheritance cannot withstand the most searching criticism which can be brought to bear upon them (the writer believes that they *can*) then the sooner they are relegated to the scrap heap the better it will be for biology. To seek to avoid criticism or to crush and annihilate it by militant rhetoric is not only futile but foolish.

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A Note Regarding Variation in the Single Combs of Fowls.

A REPLY BY "ARDENT MENDELIAN."

We are afraid that Dr. Pearl in his reply to some comments of ours, concerning the memoir published by him in "Biometrika," and which he has we cannot help thinking ungenerously called a "rather violent and caustic criticism," has not done us the honour to read that criticism with sufficient care. Had he done so, he would have learned

that none of the imputed statements were written by us. We were extremely careful, in all essential points, not to write our own interpretations of Dr. Pearl's utterances, but to quote them fully and without modification.

When Dr. Pearl accuses us of "stating that the *objects of his investigation* were (1) to answer the one question as to whether single combs or their various grades breed true; (2) to determine whether these different varieties of single comb may be hereditarily transmitted in accordance with Mendelian principles; and (3) to cast doubt upon Mendelian conclusions," he is clearly under a misimpression as to the object of our criticism. What was stated was this* : "The authors have *propounded a number of questions*, but the central thesis turns upon the one question as to whether single combs or their various grades breed true." This is quite a different thing to asserting that these questions were the *objects* of his *investigation*.

Now did Dr. Pearl propound these questions or not? If we turn either to his original article in "Biometrika" or to his reply, we shall find the answer. We may quote his statement *verbatim*. "All 'single' combs are put together in one category, all pea combs in another. But nothing is more certain than that all single combs are not alike in respect to any feature whatsoever, even including their singleness.† How much and in what way do they vary? Do the variants within the category Mendelize‡? Are all variants exactly equivalent in crossing with other categories?" In slightly different words,†‡ these are precisely the questions which we said Dr. Pearl had propounded in his paper, and it was not asserted that they were the objects of his investigation.

Let us come to what we actually did say was the object of his investigation. Since this was the crucial point around which the criticism centred, our invariable rule

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* Mendel Journal, No. 1, October, 1909, p. 183, fourth line from the beginning of the article.

† This is only another way of stating that single combs do not breed true.

‡ This is only another way of asking whether the various grades of single comb breed true, and whether they are transmitted according to Mendelian principles.

of quoting fully the statement which we purposed criticising was followed. On page 184 it was stated : " The apparently real object of the investigators, that with which they commenced their investigation, seems to be contained in the following statement : ' In this paper we have endeavoured to give a clear and, as far as possible, quantitative description of the nature and amount of variation normally occurring in a homogeneous pure-bred strain of Barred Plymouth Rock hens in respect to the form and size of the comb.' " And now, Dr. Pearl in endeavouring to put our putatively aberrant pen right, quotes in his reply (pages 190-1) this very sentence, as explaining the object of his investigation ! This is just what we said was the object of his investigation as stated in his own words. Surely Dr. Pearl must have overlooked this part of our article.

In another matter Dr. Pearl has misread the comments which we ventured to pass upon his conclusions. Nowhere was it asserted or implied that he was " so stupid as to undertake a purely descriptive biometrical investigation of variation, and only that kind of investigation, for the purpose of determining whether the domestic fowl ' breeds true ' with reference to comb characters." Our criticism was confined wholly to Dr. Pearl's own statements and conclusions, and had he carefully read page 185, he would have surely noticed the sentence in which was indicated again the real problem we had to consider. " The object then before us is a study of the range and nature of the variation in the single comb of a certain race of hens." That is precisely what Dr. Pearl tells us was the nature of his enquiries in the sentence which we have just extracted from his reply, and quoted above.

In our article two general objections to the conclusions arrived at by Dr. Pearl were raised. First, it was contended his methods were invalid. Second, that the so-called homogeneous material was not homogeneous at all but markedly heterogeneous. And that, therefore, conclusions based upon material which is assumed to be one thing but is in reality the reverse, can be of no value.

Now Dr. Pearl, in his reply (p. 191) " freely admits that there is room for a difference of opinion as to whether

the object of his investigation was biologically valid." The dominating and all-embracing object we should bear in mind was to measure (quantitatively describe) the variation of this so-called homogeneous material. Since Dr. Pearl admits it is possible that this object may have no validity there is nothing to be gained by repeating here those considerations which we gave for regarding his biometrical methods (namely, mass measurements) as wholly invalid for the purposes of the investigation.

It was further contended that the somatic condition of a character may be quite unreliable as a criterion of its gametic behaviour in inheritance. Dr. Pearl now frankly admits that it is so (p. 192). Having done this he has destroyed altogether the foundation of "Biometrics" as a valid instrument for dealing with problems of inheritance. For that which can alone measure somatic characters cannot give valid conclusions on problems of inheritance which depend upon gametic behaviour.

With regard to our second criticism, that the material investigated by Dr. Pearl was not homogeneous, he has attempted no real answer to it. He says he "did not use the term 'homogeneous' in the fancier's sense" (footnote, p. 192). But he does not tell us in what sense he did use the word. It is not, it seems to us, any reply at all, to say he could not see "just how the actual facts regarding the past history of these fowls was to be told, without describing them as a homogeneous race." But this particular race of hens is either homogeneous in some definite sense or it is not. If it is homogeneous, the first canon of science requires the strict definition of the term employed. And, if it is not homogeneous, then no matter what the inconveniences, the use of the term is unjustified. As a matter of fact, Dr. Pearl did describe the past history of the birds without reference to this term when, in his original memoir, he stated "that they had been carefully and closely selected in their breeding for more than twenty five years." This, surely, would have been sufficient description of his material, and it would, by itself, have been quite accurate, without the introduction of a term which Dr. Pearl, under the light and stress of criticism, now virtually admits has no meaning at all, in the context in which he employed it.

Another objection which was urged against the homogeneity of his material was "that we cannot by mere somatic selection, however long it may be continued, obtain a homogeneous race of single combs in respect to any of their characters, except as the result of a happy and exceedingly rare accident." Now Dr. Pearl admits the justice of this criticism, for he says, as we have already pointed out, in his reply (p. 192) "that the somatic condition of a character is, generally speaking, an unreliable criterion of its behaviour in inheritance." And this race of hens, selected on the old somatic basis in vogue twenty years ago, and which has been continued to the present, can therefore have no claim of any sort, on Dr. Pearl's own admission, to be regarded as homogeneous.

Thus, on all the essential points which we criticised, Dr. Pearl virtually admits the justice and validity of the criticism.

There is one further point in Dr. Pearl's reply which calls for notice. He says (p. 192) "it seemed to him that within the broad category of 'single comb' there might exist more than one gametic type." Now, we do not question that Dr. Pearl conceived, even before the appearance of our criticism, the possibility of there existing more than one gametic type "within the category of single comb." But we do know, as the note appended to the article in which that criticism is expressed shows, the Editor of "Biometrika" declined to allow us to reproduce the figures of four particular combs which we had selected from Dr. Pearl's illustrations. But he was willing that we should reproduce any one of Dr. Pearl's three plates complete. As a concession, and upon our suggestion, he subsequently kindly allowed us to reproduce both the four selected figures and one whole plate of figures. Now there can be only one object in desiring this reproduction of a whole set of figures, as they were arranged by Dr. Pearl, and which were quite immaterial to our purpose. And that object is to demonstrate what the Editor of "Biometrika" conceives to be a case of continuous or fluctuating variation. And, if these figures conceivably show continuous variation, where, then, we may ask, is the conception of gametic types?

Clearly, the Editor of "Biometrika" and his biometrical contributor do not see "eye to eye." But, apart from a consideration of this sort, Dr. Pearl does not anywhere in his original paper allude to gametic types among single combs. But what he does allude to are "variants." Now "variants," as used in the biometrical language of Dr. Pearl's paper, are wholly somatic, and not gametic, in conception. Whatever conceptions Dr. Pearl may have had in mind, certainly the gametic ones were not expressed in his original memoir. Perhaps in the shadow of the Biometrical "Field Marshal" they froze at their inception, and they awaited the more genial atmosphere of the "Mendel Journal," for their fruition.

Dr. Pearl seems to imply (p. 193) that we have attributed to him views regarding heredity which he does not hold. If we understand him rightly, and have correctly followed his context, Dr. Pearl appears to think that we have ascribed to him a belief in the biometrical "Law of Ancestral Inheritance." We should be exceedingly sorry to attribute to anyone beliefs which they do not hold. We can quite sympathise with Dr. Pearl's anxiety to be dissociated from that burning and blackening chestnut of the biometrical harvest. But surely it is a little ungenerous to seek an opportunity of discarding a fallen idol and a shattered faith by accusing us of statements which we have not made. We have nowhere attributed to Dr. Pearl a belief in any theory of inheritance. His methods, his statements, his conclusions, and his material were criticised, but nothing more. The fact that Dr. Pearl has spoken of our "militant rhetoric" and of the "Law of Ancestral Inheritance" suggests to us that he has confused together two distinct articles, namely, "The Present Position of the Mendelians and Biometricians" and the one entitled "Variation in the Single Combs of Fowls; some Mendelian Comments." In the latter, no "militant rhetoric" of any sort occurs, and the "Law of Ancestral Inheritance" is not mentioned. In the former article, which does not deal with Dr. Pearl, the author of this Law is mentioned, and the article was advisedly written in the style of martial imagery. But it was not intended that the cap should fit Dr. Pearl. Of course, if he should like

to put it on, only for the purpose of creating an opportunity of throwing it off, that is an exercise which he is at perfect liberty to practise. But it is unkind of him to accuse us of perpetrating the cruel act of placing such a veritable crown of thorns as the "Law of Ancestral Inheritance" upon his head.

With regard to the maintenance of a critical spirit in dealing with observations and theories in science, we are all agreed. But we may, in conclusion, express the hope that those who are subjected to criticism should endeavour to fairly read the comments and to grasp the spirit of the critic. If that were done, "militant rhetoric" would not be found where it does not exist, and "caustic comments" would assume the appearance of an honest and even genial attempt to deal with problems in which many find an interest, but who approach them from different standpoints ?

Nothing was further from our intention than to make any "caustic comments" upon the memoir of Dr. Pearl and his wife. The paper from the standpoint of Mendelism appeared to require comment. With much of Dr. Pearl's recent and experimental work we are in accord, and could only write of it in terms of the fullest admiration.

MISCELLANEA.

Apogamy and Hybridisation.

Mr. C. H. Ostenfeld, of København, who in 1906 brought out a paper on Apogamy and Hybridisation of the *Hieracia*, popularly known as Hawk-weeds, repeating some of Mendel's experiments, has just published an account of the results of some further experiments on apogamy and hybridisation of the *Hieracia* from 1906 to 1909.* The plants experimented on were obtained from a wide area, north, south, east, and west of Europe, and included many from North America, besides the offspring of previous experiments.

The method of castration was a complete shave with the razor, cutting off the top of the unopened head within two or three days of expansion; this operation removed all the stigmas and anthers. Every precaution was taken to ensure isolation of the seed, the soil in which it was sowed being first baked—very necessary when experimenting with such a common and widely distributed genus as the *Hieracia*.

The experimental results obtained by Mr. Ostenfeld as the outcome of castration of the flower heads, suggested the necessity of a cytological investigation of the embryo sac and surrounding nucellus of the ovules. Prof. O. Rosenberg undertook this. He made examinations of the developing apogamous ovules and found the chromosomes of the egg-cells unreduced, *i.e.*, their number equalled those of the body cells.

Mr. Ostenfeld carefully summarises all the current theories on the connection between mutations, their constancy, and apogamy; he regards Winkler's opinion, that in parthenogenetic or apogamous plants there is great likelihood of a mutant keeping constant, as the truest view of the question up to date, and is inclined to believe that apogamy influences the constancy of a mutant and causes its persistence, but is not the cause of the mutation.

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* Castration and Hybridisation Experiments with some species of Hieracia, by C. H. Ostenfeld. Særtryk of Botanisk Tidsskrift. 27 Bind. 3, Hæfte, København, 1906; and

Further Studies on the Apogamy and Hybridisation of Hieracia (Experimental and Cytological Studies on the Hieracia, by C. H. Ostenfeld and O. Rosenberg. Part III.), by C. H. Ostenfeld. Sonderabdruck aus "Zeitschrift für induktive Abstammungs-und Vererbungslehre." Band III. Heft 4. 1910.

One particularly interesting "sex-mutant" arose from a cross made by Mr. Ostenfeld between *Hieracium excellans*, which bears female flowers only, and *Hieracium aurantiacum*, which is hermaphrodite. The primary hybrid of F_1 was, like *H. excellans*, *i.e.*, female only. It was isolated, and 53 individuals obtained in F_2. These were, of course, developed apogametically. Of these 52 were like the primary hybrid, but one differed in several features and was hermaphrodite with copious pollen. It was sterile on isolation, and did not set any seed. Mr. Ostenfeld speaks of it as a sex-mutant.

Of special interest is his discovery that two apogamous species *i.e.*, *H. aurantiacum* and *H. piloselloides* are fertile when the latter is pollinated by the pollen of the former species. Nineteen individuals were obtained in this experiment, of which one was an indubitable hybrid.

Amongst other interesting points shown by his work, Mr. Ostenfeld found all forms of *Hieracium umbellatum* normally sexual except one which is apogamous; but he states that, setting aside this species, he believes himself justified in declaring that almost all the numerous species of the sub-genus Archieracium are apogamous. He tells us further that he found some species apogamous and capable of cross fertilisation, while others were only apogamous and incapable of cross fertilisation.

Mr. Ostenfeld points out that Professor Bateson's view that no indisputable examples of non-segregating hybrids have been found amongst plants is upheld by his experiments with *Hieracia*, which prove that the constancy of certain hybrids is due to apogamy.

Apparently all apogamous hybrids are not constant, for in Mrs. Haig Thomas' experiments with apogamous seed from an extracted F_2 white *Nicotiana*,* derived by the selfing of an F_1 hybrid of *N. Sylvestris* x *N. affinis* there were produced in F_3 all the shades of reds, purples, and whites with coloured under-surface found in F_1 and F_2. It would seem, therefore, that apogamous plants are not necessarily constant.

A beautifully executed coloured plate containing drawings of the parents and 23 hybrid offspring F_1, F_2, and F_3 of *Hieracium excellans* x *Hieracium aurantiacum* accompanies the memoir.

A New Theory of Sex Heredity.

In a letter to "Nature," February 24th, 1910, Mr. Frederick Keeble, Professor of Botany at the University of Reading, propounded a new theory of sex heredity based, he states, on the

* This plant had white flowers with coloured under surface.

presence and absence theory of Professor Bateson and Professor Punnet.

On Mr. Keeble's suggested hypothesis maleness and femaleness are independent allelomorphs, each with its presence and absence designated thus: "Mm" and "Ff." Proceeding on the usual plan, the scheme presented contains among 16 individuals nine hermaphrodites with varied possibilities, three males, three females, and one sexless. Amongst the nine hermaphrodites is one zygote, in which both sexes are doubly present, and symbolically represented as M M F F, and four of the type M m F f, where each contains only a half-dose of the two sexes. Naturally the question arises as to the actual sex which either type of zygote, *i.e.*, M M F F and M m F f, will manifest. Both contain maleness and femaleness in equal amount. Other theories of sex regard maleness as being dominant in some cases, and in others femaleness. But Dr. Keeble, falling back upon the familiar fact that double begonias which normally bear female flowers, may be induced to develope male flowers upon starvation of the plants, postulates the conception that it is external conditions, such as nutrition, which determines the definitive sex of zygotes of either these two types. He thinks that in general the number of males and females among zygotes of the M M F F and MmFf type will be about equal, though wide departures may be expected under conditions which favour the development of one sex more than another. The question is one which experiment alone can decide.

Among the remaining hermaphrodite types, *i.e.*, M m F F and M M F f, the former, it is supposed, will produce female organs and the latter male.

Four of the remaining types will be unisexual, containing only maleness or femaleness, and may be represented as M M f f, M m f f, m m F F, m m F f. The one remaining type will be sexless, *i.e.*, m m f f. Naturally it must be supposed that M M f f and M m f f will bear male organs and m m F F, m m F f female organs whatever the nature of the external conditions. The m m f f type, if viable, will be sterile.

Dr. Keeble proceeds to show that many interesting problems, such as prepotency, partial sterility, homosporous and heterosporous ferns (the latter bearing spores which produce both male and female or hermaphrodite prothalli, and the latter having two sets of spores, one large which produce female prothalli, and the other small which produce small male prothalli), the high rate of mortality accompanying spore formation, and the absence of sexual reproduction in certain algæ, may receive an intelligible explanation on the basis of his hypothesis, in part elaborated by the collaboration of his colleagues, Miss Rayner, Miss Pellen, and Mr. Jones.

Dr. Keeble's hypothesis, that it is starvation which induces the appearance of male flowers in double begonias of the postulated type M M F F and M m F f, and by implication that it is high feeding which produces the development of female flowers, while not inconsistent with the cases he had in view, is yet not consistent with the fact that another variety of the same plant, *Begonia Gloire de Lorraine*, so much cultivated as a stove plant, forced under heat and richly fed, which produces only male flowers until the end of the flowering season, when one or sometimes two female flowers appear. The cultivation of the melon furnishes another example of the appearance of abundant male flowers in spite of rich feeding, the female flower being much rarer and appearing later.

The following is Mr. Keeble's scheme, showing the distribution of the nine different types of zygotes among every 16 individuals, which accompanies his paper in "Nature," Vol. 82, No. 2104, February, 1910:—

Dihybrid Scheme.

1. M M F F		
2. M M F f		9 M F.
2. M m F F		
4. M m F f		
1. M M f f		3 M f.
2. M m f f		
1. m m F F		3 m F.
2. m m F f		
1. m m f f		1 m f.

A Note on Mrs. R. Haig Thomas's Article on Parthenogenesis.

In her interesting paper on "Parthenogenesis in Nicotiana" in the *Mendel Journal* for October, 1909, Mrs. R. Haig Thomas suggests that *Nicotiana Sanderae* may be simply the result of parthenogenetically produced seed of *N. Forgetiana*, not the result of crossing that plant with *N. affinis*, as has been alleged.

The chief ground for her belief that *N. Sanderae* is not a hybrid appears to lie in the fact that it breeds true from seed, a "phenomenon which is not expected to occur, on Mendelian principles of gametic purity and segregation."

Now the authoress shows that *N. Sanderae* is capable of forming seed parthenogenetically, and suggests that it is for this

reason that even when growing alongside of other "varieties" (species ?) "of *Nicotiana* in the open it always seeds true," and this suggestion carries with it the implication that all the seed of *N. Sanderae* are produced parthenogenetically.

Mendelian segregation is not to be expected in the case of parthenogenetically produced offspring. Thus the evidence on which *N. Sanderae* is regarded as not a hybrid is, to say the least, somewhat weak.

Furthermore, it is not shown that *N. Forgetiana* exhibits the phenomenon of parthenogenesis, although this is of course possible. If this were shown, greater weight might be attached to the suggested origin of *N. Sanderae*, but in the absence of this proof the origin of *N. Sanderae* seems certainly not less "shrouded in mystery" than it was before.

It is conceivable that a hybrid, such as *N. Sanderae* is alleged to be, should breed true by producing seed parthenogenetically.

Royal Horticultural Society. FRED. J. CHITTENDEN.

A Reply to Mr. Chittenden's Note.

MR. F. J. CHITTENDEN's criticism of the doubt which I expressed respecting the hybrid nature of *Nicotiana Sanderae*, in a short paper on "Parthenogenesis (Apogamy)" in "Nicotiana."† is based upon the supposition that "Mendelian segregation is not to be expected in the case of parthenogenetically produced offspring." But within my experience segregation has occurred among apogamous plants. I will cite the case:

An F_2 *N. sylvestris* × *N. affinis*; white upper surface, coloured under surface, was treated for apogamy, by cutting off the anthers and stigma in the young bud, and ripe capsules were gathered on the 31st of August, 1909. The seed was sown 4th Sept.; and germinated 12th Sept. 1909. In May, 1910, all these F_3 apogamous plants, some sixty in number, flowered under glass, and split into the reds, purples, and whites with coloured under-surface which we would expect in the case of self-pollinated seed; unfortunately, the ratio of the different colours was not counted before the plants went off flower, but in June fourteen of them were planted in the open border. These flowered again in September, and the colours were counted:—

White, with coloured under surface.	Red.	Purple.
10	3	1

†"Mendel Journal," October, 1909.

Further, the ripe seeds in an apogamus capsule are usually fewer in number than those in a selfed capsule, and it may therefore be inferred that all the ovules are not capable of apogamy. Given this inference, it is not difficult to imagine that some *N. Forgetiana* were reproduced apogamously among the hybrids made by Mr. Sander, and chosen for their deeper colour to continue breeding from.

I fear it is not possible, until some traveller again imports it from South America, to ascertain positively that *N. Forgetiana* is apogamous, for in reply to a letter of enquiry Mr. Sander wrote to me April 23rd, 1907, as follows :—" We regret to say we have neither seeds nor plants of *N. Forgetiana*, as the plant was deemed not worth keeping after the hybrids were obtained from it." It seems a pity that a plant which produced such splendid results horticulturally and financially in the cross with *N. affinis* should be destroyed instead of being crossed with other varieties of Nicotiana. Again, all the Nicotiana species and varieties I have observed under cultivation are each of them distinguished by three gradations of colour—pale, medium, and dark in distinct shades, the medium shade most numerous, the pale and dark far less frequent ; and these same three grades of colour exist on the under surfaces of the so-called White Nicotianas, *Affinis* and *Suavolens*. Now, it is conceivable that Mr. Sander's traveller may have brought over *N. Forgetiana* in one only of these colours, the medium, which might further confuse the issue, as it would seed all three shades. This I have found experimentally in selfed plants.

Varieties of Nicotiana seeding true in the open ground subjected to insect crossing, and growing within two feet of one another, seemed to demand some explanation ; in the case of the *N. Tabaccums*, this is found in the fact that fertilization takes place just before the bud expands, but *N. Sanderae* and others have not that habit, and apogamy in these might partly explain it, though not altogether, as apparently all the ovules are not apogamic.

Rose Haig Thomas.

Appendix to Mr. Mudge's Rejoinder to the Criticism of Miss Wodehouse.

The Incidence of Plague and the Relative Insensibility of Hooligans to Pain.

POSTSCRIPT TO PAGE 151. "*Thus the plague is benign to the race in the long run,*" &c.

WHILE these pages were passing through the Press, I felt curious to ascertain whether as a matter of history, quite apart from biological considerations, however plausible and cogent they may be, the plague does actually tend under the ordinary conditions of life, to eliminate particular classes of people, and thereby to exercise a selective value in the evolution of a community. During my quest I came across, among others, a well-known book, namely, Daniel Defoe's "Journal of the Plague Year."* In order that I might gather a right impression of the events of that period, namely, 1665, and obtain a true conception of the nature of the author's experiences, and of the observations which he had gathered from eye-witnesses, I carefully read the book through. I think anyone who will do the same will be able to arrive at a general conclusion something like this: that in the course of a great and terrible epidemic such as the plague of 1665 was, there is an actually heavier incidence of the disease upon the foolish, the reckless, the criminals, and those who are of dirty instinct or habit.

Let us deal with the foolish and reckless first. Defoe described certain precautions which were enjoined upon the people, and he tells us "It must be acknowledged that when people began to use these cautions they were less exposed to danger, and the infection did not break into such houses so furiously as it did into others before; and thousands of families were preserved by that means. But it was impossible to beat anything

* As scientific evidence this book cannot of course be quoted. But it does not necessarily follow that all evidence which is not strictly scientific is not true. Defoe was only six years old during the Plague year, but he had an uncle and other relatives who were in London, at the time, and he became acquainted with others who were intimate with the incidents of that year. Moreover, he had access to volumes, some written by medical men, which appear not to be obtainable now. There is, therefore, no reason to doubt the essential accuracy of his records, especially as the book appears to have been written as a means of administering precaution and tendering advice to Londoners in view of the threatened extension of the plague which was raging at Marseilles in the years 1720-21.

into the heads of the poor. They went on with the usual impetuosity of their tempers, full of outcries and lamentations when taken, but madly careless of themselves, foolhardy, and obstinate while they were well. This adventurous conduct of the poor was that which brought the plague among them in a most furious manner, and this, joined to the distress of their circumstances when taken, was the reason why they died so by heaps; for I cannot say I could observe one jot of better husbandry among them—I mean the labouring poor—while they were all well and getting money than there was before, but as lavish, as extravagant, and as thoughtless for to-morrow as ever; so that when they came to be taken sick they were immediately in the utmost distress, as well for want as for sickness, as well for lack of food as lack of health."

This, then, is the contemporary, or nearly contemporary, record of the incidence of the plague, and it seems to have fallen upon the foolish and the reckless—which then, as now, appear to be more frequently found among the poor than in other social classes—"so that they died by heaps." It is pathetic, no doubt; but we must not overlook the real significance of this incidence because of that. The gist of the matter, indeed, is very simple. Foolishness and recklessness are not the products of poverty, but poverty is the consequence of inherently foolish and reckless natures'. That is why the poor are poor; that is why civic defectiveness is more largely developed in them as a class than in other classes; it is because the inherently defective tend to sink downwards into the lower social strata. The twentieth century does not differ from the fifteenth in this matter. And, if a great epidemic were to sweep over England to-day, the Defoe of our time would write precisely what the Defoe of two hundred and forty-five years ago wrote. The "poor are always with us," but it is too often forgotten to add, "and are always the same."

As a further illustration of the reckless and wasteful nature of the poor we read: "There was a most excessive plenty of all sorts of fruit, such as apples, pears, plums, cherries, grapes, and they were the cheaper because of the want of people; but this made the poor eat them to excess, and this brought them into fluxes, griping of the guts, surfeits, and the like, which often precipitated them into the plague."

In yet another part of the "Journal" we read as follows: "They not only went boldly into company with those who had tumours and carbuncles* upon them that were running, and consequently contagious, but ate and drank with them, nay, went into their houses to visit them, and even, as I was told,

* Of the Plague.

into their very chambers where they lay sick." "Nay, even into the same beds, with those that had the distemper upon them, and were not recovered."

Now let us consider the criminals, and the incidence of the disease upon them. "That there were a great many robberies and wicked practices committed even in this dreadful time I do not deny. The power of avarice was so strong in some that they would run any hazard to steal and to plunder; they would break in [to houses] at all hazards, and without regard to the danger of infection take even the clothes off the dead bodies." "It is, indeed, to be observed that the women were in all this calamity the most rash, fearless and desperate creatures, and as there were vast numbers that went about as nurses to tend those that were sick, they committed a great many petty thieveries in the houses where they were employed." These statements speak for themselves, and I need not further comment upon them, except to say this: that very probably a great number of persons of thievish propensities became nurses, not to nurse, but in order to utilize the opportunities for stealing. If these passages and others in the "Journal" have any meaning, they signify that the thieving and pilfering instincts took their possessors into danger, and led the majority of them to their destruction.

It is often said that the plague strikes all persons alike, irrespective of strength or weakness. There are reasons to justify us in doubting this belief, and Defoe's "Journal" strengthens these doubts. There is no reason why we should doubt that the plague, like malaria, inflicts some fatally, others not fatally, and others not at all. In other words, the plague, not only socially but constitutionally, exerts an evolutionary influence, eliminating the susceptible and leaving the immune. There is, for instance, the case of John Hayward, under-sexton, gravedigger, and bearer of the dead. "This man carried, or assisted to carry, all the dead to their graves, and who were carried in form (that is bodily and individually), and after that form of burying was stopped, went with the dead-cart and the bell to fetch the dead bodies from the houses where they lay, and fetched many of them out of the chambers and houses; for the parish was remarkable for a great number of very long alleys, into which no carts could come, and where they were obliged to go and fetch the bodies a very long way." "Which work he performed and never had the distemper at all, but lived twenty years after the Plague." "His wife at the same time was a nurse to infected people, and tended many that died in the parish, yet she never was infected neither."

Then there is the case of the "piper," who one night having drunk too much, lay down upon a stall in the street, and was taken

up for dead by the bearers, placed in the dead-cart, and upon and around him a number of dead were laid. All the while that the bearers were collecting their gruesome cargo, and piling the bodies into the dead-cart, the "piper" slept soundly, surrounded on all sides by the plague-stricken dead. He was nearly buried alive. But in spite of his intimate contact with the dead, he did not contract the plague. It should also be remembered that this "piper," from the nature of his calling, mingled freely with people of the lower classes, upon whom, according to Defoe, the incidence of the plague was very heavy, and must have come many times into contact with persons suffering from the disease, either manifest or not.

From Dr. Boghurst's "Loimographia" we may extract the following, which shows how though some persons may come into the most intimate and prolonged relationship with the infected, throughout an extended period of time, in fact during the whole of the plague year, yet they do not contract the disease. "I commonly dressed forty sores in a day, held the pulse of patients sweating in their beds half or quarter of an hour together, let blood, administered clysters to the sick, held them up in their beds to keep them from strangling and choking,* half an hour together commonly, and suffered their breathing in my face several times when they were dying; ate and drank with them, especially those that had sores; sat down by their bedsides and upon their beds, discoursing with them an hour together."

It is not necessary to give any further quotations. These serve to indicate my general purpose. It seems, therefore, there are those who are immune to the disease; those who are only difficultly susceptible; those who fall easily. The quotations further show—and a full reading of the "Journal" would demonstrate this even more cogently—that the disease does manifest an incidence in its effects and carries off the foolish, reckless, vicious dirty, and thievish in greater numbers than those who are wiser, more discreet, cleaner, and more capable of adjusting themselves instinctively or intuitively to sudden emergencies and new conditions. In as far as it does that, plague serves to purge a community of undesirable units and to leave it sounder in those attributes that are essential in civilised communities. It exercises a selective and therefore evolutionary effect upon the race which it visits. I have been assured by travellers, some of them medical men, that in other epidemic diseases, such as yellow fever, the incidence of the disease is very much heavier upon those whose habits are dirty and licentious

* This evidently indicates the pneumonic or more infectious form of plague. To allow such to "breathe in one's face" means almost certain infection to susceptible people.

than upon the clean and those of temperate habits. The latter, in fact, are relatively seldom attacked. Whether by these means we pay too high a price for evolution is not a question with which I am now concerned. Perhaps the annals of the future may supply the answer, when our defective citizens—defective in clean instincts, in discreetness, in thrift, in ordinary judgement—have so accumulated that our army of sanitary officials shall have become so augmented that it cannot be further enlarged on account of its prohibitive cost, and it will therefore be unable to suppress or control the manifestation of the defective qualities of the accumulated horde of defective citizens. Then these imperfectnesses of character may manifest themselves in a way which will surprise us. I am only desirous to point out the dangerous nature of the road along which our modern sentiment is urging us, and to emphasize the consideration that all its hopes are futile, because it has overlooked the fact that in saving certain citizens from the consequences of their own spontaneous deeds, this sentiment is evolving a sadly defective race.

Postcript to page 168. "*It is one of the most fatal mistakes to imagine that because to fight and be lacerated would be to some people a painful and fearful thing, that therefore it is so throughout the whole animal kingdom. It is not, I believe, even true for Man,*" &c. Pages 168-9. "*There can be little doubt that prize-fighters and others,*" &c.

Since these pages were in the Press, there has been published in the "New York Medical Record," an account of an extremely interesting psychological study of the gang of hooligans who have recently terrorised New York. These researches were made in the Department of Psychology of Columbia University by Dr. Siegfried Block, A.M., M.D. The research raises many problems of serious import to civilisation, and it entirely confirms the general contentions which were put forward in my original article and in my reply to Miss Wodehouse's criticism. But with the general results we shall hope to deal in our next number. Only one point it seems necessary to direct attention to now, because it is an *experimental* verification of the contention, that all animals, and certainly all men, are not equally susceptible to pain, and that any argument based upon the assumption that they are, such as that urged by Miss Wodehouse, is invalid. Dr. Block tells us that these hooligans "*are remarkably insensitive to pain.*" This being so, Miss Wodehouse's lamentation over "mortal combats between jealous wooers and the extermination of cattle by the bite of an insect" loses the pathos it was no doubt intended to excite, and the point of the argument is so truncated that it is no longer capable of cogent application.

REVIEWS.

Mendel's Principles of Heredity.—*By W. Bateson, M.A., F.R.S., V.M.H., Fellow of St. John's College, Professor of Biology in the University of Cambridge. Cambridge, at the University Press.* 1909.

PROFESSOR BATESON'S book will remain as the classic exposition of Mendelism. No man more than he has served by the brilliance and patience of his work, and the insight indicated by his methods, to place Genetics, or the study of Heredity, upon what is in reality its first enduring foundation. Whatever may be the amplifications and extensions which future investigation may necessitate, we do not think there is much doubt that the basis of the Science of Inheritance, which is now being reared by the Mendelian School, will remain as a permanent edifice in biological science. The conceptions of gametic segregation and gametic purity have come to stay. That the processes embraced within these conceptions play a part in inheritance and in evolution is clear enough. Whether they are the sole agencies at work, or whether their part in Nature, though limited, is of wide extent, is a question for the future to answer. But no conception of Nature which omits them can be complete, or have any pretence to be regarded as full or sound.

If inheritance and evolution are wholly or in part based upon gametic segregation, then our conceptions of the essential nature of the phenomena of variation will be necessarily profoundly modified. For, as is pointed out in the article in this Journal on "Mendel's Life and Work," it is difficult to picture the process of evolution as merely a matter of the blending of minute incremental variations all tending in one direction, while the processes going on in the sex-cells are those of segregation. We may as well try to imagine that St. Paul's Cathedral was reared by a continuous welling upward motion of liquid stuff, while all the time we know that it was built up of solid, separate blocks, deftly laid and arranged, one by one. It was not constructed by a flowing process, but by the sequential addition of separate though small steps, each block and brick of its masonry representing a step upward.

Thus we have before us these two conceptions of evolution as a process based on variation. It is clear, to start

with, there can be no evolution without change. A change in structure or function is variation. The direction of evolution will necessarily depend upon the " selection " of certain changes and the "elimination" of others. The great biological controversy of the present time centres around the question, What is the nature of these changes or variations ? If there are two species, one bigger and the other smaller, one swift of foot and the other less swift, the essential problem before us is, Were the changes by which the smaller and less fleet of foot evolved into the bigger and the swifter species of the nature of a thousand and one imperceptible stages, or did they partake of the nature of a series of definite and appreciable steps ? Those who hold to the view of " continuous variations " maintain the former thesis, while those who believe that " discontinuous variations " supply the material of evolution, hold to the latter view.

Let us try and form a tangible mental picture of the difference between an evolution proceeding on the basis of continuous variations, and that which proceeds on the basis of discontinuous variations. In the evolution of the larger from the smaller species are we to picture a number of the individuals among this species as varying to an inappreciable degree in size in the direction of being larger, and that as the result of the mating of such individuals there arises a race, definitely and permanently larger than the parent species, though so slight in extent as to be barely appreciable ? And, accepting Darwin's proposition, that it is conceivable a variation once arisen may continue to vary for many generations in the same direction, and, therefore, in the course of time the summation of a series of barely appreciable differences will ultimately result in an appreciable advance in size. Or, are we to believe that the difference in size arose by either a single jump or by a series of small but appreciable steps ?

Now, at this point we need to be careful to draw another mental picture of the difference between the two processes. So far, we have as it were, pictured the outside of the process ; but it is very necessary to picture also that of its inner mode of working. When we picture the outside of the process, we have in mind the body itself of the individuals. But the inner picture focusses our attention upon their reproductive cells or gametes. Now, when an individual's body varies in size, even to an inappreciable extent, upon what does this variation depend ? Upon the answer to this question rests or falls the case for evolution by continuous variations. Is the change a permanent one and maintained in inheritance ? For if it be not preserved in hereditary transmission, it has no evolutionary value, since evolution is simply progress in form and time. Or is it merely a temporary one—a passing response to environmental conditions, and

merely an expression of that plasticity which evolution has imposed on all living protoplasm? In other words, does it depend upon some definite change in the architecture of the germ-cells and is therefore transmitted from one generation to another? Or is it simply a matter of a nutritional responsive capacity of the body-cells, an event for this generation only, and the outcome of a passing influence of environmental combinations? If it be that, its manifestation through succeeding generations will depend upon the continued presence of these same environmental influences. But if it is not that, then its manifestation will be independent of particular combinations of environmental influences, and throughout long ranges of geographical distribution, and under diverse conditions, will exhibit its particular quality, whether it is size or any other.

Now, it is known that there are species, the individuals of which live under very different conditions, and yet preserve an identity of specific characters. We may cite the case of a small snail (*Helix arbustorum var alpestris*) which is found living above the snow line on the Alps and also in the marshes of Hoddesdon, in Herts. Notwithstanding that these conditions are wholly different, the individuals living in the one region are not distinguishable from those living in the other. Or, we may show the same thing by taking a converse case, namely, where distinct forms within a species maintain their characteristic differences though grown under the same conditions. The vernal whitlow grass is a case of this sort. This species has a large number (about 200) of forms which differ from each other in several features. These forms breed true, both in their local stations and when brought together and reared in botanical gardens. Take, for instance, two of these forms, one with narrow leaves and the other with a broader foliage. They have been cultivated side by side, and yet each form retains its own peculiar features in a quite uniform way. In Nature, on the geographic borders of the limits of the different forms, intermediates are not known. Hence, it is clear that some minor and greater differences of character which mark the individuals of many species are of fixed gametic origin, and though in some cases small in amount, are yet quite distinct and unblendable. Experiment alone can decide for certainty whether these forms are really unblendable when crossed. For the present, we have only the evidence of the absence of intermediate forms on the geographic limits of the overlapping groups. But still it is known that in other species similar differences do not blend in inheritance.

Then there is the wide range of comparative studies, where in comparing the extremes of numerous variations of very different kinds, we fail to find evidence that intermediate stages

exist. In the case of the dog-whelk, very common around the English coast, the differently coloured and banded varieties are distinct, and though they have been seen breeding together, nothing in the nature of blends or intermediates occur.

It is therefore clear that the differences which distinguish some forms from others are "discontinuous" and do not blend in the hereditary transmission, but segregate. The question which the future has to answer is no longer whether discontinuous variations exist at all, nor is it any longer a matter of upholding in an unqualified fashion the contention that evolution is based wholly on the blending and summation of continuous variations, for the existence of discontinuous variations is now a fact. But the question is one of whether evolution is wholly a matter of discontinuous variations, or whether it is partly this and partly due to continuous variations.

All that can be said in the way of general considerations and of essay speculations has already been written. The answer of the future to these questions lies in experiment. No doubt, observation of Nature has its value in suggesting new problems and in controlling the interpretations of the experimental evidence. But, by itself, it is insufficient to permit us to arrive at definite and certain conclusions, which can be regarded as incontrovertible.

Professor Bateson's book on "Mendel's Principles of Inheritance" thus comes as the exposition of the experimental method in biological science. In effect, he says: "We have done with essay-writing, and the time is ripe for the application of experiment to all the problems of inheritance, variation, and evolution." In the present book the author has preferred to restrict his treatment of the subject to the concrete facts of inheritance, as they have been ascertained by experiment. The consideration of the theoretical conceptions which those facts suggest he has left to be dealt with in another book.

We may now glance, in a general way, at the contents of the book and some of the problems with which it deals. In the introductory chapter there is an account of some pre-Mendelian writings, and it includes Darwin's "Origin of Species," and his "Animals and Plants Under Domestication," as well as those of Francis Galton, Weismann, and some of the earlier works of De Vries. The account of the pre-Mendelian writings is followed by a statement of the way in which Mendel's paper was rediscovered and of the new methods which he introduced into the study of inheritance. There is also, in this chapter, a description of the Mendelian principles illustrated by reference to some recent experiments in breeding hybrids derived from a cross between tall and dwarf sweet peas.

In many ways Chapter II. is an important one, since it describes the nature of the material with which Mendelian experiments have dealt, and to which Mendelian principles have been shown to apply. Some of the characters, the inheritance of which have been studied by Mendelian methods, are not only structural in nature, but several of them are physiological. The different habits assumed by plants are among some of the characters which have been investigated. Some individuals in a race are tall and others dwarf; some have a branching habit, others are unbranched; some straggle and others are erect; some are biennial and others annual; some wheats are susceptible to the attacks of the "rust-fungus," while others are immune. These habits, with the exception of the biennial and annual habits, and the straggling and erect habits, which require further investigation, behave in inheritance according to Mendelian principles. Among animals, the peculiar round-about motion of waltzing mice and the normal habit are known to be transmitted in Mendelian fashion. Hairiness and smoothness of the epidermis of plants, the prickliness and smoothness of fruits, lax and dense ears in wheat, starchy endosperm and sugary endosperm, short hair and long hair in rabbits and guinea-pigs, the various forms of combs in fowls, and eye-colour and various abnormalities in man, and a large number of other characters, for which the reader should refer to the book itself, have already been shown to behave in inheritance as Mendelian factors.

How extensive has been the work which has dealt with the transmission of colour in plants, animals, and Man alike, may be gathered from the fact that four chapters, namely, four to eight inclusive, are wholly concerned in describing the phenomena relating to colour-transmission. Perhaps in no other character is a knowledge of inheritance so certain, and of such a great degree of refinement, as in that of colour. Heredity and sex, and some interesting problems related to the subject, are dealt with in Chapter X. The existence of Mendelian inheritance in Man is discussed in Chapter XII. In Chapter XV. the bearing of Mendelian principles upon old biological conceptions is dealt with in a tentative and suggestive manner. Here we find a discussion of the nature of Mendelian units and of segregation; the possible moment at which segregation occurs is indicated; the nature of reversion and variation, discontinuity in variation and the relationship of Mendelism to Natural Selection is dealt with in a lucid and inspiring fashion.

The final or sixteenth Chapter deals with the practical application of Mendelian principles. In this chapter we are introduced to several extremely interesting matters. We learn that our old conception of a pure-bred race or individual must be profoundly

modified. Here the art of the breeder is at once touched by the introduction of a new standard of precision. The possibilities of raising horticultural novelties is much increased; "rogues" may be with more certainty eliminated or prevented from appearing; and "types" may be fixed within a few generations, where previously years were spent, and, even then, in many cases only to end in futile results. Most important, perhaps, is the lesson which the newer knowledge of Genetics has to teach us, that some types are unfixable.

The last section in this chapter deals with the sociological application of Mendelism. We commend every advocate of social panaceas and of legislative interference with natural processes to read this part of the book. In a few well-chosen sentences, the author gives expression to the judgement of every biologist, alike of the present and the past, who has given to social problems adequate and unbiassed thought. For nothing is more evident to the naturalist, than that we cannot convert inherent vice into innate virtue, nor change "leaden instincts into golden conduct," nor "transform a sow's ear into a silken purse," by any known social process. Our vast and costly schemes of free compulsory elementary education, of County Council Scholarships and evening classes, which are among these social processes supposed to possess the magic virtue of transforming the world into a fairy land, may be a delusion and a danger. And so, too, may be all the other well-intentioned but costly panaceas that harass and tax and eventually destroy the fit in order to attempt—for they can never achieve—the salvation of the unfit.

The chapters which we have indicated, namely, I.—XVI. are included in Part I. of the book. Part II. cannot fail to be of general interest. It gives an account of Mendel's life, and shows what sort of man he was, and what were the methods of his work.

Gregor Mendel was during part of his life an Abbot, yet there is little in his appearance, as it is depicted in the portrait reproduced in Professor Bateson's book, to suggest the priest. But there is something manifested in his countenance which escapes analysis: it is evident that he was a kindly and tolerant person, for although the face is wholly masculine, there is that in it which indicates a feminine gentleness and patience with little things. His character was doubtless of a complex nature: possibly there were combined in it the sternest resolution and the sweetest docility; and the academic spirit of the Cloister and the College were commingled in happiest harmony with the utilitarian capacity and clear thinking of successful practical life.

Gregor Mendel was not eminent in the popular sense, for he was not a "rope-dancer in the market place," nor a charlatan standing on a self-exalted pedestal, but he was great because he

was what Nietzsche would have called a "Creator." He came into a branch of knowledge that was in chaos and he gave us order; he found there stagnation and he converted it into progression; he saw multitudinous facts dead because they had no consistent interpretation, and he gave them life. The magnitude of the task that he accomplished and the nature of the debt that posterity owes to him, only those who fully know the state of hereditary studies in his day can properly appreciate.

The reader of Professor Bateson's book will find a great deal of matter of absorbing interest. He will find problems that have escaped elucidation for centuries glowing under the newer light of the Science of Genetics. At last we see the only way by which problems of inheritance can be effectually and scientifically attacked. The facts of geographical distribution, of the interrelationship of species on the overlapping bounds of their common territory, may suggest problems for investigation, but they cannot by themselves supply a truthful answer. The problems of evolution and of variation to-day give promise of receiving correct answers. We cannot, of course, yet close the book of knowledge, for we have only just passed its preface and reached its first real page; but we no longer grope in semi-darkness, for now we have a method by which we can put to Nature a single definite question, and get from her a single and definite answer to every question we choose to put. That is an enormous gain. This method is the most powerful instrument biological science has ever wielded, and the intellectual conception that lies behind it and supplies the motive power must be ranked among the greatest of her victorious achievements.

With regard to Man, it is now clear that what medicine, social reform, legislation, and philanthropy have failed to accomplish, can be achieved by Biology. Tell the student of Genetics what type of nation we desire, *within the limits of the characters which the nation already possesses*, and confer upon him adequate powers, and he will mould it, by the process of elimination. It is not too much to say that if he were instructed to evolve a "fit" nation—that is, one of self-reliant and self-supporting individuals—in the course of a few generations there would be few workhouses, hospitals, unemployables, congenital criminals, or drunkards.

The Family and the Nation.—*A Study in Natural Inheritance and Social Responsibility. By William Cecil Dampier Whetham, M.A., F.R.S., and Catherine Durning Whetham, His Wife. Longmans, Green, & Co., London. 7s. 6d. net.*

Perhaps in no book of its size has so much information of the greatest value, from the civic standpoint, been incorporated, nor

written in such a charming style, as Dr. Whetham's "Family and the Nation." From its Preface to its concluding page are to be found felicitous phrases that serve to carry home and fix upon the mind ideas which had but vaguely floated before it. There are many in this nation of ours who feel strangely perturbed, haunted by vague fears that something is wrong in the body politic, and yet who cannot define their fears or express the nature of the intellectual restlessness which they feel whenever they think of our social problems. To them the book of Dr. Whetham and his wife will give coherency of thought, definiteness of idea, and will express their experience in a beautiful language. It may do more even than that. It is to be hoped it will arouse the nation—or at least those who have undertaken to lead opinion and initiate action—to a sense of the danger of the over-valued altruism and emotional sentiment which seems to be guiding its destiny now.

The general aim of the book can be best indicated by a few sentences culled from the Introduction: "The efforts of men of science, philanthropists, and statesmen have been directed for centuries towards improving the general environment of the race, and of late years with conspicuous success." . . . "But in our wise and beneficent search for better conditions of life, we must not forget the other influence which, even more than environment, goes to make personality. To improve the conditions in which life is passed and by which it is moulded, is but part, probably by far the smaller part, of the problem. The deeper question, the conscious solution of which is opening out to all civilised nations, is how to maintain, and if possible to improve, the innate quality and character of the life itself." The answer to this latter question is given in the book itself. The maintenance of quality is achieved by inheritance alone. In the Introduction the authors point out that enough is known of the problems of heredity in lower forms and in Man, "to give us, here and there, certain principles which should be borne in mind when we are considering proposals for legislative or social action." They point out that until recently the effects of individual conduct or of social legislation on the innate qualities of a people have been ignored, perhaps not even suspected. We suppose that deficiency in the knowledge of these effects must apply to our statesmen, philanthropists, and the community generally, for fifty years ago, and again sixteen years ago, Herbert Spencer warned his nation, in his characteristically vigorous and lucid language, not only of the futility of legislation, but of its disastrous tendency in creating new evils as well as in failing to remedy those it set out to mend. He it was who asked us to "Change our vague idea of a bad law into a definite idea of it as an agency operating on people's lives,

and we see that it means so much of pain, so much of illness, so much of mortality. A vicious form of legal procedure, for example, either enacted or tolerated, entails on suitors, costs, or delays, or defeats. What do these imply? Loss of money, often ill-spared; great and prolonged anxiety; frequently consequent illness; unhappiness of family and dependents; children stinted in food and clothing—all of them miseries which bring after them multiplied remoter miseries. Even to say that a law has been simply a hindrance, is to say that it has caused needless loss of time, extra trouble, and additional worry; and among over-burdened people extra trouble and worry imply, here and there, breakdowns in health with their entailed direct and indirect sufferings. Seeing, then, that bad legislation means injury to men's lives, judge what must be the total amount of mental distress, physical pain, and raised mortality, which these thousands of repealed Acts of Parliament represent!"* This warning was published in a magazine of wide circulation in this country; but it appears, as Herbert Spencer fully expected, to have received no adequate heed. It is, therefore, all the more to be welcomed that Dr. Whetham and his wife should once more, in a rather different way, and with a wider experience and an expanded knowledge before us, repeat the warning, and ask anew our consideration of the problem.

It is clear we cannot continually harass through the agency of taxation and by other means the more self-respecting, the self-supporting, and generally better portion of the community, for the benefit and maintenance of the thriftless, the foolish, the lazy, and drunken, without in the long run definitely modifying

* Herbert Spencer had been calling attention to the fact that an Act of Parliament would not be repealed if it were beneficent in operation, and that it can usually only be repealed with difficulty—as witness many vicious unrepealed and active laws of our times—and then only after it has inflicted intolerable wrong and prolonged suffering. Then he proceeded to show how vicious the great mass of legislation was by citing the remarkable fact that since the Statute of Merton (20 Henry III.) to the end of 1872, there had been passed 18,110 public Acts: of which, Mr. Janson, Vice-President of the Law Society had estimated that four-fifths had been repealed. The same gentleman also stated that during the three years 1870-71-72, 3,532 Acts had been repealed, partially repealed or amended, and of these 2,759 had been wholly repealed. In order to ascertain whether this rate of repeal had been maintained, Herbert Spencer referred to the volume of "Public General Statutes," which is issued annually, and noted the number of repealed Acts for the last three Sessions of Parliament. These would be the three Sessions immediately preceding the year 1884. Leaving aside numerous amended Acts, the result was that in this short period 650 Acts were repealed. Doubtless a number of them were laws which were obsolete; others had been demanded by change of circumstances, but seeing how many belonged to the current reign and were of quite recent date, it must be inferred that in multitudinous cases, repeals came because the Acts had proved injurious.

the innate nature of the race. For, as the authors point out, the struggle for life, incessantly at work in the lower world, affects Man also. Selection will tend to modify the character of a nation as it modifies the flora and fauna of a country.

Therefore, the first care which we must manifest with regard to every legislative proposal or contemplated social change is: "Will it tend to favour the growth of those elements of the population which already are known to be of national worth?" "Will it tend to check the reproduction of those who the present fragmentary knowledge already points out as detrimental to the community?" We have but to ask these questions, and then to look at our legislative machine, our party caucus, to the hungry and penurious portion of our Democracy, and to such of our unscrupulous politicians as are working their way to the front, to find a sufficiently clear answer. But when to these agencies, all working for evil, we add the achievements of philanthropists, of hospitals, of infirmaries, of workhouses, of the boarding-out system, and of the socal agitation in part led by certain medical men and in part by Fabians, we have not only an answer but a demonstration.

But there is another question of very grave importance and without the existence of which that of selection has no national meaning. There is nothing gained by a rigorous selection of the best, unless it is propagated in such numbers that the individuals representing it are passed on to succeeding generations in adequate numbers. There must not only be success in life, but also a "dominant fertility" of those who succeed. It is obviously useless for the best citizens to succeed if they leave the breeding of the race to the worst citizens. Clearly, it is even more disastrous if, by legislation and altered social customs we allow the degenerate types of citizens to both succeed and breed. Yet to us it appears that this is precisely what we are doing in many walks of life. In politics, personalities of the type of Thersites flourish, and are raised to positions of eminence:—

> "Loquacious, loud, and turbulent of tongue:
> Awed by no shame, by no respect controll'd,
> In scandal busy, in reproaches bold:
> With witty malice studious to defame,
> Scorn all his joy, and laughter all his aim:—
> But chief he gloried with licentious style
> To lash the great, and monarchs to revile.
> And much he hated all, but most the best:
> Long had he lived, the scorn of every Greek,
> Vex'd when he spoke, yet still they heard him speak.
> With wrangling talents form'd for foul debate,
> He's but a factious monster born to vex the State."

Not only in politics of a period not more ancient than the last election, but, we are afraid, in Nonconformist pulpits, types deserving of the anger of a Ulysses are also to be found. The sterner but loftier sentiment of three generations back would have made short work of that degenerate type of leader which we now usually call a demagogue. That these men escape punishment and suppression, not only in England, but in most of Western Europe, is one of the signs of the weakening and degeneration of national tone. And this tone, we are afraid, but reflects the intellectual attitude of the nation which tolerates it. Whether this is due to an inherent incapacity to foresee its dangerous consequences or to an innate slothfulness that must see the very edge of the precipice before it will resort to action, time and the result can alone decide. In lower walks of social life the same success of degenerate types is seen. They reap the fruits of harvests which others have sown. They are accommodated, clothed, washed, and fed in palatial workhouses; they are treated with the minutest care and the best skill in infirmaries, which are much more than comfortable; their slightest ailment is attended to with expedition and dispatch; they are visited by district visitors, and attended by district nurses; charitable institutions vie with each other in relieving their merited miseries; religious bodies convert them into cunning hypocrites, so that every family contains a Roman Catholic, an Anglican, an Episcopalian, a Presbyterian, a Methodist, a Baptist, a Calvinist, a Jesuit, a Wesleyan, a Primitive Methodist, and a Jew. These families doubtless would also contain a Mahommedan, a Buddhist, a Confucianist, and a Laotryist, but that these are creeds outside the range of charitable disbursements in this country. Their children are educated for nothing. An attempt is made to shoot them up the social rampart against their wills in a remarkably expeditious lift, called "Scholarships." They are fed for nothing and invited to come again. At somebody else's cost, they are medically inspected, the only intelligible reason for which is that more officials—another degenerate type of citizen—may be given employment; for certainly we cannot suppose that a cleft palate, a congenitally malformed heart or brain, a club foot or scattered and prematurely-decayed teeth are going to be remedied by a doctor's inspection. Not only are we not satisfied in a futile medical inspection, but now we are going to medically *treat* them for ills which cannot be cured. And so on, doing everything for them—even liberating them from prison when a Home Secretary, dazzling in the limelight, pays a visit to the prisons, never remembering that the leopard cannot change his spots, and that where we have social chamœleons, the birch sometimes succeeds in obtaining the requisite change of colour where peurile sentiment fails.

But to come back to the book. Considerations of this sort and many others justify us in feeling disquieted as to the direction in which the evolution of our nation is tending. We clearly cannot go on restricting the birthrate of our better citizens, hampering them increasingly in the struggle of life, while at the same time the birthrate of the inferior stocks is rising and their deathrate decreasing, and life is made easier for them by the compulsorily enforced altruism of the other citizens, without some day in the near future being called upon to face disaster.

Life, no doubt, is a difficult and stern affair. But, as the authors of *Family and the Nation* say in a passage of singular beauty: "Out of the very agony and weariness of the strife, is born that social and moral sense which gives to man his highest attribute and noblest reason for existence." If before the goal of human perfection is reached we reduce this agony and weariness, or lessen the strife, we are undoing that process which raised man to his present state. But notwithstanding this, as the authors are careful to point out, "of late years, the means of keeping alive the falling and fallen have grown with ever-increasing speed. Each advance in medical skill, in knowledge of Pathology or Hygiene, each new moral effect to cope with external evil, results in prolonged life for the members of weak and unsound stock, and still more significant, a lessened mortality among their children. There is often an inclination to deprecate the struggle for life, an endeavour to minimise its effects, to mourn with the loser rather than to rejoice with the winner."

When we endeavour to trace the origin of this softened sentiment which is so solicitous of the defective and the weak, so strangely heedless of the beauty of the strong, we are brought face to face with religious conceptions, religious change, and religious decay. In the older days, well within the memory of some of us as children, the dominant conception of our religion was that this life was a "probationary training ground for a higher one which was to succeed it." For better or worse, this conception is decaying, and no worthy ideal has taken its place. The cry of resignation "Thy will be done," or of that "I am in the hands of the Lord," uttered in almost every home within the land, and inscribed on many tombstones, not more than forty years ago, is now heard in but a few, though its inscription still continues. We apparently no longer believe in that great belief, carrying within it the germ of national salvation, and anticipating by many centuries the biological truths of to-day: "My God, "in Him will I trust. Surely He shall deliver thee from the "snare of the fowler, and from the noisome pestilence. Thou "shalt not be afraid for the terror by night; nor for the arrow

" that flieth by day ; nor for the pestilence that walketh in " darkness ; nor for the destruction that wasteth at noonday. " A thousand shall fall at thy side, and ten thousand at thy right " hand ; but it shall not come nigh thee ; there shall no evil " befall thee, neither shall any plague come nigh thy dwelling." Were it necessary to preach biological truths in the exalted language of imagery and addressed to a people to whom the methods and aims of science were unknown, is there any better language than this of the Psalms, to preach to men a sublime resignation to the powers of their own fitness or unfitness ? If a man is naturally alert and wise of understanding, he is not caught by " the snare of the fowler." If he is innately brave, the " terrors of the night " have no meaning for him. If he be constitutionally strong, having that chemistry of blood and tissue which renders him immune or only difficultly susceptible to disease, he will escape the " pestilence that walketh in darkness and the destruction that wasteth at noonday." And, if he be not wise and strong, do we want him to be a breeding centre for our nation ? The Psalmist in effect said " No," but modern social sentiment says " Yes." Here, then, is an opportunity for the Church. It may further justify its existence by its social service. It is not ritual but guidance that the nation wants. Behind the Psalmists and the Prophets stands the justification of Biology.

We now no longer resign the faltering to the call of the Inexorable. Save life at all costs, and disregardful of every national requirement, prolong it under all circumstances, is the attitude we have substituted.

In appealing to the community to face the social problem which confronts us, two classes of men—each characterised by its own code of belief—present themselves. There are those who have no belief in a future State. " To such minds," the authors believe, " pending the revival of a deeper faith, the thought of the future welfare and improvement of the nation may supply the ideal necessary for a worthy life." But those " who regard each man's frame as the dwelling-place of an immortal soul, will feel more the awful responsibility that is ours to determine, by our individual and corporate action, whether or no the bodies and minds of succeeding generations shall be fit temples for such sparks of the divine, fit habitations in which they can expand and develop until they are worthy of a sublimer sphere." Those who for years have been engaged in an attempt to obtain a recognition of more rational methods in dealing with the congenitally hopeless, civic and physical wrecks of civilisation, have long recognised that one of the greatest difficulties athwart their path is the theological conception of man's relation to a future state. The pure biologist can, of course, express no opinion upon this

conception. It lies outside his legitimate domain. But it is perhaps permissible for him to accept the conception as it stands, and to plead that it is a nobler thing to invest the "divine spark" in a beautiful and healthy body, than in an ugly and repulsive one. If the great object of religion is to teach a noble ideal of life, and is to lead men to love the beautiful in conduct and thought, surely that end will be the better attained, if the love of beautiful bodies and repugnance of stunted, distorted, and abnormal ones, becomes a prevailing tone of Society. That frame of mind which loves beauty in structure will be less apt, we should imagine, to love ugliness of conduct, than minds which delight in "broken reeds." And this prevailing tone is only possible when those who "give tone" to Society are themselves so innately constructed that beauty spontaneously appeals to them. But quite apart from an abstraction of this sort, there remains the consideration that if there be any relationship at all between the merits of men on earth and their transformed selves in another sphere, then it is the bounden duty of the Church to exert itself to the utmost to see that only a sound and noble stock is propagated on the earth. For surely it is the highest duty to ensure that its efforts shall stock that sublimer sphere with high souls worthy of their place. But if there be no such relationship, then the Church and its present efforts are futile. They would have no meaning. If it does not matter what our bodies and conduct are in this world, because irrespective of anything we have been or of any deeds we have done on earth, we are all equally to be angels in a higher sphere of life, then the whole base upon which the teaching of the Church is founded falls away. It has nothing which can appeal to man. Such an attitude is, of course, hopeless and spiritually meaningless. Therefore, it seems to us, that the Church has by the progress of biological knowledge been brought face to face with a problem to which it must give at once a definite "Yea" or "Nay," or cease any longer to be either the spiritual or material guiding light of its nation. It is in duty bound to fall into line with the Eugenic movement of to-day, and in season and out of it, rising supreme over every other consideration, to preach that it is sinful for defective stocks to propagate and equally sinful for sound stocks not to multiply.

It will thus be seen that the book of Dr. Whetham and his wife raises within its pages many interesting problems. To those who read it, many others will present themselves, as they have to us. The introductory chapter is followed by one on the "Scientific Study of Variation and Heredity." Chapter III. deals with "Inheritance and Variation in Mankind." In Chapters IV. and V. the subjects are "Inheritance of Mental Defect,"

and "Ability" respectively. Chapters VI. and VII. consider the important questions of the "Rise" and "Decline of Families" respectively. Chapters VIII., IX., and X. deal with problems of the "Birth-rate," the effects of a selective birth-rate, and the causes of a declining one are very fully considered. Chapter XI. gives general conclusions, and summarises the previous chapters.

G. P. M.

Charles Darwin and the Origin of Species.—*Addresses, &c., in America and England in the year of the two Anniversaries. By Edward Bagnall Poulton, D.Sc., M.A., F.R.S., Hope Professor of Zoology in the University of Oxford. Messrs. Longmans, Green, & Co. Price 7s. 6d. net.*

This book is dedicated to Alfred Russel Wallace. It is divided into seven Sections or Addresses, and there are four Appendices.

The first Section attempts to give a brief account of the history which led up to and followed the publication of the theory of Natural Selection and the *Origin of Species*. It is entitled "Fifty Years of Darwinism," and was one of the centennial addresses in honour of Charles Darwin which were read before the American Association for the Advancement of Science, at Baltimore, on January 1st, 1909. To the present generation, that has been born long after the struggle which the enunciation of the *Origin of Species* and the *Descent of Man* called into tempestuous existence, and that finds itself living in a period of complete intellectual liberty—perhaps in some directions too complete—this Section of the book cannot fail to be of the greatest interest and stimulus. It is difficult for the young men of to-day to fully appreciate the magnitude of the great victory which was achieved for freedom of thought during the earlier half of the fifty years that began with the publication of the *Origin of Species*. So deeply rooted were the religious convictions of men, so firm a hold had the orthodox conception of Man's origin upon men's minds, that even some of the scientific savants of the period who eventually recognised the success of the new doctrine of Organic Evolution, did so with feelings of regret, and others who ultimately accepted it, felt a reluctance in so doing. How fearsome was the spirit that pervaded many—even men of science—at that period, may be gathered from a quotation of a letter which Darwin sent to Fawcett on September 18th, 1861: "Many are so fearful of speaking out. A German naturalist came here the other day, and he tells me that there are many in Germany on our side, but that all seem fearful of speaking out, and waiting for some one to speak, and then many

will follow. The naturalists seem as timid as young ladies should be, about their scientific reputation." Fortunately, the period produced its man; one who, having become convinced of the verity of organic evolution, fearlessly disregarded conventions and consequences, and became "the great and beloved teacher, the unequalled orator, the brilliant essayist, the unconquerable champion and literary swordsman." Thus has Thomas Henry Huxley been happily described by Sir Ray Lankester.

The influence which his friends exercised upon Darwin, and especially the debt which in one direction he owed to Lyell, and in another to Huxley, is also dealt with in an interesting manner in this Section of Professor Poulton's book. Here, too, we read a vivid account of the almost inexplicable opposition which was offered to Evolution by the great anatomists, Richard Owen and St. George Mivart.

In this Section, Darwin's attitude towards the idea of evolution by means of mutations is set forth, and the fact that he had considered the possibility of the progress of evolution by large variations is considered. And, in order to leave his reader in no doubt as to Darwin's repudiation of "mutations" as factors in evolution, Professor Poulton cites a paragraph from a letter which Darwin sent to Lyell, in which he criticised some statement of the late Duke of Argyll. Here Darwin spoke of "the variation in the bill of a bird 'born' with a beak the one-hundredth of an inch longer than usual." The letter then proceeds to say, "The more I work, the more I feel convinced that it is by the accumulation of such extremely slight variations that new species arise." Apparently, one of the chief considerations which influenced Darwin to repudiate mutations as steps in evolution was "that it seemed to him in almost every case the adaptation of structure was too much, too complex, and too beautiful to believe in its sudden production." It is, of course, clear from this statement that Darwin regards a mutation as being necessarily of large moment, a kind of sport or monstrosity. That is not the view which Mendelians hold of mutations. But to this point we shall return.

Lamarck's hypothesis of the hereditary transmission of acquired characters and the recent attempt of Francis Darwin to amplify it, together with Weismann's theory, are also considered in this Section. The chapter is brought to a conclusion by a singularly eloquent tribute to the pathos of the dramatic conflict, out of which intellectual emancipation arose. "The distance from which we look back on the conflict is a help in the endeavour to realise its meaning." . . . "We have passed through one of the world's mighty bloodless revolutions; and now, standing

on the further side, we survey the scene and are compelled to recognize pathos as the ruling feature." . . . "There were sons of great men to whom the new thoughts brought deepest grief, men who struggled tenaciously and indomitably against them. And full many a household unknown to fame was the scene of the same poignant contrast, was torn by the same dramatic conflict."

Section II. deals with the personality of Charles Darwin. This is perhaps, from the general standpoint, one of the most fascinating chapters in the book.

In Section III. Professor Poulton controverts, and we think successfully so, the old and oft-repeated error that between the love of science and the love of literature there is an incompatibility which renders the pursuit of both impossible. The author goes to great pains to show that Darwin's attitude to art and literature in his later life has been greatly misrepresented, and even indeed perverted. There seems to be a widely prevalent idea that the nearly exclusive study of science leads to a loss of appreciation of poetry or music. Probably no greater error was ever promulgated as a general statement, and certainly not as a particular one applicable to Charles Darwin. Professor Poulton shows that it has no foundation. On the other hand, we believe there are, perhaps, some cases where it could be more truthfully contended that the study of science succeeded in arousing dormant faculties and in exciting a love of music, poetry, and literature. For there have been persons in whom there existed little appreciation of the beauties of the arts until they had succeeded in arousing their interests in science. But the truth of the matter is probably not to be found in considerations of the influence of the pursuit of science upon literary faculties, or *vice versâ*. It is probably not a matter of training or of general education. It is one which doubtless finds its explanation in the nature of the person himself. If a man inwardly loves both science and literature he will appreciate both. If he has a passion for the rigid analytical work of science and also for the flowing rhythm and expansive conceptions of poetry, he will find pleasure in the practice of both, in their due seasons. It would not be difficult to name an eminent German scientist, whose love of art was so great that the precision and accuracy of his scientific records were sometimes in danger in consequence. We need only recall the name and work of Tyndall to remind us that beauty and fire of literary expression, combined with a poetic fervour, are consistent and compatible with a scientific training and a life devoted to science. But after all, however varied may be our interests, what we can actually accomplish is determined by the shortness of life and the

limitations of daily time. *Ars longa, vita brevis* is true in this respect, as in others. Many men accomplish very little of a definite nature in life, not because they are incapable or unworthy, but because they attempt the impossible and endeavour to express in deeds their all too varied interests. Such men are not uncommon in the scientific world. It was once said of Lord Brougham "that he would have been an excellent lawyer had he known a little law." There are, unfortunately, men in every calling of life, who like Brougham have such a varied interest in things that it may be said of them, "they would be better workmen at their calling if they but knew a little more of their work." Men of science, as well as those of art, have to remember this, in order that something definite and tangible may constitute the edifice which their life leaves behind it.

In Section IV. Professor Poulton gives an account of Darwin's connection with the University of Cambridge, one of whose great and illustrious sons he was. It would be impossible here to give a proper idea of the fascinating interest of this chapter.

Sections V. and VI. deal respectively with the "Value of Colour in the Struggle for Life" and of "Mimicry in the Butterflies of North America." Both these sections are largely written from the historical standpoint, in order to restate, presumably in light of present-day controversies, the bearing which the facts of colour and mimicry have upon the conception of evolution by means of continuous variations and of the origin of adaptations through Natural Selection.

Section VII. cannot fail to be of the greatest interest, since here, we understand, a series of characteristic letters which Darwin sent to Mr. Roland Trimen are published for the first time. As we read Mr. Trimen's account of his first acquaintance with Darwin, there are revived again the memories of the old prejudices which the appearance of the *Origin of Species* aroused in 1859. And, as for the letters themselves, "we feel, again and again, as we read them, the presence of the bright, courageous spirit that could pierce the dark shadow of lifelong pain and discomfort, and preserve undimmed its humour and its breadth of view."

The Appendices A, B, C, and D are a little polemical in nature. Appendix A revives the old controversy as to the existence of single or multiple origins of species. Appendix B considers Darwin's attitude towards the question of evolution by mutation. Professor Poulton points out that Darwin's critics and others from time to time called his attention to the possibility of evolution through the agency of "monstrosities" or "sports," and that Darwin very carefully at different periods

considered the matter, and decided that, in his opinion, evolution did not proceed in Nature by such means. Professor De Vries' statement that Darwin believed evolution occurred both by mutations and by the accumulation of fluctuating variations, is traversed by Professor Poulton. If we read him rightly, his attitude is that Darwin denied altogether the probability of evolution in Nature by mutation. It is certain that in some of his letters, Darwin has on more than one occasion expressed, what was for him, very decided opinions adverse to modifications of species by sudden jumps. But it is only fair to bear in mind, that Darwin himself said "that no definition can be drawn between monstrosities and slight variations (such as his theory requires) though he suspected there was some distinction." That was in the year 1860. In 1873 he said, "It is very difficult or impossible to define what is meant by a large variation. Such graduate into monstrosities." We cannot, therefore, very clearly see the real justification for believing that evolution can proceed by slight variations and yet not by larger ones or monstrosities, when no definition which can express real differences between them can be framed. Here, in fact, Darwin was face to face with the difficulty which faces us to-day. Are these small variations—the so-called fluctuating variations—in reality, in some cases at any rate, simply small mutations? That is, variations which really express definite features due to gametic factors and which are capable of hereditary transmission. And, further, is it possible that under the one term, namely, "fluctuating variations," we have been all along including two externally similar but intrinsically different phenomena? Are some of these variations really small mutations and the others merely individual responses of a plastic organisation to differences in environmental influences? Professor Poulton becomes satirical upon this point, and in alluding to the fact that the Mendelian has recognised the possibility that some small variations are in reality mutations of small moment, has twitted them with the hope that they may yet "save their face" by calling the same thing (Darwin's small variations) by another name (small mutations). But this mode of controversy is a double-edged sword. For suppose the future shall demonstrate that there are two kinds of small variations; those which are germinal and those which are environmental. The latter are not inherited, the former are. We must distinguish between them. For is it not the function of science to analyse? The difference between the Present and the Past in our conception of these things is that now we are beginning to perceive the possibility that there are two kinds of fluctuating (small) variations and propose to distinguish them in our minds by calling them different names. Is that scientific or not? At

any rate, if there are those who " save their face " by resort to an ordinary scientific habit, namely, that of distinguishing different conceptions by different names, there are certainly those who may be in danger of losing their reputation for scientific acumen by seeing only one phenomenon where two in reality exist.

However, apart from these polemics, which are inevitable in all new forward movements, there is much, indeed the greater part of the volume, which is of more peaceful interest to all who would know something of the great controversy which began fifty years ago and of the works of the eminent man, out of and around which, the conflict arose and centred. For those of this generation, the battle is practically over, and the fight for intellectual emancipation has ceased. But how that battle began, how it was fought, who were its great leaders, who the faithful and unfaltering, who the doubtful and seceders, and how the great victory was finally attained, and at what cost, are questions of abiding interest that must appeal afresh to every succeeding generation. In the pages of Professor Poulton's book, once more the great controversy is unfolded before our eyes, and we can almost feel again its pathos and its tragedy, but yet too, we feel the glory and the thrill of battle, and rejoice in the conquest which the great leaders won for all who shall succeed them.

Mental and Moral Heredity in Royalty.—By Frederick Adams Wood, M.D., *Lecturer in the Biological Department of the Massachusetts Institute of Technology ; late Instructor in Histology and Embryology in the Harvard Medical School. With One Hundred and Four Portraits. Henry Holt & Co., New York.*

Of all subjects that deserve treatment by scientific methods we may, perhaps, regard History as having a paramount claim. So great is the influence of the events which it is its especial function to record, and so important to the welfare of mankind is a correct appreciation and interpretation of its facts, that not only is no apology needed for an attempt to deal with History by right methods, but it is a matter of surprise that no attempt to deal with the subject on anything like scientific principles was made until John Richard Green published his " History of the English People " in 1888. It is perhaps a matter of significant value as showing how great things are done from inner promptings, and not from extraneous sources—be they designated educational or otherwise—that with Green, the historian, " the study of history was with him never a matter of classes and fellowships, nor was

he in his work touched by the rivalries, the conventional methods, the artificial limitations, and the utilitarian aims of the schools. College work at Oxford and history work went on apart, with much mental friction and difficulty of adjustment and sorrow of heart. Without any advisers, almost without friends, he groped his way, seeking in very solitary fashion after his own particular vocation."

To those who regard History—as we all do nowadays—as something more than a record of Kings' births, ascents to the Throne, and descents to the grave, or desultory or vivid descriptions, varying according to the temperament of the writer, of battles and disasters, retreats and victories, Richard Green's attempt to present the facts of English History not as isolated events, but as factors in a process of evolution, came as a welcome departure from the time-worn method in vogue in his day. He endeavoured to show the relationship of deeds accomplished in the Past to the consequences which the Present paid for them and the Future reaped. To himself he would say: "A State is accidental; it can be made or unmade, and is no real thing to me. But a nation is very real. That you can neither make nor destroy." To him History had its philosophy, and whether he was conscious of it or not, this philosophy centred around the idea of evolution. To him the story of a great people is not to be found in the ecclesiastical annals of a period which are too often but the records of the mere squabbles of priests, but must be sought in the things men did, in the institutions they evolved, in the names of their streets, in the memorials of their guilds, in the nature of their market-places, in their struggles over poll and toll and tax, and in their social customs.

Dr. Frederick Adams Wood, dealing not with the history of a nation but of a social class, has endeavoured to further extend the application of scientific methods to the elucidation of historical problems. There is no social class that lends itself so well to scientific treatment as the Royal Class. In the first place, it is limited in numbers, and, therefore, the whole field of investigation can be more completely explored than any other. In the second place, its individuals live in such publicity, and their deeds are of such interest or importance, that they are more fully recorded than those of any other class; and through these records an estimation of the character of the men and women who committed them can be formed. Court diaries and biographies, moreover, contain the expression of personal judgements which contemporaries were able to form of the character of the royal personages with whom they came in immediate contact. It seldom happens that a King has escaped such an estimation of his character by one contemporary alone, and it is, therefore,

possible to weigh several estimates of a particular royal personage, and, to obtain a kind of mean result. For practical purposes such an average judgement will doubtless approximate very nearly to the truth. Certainly it is as near to the truth as we can possibly get. By thus endeavouring to weigh the intellectual, moral, and physical worth of the kings, queens, princes, and princesses whose characters Dr. Adams Wood has attempted to investigate, he has eliminated from his enquiry that most disturbing of all factors—the personal equation. By standardising, as it were, the average judgements thus obtained, it is possible to trace the hereditary descent not only of definite types of intellectual, moral, and physical traits, but also within a margin of error, of the different degrees of their developement. Dr. Adams Wood's method of ascertaining facts and arriving at conclusions, are such that " the basis of his book is placed in the hands of his readers, so that anyone doubting the truth of his assertions can easily take a few characters at random and look them up." We have so looked up his estimations of the chief Portuguese royal personages which are described in the three pedigrees on pages 209, 210, and 213, and, on the whole, we endorse the values which the author has assigned to them. They represent, we think, very fairly the general verdict of History.

Now, how far do these three pedigrees justify us in believing that intellectual and moral qualities are transmitted by inheritance with the same degree of inevitableness and inexorableness as the physical characters ? We think they fully justify such a belief. If we start with Alfonso I., the founder of the Kingdom of Portugal, we shall notice that while his father, Henry of Burgundy, is valued highly in the scale of morality, his mother Theresa, though a very able and accomplished woman, had a bad character, and was violent and passionate. Her low grade of moral character is indicated in the pedigree by the symbol X. We can trace this symbol throughout all the three pedigrees, marking in a symbolic way the relentless operations of inheritance, and justifying the old adage " That what is bred in the bone comes out in the flesh." But what is yet more important, since it shows that neither sex nor environment are answerable for moral character, is the fact that this symbol sometimes attaches to a woman's name and sometimes to a man's. Moreover, it may indicate a particular brother or sister, while the others are not so stigmatised. Yet they have been reared under the same influences. Coming back, then, to Alphonso I., we find him of decidedly higher morality than his mother but not so high as his father. In neither of his three children does his mother's low degree of morality appear. He married Matilda, of whom little is known. It may be taken that her moral nature was not notorious or some record would

have certainly been forthcoming. In the third generation of descendants from Alfonso I. we find Alfonso III., who, though a great warrior and statesman, was an unprincipled tyrant, again indicated by the symbol of low morality. Now, there is no other person in the pedigree, except Theresa, where this "unprincipled tyranny" and low morality could have emanated. For the various families which were introduced by marriage into the line of the descendants of Alfonso I., were of a high grade of morality, or their general character was such that they have been described as "kindly and pious," "virtuous and highly eulogised," and "able and moderate." Following the descendants of Alfonso III. we find the same symbol of low morality appearing by the side of the name of Alfonso IV., who was brave and able, but cruel and tyrannical, and by that of his daughter Mary, who was a wicked and revengeful queen, while her brother, 'Peter the Rigorous,' was a wise, able, and just ruler. Following down the line of descent of 'Peter the Rigorous,' through the next two generations, where the pedigree terminates, we find that his descendants are, with but two unindicated cases, all of high order from the moral standpoint. They are "moderate and enlightened" or "liberal and accomplished" or "accredited with the highest praises for the possession of many virtues." It is interesting to note that much of this high standard may be due to the introduction of Philippa, sister of Henry IV. of England, who had an elevated character and many virtues, into the pedigree, by marriage with John I. of Portugal. This fact is worth emphasizing, because there is a tendency in the popular conception of these problems, to regard the female part of the line as having less influence than the male. The pedigrees contained in Dr. Adams Wood's book will dispel this erroneous idea if they are studied carefully enough.

The appearance of many new characters, intellectual and moral, in the various lines of descent which are traced in these pedigrees, are accounted for clearly by feminine introduction. Such a case occurs, for instance, in the pedigree of Maria Theresa, on page 180, She married Francis of Lorraine, in whose ancestry individuals of intensely religious feelings do not occur. But both the mother and the grandmother on the father's side of Maria Theresa were intensely religious. Maria Theresa herself is not so indicated. But her son, Joseph II. of Austria, was austere, and his interest in religious matters as manifested by his ecclesiastical policy was deep and intense. We may in passing say that we cannot agree that the epithet "bigoted" which is applied to him by the author is altogether justified. We do not think that a Holy Roman Emperor, such as Joseph II. was, who brings himself into conflict with the Holy See, by issuing an edict of toleration, granting freedom of worship to all Protestants and to members of the

Greek Church within his realm, can be truly regarded as bigoted. Neither can we think that an Emperor who transfers the censorship of books from the clergy to laymen of liberal sympathies can be regarded as a bigot. We are inclined to think this epithet is accidentally misplaced. Thus the descendants of Charles V. of Lorraine (great-grandfather of Joseph II. along the father's line) manifested no decided religious bent until it was introduced through the Empress Maria Theresa, from her mother and grandmother.

There are those who believe that indolence, viciousness, licentiousness, dissoluteness, inferior capacities, stupidity, imbecility, and insanity are the products of the environment, and are begotten in the slums of cities or engendered in the stress of life. To them we commend a careful study of the "Chart of Modern Spain," which faces page 154 of Dr. Adams Wood's book. This chart deals solely with the descent of royal personages, to whom were accessible all the charms that render life smooth, whose wishes were commands satisfied in the moment of their utterance, to whom all activities were open and the means of participating in them were freely forthcoming. Yet, living under the best conditions that the civilisation of their time knew, the Royal Stock inscribed upon this chart contained individuals manifesting the undesirable qualities we have just enumerated. It may be contended, as we are afraid it has often been by people who are content with unsupported assertions that carry comfort to their own preconceived ideas of the nature of things, that licentiousness and imbecility are equally the product of a Royal Court and the slums. That such a contention has no validity, notwithstanding its wide acceptance, is clearly shown by the fact that in the Royal family which contains violent, passionate, licentious, and feeble-minded members, there are others who are normal, virtuous, amiable, and wise. But what this chart does show, is that once degenerative characters are introduced into a line of descent, and individuals of degenerate families are married to each other, they may be perpetuated through succeeding generations. In this chart, for instance, they pass down through six generations, and would continue to pass on indefinitely, so long as the mode of selection of the worst individuals, and the inbreeding which the chart shows to have occurred, are continued. Historians and others have been too apt to assert that inbreeding which is more common in Royal families than in others, leads to sterility and the termination of the stock. But, as Dr. Adams Wood points out, there is not only no indication of it in this case, but on the contrary, nearly every marriage was prolific of children, even amongst the closest blood relations.

That the circumstances which are attendant upon Royal Courts have nothing whatever to do with the origin or encouragement of licentiousness or weak lives, but that the mode of life, the mental level of the persons concerned, their ambitions and aspirations, and their capacities or incapacities are dependent upon their inherent nature, is clearly demonstrated by an examination of the Pedigree which contains the individuals of the House of Orange. Probably, in the history of the world there has never been a family which in its civic attainments and its high abilities of a varied order has surpassed that of this great and noble house. Their members have reigned in different countries, and yet the rough manners and low standards of the sixteenth and seventeenth centuries did not produce one depraved Prince of Orange, and the daughters of the house were noted for their many virtues.

The same lesson of the futility of environmental influences to overcome the congenital and inborn qualities, be they physical, mental, or moral, is shown by the remarkable and, we may add, cogent case of the descendants of a brother of Peter the Great, who are described on page 221 of Dr. Adams Wood's book. For political reasons five of these descendants, all brothers and sisters of one family, were, while infants, imprisoned. Their imprisonment lasted for thirty-six years. One of the five children was almost an imbecile and showed occasional symptoms of insanity. He was violent and eccentric. Now if this child stood alone we should no doubt be told by the people whose faith is pinned to environmental influences as a causal agent, that this imbecility was the consequence of thirty-six years of severe imprisonment. But it so happens there were four other children imprisoned under the same conditions and for the same period of time. Yet they were all normal. Indeed, one was much above the average and at the time of her release was "a woman of high spirit and elegant manners." Upon her release "she wrote a letter of thanks to the Empress so well expressed as to excite admiration how she could have obtained sufficient instruction during her long confinement." The explanation of this remarkable case is clear upon an examination of the Pedigree. Inheritance is its keystone. The father of these children was Anthony Ulric, of Brunswick, an excellent but mediocre man. Their mother was Elizabeth Anne, eccentric, extremely capricious, passionate, and indolent. Their maternal grandmother was Catherine, lively and good humoured and not peculiar in any way. Their maternal great-grandmother was the Empress Ivan, who was an imbecile, was epileptic, and pious. Thus the imbecility of the eldest of the five children is derived from the maternal great-grandmother, while the high spirit of the youngest child

appears to have descended from the maternal grandmother. The mediocre qualities of the other three children were derived partly from the father and partly through the mother.

One other point of great interest, because it confutes a dogma too well but not too wisely loved of Democracy, is revealed by Dr. Adams Wood's research. This dogma is to the effect that royal and aristocratic blood is only saved from degeneration by occasional sprinklings of plebian blood.* Unfortunately for this poetic conception an appeal to fact lays it low in a prosy dust. Peter the Great, though a man of "extraordinary will and energy," was at the same time violent and epileptic. His brother, Ivan, was an imbecile and epileptic. Another brother, Feodor, was also an imbecile. His sister Sophia had great force of will and ambition, and was intriguing and cruel. Now, it happens that the father of these children was the Czar Alexis, who was wise, temperate, and virtuous. In his day "it was the custom for the Czars to choose their wives from a large number of their subjects. The most charming girls in the kingdom were brought to the court for their sovereign's inspection, the most beautiful of all being selected and made legal queen. Alexis was married twice, and on both occasions to a peasant girl, as was his father before him. Yet, despite this treble introduction of plebian blood, epileptic children came from both his marriages, and the epileptic and imbecile psychosis has been transmitted to succeeding generations. Now the facts suggest that the degenerative character was not introduced by the peasant girls who married Alexis, but probably came from some of his obscure ancestry. Healthy though the peasant stock was, we see that it was ineffectual in regenerating a defective royal stock, and, it therefore follows, as a logical corollary, that royal and aristocratic blood, when it retains its virility and greatness, does so in virtue of its own innate qualities.

Dr. Adams Wood's book contains not only much of the greatest interest, but of the highest importance. For the first time there has been brought together the pedigrees of Royal Houses, so arranged that the relationship of the various members can be seen at a glance or easily ascertained. Moreover, for the first time, we have before us a definite standard by which we can measure to an approximate degree of accuracy the moral and

* It was to us a matter of great surprise, when a short time ago we read a sentence written by the Right Hon. G. W. E. Russell, in a little book entitled "Collections and Recollections," second series, in which this popular delusion was again expressed. We will give the sentence as it stands: "The ever-increasing dilution of the English aristocracy with elements drawn from other strata has prevented, or at least arrested, decadence." We should certainly like to hear the scientific evidence in support of this statement.

intellectual worth of royal and other individuals. In the light of this Biologico-Historical research, we must as a nation recast our too prevalent modern ideas, and in regard alike to Democracy, Aristocracy, and Royalty, seek to place our recent and developing national policy upon a sounder and more rational basis. We shall learn that our oldest traditions, fast losing their hold upon a people who have passed through the stage of being a nation of shopkeepers and are fast becoming a nation of slaves—slaves to democratic demands of rights and neglects of duties and to conceptions of the equality of men, and to the system of the propping up of weaklings by the steel props of officialism—have been more firmly and truly based on the solid ground of Nature than we at present conceive. Good blood first, good blood last, good blood always, that is the lesson we can gather from Dr. Adams Wood's research. Degeneracy in Royalty like the same quality in lower social classes, is due to bad blood. Greatness and nobleness in Royalty is due to good blood. Good stock may be spoiled by contamination with bad blood is the lesson engraven in deep-set inscriptions on almost every page and every pedigree in Dr. Adams Wood's book. No greater lesson exists to be learned by any nation. For what is true of Royalty is true of humbler classes. The nation which shall first learn the lesson and apply its knowledge, pressing forward ruthlessly and fearlessly to the goal of a worthy Manhood, an efficient and noble Kingship, and a patriotic Citizenship, is the nation that will win and hold the world. G. P. M.

Herbert Spencer and Animal Evolution.—*The Herbert Spencer Lecture, delivered at the Museum on the 2nd December, 1909, by Gilbert Charles Bourne, M.A., D.Sc., Oxford, at the Clarendon Press, MCMX. Price 1s. 6d. net.*

This booklet contains the substance of the first Herbert Spencer lecture which is to be annually delivered at Oxford University. Professor Bourne, who is the Linacre Professor of Comparative Anatomy at Oxford, has in a very interesting way brought some of Herbert Spencer's conceptions into the light of modern biological knowledge. More especially has he dealt with that clearer light which experimental Embryology throws upon some of the most fascinating problems of Biology, and incidentally upon Sociology.

The first part of the Lecture, among other things, deals with Herbert Spencer's belief in the transmission of acquired characters. This is followed by an account of Weismann's refutation

of the doctrine. Then comes the most interesting and central part of the Lecture. Essentially it is a consideration of the nature of the germ-plasm as we can elucidate it by our knowledge of experimental Embryology. This part of the Address is not long, but it is very clear and is illustrated by several figures. It is moreover of great importance on account of the light which it casts upon the nature of the germinal substance. It should be read by every statesman, politician, social reformer, and philanthropist.

The central feature of the embryological experiments here described is that what the organism will become, depends not on the environment, but on the nature and constitution of the germ-plasm. Let it be remembered, this is not an assertion, not a conception born of emotion, not a daughter-expectation begotten of a father-wish, but a simple, demonstrable fact observed and always to be observed, in the experimental laboratory. It was ascertained yesterday, it can be verified to-morrow.

Professor Bourne speaks not as a Sociologist but as a Zoologist. His attitude to social problems—which inevitably Zoology must trench upon—is indicated in the following sentence : " It is not the business of a zoologist to offer solutions of social questions. But he is within his right if he tenders to those whose business it is to study these questions such evidence as is relevant." We are not altogether in agreement with this attitude. It seems to us that those who can best solve social problems are they whose work and experience have brought them into contact with the actual foundations upon which they rest. We do not say biologists are the only persons who can approach them. But we do think that the certainty of experience and the strength of conviction which arise as the outcome of first-hand knowledge of life in its widest manifestation are essential to a full, a right, and a fearless understanding of these difficult questions. Doubtless there may be room for two classes of biologists : those who decide to remain in the calm and dispassionate air of the laboratory and those who prefer to enter the complex arena of human life outside, and to apply their academic knowledge and experience to the questions which there arise. There is an advantage certainly in such a subdivision. It may be that the conclusions of those who have brought their biological knowledge to bear upon human affairs, may be the more readily accepted by Society generally, if they are supported by the evidence of those biologists who have stood outside the strife and heat of the arena, where scientific knowledge and method must inevitably come into conflict with partisanship, and with those preconceived ideas upon the rise or fall of which, Schools and Creeds rest their existence.

Regarding Professor Bourne's Address in this light, accepting his conclusions as those of an eminent Zoologist who views social questions from the calm perspective of the laboratory, we note two conclusions which seem to us of special value. The first is this : " We should hold fast to Herbert Spencer's conviction that mankind is governed by the same laws as govern the animal kingdom, and that no true system of Sociology can be offered which does not take full account of those laws." This, we may say in passing, is one of the facts which is among the objects of the *Mendel Journal* to bring home to the minds of men. We welcome this pronouncement of Professor Bourne all the more, that it comes from him as a Zoologist purely and not as a Sociologist.

The second conclusion to which we refer is stated in the following sentence : " You will probably be inclined to the opinion that the conclusions to which Zoology has arrived are not sufficiently secure to warrant an attempt to apply them to affairs of State. Be it so. But it is a fact commonly overlooked that ideas derived from biological science *are* being applied to the affairs of the State, and that some who would hurry on the march of progress wish, consciously or unconsciously, to apply them still further. But these ideas are founded on the conclusions reached fifty years ago, and science has moved far forward since then. It is to be feared that much that still passes for "progress" is really regress, for it is founded on mistaken conceptions of the operations of Nature."

The chief idea to which this sentence refers, is the possibility of the transmission of acquired characters. If we have to discard this idea, in the form in which it has been promulgated since its conception, it may be asked how we can rely upon any other, even the latest of biological conceptions. If this one must be thrown over, what guarantee have we, that those which occupy the pedestal of thought to-day, may not to-morrow, in their turn be tumbled into fragments on the ground. It should, however, be remembered that the hypothesis of the transmission of acquired characters, arose as a speculation and was tacitly accepted as a supposed self evident truth. It was never based upon unquestionable experimental evidence. The newer conclusions of to-day are. They are the products of the experimental laboratory and the experimental garden. The facts thus ascertained are enduring ; fresh knowledge cannot alter old facts. If we must apply biological conclusions to human affairs, and if we are impelled to hurry on the march of events, let us, at any rate, be sure we are applying the enduring portion of Biology, and that we do not advance beyond its well ascertained facts. That in essence is the plea of Professor Bourne, and everyone will agree it is the soundest that can be made.

The conclusions of modern Biology, those which rest upon the irrefutable basis of experiment, certainly lend no support to the cardinal feature of present social sentiment. Not only do they not render any corroboration of the ideas upon which this sentiment is based, but they indicate clearly enough that these ideas are diametrically opposed to truth, and are largely the outcome of a confusion of cause with effect. G. P. M.

The Late SIR FRANCIS GALTON, F.R.S.

News of the death of Sir Francis Galton reaches us as the pages of this Journal pass to the press from their final revision. We have not therefore the opportunity of presenting to our readers a full account of his scientific and other work which is so intimately concerned with problems of Human Inheritance. But the death of one who, more than any other person, called attention in the early forties, to the importance which heredity played in the phenomena of life in general and of human attributes in particular, cannot be passed over without some word of appreciation, however imperfect, in a Journal which deals with problems that centre around the application of Heredity to human affairs.

Photo] *[Elliott & Fry.*

Sir Francis was in his own personality a very clear illustration of that process of inheritance which he did so much to expound. His maternal grandfather was Erasmus Darwin, the poet-naturalist, and both his father and paternal grandfather were men of scientific ability. Erasmus Darwin married twice, and Sir Francis Galton was his grandson by one wife, while Charles Darwin was his grandson by the other. Sir Francis and Charles Darwin were thus cousins. The Galton-Darwin-Wedgwood alliance of families was indeed a striking example of the influence of stock in the production of eminent men. The Galton family became connected through marriage with the Darwin family, by the marriage of Mr. S. T. Galton, of Duddeston, Warwickshire, with a Miss Darwin, daughter of Erasmus Darwin. The Wedgwood family

entered the alliance through the marriage of a Miss Wedgwood with Dr. Robert Darwin. Of this marriage there were six children, and Charles Darwin, the great naturalist, was one of them. From these three allied families, in five generations, comprising about sixty individuals, there are no less than sixteen eminent men, showing exceptional scientific ability. Of these, nine are Fellows of the Royal Society.

Sir Francis was born in 1822, and died on Tuesday, January 17th, 1911. He was thus eighty-nine years of age at the time of his death. He was one of the many eminent men of science which Cambridge University can claim for its own, since he graduated at Trinity College in 1844.

He was a man of very varied attainments, to which we are afraid the popular conception of his achievements does but little justice. In the popular mind he is mainly remembered as the author of the finger-impression method of identification. Yet this work, valuable though it be, is but a small part of his contribution to knowledge or to human affairs. His earlier days after leaving Cambridge were devoted to travelling and to the publication of his observations. He was indeed a pioneer among the explorers of the Dark Continent, for in 1844 he spent two years in ascending the White Nile. This was followed in 1850 by an exploration, in company with Dr. John Anderson, of Damaraland and the Ovampo country in South-West Africa. He next turned his attention to Meteorology and made the first serious attempt to chart the weather on an extensive scale. Subsequently, in about 1863, he devoted himself to the study of inheritance, and ever since his life has been occupied in working at the subject. Space does not now permit us to deal with this part of his work as it deserves. No real conception of it can be conveyed to the mind of readers in a short dissertation. But we hope in our next number to publish a full account of his work upon inheritance and of the fruits which

it has borne, and the harvest it is yet destined to produce.

Meantime, we can perhaps give no better indication of his real greatness, as distinguished from eminence, than by reciting his utterances of virility, pronounced as recently as 1909, which are among the last he has given to his countrymen. In a recent article* he wrote: "I have studied the causes of civic prosperity in various directions and from many points of view, and the conclusion at which I have arrived is emphatic, namely, that chief among those causes is a large capacity for labour—mental, bodily, or both—combined with eagerness for work." "A prosperous community is distinguished by the alertness of its members, by their busy occupations, by their taking pleasure in their work, by their doing it thoroughly, and by an honest pride in their community as a whole. The members of a decaying community are, for the most part, languid and indolent, they shirk work when they can do so, and scamp what they undertake." "Prosperous communities are also notable for enjoyment of life, for though their members must work hard in order to procure the necessary luxuries of an advanced civilisation, they are endowed with so large a store of energy that, when their daily toil is over, enough of it remains unexpended to allow them to pursue their special hobbies during the remainder of the day. In a decadent community, the men tire easily and soon sink into drudgery; there is consequently much languor among them and little enjoyment of life."

Recalling these words now, when he who wrote them has passed from the world, leaving the memory of his greatness and social insight, they remain as a warning to a nation, which by the signs of its times already shows evidence of that decadence

* "The Eugenics Review," July, 1909.

and absence of strenuousness that mark the road along which a nation marches to its destruction. It is a significant matter that in the newspapers which recorded his death there also occurred the records of "that sinking into drudgery, of that languor, of that little enjoyment of life, of that indolence, of that shirking" which the great, but alas, during life too silent, counsellor indicated as the sure signs of communal decadence. His wise counsel and sure intellectual penetration are no longer with us in the flesh, but his work lives, and they whom that work has inspired remain to carry it, if possible, to fruition. In that sense, this Journal, in part, may claim to be one of his heirs.

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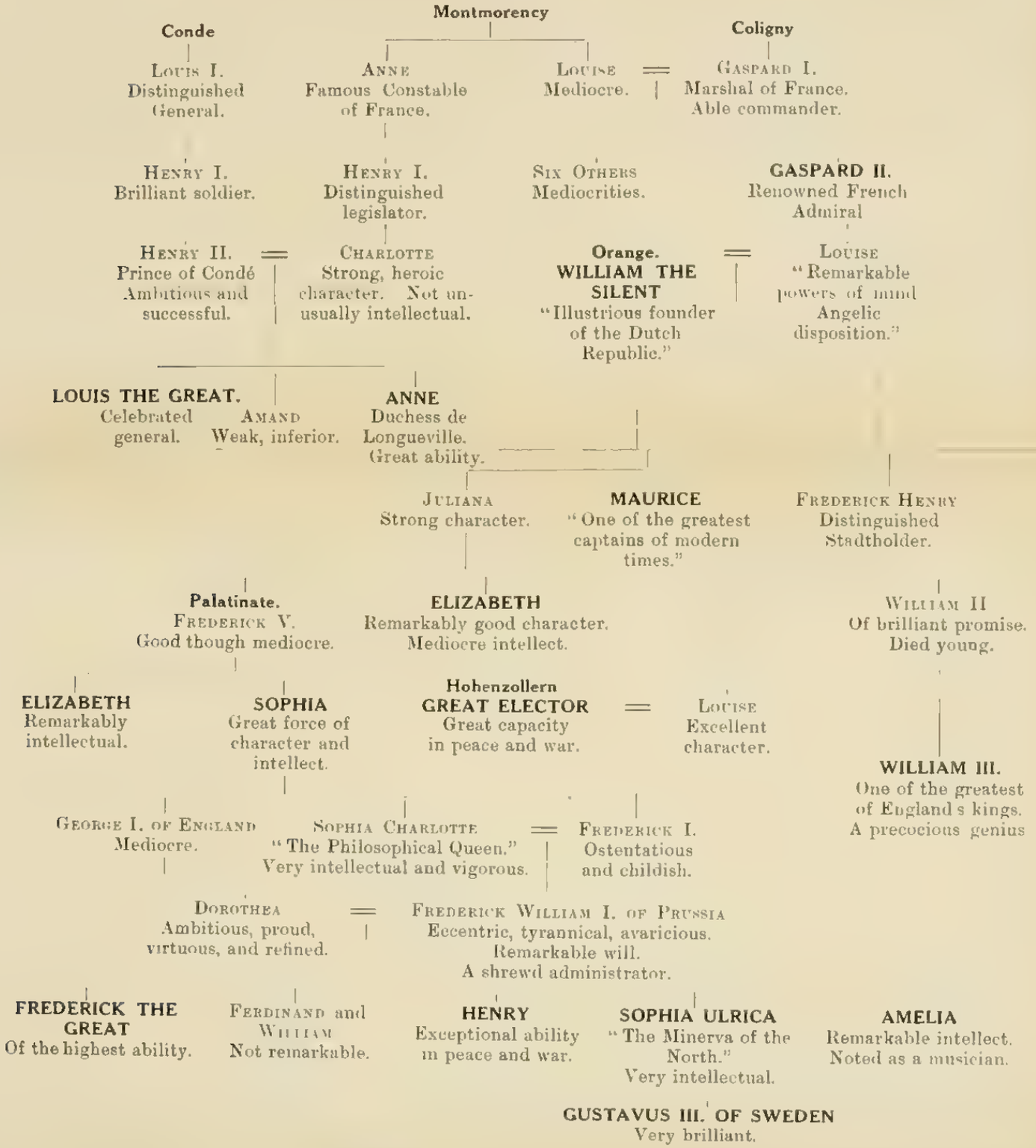

The
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No. 3 September, 1912

ALTERNATIVE HEREDITY OF MENTAL TRAITS.

BY
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Lecturer in the Biological Department of the Massachusetts Institute of Technology; late Instructor in Histology and Embryology in the Harvard Medical School, Boston, U.S.A.

THE sharp contrasts in traits of character between children born of the same parentage and educated under the same surroundings is often a matter of wonderment, and such variations in the human strip, reckless as they at first sight seem in their wide individualistic expressions, have often deterred belief in the value of heredity. The real lesson is quite the reverse, and these same contrasts, when rightly understood, form, perhaps, the strongest argument in favour of mental inheritance. They support the belief in the essentially predetermined nature of such differences as commonly exist between man and man, and bring the whole question of family and individual vicissitudes within the scope and understanding of the germ-cell theory.

Alternative heredity is exemplified when any two contrasted qualities are present in a stock, either as outward body manifestations or as inward germ-cell determinants, and these qualities are passed onward from generation to generation without neutralising each other, or, in other words, without mutually

destroying the contrast. Qualities black and white, good and bad, are in the parentage and in the stock. The offspring must be made up and entirely compounded out of either black or white or good or bad, one or the other, this is the "alternative." In the case of sheep the creature must be, as we all know, clearly and absolutely either one thing or the other. This is *alternative heredity* working at its perfection.*

How far is this true for mental and moral traits? Though man cannot be divided into good and bad, great and small, there is nevertheless much more alternative heredity at work than is commonly supposed. This can be the more readily demonstrated if one divides individuals into three classes. Let the great mass be placed in the middle or common grade, and then watch the appearance and reappearance of either one of the types belonging to the extreme ends of the scale, either the rare superior or the rare inferior. If these types are traced through long pedigrees of human beings by studying intensively families where most of the brothers and sisters and uncles and aunts can be traced and accounted for, the phenomenon of alternative inheritance can be seen to have an universal value. Everywhere we find certain children inheriting the peculiarity in question, while, if the records are complete, it is equally clear that others do not.

* The phrase, alternative heredity, was in use before the rediscovery of Mendel's law in 1900. It is a broader, looser term than strict Mendelian heredity, and does not raise the question of dominance and recession. It does, however, involve the idea of segregation of the germ plasm, and is a convenient term to employ when "factors" and "units," "dominance" and "recession" have not yet been unravelled.

No pedigrees are better preserved than those of Royalty, the names and dates being quite complete. As I have elsewhere offered proof of the essential validity and general utility of the historical and biographical materials,* I will cite some instances to prove how frequently a peculiar or exceptional mentality shows its presence in one member of the family while its absence is found in the very close of kin.

The House of Hanover had a sprinkling of pleasure-loving and dissipated princes, but the majority were quiet and domestic. Frederick Prince of Wales, Frederick Henry Duke of Cumberland, and Edward Duke of York, and Frederick Duke of York resembled George IV. in this particular, and in their moral nature stood in sharp contrast to George III., William Henry Duke of Gloucester, Edward Duke of Kent, and Adolphus Duke of Cambridge.

The Hohenzollerns in Prussia have had among their number a few men and women of remarkable mental endowments, and these also well illustrate the action of alternative inheritance, the genius springing apparently from the Houses of Orange and Coligny with Montmorency in the background (see Pedigree). This first appeared in the Great Elector of Brandenburg, a son of Elizabeth of the Palatinate, who was one of the thirty-two grandchildren of William the Silent—four only of whom had shown the family genius. The parents of the Great Elector were neither of them

* "Heredity in Royalty," Henry Holt, New York, 1906, and "Historiometry as an Exact Science," "Science," New York, April 14th, 1911.

more than mediocre. His son Frederick I. was but a vain and ostentatious figure-head, but an intermarriage caused at least a partial reappearance of the exceptional type in Frederick William I., of Prussia, and a second intermarriage produced Frederick the Great. Four of this generation were almost equal to Frederick the Great himself. His brother Prince Henry was a great strategist. His sisters Sophia Ulrica (Queen of Sweden, "The Minerva of the North"), Charlotte of Brunswick, and the Princess Amelia, were noted for their intellect, while at least four of the other five brothers and sisters could not possibly be placed in the same class. Out of all the nieces and nephews of Frederick the Great it is quite easy to pick seven as showing in one form or another the family brilliancy, while at the same time the feeling is strongly forced on one that, with one or two partial blendings as exceptions, the others do not. The seven whom I have included in this group which seem characterised as "brilliant" are Gustavus III. of Sweden and his sister Sophia Albertina; Augustus Frederick of Prussia, reputed the best artillery officer in the Prussian army, who died young; Louis, a son of Ferdinand of Prussia; Amelia Duchess of Saxe-Weimar, the distinguished patron of genius and learning, of Wieland, Herder, and Goethe; sixth, the celebrated commander Charles William Ferdinand of Brunswick; and seventh, his brother William Adolphus.

The House of Montmorency shows in the same way that genius tends to hold itself as a single entity and

skip about in course of descent. From Eberhard Montmorency, contemporary of Hugh Capet, to Anne, the great Constable of France, there were eighteen generations—one hundred and seven individuals—yet only two great names, Mathew I., who died in 1151, and his grandson Mathew II., who died in 1230. From his death to the birth of the celebrated Constable Anne two hundred and fifty-three years passed, and then a new centre of genius appeared which probably had nothing to do with the earlier manifestation, though it may, of course, have been an extreme reversion. Anne's second son, Henry I., Duke of Montmorency, was a distinguished legislator, being the only one of seven mature children to attain high fame, the others representing the mediocre ancestors. Henry II., the representative of the next generation, was even more eminent than his father. He was the only son to reach maturity. His three sisters were not distinguished for intellectual qualities. One of these sisters, Charlotte, married Henry II., Prince of Condé, and became the mother of Louis the Great Condé, and also of Anne Duchess of Longueville, celebrated for her beauty, tact, and diversified talents. The youngest of the children, Amand, Prince of Conty, in no way inherited the same qualities. He was an utterly weak and insignificant person. The marriage of Louis II., the Great Condé, with Clemence de Maillé de Brezé brought insanity and degeneracy into the line, and for three generations brilliancy, debauchery, and eccentricities ran rampant. There were, however, five other members

of the family about whom little is known, or at least nothing is readily obtained. The presumption is that these were mediocre normal and negative persons, who, having neither gifts nor vices, did not interest the gossips of the time.

The talent in the House of Bourbon also shows an alternative tendency, especially in the capacity found among the relatives of Henry IV. of France. His father was mediocre, but his mother, Jeanne d'Albret, one of the chief supporters of Protestantism, was distinguished for her talents, virtues, and heroic qualities. Her father, Henri d'Albret, was of little account, but her mother was Margaret d'Angoulême, famous for her literary work, and gave chief refuge to the advocates of the reformed doctrine in her time. Among the five children of Henry IV. Henrietta, who married Charles I. of England, seems to have had the brains and spirit, while Louis XIII. and Gaston of Orleans were unusually weak. In the next generation Anne Marie amply represented the genius of her grandfather. She was Duchess of Monpensier—generally known as "Mademoiselle"—and belongs among the few famous military leaders who have been women. The spirit and daring of "Mademoiselle" were indeed remarkable, especially at the capture of the town of Orleans, where she rendered important service. She also wrote memoirs which are interesting reading.

This type of active mentality disappeared in the subsequent generations of the French royal family. I have accounted for this in "Heredity in Royalty,"

by the force of selection. The marriages of Henry IV., Louis XIII., and Louis XIV. were especially disastrous, since they introduced the Italian and Spanish, Hapsburg psyco-neurosis. Degeneracy and viciousness thus introduced did not appear in Elizabeth, eldest daughter of Henry IV., or in Louis Duke of Burgundy, father of Louis XV. There is no good evidence that the two youngest daughters of Louis XV. were otherwise than normal, and in the last generation Louis XVIII. and his sisters, Princess Adelaide and Princess Elizabeth, seem to have entirely escaped the family blight, which in one form or another had affected certain individuals among their ancestors since the days of Joanna "the Mad," who died in 1555.

The Regent Philip of Orleans was notorious for his vices, and his daughters have left a bad name; but it must not be forgotten that Louis his son and Philippine the youngest of the daughters were as remarkable for their virtues as Marie, Charlotte, and Elizabeth were for their depravities. The mother, Frances Marie de Blois, was a good character, and the explanation from alternative heredity, of course, is that, in morality, some resembled the father and some the mother. The later history of the House of Orleans presents an exception inasmuch as normality was universal in the two generations following the debauched "Egalité." There were eleven children, and one or two might have been expected to repeat the degenerate type.

In the early history of Spain, at the time of the

Moorish wars, strength alternated with mediocrity. After the time of Charles V. an unfortunate ancestral combination gave nothing for mediocrity to alternate with as far as mentality was concerned, save insanity and imbecility. A few fine moral characters are still found here and there rising conspicuously among a mass of lazy or cruel degenerates.

The heritage from the mad Joanna also appeared in the Austrian line of the Hapsburgs, but did not at first affect many members. Selection was almost entirely through normal parentage. Vehse, in his well-known "Memoires of the House of Austria," refers to the more recent reappearance of the ancient ailment, "Whereas the children of Maria Theresa were all of them healthy, the sons of Ludovica were afflicted with the hereditary evil of the Spanish Bourbons, convulsions and epilepsy. The Archduke John alone was free from it, and all the other sons suffered more or less from the terrible malady; the Archduke Charles very badly, most of all the Archduke Rodolph. Ludovica's daughters were free, but the malady reappeared in the grand-daughters—as, for instance, in the Archduchess-Co-Regent Caroline of Saxony."

The history of Portuguese royalty is easily divided into two main periods, that prior to Emanuel the Fortunate, who died in 1521, which was an era of great kings, and the subsequent generations composed of little or mediocre personages. In the early period Ferdinand I. and Alfonso V. stand out in contrast, owing to their weakness; while in the latter period

much alternative heredity is seen on a close analysis of all the family members, especially evident in the different types of moral character. In fact, much the same picture is seen here as among their close cousins, the Spanish, Hapsburg-Bourbons. Since the middle of the nineteenth century the Saxe-Coburg and best Orleans blood has eliminated the insane and abnormal types, but has not introduced any ancestry containing great ability.

The early Romanoffs in Russia show a cruel, passionate, violent, and often epileptic type. This was absent in the Czars Michael and Alexis, appeared in Peter the Great, his brother Ivan and sister Sophia, and in two of his children, but not in his daughter Anne. The children of another Anne, who had herself inherited the Romanoff eccentricities and had married the excellent though mediocre Anthony Ulric of Brunswick, are especially interesting, as they show what the outcome may be after a very exceptional environment. All the children were taken when infants and for political reasons imprisoned for thirty-six years. Ivan, the eldest, was almost an imbecile, and showed occasional symptoms of insanity. This imbecility might naturally be attributed to the imprisonment, which was extremely severe; but the characteristics of the other four children make one suspect alternative heredity pure and simple. "Elizabeth, the youngest sister, was a woman of high spirit and elegant manners. On being released she wrote a letter of thanks to the Empress so well expressed as to excite admiration how she

could have obtained sufficient instruction during her long confinement."

The other children were mediocre, and in no way peculiar. "They amused themselves with reading, playing billiards and cards, riding, and walking. They walk about the town and in the environs, and drive out in carriages; the princes frequently ride, and particularly Alexis, who is very fond of that exercise, and said to be an expert. They not infrequently pay visits in the country and dine with the neighbouring families."*

Thus among five children exposed to a very unusual environment from infancy, we find a result showing little influence other than should be expected from heredity. Three were mediocre, representing the majority of the strain; one was an imbecile, corresponding to his mother and great-grandfather, Ivan; and one was spirited and cultivated in spite of it all, and rose very nearly as high as any of the immediate ancestors. Of course, such remarkable circumstances must have modified the characters of the four normal children, to some slight extent at least; still, even these exceptional cases deviate very little from what is to be expected from the force of heredity, if segregation of the germ-cells be taken into account.

The early history of the House of Oldenburg, in Denmark, shows little alternative heredity of a striking sort, but this is because there is little deviation from mediocrity or the average. Of course, if

* Coxe, "Travels," Vol. V., p. 19.

no marked peculiarities are obvious, then there is no opportunity to contrast its absence. Sweden, on the contrary, had, just before and after the time of Gustavus Adolphus, several very remarkable persons in the royal family. These are quite clearly differentiated from the mediocre types, though transitions also exist. These transitions naturally exist to considerable extent in all families. It is impossible to always satisfactorily estimate, classify, and grade mental and moral differences; but I believe these transitional individuals are much rarer than is commonly supposed, and my purpose in writing this article is merely to call attention to the universal tendency in mental heredity to give at least a partially perfect alternative inheritance. I have often looked for demonstration of pure Mendelian dominance and recession in psychic heredity, but have never found it in the material I possess. It may very likely be that a further splitting up of such rough first approximations and classifications as I have been forced to make will bring to light some unit character that will prove dominant or recessive. It would seem as if the mental qualities were more or less formed into one unit, and certain specific moral types into another, and as if the germ-cells were trying with more or less success to segregate these units.

The appreciation of the general principle of alternative heredity in human mentality is at least a valuable consideration, because it is something which environment cannot, we must think, tend to cause,

but rather would tend to obliterate had environment a power to do so. The fact that these differences are not obliterated, even among those living in the same social atmosphere, is a strong argument in favour of germ-plasm causation. The more minutely one studies separate families and traces them through succeeding generations the more one is convinced that the welfare of the family and consequently the nation depends on stock. At the same time, the more one seeks an explanation for the facts of family variation in any humanly imposed or artificially created atmosphere of surroundings the more one finds his expectations fail.

Some Observations on the Segregation of Human Types in Spain.

By ROSE HAIG THOMAS.

A PAPER on "Heredity and the Jew," by Dr. Redclyffe N. Salaman, appeared in "Genetics," Vol. I., Part III., in which the author dealt with the Ethnology of the Jewish people, first historically tracing the probable origins of the Sephardic and Ashkenazic, the two Jewish types, and illustrating with photographs of ancient Assyrian sculpture and modern portraits the persistence of the Jewish type from those early times down to our own. He pointed out evidence of the working of Mendel's law in the offspring of marriages between Jews and Gentiles, and came to the conclusion that the Gentile facial characters were dominant over the Jewish.

The great interest of Dr. Salaman's article recalled to my memory some observations made on human type segregation in Spain contained in the following notes, which do not pretend to be more than a mere sketch; the notes were jotted down during a journey through Spain in the spring of 1908 as an attempt to record whether the two types, *i.e.*, Visigoths and Moors, could still be traced, each preserving something of its purity of race.

The reader should be reminded that the first invasion of the Moors took place about A.D. 711, and the Moorish dynasties in Spain extended over a

period of nearly eight centuries, terminating with the fall of Granada in 1492. Nevertheless, the segregation of the two types may often be clearly seen, even among the members of a single family. And it is interesting to remember that this segregation occurs even though more than four centuries have elapsed since the commingling of the two races in Spain. In Granada, Cordova, Seville, Malaga, and all the places visited in Southern Spain, the extraordinary dissimilarity of skin, eye, and hair colour between members of the same family showed a "split" of the most pronounced divergence of type which drew the notice even of travellers who were not (as I was) looking for it. One American lady was heard to express her surprise "at not finding the dark race of Spaniards" she expected to see, adding "there are many as fair as at home."

The Province of Barcelona.

The Moors never penetrated so far North-East as Barcelona. The town of Barcelona submitted to Moorish rule after the sacking of Tarragona, and thus escaped the mixture of races which occurred further South. Here about four-fifths of the population seem to have brown hair of varying shades; the children and some adults have often quite flaxen hair, the complexions are generally fair, and the olive skin is rarely seen.

From Barcelona to Valencia.

Travelling along the coast line through Tarragona and Tortosa to Valencia on a Sunday, crowds of

people were collected at every station to see the train, giving ample opportunity to observe the proportion of fair to dark, which apparently preserved much the same ratio as in Barcelona, about three-fourths fair to one-fourth dark. In Valencia no observations were made on the colour of the population.

Province of Ciudad Real.

Alcazar is a junction between the East coast line and the main line from Madrid to Seville, and here we had a seven hours' halt. A man came into the waiting-room, remaining some time cleaning the lamp; he had a perfect Nigger crop of black, woolly hair, tight frizzled curls close to the scalp, also a very dark skin, but the *features were not Negro type.*

At Manzanares, further South on the line, was a twenty minutes' halt. Here was seen another man with an absolute Nigger crop of tight, black, woolly curls. Otherwise all along the line fair-haired children and blue-eyed, brown-haired adults abound. At a station, looking out at a standing train which had come from the South, amongst a number of heads thrust out was that of one man with a face almost pure Negro in type of feature and skin colour, with prognathous jaw; but the hair, though black, was *smooth and straight.* In the three men thus located in different parts of Spain, the hair appears to segregate independently from the features, and also hair colour and hair structure seem to be two independent factors.

PROVINCE OF MALAGA.

Near Palo, a small fishing village four miles from the town of Malaga, at the door of a cottage, lying basking in the hot sun, was a family of five small children—two flaxen-haired, two brown-haired, and one with coal-black hair and very dark brown skin, in marked contrast to his brothers and sisters. They were evidently of one family, being in steps of age upwards, from the flaxen-haired baby of a few months old who was rolling just inside the door.

In the town of Malaga was noticed a man with Negro features and skin colour. He was photographed. Amongst ten men sitting on a wall in the sun at the end of the Alameda, one was a Negro in feature, skin colour, and prognathous jaw, but the black hair was *smooth*. On the Alameda at the same hour, were two boys with Negro skin colour, hair colour, and eye colour, and one girl having the same Negro characters.

At Mass in the Cathedral was a school of twenty girls—amongst them one partially negro featured with Negro skin colour and prognathous jaw.

One man in the street with exact Negro features, the wide-spreading upturned nostrils ; but his black hair was *straight*, and he had a beard.

Another man with the high nose and Arab type of features, but with a Nigger crop of woolly curls. Here again we appear to have evidence of the segregation of the factors for facial features from the factors for hair.

On the Alameda one of the boot cleaners seen sitting in the same place every day, was a Mulatto. (On enquiry it was found he was a Cuban who had been forty years in Malaga.) He was not married.

Walking on the Mole of the harbour was a very dark Arab type of Spaniard, raven-black hair and eyes, carrying a year-old boy with flaxen hair and a skin of milk and roses. No fairer child could have been seen in England, and it was evidently his own from the tender care he bestowed upon it.

In one or two of the dark-skinned, thick-featured, prognathous types was noticed a long, flat nose with scarcely any bridge (not projecting from the face), but *without the wide-spreading, upturned* nostril.*

We met a woman in Arab dress, with very dark, high features, raven-black straight hair, and there was also a woman selling oranges and vegetables in the market on the river bank, whose features, hair, and skin colour were all pronounced Negro type. Both were photographed.

An old man near the market showed the Negro type strongly in feature, hair, skin, and prognathous jaw.

The extremes of fair and dark are very much greater in Southern Spain than in France or England, as the dark types are many shades darker than any to be found in the latter countries, and the features, more particularly in Malaga, have often a resemblance

* This seems to suggest, as we should on general grounds expect, that the form of the nose is due to many factors and that these are capable of segregation. See also "Miscellanies," page 200.—[Editor.]

to those of a Negro race. On the other hand, one would describe the Spaniards, from Barcelona down the East coast to Valencia and in the centre of Spain, as a fair race, among whom brown or grey eyes, with fair skin and brown or fair hair, are the most frequent combinations, though blue eyes are quite common. In reckoning with the number of Negro features and characters found in Malaga, it must be remembered that commerce with Cuba has played some part here, and that there has probably for two hundred years or more, been a constant intercrossing with the African races from that island. The mixtures of race, however, probably ceased with the cession of Cuba to America twelve or thirteen years ago.

The Province of Cadiz.

Amongst the military in Algeciras a great divergence of type is observable. An old woman with Negro hair and skin colour, but not Negro features, was noticed in the town.

A very dark Arab type of woman (possibly a gipsy ?) was seen in Algeciras carrying a flaxen-haired rosy-hued, white-skinned baby, and evidently her own, from the care and affection bestowed upon it. Both in Gibraltar and Algerciras a considerable number of real Moors in Eastern dress come over from Morocco. These are nearly as varied in colour and type of feature as the people of Southern Spain. One of the Moors had yellow hair and blue eyes ; another, shorter by a head than several he walked with, resembled in colour and feature the Negro type.

The greater number were high featured, dark, and tall.

No observations were made at Ronda.

The Province of Granada.

In the Arab quarter of the town of Granada called the Albaicin, the descendants of the Moors live separately from the Spaniards with whom, however, they occasionally intermarry.

The first interesting case in the Arab quarter was a blue-eyed child with dazzling white skin, rosy cheeks, and an aureole of golden curls, a boy of about eighteen months old in the arms of a woman of true Arab type, possessing black hair, dark olive skin, and black eyes. When asked, she said the child was her own, and that her husband was "rojo" (fair); the sight of this little fair-skinned child nestling its golden curls against the mother's black head was striking. A photograph was taken of the two. The guide now led the way into an old Mauresque house. Over the balcony hung a fair girl who beckoned us up. We entered, and found her brother to be a pure Arab, smooth, raven-black hair, black eyes, and a skin of the deepest olive brown. The mother stood beside them, also Moorish type, black hair and dark skin, but *the eyes were grey*. I asked what the father was. The reply was "Moreno" (black) Arab type. To a question about the grandparents, the mother replied that her father was "Rojo," a fair man, from whence came her grey eyes. Thus in part was the fair grandchild accounted for. The segregation of hair colour and

complexion here seems undoubted. The name was Gomez. Of this family six photos were made. One of the girl alone, two of the girl with her mother, one of the boy alone, and two of the boy with his father. None of these give the true sharply-defined contrasts between these people, because the photographs were taken on ordinary films instead of on panchromatic plates with colour filters. The expression "Arab type" is a common one in Southern Spain, where the type is found.

The following photos were made in the Arab quarter: Two "Arab type" boys not brothers; another dark Arab type woman with her child, the latter with flaxen hair and very white skin. The father, she said, was fair. There were also taken three sisters—one pure Arab type, one intermediate and a fair baby. (Illustration 1.) A group of people in the Arab quarter, was also photographed, and a man with his wife, both very dark, and their child, a flaxen-haired little one with light-blue eyes.

The larger number of dark over fair persons in Granada was noticed from the first, and confirmed by daily observation.

A plan for the more accurate reckoning of the colour-types of the population was arranged by counting the persons met with in the streets. My niece counted the dark and I the fair. We both walked upon the same side of the street, started and stopped the reckoning at a given moment. Mixed types which combined blue or grey eyes, with very dark skin and black hair, or fair skin with black hair

Illustration No. 1.

and very dark eyes, were not reckoned. The following groups of reckoning show results in the total of about three-fourths dark to one-fourth fair, being the reverse of Barcelona, where the population was approximately one-fourth dark to three-fourths fair.*

	Wholly dark.		Wholly fair.	
1st reckoning	10	..	4	
2nd ,, ...	5	..	6	
3rd ,, ...	20	..	2	
4th ,, ...	16	..	4	
5th ,, ...	31	..	10	
	—	..	—	
Total	82	..	26	= 108

The extremes were widely separate in shade of colour, the appearance being that of two separate races. This was the first fairly-accurate reckoning made, all other observations on the apparent numbers of dark and fair were based upon impressions, after walking the streets of the various towns ; as the above agrees with the impression I had set down of the proportion of the population colour-segregation in Granada, it is probable I may have formed a fairly correct estimate in other towns.

A very fair, flaxen-haired little girl of about seven years, was photographed alongside of a dark woman (her grandmother). Her mother was afterwards seen, and found to be also dark. The father was "Moreno," (dark), too, but he was not seen. There were six more children of this family, all of whom were said

*The Barcelona approximation was arrived at by general impression only.

to be fair, but we were unable to see them, and could not judge of their fairness.

In a doorway were three girls, evidently sisters. Two were blue-eyed with flaxen hair, one with black eyes, olive-brown skin, and black hair. These extremes in one family are the rule in Southern Spain. I have only once or twice seen a family all fair children, or a family of all dark. The manager of the hotel at Alhambra was a man with dark hair and dark eyes, but of fairish complexion ; he had a very dark Arab-type wife, yet their two children, a girl of four years and a boy of three years, had golden hair and light-blue eyes.

The Negro crop of tight woolly curls so often observed in Malaga was not once seen in the town of Granada. Intercrossing with Cubans is probably the explanation of its frequent occurrence in the seaport town.

Referring to the observations made up to the present, I am inclined to believe that the grey eye is intermediate, and will breed both blue eyes and brown eyes, and that probably of these latter the true-blue breed pure. The percentage of dark over fair persons in Granada is considerably higher than in the previous towns visited in Southern Spain, such as Malaga and Algeciras. This is more particularly true of the Albaicin or Arab quarter in Granada, where the Moors from Baeza took refuge, when that city fell into Catholic hands, in 1227.

The Province of Seville.

In the city of Seville several reckonings were made

in the Moorish quarter in the same manner as in Granada, the very old people and the types having combinations of dark hair and fair eyes and complexion, or *vice versa*, being left out. The result was :—

	Wholly dark.		Wholly fair.	
In one reckoning ..	70	..	22	
In another reckoning	72	..	13	
	—	..	—	
Total	142	..	35	= 177

Roughly, four-fifths dark to one-fifth fair. This is approximately the same as in Granada.

In the tobacco factory at Seville, where only women are employed, the percentage of dark over fair was much higher ; this might have been due to the employment of gipsies, but the question was not asked.

In Seville City, one prognathous-jawed woman with Nigger type of hair and skin was seen.

Several times I made reckonings of the Seville population-colour, and they always came out in the same proportion of approximately four to one.

Madrid.

In Madrid, the extreme contrasts of dark and fair are not so noticeable ; there are so many more intermediate in all gradations, and the gulf between the two extremes is not so wide. There was nothing I observed calling for remark.

General Considerations.

In most cities in Southern Spain there is a gipsy quarter generally immediately outside the town.

In Granada, the gipsy quarter is adjacent to the Arab quarter, and consists of a long line of caves hollowed out of the mountain. The tribe have a chief and keep themselves socially, entirely separate from the Arab inhabitants of the Albaicin. They only marry among themselves. To this custom of intermarriage is perhaps due the fact that their numbers never increase, but remain at about eight hundred.

Though informed by the guide that these gipsies never intermarried with the Spaniards in the Arab quarter, close questioning revealed the fact that he knew a Spaniard who had married a gipsy girl, and that they had three children ; the man, he said, was fair, and the gipsy very dark. As this promised to be an interesting family group, a visit was arranged up the dirtiest, narrowest streets in the Arab quarter to their dwelling behind the Church of San Nicholas. There was found a dark-skinned, black-haired, black-eyed gipsy woman, with her three children. The eldest was a very fair girl with flaxen hair and blue eyes. The second was a boy, with black eyes, black hair, and an olive skin, as dark as the mother's. The third was a flaxen-haired, blue-eyed, fair-skinned baby girl of eighteen months.* The gipsy mother said the father's eyes were blue. It will be observed that the coloration of the two girls resembled the father, and that the boy took after his mother in colour. Of course, we should not infer from a single case that

*We may accept it as pretty certain that the eldest girl of this group was an actual fair type, and not merely fair because she was young. The second brother, younger than his sister, was already dark in all characters. What the youngest member of eighteen months really is, can only be determined when she is seen at a later age.—[Editor.]

Illustration No. 2.

there is a definite correlation of eye and hair colour and complexion between the males of one generation and the females of the next. I had no chance of adding the father to the group, since he was in the Customs Service, and never came home till 8 p.m. Four photographs were made of the gipsy mother with her three children, of which one is reproduced in Illustration No. 2.

The guide related a curious form of marriage. It is the only form prevailing amongst the Granada gipsies. When a young man falls in love with a girl he elopes with her, and carries her off into the hills. In seven or eight days he returns, and obtains her parent's consent. There is no other ceremony. When children are born, however, they are baptised.

In a drive through the gipsy quarter outside Seville, in a long street of wretched houses, were seen at least seven or eight Negro-type gipsies, idling among the lounging population of these very dirty dwellings. The Seville guide confirmed the information of the Granada guide, that the gipsies and Spaniards never intermarry, but stated that Cubans and gipsies have interbred, which one could see was true. The Guadalquiver is a tidal river. Seville being thus a virtual seaport, the Negro of Cuba has left his traces here as in Malaga.

PRIMITIVE EUGENICS.*

By E. TORDAY.

A FEW days ago the daily Press recorded that in 1911 London alone gave the respectable sum of four-and-half million pounds to charities. A million and a half of this went to the hospitals and three millions were given for missionary work. As a considerable part of this money finds its way to parts of Africa, with which I am well acquainted, I am desirous, in the interest of knowledge, of discussing the expediency of such an outlay. Missionary work is of two kinds; first, it aims to spread *our* moral ideas among the savages; and, secondly, it seeks to confer the blessings of *our* civilisation upon the benighted heathen. I will not dwell on this first aim, but I cannot pass it over without mentioning that there are tribes in Central Africa whose moral code is inferior to none, and any European living according to its precepts would richly deserve a "*prix de vertu*." It is principally with the second aim that I desire to offer a few critical considerations.

I have travelled in Central Africa for about ten years and have visited places where European influence has long been established, where natives wear trousers and top-hats, go to chapel and drink gin, and where workhouses and prisons flourish; or, in other words,

* This paper is a resumé of a chapter in a forthcoming book on his travels, which Mr. Torday hopes to publish in the Autumn.—[EDITOR.]

places which must be considered as highly civilised centres. I have visited countries where no European had been before me, and I have been on such good terms with the natives that I was enabled to get a thorough insight into their ways of thought. As I occupied no Government position, I was the favoured witness of ceremonies carefully hidden from the vigilant eye of the official and the missionary. Sitting around the camp fire I have discussed with the negroes many of the social problems that one hears talked about even in the most select drawing-rooms of London "Suburbia." The summary of what I have seen and what I have been told on this subject may well be called "Primitive Eugenics."

It is a well-known fact that the child is the real aim of marriage among the negroes; tribes exist where a man can divorce his wife at will if she has not borne him a child, whereas the birth of one at once makes the union indissoluble. Thus the *racially* valueless sterile females are not a drag upon the husband's energies, disappointing his hopes, and cutting short the stock of which he is the representative and the gametic bearer. It is considered a shame as well as a misfortune to be childless. I mention this, because it is necessary that the reader should understand that the savage negro is not less fond of his progeny than the European parent. But fond as he is of his children, the welfare of his *race* is still *instinctively* dearer to his heart, and I want to show in this paper by what means he manifests this love of his race.

Initiation ceremonies have often been recorded by travellers, but few have understood their real importance. Their true purpose is to aid the boy and the girl in the fulfilment of their duties as husband and wife ; in the case of boys this may seem unnecessary in a country where even the smallest children possess knowledge which is carefully concealed from our own. But the girl, when she leaves the " fattening house " or the bush, wherever the initiation takes place, knows how to conduct her life, so as to have every chance of fulfilling the expectations of her race. She also has been taught what her conduct must be during the quickening of a life yet forming, and how to prepare for its advent to the world. Thus the life of the first-born child is more likely to be preserved than among the ill-informed and needlessly apprehensive young European mothers. When the infant is born, it is examined carefully ; if it is weak or deformed, then in one way or another, it is no longer allowed to burden its own life nor handicap its race in the struggle for survival. This is the reason why one sees no cripples or other kind of defective persons in Central or West Africa ; this is the reason why man there is a *man*, virile in habit, strong and lithe in body.

No woman has marital relations with her husband during the suckling period, which lasts from three to four years ; this means that an interval of four or five years must elapse between the advent of the first and second child. Not only can the mother, therefore, bestow all the necessary care on each child,

but she herself is enabled to recuperate for several years between one birth and another. There can be no doubt that this gives the progeny a good chance, for they are fed through a sufficiently prolonged period with natural food. But of necessity it must tend to the production of small families. This is remedied by polygamy.

Westermarck and others have pointed out that in a country where intermarriage is prohibited and where prosperity reigns, the number of female children who survive is considerably greater than the number of males. This must be the case in Central Africa, where marriage between people who can trace the slightest relationship to each other, or who belong to the same village, or the same clan, or who own the same totem, is looked upon as incest. And in this region of Africa incest is considered the most despicable of crimes, and poverty is unknown.

It is in the name of the poor black woman that the abolition of polygamy is claimed by philanthropists in Europe ; in Africa it is not so much the negro man who objects to the introduction of monogamy as the woman. It is easier for a negro to get a second wife than a first, and his wife will soon urge him to give her a companion. I remember a case where a black trooper presented himself before the Registrar in order to have the banns of his contemplated marriage with a dusky beauty published. The only paper of identity he could produce was his marriage certificate with another woman ! He was greatly astonished when informed that, although his real wife was

sterile, he could, according to European laws, marry no other woman as long as she lived. It is a fact, significant of the futility of legislation to control social customs, that shortly afterwards the legitimate spouse disappeared in a mysterious way.

The negroes have their rough rules of hygiene, which are now forbidden by the rules of the white man. The Batetela north of the Sankuru river remember a terrible epidemic that raged in their country some thirty years ago. As far as I can make out it was akin to cerebro-spinal-meningitis. All the afflicted were sent into the forest, and if they showed themselves in the villages they were killed. Food was deposited for them at a certain place; when they no longer came to fetch it they were considered dead and duly lamented. The epidemic not only disappeared, but it never spread over more than a limited area. The same was the case with sleeping sickness; it existed before the white man arrived, but never got a chance of spreading until the European Governments put down the ancient and barbaric but none the less effectual custom. Since then half of the population of Uganda has perished from it. Among the Baluba, even now, any person who is known to have given syphilis to another is executed.

Criminals—by whom I mean people who habitually committed larcenies, or were guilty of some serious crime, were sold as slaves to a strange tribe. Instead, therefore, of being a burden to the law-abiding population as our criminals are, they were made use of to compensate their victims, to whom the purchase

money was paid. They were also prohibited from transmitting their instincts to the children they might have begotten in their own tribe. This latter consideration was to such an extent part of the reason for this form of punishment that among certain tribes (as those of Manyema) criminals were regularly neutralised. Slaves who misconduct themselves are often slain and eaten in cannibal tribes and most tribes in Central Africa are anthropophagous.

All this may seem cruel and barbarous, but it cannot be denied that, once we are away from the civilised centres in Central Africa, there are no beggars to be found. Among the Bayaka, a person who asks for alms becomes, *ipso facto*, the slave of the man to whom he addresses his request. There is no such thing as real poverty. Very few crimes are committed, for it does not pay to play that game, and the native laws are enforced without regard to the sentimental considerations that weaken or nullify the influence of our own.

It is not in accord with my present desire to draw any conclusions from what precedes; the sole task of the traveller consists in recording facts. None the less, I may be permitted to express the fear that the disadvantages and dangers of our civilisation as applied to Africa will be only understood when it is too late and when the natives will have become civilised out of existence like the Tasmanians and other unfortunate races, who had committed no greater crime than having been the product of a line of evolution wholly different from our own. The policy,

too often, alas, inflicted in the name of humanity and civilisation, of compelling so-called savage tribes to conform to habits to which they were not born, to live up to aspirations that Nature has rendered alien to their understanding, and to burden them with luxuries which carry the seed of racial sterility, is a punishment too heavy for any race to bear.

The Heredity of Racing Stamina in the Thoroughbred Horse.

By J. B. ROBERTSON.

In the literature of inheritance there is a trite observation that speed and stamina in the racehorse are not separate characters, but the manifestation of a combination of characters, involving bone, muscle, internal organs, and a capacity for making acquirements. This, however, reads rather like a loose excuse for not coming to close quarters with hereditary characters in the racehorse. A capacity for making acquirements is a convenient phrase which can be applied equally well to a senior wrangler or a bucking broncho in a Wild West show. All the higher animals have a capacity for making both mental and physical acquirements; but the capacity is subservient to the hereditary morphology and physiology of the tissues. The merest tyro can see that the bony skeleton of the thoroughbred and his relatives, the Arab and Barb, differs very materially from that of shires, Clydesdales, hackneys and so forth. Indeed, the crude ungainly bones of the shire, which give one the impression of a horse in the making, would be an impossible proposition in a racehorse. In certain racial characters the bones of thoroughbreds are alike. Nevertheless, individual differences are very great. But in the passive skeleton these differences rarely offer any explanation for the extremely wide diver-

gencies in both speed and stamina seen in various thoroughbreds. The length, width, and circumference of the bones, and the surfaces which they present for muscular and tendinous attachments have differed as widely among classic winners as between them and common selling-platers, or horses too slow for racing. It is not a very rare occurrence for well-grown, good-looking yearlings with good action to realise at public auction anything from one to two thousand guineas, and yet prove absolutely useless for racing purposes. Their failure is due not to want of symmetry, lung space, and so forth, but, as a rule, may be set down to one or other of the two following causes, or sometimes both, viz.: Insufficiency of what, for want of a better term, we may call nerve energy, and inability of the voluntary muscles to respond to stimulation for more than a short period; that is to say, these horses are either too slow or do not stay the minimum racing distance of four furlongs for two-year olds, and five furlongs for older horses.

It seems necessary to emphasise the fact that muscular movements play a leading part in all the great bodily functions. The circulation of the blood and respiration, the two functions most essential to the maintenance of life, are kept up by the alternate contraction and relaxation of muscles. The control of the blood supply to all parts of the body, and the movements of the alimentary canal, in addition to that immense class of movements which are more or less under control of the will, all depend upon muscular

action, which, in its turn, owes its initiation to stimuli passing along the nerve fibres from the central cells in the brain or spinal cord. All muscular tissues have certain common properties, such as excitability, contractility, growth, and respiration. All are made up of units which readily take up oxygen and give off carbon dioxide. But there is, nevertheless, a wide divergence between the muscles which are known as involuntary, or smooth, and found in the walls of the blood vessels and alimentary canal, and those of the heart and the skeletal or voluntary muscle group. The smooth muscles do not concern us here, but, in order to make the subject of stamina in the racehorse intelligible, it is necessary to set forth a brief account of the structure and functions of the unit of voluntary muscle, the muscle fibre or cell.

In regard to microscopic structure, it cannot be said that finality of opinion has yet been reached. Eminent observers differ widely as to what is to be seen, and it would be out of place here to enter upon a discussion of the points in dispute. All, however, are agreed that the muscle cell, or fibre, is surrounded by a structureless membrane, the sarcolemma, and that the fibre shows alternate dim and bright transverse bands, and can be split up into discs by certain reagents such as osmic acid. It is also agreed that the fibre shows longitudinal striations which appear to separate it into fibrils ranged side by side. "The contents of the muscle fibre appear to consist of two functionally distinct substances, a contractile substance and an interstitial, perhaps nutritive non-

contractile material of a more fluid nature. The contractile substance is arranged as longitudinal fibrils embedded in interfibrular matter."*

Oxygen and a carbo-hydrate in the form of glycogen are essential to muscular life and action. A stock of both, but principally of oxygen, is taken up and stored in the essential constituents of the muscle fibre. These constituents are broken down during muscular contraction, and more slowly during rest, carbon dioxide and lactic acid being the end products. Ranke has shown that the exhausted muscles of a frog can be restored by washing out the vessels with physiological salt solution. He claims that it is the removal of carbon dioxide and lactic acid, the products of contraction, which immediately restored the muscle. The injection of arterial blood, or of an oxidising agent, like potassium permanganate, into the vessels of an exhausted muscle also causes restoration†.

When a muscle fibre is stimulated it increases in cross sectional area, but the volume remains constant, contraction commencing in the middle of the fibre and passing to the ends.

The Varieties of Muscle Fibres in the Race Horse.

So far we have considered the structure and properties of what may be termed typical striped muscle fibre. But it has been clearly established that there are two varieties of striated muscle, namely pale and

* Stewart. † Kronecker.

dark red. In some fish, the domestic fowl, and in the greater part of the musculature of the rabbit, the muscles are of an extreme pale variety, whilst in the falcon, and other birds which possess the faculty of long-sustained flight, the muscle fibres are almost exclusively dark red. In the horse the two varieties of muscle fibre are found, but not nearly so widely separated in structural appearance as are the muscles of the domestic fowl and the falcon. I shall endeavour to show, however, that the physiological properties of the two types of muscle are in the horse very widely differentiated, the dark red type being correlated with the capacity for long-sustained muscular effort, whilst the presence of an excess of the pale variety is correlated with inability to respond to repeated stimulation continued for more than a very short period. The difference between the two varieties of muscle fibre may be summarised thus :—

Pale Red Fibre.	Dark Red Fibre.
1. Pale red in colour.	1. Dark red in colour, and in transverse section granular in appearance.
2. $\frac{1}{400}$th part of an inch or more in diameter, and cross sectional area large in proportion to length.	2. Less than $\frac{1}{500}$th of an inch in diameter, and cross section small in proportion to length.
3. Transverse stripes very plainly marked, but longitudinal stripes indistinct.	3. Longitudinal stripes very plainly marked.

Pale Red Fibre.	Dark Red Fibre.
4. Owing to the bulk of the fibres in cross section, the capillary network of blood vessels running between them is narrow.	4. The capillary network exceeds in size the ordinary capillaries, and is dilated into little reservoirs.
5. Stimulation repeated at short intervals rapidly produces exhaustion.	5. Stimulation at short intervals does not rapidly produce exhaustion.
6. Moderate stimulation is sufficient to produce a maximum contraction.	6. Requires powerful stimulus to ensure a maximum contraction.
7. In the living animal, the essential constituents of live muscular substance are rapidly broken down during continued muscular contraction. Carbon dioxide, together with other end products collect in the muscle, producing the phenomena of tissue asphyxiation and fatigue.	7. In the living animal, the essential constituents of the live muscle appear to possess an accentuated faculty of storing up oxygen, and perhaps glycogen, in some compound or compounds. The amount of carbon dioxide given off by the muscle during work is at the same time comparatively small, and passes immediately into the dilated capillary loops, in place of lodging in the muscle fibre itself. Tissue asphyxiation and fatigue is, consequently, long delayed.

From the above it will be gathered that, in the case of a horse whose muscles are principally, if not solely, of the pale red variety, the materials necessary for contraction are used up during continued fast work much more quickly than they can be renewed,

and, secondly, that waste products are formed by the muscle faster than they can be removed. It is, of course, true that fatigue does not wholly depend on the lodgment in the muscle fibre of carbon dioxide and other broken down products, for experiments clearly show that these substances also act on the central and peripheral nerve tissues; but in the racehorse, muscle asphyxiation is the primary cause.

The muscles in the heavy breeds of horse appear to be exclusively of a pale red variety. The shire horse, even when thoroughly conditioned and fit, rapidly suffers from muscle asphyxiation if made to gallop at his top pace, poor as it is, for a short distance. In some individuals a furlong will suffice to produce distress.

In the thoroughbred both varieties of muscle are to be found in the same individual, and, although in this class of horse the opportunities for making microscopic examinations are few, it is permissible to conclude from the specimens of muscles which I have examined that the dark red fibres greatly preponderate in the muscles of a "stayer" and the paler variety in the muscles of a "sprinter."* At all events, I trust it will be clear from the evidence which I shall advance that the physiological properties of the muscles of certain racehorses are sharply

* Mr. Robertson tells me, in answer to my inquiry, that he thinks about 75 per cent. of the muscles of a true "sprinter" are of the pale variety. In an "Intermediate" racer he thinks there is about half and half, and in a "stayer," such as *Willonyx*, he would say that 90 per cent. of the fibres are of the deep red variety. These statements are of course very approximate, and they partake of the nature of constructing the whole horse from a microscopic examination of a few muscles only.—EDITOR.

defined from those of others, and that the gametic composition of these individuals is in accordance with a Mendelian conception of alternative unit characters.

In the laboratory it is not difficult to demonstrate, either on the living subject, or by electrical stimulus applied directly to excised muscle, or indirectly through its nerve, the amount of work a particular muscle is capable of performing before fatigue is induced. In man muscular fatigue can be directly measured by means of an ergograph. By this instrument a record of successive contractions repeated at regular intervals, say of one of the flexor muscles of the finger in raising a weight, is taken on a revolving drum. So long as the muscle is working within its capacity, no alteration is observable in the successive curves traced out; but, when the muscle becomes at all fatigued, the diminished height of contraction is at once indicated on the drum. The actual amount of work which the muscle can perform at a given frequency of stimulation before fatigue is induced is, of course, obtained by multiplying the weight by the total distance through which it is raised. This method of testing muscles and measuring their work is really only applicable when the subject brings intelligent co-operation into play. It is inapplicable to the horse, and, more particularly, to the thoroughbred horse. Then, too, there is the serious drawback that the behaviour of only a few muscles can be studied, whereas what is really required is a record of the amount of work performed in a given time in executing a series of co-ordinated and strenuous

movements such as are brought to play in galloping at racing pace. Here we are not concerned with the staying power of an individual muscle, but with that of almost the whole of the striated skeletal group, and the heart included.

The Severity of the Racecourse Test as a Selective Agent.

By the racecourse test, it is true, we are not able to measure in foot pounds per second the amount of work done by the muscles of a racehorse before fatigue (*i.e.*, muscle asphyxiation) commences; but the graduated series of distances over which races are run in Great Britain and Ireland provides a test which, though perhaps not entirely free from error, is, nevertheless, a reliable standard by which an individual horse's racing stamina can be assessed.

In order to appreciate fully the part played by the racecourse as a selective agent for stamina, it is necessary to draw attention to the rules of the Jockey Club bearing on the distances to be run and the apportionment of stake money for these distances. These rules provide :—

(1) That no horse or mare of three years old and upward shall run over a less distance than five furlongs.

(2) There shall not be more than two races per diem of less distance than seven furlongs, only one of which may be of less distance than six furlongs.

(3) There shall be two races per diem of a minimum aggregate distance of $2\frac{1}{2}$ miles, differing at least one furlong in distance.

(4) At every meeting one half at least of the total amount guaranteed for prizes must be apportioned to races of a mile or over for three year olds and upwards, and of this not less than a moiety shall be for races of $1\frac{1}{4}$ miles and upwards.

It will be observed there is every inducement from a monetary point of view to run a horse in a race of $1\frac{1}{4}$ miles or upwards. The effect of the distance rule is forcibly illustrated by comparing the present distribution of races over the different distances with the distribution prior to the introduction of the above rules in 1899. To facilitate comparison, the figures have been given in the form of percentages in Table I.

It will be observed that for some years prior to 1899 the opportunities of running a horse over a greater distance than a mile were few. Selective agencies did not favour the horse which could stay $1\frac{1}{2}$ miles or over. Indeed his opportunities were so few that there is considerable difficulty in picking him out from his fellows who were non-stayers. For this reason the records for the decade prior to 1899 are of little value for our purpose, and excepting in the case of the produce of a few notable stallions they have not been taken into account.

Yet another factor which seriously discounts the value of the early data is found in the consideration

TABLE I.

Distances of Races in Furlongs.

Year.	5	6	7	8	9	10	11	12	13	14 &15	16 over 16		Percentage of races run over the different distances.
1898 (1,230 races run)	29·5	9·8	2·1	41·2	2·4	4·3	1·6	4·8	0·5	1·1	2·0	0·7	
	41·4 per cent.			49·5 per cent.				9·1 per cent.					
1900 (1,221 races run)	18·4	14·4	5·6	25·7	3·0	8·7	5·3	14·2	1·4	1·0	1·6	0·7	
	38·4 per cent.			42·7 per cent.				18·9 per cent.					
1909 (1,260 races run)	17·5	13·5	6·0	24·5	1·7	11·2	4·7	14·8	1·6	1·3	2·0	1·2	
	37·0 per cent.			42·1 per cent.				20·9 per cent.					

that, prior to the year 1899, races exceeding six or seven furlongs in length were rarely run at a true pace; consequently they were not infrequently won by horses running beyond their proper distance. In the early part of the contest the competitors were not called upon to do serious work. But the advent of Sloan and other American jockeys revolutionised the methods of race riding in this country, and from 1899 races have been run at a true pace. The crouching position of the latter-day jockey is partly responsible for the difference in times shown in Table II.; but, as practically all horses have been ridden alike during the last ten years, it is unnecessary to refer further to the jockey's seat in relation to bio-mechanics.

The Essential Qualities of a "Stayer."

It has frequently been asserted that the physiological properties of muscle are largely, though not wholly, acquired characters, being dependent on nurture and use. Whilst this contention in a qualified sense is true, it does not apply to the subject under discussion. Racehorses are skilfully trained, and no stone is left unturned to bring them to their best. Environmental conditions are, in almost all cases, equally favourable; but nevertheless, it has been demonstrated times without number that a horse has his course, and, notwithstanding the most careful management and scientific training, he cannot stay beyond it. Training and racing merely bring out the full potentiality of inherited characters; but whether a horse is able to win over a long or short

TABLE II.

RACE.	Distance.	Prior to Introduction of American seat.		After Introduction of American seat.		Difference.
		Years Taken.	Mean of Time.	Years Taken.	Mean of Time.	
			m. s.		m. s.	sec.
Batthyany Stakes (Lincoln Spring).	5 fur.	1886-1892 inclusive 1896-1899 inclusive	1 5·75	1901-1909 inclusive	1 3·84	1·91
Middle Park Plate	6 fur.	1887-1897 inclusive	1 18·58	1900-1910 inclusive	1 15·80	2·78
Stewards' Cup, Goodwood	6 fur.	1887-1891 inc., 1893, 1897-1899 inclusive	1 16·82	1900-1902 inclusive, 1904-1908 inc., 1910	1 14·51	2·31
Crawfurd Handicap (Newmarket).	6 fur.....	1886, 1889, 1891-1894 inc., 1896-1899 inc.	1 18·48	1900-1909 inclusive	1 16·08	2·40
Dewhurst Plate	7 fur.....	1886, 1889, 1893, 1894, 1896, 1897, 1898	1 33·23	1901-1907 inclusive	1 30·51	2·72
Two Thousand Guineas ..	1 m. 17 yds.....	1889-1899 inclusive	1 45·82	1900-1910 inclusive	1 41·32	4·50
One Thousand Guineas ..	1 m. 17 yds.....	1889-1899 inclusive	1 47·45	1900-1910 inclusive	1 42·78	4·67
Eclipse Stakes	about 1 m. 2 f. ..	1888, 1889, 1891-1899 inc.	2 13·33	1900-1910 inclusive	2 10·8	2·53
City & Suburban Handicap	1 m. 2 fur.	1889-1899 inclusive	2 13·73	1900-1910 inclusive	2 8·93	4·80
Liverpool Spring Cup	1 m. 3 fur.	1889-1893 inc., 1895, 1897-1899 inclusive.	2 30·07	1900-1908 inclusive	2 27·07	3·00
Hardwicke Stakes	1 m. 4 fur.	1886-1891 inc., 1895-1897 inclusive	2 46·62	1900, 1902, 1903, 1905-1910 inc.	2 38·52	8·10
Derby	1 m. 4 fur.	1889-1899 inclusive	2 45·55	1905-1910 inclusive	2 40·98	4·57
Oaks	1 m. 4 fur.	1889-1899 inclusive	2 46·15	1900-1910 inclusive	2 42·27	3·88
St. Leger	1 m. 6 f. 132 yds...	1889-1899 inclusive	3 16·27	1900-1910 inclusive	3 7·18	9·09
Ascot Stakes	2 miles	1889-1899 inclusive	3 42·76	1900-1910 inclusive	3 30·00	12·76
Ascot Gold Vase	2 miles	1889, 1891, 1892, 1894-1898 inclusive	3 46·37	1900-1910 inclusive	3 34·47	11·90
Cesarewitch Stakes	2 m. 2 fur.	1888-1893 inc., 1895-1899 inclusive	4 3·89	1900-1910 inclusive	3 57·22	6·67
Gt. Metropolitan Handicap	2 m. 2 fur.	1889-1899 inclusive	4 18·31	1900-1910 inclusive	4 10·75	7·56
Chester Cup*.....	about 2 m. 2 f.	1889-1899 inclusive	4 6·94	1900-1910 inclusive	4 5·22	1·72
Ascot Cup	2 m. 4 fur.	1889-1899 inclusive	4 40·82	1900-1910 inclusive	4 31·00	9·82
Goodwood Cup	2 m. 5 f. 83 yds...	1888-1894 inc, 1897-1899 inc	5 12·24	1900-1909 inclusive	4 59·82	12·42
Alexandra Plate	2 m. 6 f. 85 yds...	1886, 1887, 1889, 1890, 1891, 1894, 1895, 1898.	5 34·08	1900, 1902-1908 inc.	5 21·05	13·03
Lincolnshire Handicap ..	1 mile	1889-1899 inclusive	1 45·27	1900-1910 inclusive	1 46·38	1·11

*The time difference in the case of the Chester Cup is small, but this is explained by the circumstance that in 1900 the distance was changed from slightly under 2 miles 2 furlongs to 2 miles 2 furlongs 154 yards. Now in the later period the rate of progression is 13·12 secs. per furlong, and 154 yards at this rate gives 9·18 secs.; the difference for the Chester race is, therefore, in reality 10·90 sec., and not 1·72 sec.

course is determined by the bodily qualities brought in by the gametic factors of his parents. In other words, it is determined by the germ plasm. It is a congenital matter.

In Table III. all two year old running is ignored, as horses of that age are rarely asked to race over a greater distance than five furlongs, and further at this period of their existence the tissues, and especially the dark red muscle fibres, are immature. If a horse does not run after two years it is difficult to arrive at his somatic composition as regards muscle fibre. The same difficulty arises in connection with those individuals which run after that age but do not succeed in winning a race on the flat over any distance. Not infrequently the interactions of nerve force and muscle fibre lead to some strange and unexpected results. A horse may have all the physical and physiological properties of a "stayer," but be too slow for racing, owing to his motor-nerve cells discharging with insufficient force or rapidity. On the contrary, he may possess nerve cells capable of discharging in a superlative degree, but carry muscle fibres which become asphyxiated before he has run, at racing pace, the minimum distance of five furlongs. To include these two classes would introduce a large element of personal opinion, therefore they have been omitted from the produce groups of the various stallions. In regard to horses and geldings the slow staying group is undoubtedly the more numerous, but in mares the reverse condition obtains. Thus though the sexes are produced in approximately

equal numbers, and the number of fillies which win at two years of age slightly exceeds the number of colts, at three years of age and upwards horses and geldings which are successful outnumber the mares by nearly two to one. The fillies are, in fact, quite as if not more speedy than the colts, but some factor or factors correlated with sex inhibit in certain, but not in all, cases the physiological properties of a mare's dark red muscle fibre. It is a matter of common knowledge that some fillies stay even better at two years than later in life.

The Types of Racehorses.

In Table III. (page 81) the sprinters, symbolically indicated by PP, represent a group of horses unable to run at true racing pace more than seven furlongs; intermediates, indicated by PR, a second group which stayed from 8 to 11 furlongs ; and stayers, indicated by RR, a third group which won at twelve furlongs and upwards. This classification is to a certain extent arbitrary, and possible sources of error lie in the border lines between sprinters and intermediates, and intermediates and stayers. It is probable that the limit of the sprinters is fixed a little too high, and that winners at seven furlongs on straight and rather severe courses, such as Newmarket, Ascot, Newbury, and Gosforth Park, are really intermediates. Then it may be open to question whether twelve furlongs provides a sufficiently severe test to distinguish in all cases between horses of PR and RR composition. The Epsom twelve furlongs most certainly does not ;

hence *Ayrshire*, *Ladas*, and some other Derby winners cannot be classified as stayers. In the table the probable gametic composition of each stallion is affixed, though it will be observed it does not always coincide with his manifest somatic character ; that, of course, is a familiar Mendelian phenomenon.

Let us assume for the moment that the dark red fibres, symbolised as R, and the pale red fibres, as P, are represented in the germplasm by alternative unit characters, and that neither factor in the body cells is dominant. In these circumstances, granted that sharp and clearly-defined segregation, both in the sex and body cells, took place, we may conceive theoretically that three possible somatic combinations of the pale and red fibres would occur. They are shown in the following table :—

SPRINTERS = PP, *i.e.*, theoretically all their muscles are of the pale variety.

INTERMEDIATE = PR, *i.e.*, theoretically their muscles are a mixture of the red and pale varieties.

STAYERS = RR, *i.e.*, theoretically their muscles are assumed to be of the red variety.

Nature of Mating. Parents.	Result of Cross. Offspring.
PP × PP =	PP only.
PP × PR =	PP and PR in equal numbers.
PP × RR =	PR only.
RR × RR =	RR only.
RR × PR =	RR and PR in equal numbers.
RR × PP =	PR only.
PR × PR =	PP, PR, and RR in the ratio of 1 : 2 : 1.

It will be apparent that here, as in other Mendelian characters, it is possible considerable variations in the relative proportions of the two kinds of fibre may exist.

The variation in the amount of black pigment on the limbs of bay horses may offer an analogy comparable with the variation of the specific properties of the dark fibre. In certain bays black pigment is absent on the limbs, in others it is scanty, whilst a third variety shows intense pigmentation. All, however, behave in the same manner in relation to the recessive character, chestnut, which, in itself, has a wide range of tint. Individual horses may be arranged in such a manner as to form a graduated series, commencing with very light yellow chestnut with flaxen mane and tail, and yellow almost albinistic limbs (reference is not made to white stockings, which are truly albinistic) and ending in dark bay with intensely melanistic mane, tail, and limbs. At times it is difficult to decide whether a horse is a rufous bay or a chestnut, and still more difficult to distinguish between a dark chocolate chestnut and a black. It will be readily understood that in the characters of muscle fibre, of which the heterozygous form is evidently a mosaic* of both characters, it is not possible to fix a hard and fast distinguishing line between the muscles of the three types of racehorses, namely, sprinters, intermediates, and stayers.†

*Mr. Robertson tells me that in the horse, unlike the rabbit, the red fibres are not found in one muscle and the pale in another, but that they both occur in varying proportions in the same muscle.—[Editor.]

† See footnote, page 43.

Sex Limitation in Staying Powers: Horses v. Mares.

As already stated, the sexes are foaled approximately in equal numbers. There is also but little difference between the number of colts and fillies which win at two years of age. Thus the winning two-year-olds sired by the stallions set forth in the table are made up of 980 colts and 1,050 fillies. But in the whole of the winning stock of three years and upwards by the same stallions the sexes are far from being equally represented.

It may be urged that the falling off in the number of mares which win after two years of age is the direct result of a corresponding decrease in the number of mares which start at three years old and upwards. To argue thus, however, would merely be to confuse cause and effect; for it must be remembered that the trial ground, as well as the racecourse, is selective. Owners, quite naturally, do not desire to pay training expenses, entrance and jockeys' fees for mares or fillies which, through lack of stamina, are unlikely to win races. Were the number of races won by each individual the subject of inquiry, the disparity in the numbers of horses and mares which start after two years might have to be taken into account. But this is beside the point, for in the foregoing stamina tables a winner of one race after two years has the same numerical representation as a winner of eight or a dozen races. Actually it is found that two-year-old colts and fillies start in equal numbers, but that after two years of age the horses which start out-

number the mares by two to one. The following figures, taken for the last ten years, show the ratios of horses to mares for the different ages, the mares at each age being represented by unity :—

2 yrs.	3 yrs.	4 yrs.	5 yrs.	6 yrs.	7 yrs. and upwards
1	1·6	1·9	2·4	3·4	5

In the following Table IV. is shown the proportion of winning colts to fillies from three years upwards of the progeny derived from the stallions numbered one to sixty in Table III.

TABLE IV.

TABLE SHOWING THE PROPORTION OF WINNING COLTS TO FILLIES, FROM THREE YEARS UPWARDS, ALL FROM THE STALLIONS SHOWN IN TABLE III.

	Colts.	Fillies.	Ratio {Colts / Fillies
From RR stallions* ..	540	270	2 : 1
,, PR ,, ...	1,065	557	1·91 : 1
,, PP ,, ...	95	78	1·22 : 1
From all stallions.....	1,700	905	1·88 : 1

* *St. Simon's* winners prior to 1899 are not included in the calculation.

The above figures show that factors are at work which place the mares at a disadvantage as racehorses after two years of age. One of these factors, manifestly, is a sex limitation, *in certain mares*, of the specific functions of the staying muscle fibre.

A further proof of sex limitation for this character is obtained by taking out, from the produce Table III. of the specified sires, the ratios of horses to

mares which have won at the different distances. Thus :—

At 5, 6, and 7 furlongs the ratio of horses to mares is	394 : 318, that is 1·24 : 1
At 8, 9, 10, and 11 furlongs the ratio of horses to mares is	702 : 366, that is 1·92 : 1
At 12 furlongs and upwards the ratio of horses to mares is ..	604 : 221, that is 2·73 : 1

These results include races open to mares only. In this category are the One Thousand Guineas, the Coronation Stakes, and the Irish Oaks (all eight furlongs), the Yorkshire Oaks and the Atalanta Cup (ten furlongs), the Epsom Oaks and the Nassau Stakes (twelve furlongs), the Park Hill Stakes and the Newmarket Oaks (fourteen furlongs). It is highly significant that the subsequent running of many mares which have won one or more of the above races has shown them to be incapable of staying their accredited distances. If races open to mares only be excluded from calculation, though no appreciable alteration is produced in the proportions of sprinters and intermediates, the disparity at twelve furlongs and upwards is still more marked than as given, viz., 3 horses : 1 mare instead of 2·73 : 1. Here it is convenient to mention that in all weight-for-age races mares receive an allowance of 3lbs.

The foregoing data refer solely to the offspring of the sires enumerated in Table III. I will now give a few figures dealing with a wider field. The annexed Table V. has been obtained by extracting the maxi-

Table V. Showing

The Relative Staying Powers of Mares and Horses.

	Distances in Furlongs.									For All Distances.
	5	6	7	8	9 and 10	11	12	13-15	16 & above	
Numbers of Winning Horses at the Respective Distances ..	187	214	86	415	234	84	370	109	147	1,846
Numbers of Winning Mares at the Respective Distances ..	193	114	43	176	86	30	118	33	43	836
Ratio of Winning Horses to Winning Mares	1 : 1·03	1·88 : 1	2 : 1	2·36 : 1	2·72 : 1	2·8 : 1	3·14 : 1	3·3 : 1	3·42 : 1	2·2 : 1

mum winning distance of every horse and mare over two years of age which has been successful under the Rules of Flat Racing in the years 1906–1910 inclusive, Irish races of less value than 90 sovs. being ignored.

For one hundred and thirty-nine winners of the most important and highest class long distance races of fourteen furlongs or over, 1900–1911 inclusive, the ratio of horses to mares is 112 : 27, that is 4·15 : 1.

It would appear that the inhibition of the staying factor is due to the presence in *certain* mares of a unit character. The exact nature of this character can only be guessed at, but it is permissible to surmise that it is an enzyme connected with an ovarian secretion. Evidence goes to show that a mare possessing the inhibiting character may transmit it to both her sons and daughters, and that it is dominant in mares, and, necessarily, recessive in horses. In the absence of this character mares carrying one unit of R and one unit of P will be intermediates, and those carrying two units of R will be stayers. Conversely, the presence of the inhibitory factor, I, would reduce an intermediate mare carrying R and P to a sprinter, the R factor being inhibited in her soma. But, nevertheless, a mare of this composition would give off gametes containing R and P alternatively and approximately in equal numbers. The effect of the presence of inhibition and two units of R is not quite clear. Theoretically the inhibitory factor should have the same effect on the muscular tissues of mares of RR as of PR gametic composition. But the facts do not

altogether show that is the case, and doubtless some further factor is concerned.

Do Stallions Transmit the Female Inhibitory Factor ?

The interesting question now arises, do mares transmit the inhibitory factor to their sons, and do the gametes of the staying and intermediate stallions pass it on to some of their daughters ?

In Table III. it will be noted that the daughters of some horses, in regard to stamina, fall far below the standard of the same stallion's sons. *Martagon* is a marked case. *Count Schomberg*, *Marco*, *Collar*, and *Wolf's Crag* are others. The dams of these horses were all non-stayers. An analysis of the produce of these mares may help to a solution :—

TABLE VI.

Offspring of the Dams of the Five Stallions.

Dams of the Five Stallions.*	Sprinters.		Intermediates.		Stayers		Produce which Cannot be Classified.	
	Colts.	Fillies.	Colts.	Fillies.	Colts.	Fillies.	Colts.	Fillies.
Tiger Lily (*Pri*) ..	3	1	—	—	2	1	—	—
Clonavarn (*Pri*) ..	1	2	2	1	3	1	—	—
Novitiate (*Pri*) ..	1	4	2	—	1	—	2	—
Ornament (*Pri*) ..	—	5	2	—	3	1	—	—
Lucy Ashton (*Pp.*)	3	5	4	—	—	—	2	—
Total	8	17	10	1	9	3	4	—

Total fillies = 21. Total colts = 31.

*To which stallions these five dams were respectively mated may be seen in Table III., by looking down the first column for the stallion's name.

It is at once apparent that *Tiger Lily*, *Clonavarn*, *Novitiate*, and *Ornament* are not of the same composition as *Lucy Ashton*, although all were sprinters. Out of fourteen matings *Lucy Ashton* did not breed a stayer of either sex, though mated nine times with horses of PR and five times with horses of RR composition; therefore it is clear that she was a mare of gametic composition PP. The other four mares all bred stayers, and, manifestly, were of gametic composition PR; but since they were sprinters, the R was inhibited in their soma by the sex limiting character, and we may therefore symbolise them as PRI. *Lucy Ashton* may also have carried I, but there is no evidence to show that this was the case. We cannot conclude, therefore, that she transmitted it to her son *Wolf's Crag*. But it is permissible to surmise that the stallions *Martagon*, *Count Schomberg*, *Marco*, and *Collar* all derived this inhibiting factor from their dams, carried it as a recessive, and, in due course, passed it on to certain of their daughters, in which it was again manifested.

Sex limitation at once explains the paradox that many of our best racehorses and most noted stayers have been out of mares which were almost useless on a racecourse after two years of age, principally, if not solely, owing to lack of stamina. Some were absolutely useless. Here are a few examples which may be of interest. They are by no means exhaustive. If space permitted numerous additions could be made. See Table VII.

TABLE VII.—Showing the Origin of Good Stayers from Dams having Indifferent Racing Records.

Winner.	Max. dist. in fur. of winner.	Sire.	Max. dist. in fur. of sire.	Dam.	Performances of Dam.
St. Simon	21	*Galopin*	12	*St. Angela*	Could barely stay 5 furlongs.
Isinglass	20	*Isonomy*	21	*Deadlock*	Never ran.
Carbine	20	*Musket*	24	*The Mersey*	Never ran.
Spearmint	15	*Carbine*	20	*Maid of the Mint*	Never ran.
William the Third	22	*St. Simon*	21	*Gravity*	Unplaced at 2 years. Did not run afterwards.
Willonyx	20	*William the Third*	22	*Tribonyx*	Unplaced. Did not run after 2 years.
Love Wisely	20	*Wisdom*	8	*Lovelorn*	Never ran.
Amadis	18	*Love Wisely*	20	*Galeta*	Unplaced.
Santoi	20	*Queen's Birthday*	16	*Merry Wife*	Unable to win a 5 furlong selling race.
Radium	21	*Bend Or*	12	*Taia*	Unable to win a 5 furlong plate.
Cyllene	20	*Bona Vista*	8	*Arcadia*	Could not stay more than 5 furlongs.
Bronzino	17	*Marco*	12	*Flitters*	Could not stay more than 5 furlongs.
Doricles	15	*Florizel* II.	21	*Rosalie*	Could barely stay 6 furlongs.
Velocity	16	*Speed*	8	*Ballast*	Never ran.
King's Messenger	18	*King Monmouth*	8	*Swiftsure*	Never ran.
Bomba	20	*Carbine*	20	*St. Neophyte*	Never ran.

TABLE VII.—*Continued.*

Arizona	22	*Omnium II.*	16	*Attractive*	Never ran.
Ravensbury	22	*Isonomy*	21	*Penitent*	Unplaced. Did not run after 2 years.
Sandboy	18	*Ravensbury*	22	*Sandblast*	Placed as a 2 year old. Did not run after.
Royal Realm	22	*Persimmon*	20	*Sandblast*	Placed as a 2 year old. Did not run after.
Troutbeck	16	*Ladas*	12	*Rydal Mount*	Unplaced. Did not run after 2 years.
Challacombe	15	*St. Serf*	10	*Lady Chancellor*	Never ran.
Pillo	16	*Raeburn*	8	*Golden Horn*	Never ran.
Count Schomberg	21	*Aughrim*	*	*Clonavarn*	Unplaced at 5 furlongs.
Up Guards	18	*Aughrim*	*	*Clonavarn*	Unplaced at 5 furlongs.
Golden Measure	16	*Florizel II.*	21	*Fairy Gold*	Won at 6 furlongs at 3 years.
Pure Gem	22	*Isinglass*	20	*La Vierge*	Unplaced. Did not run after 2 years.
Magic	21	*Martagon*	21	*Sesame*	Never ran.
Rabelais	21	*St. Simon*	21	*Satirical*	Won at 5 and 6 furlongs.
Perseus	21	*St. Simon*	21	*Andromeda*	Unplaced. Did not run after 2 years.
Fortunatus	21	*St. Frusquin*	10	*Lucky Lady*	Never ran.
Beppo.	14	*Marco*	12	*Pitti*	Never ran.
Verney	18	*Veronese*	13	*Repletion*	Never won. Started once.
St. Amant	14	*St. Frusquin*	10	*Lady Loverule*	Never ran.
Soliman	18	*St. Simon*	21	*Alibech*	Did not run after 2 years.
Yentoi	18	*Santoi*	20	*Rot*	Ran up to 5 years. Could not stay more than 5 furlongs.
Ard Patrick	13	*St. Florian*	8	*Morganette*	Never won. Started once.
Galtee More	15	*Kendal*	†	*Morganette*	Never won. Started once.

* Never won. † Did not run after 2 years.

Offspring of RR Sires Mated to Random Selection of Mares.

When we consider the nature of the expected offspring from RR sires mated with random samples of mares, several interesting points present themselves for consideration. We should, for instance, expect the offspring to consist of stayers and intermediates, but no sprinters. There are, however, one hundred and four of the last group. The stallions numbered 1–17 in Table III. may be taken as RR composition. *Diamond Jubilee* and *Bay Ronald* are possible exceptions, because their produce are rather too few to enable us to form a reliable estimate. The offspring of these seventeen stallions is as follows :—

	Sprinters.	Intermediates.	Stayers.
Colts	61	195	284
Fillies	43	115	112
	104	310	396

Dealing now with the appearance of the one hundred and four unexpected sprinters, we have no difficulty, so far as the 43 fillies are concerned. Three of them won at seven furlongs, the arbitrary upper limit which is fixed for sprinters, and were placed at a mile or over in good company. Their proper position virtually is in the intermediate group. In regard to the remaining forty, it is highly probable, from the evidence I have already submitted, that all mares sired by RR horses, which do not themselves manifest the R factor, either as intermediates or as stayers, carry a factor for inhibition ; that is to say, these

mares derived an R factor from their sires which their sex inhibited, and a P factor from their dams, which is manifested as the somatic character. They are therefore really PR in composition, as we expect, but the R factor is inhibited by the sex inhibiting factor, and they therefore behave as though they were PP in somatic composition and were sprinters. These forty fillies, however, do not comprise the whole of the sex limited offspring of their sires. The balance is to be found in those mares which, through failure to stay the minimum distance of five furlongs, did not win after two years of age. Many were unable to win at two, and others never appeared in public.

In regard to the appearance of the sixty-one sprinting colts, certain circumstances call for consideration. Twenty-five were unlucky not to win at eight furlongs or over,* six at least were roarers, one broke blood vessels, and another, *Longy*, by *Trenton* out of *Saintly*, became extremely obstinate and mulish after two years of age. The race he won at six furlongs was in the nature of a fluke, and much against his will. It was not so much stamina that he lacked as a willing disposition. Thus, taking into account the five hundred and forty matings which produced colts, there are twenty-eight apparent exceptions to the rule that RR $\times$ PR and RR $\times$ PP should give RR and PR solely. Having regard to the fact that the disposition of the skeletal levers

* Of these twenty-five, fourteen won at seven furlongs, and were placed at longer distances, and eleven won at six furlongs, and were placed at eight furlongs, in good company. Some of the twenty-five were merely beaten by short heads over a greater distance than seven furlongs.

plays a part—though in the thoroughbred only a minor one—in relation to the rapidity with which muscles tire during work, and allowing for other disturbing factors, the twenty-eight exceptions are not beyond reasonable expectation. A more exact knowledge of the individual horses would probably reduce these twenty-eight exceptions to a negligible quantity.

The objection may be urged that the winning male stock of a stallion are a selected sample, and that the incidence of non-stayers will be heavier in those of his produce which never run on the flat. This is true in the case of non-staying sires, but the argument has little or no weight in regard to RR stallions. Slow their colts may be, and not infrequently are, but it is rare in the absence of disturbing conditions, such as roaring or blood vessel breaking, to find one which cannot stay five furlongs.

The Mating of Sires and Dams both of Known Performances.

So far, we have only considered the results of mating the seventeen sires of known staying capacity, shown in Table III., with a random sample of mares whose performances were not considered. But now it is necessary to study the results of mating sires of known performances with dams also of known performances.

As already stated, no great reliance can be placed on public form, as regards stamina, prior to 1899. The greater part of these dams were in training prior

to 1899. Many ran in the eighties, and a few as far back as the seventies.

Following the plan adopted in Table III. with the stallions, these dams are divided in Table VIII. into groups of sprinters, intermediates, and stayers. A fourth group comprising those mares which never started is added. The sprinters, in addition to actual winners, include all runners of two years which did not reappear as three-year-olds, and also mares which ran over courses of seven furlongs and under without winning a race.

Excluding from the offspring columns of the table the 43 fillies, the six roarers, the blood-vessel breaker, and *Longy*, and adding to the intermediates the twenty-five colts which won at seven furlongs or were placed at one mile or over, we obtain the following results.

TABLE VIII.

	MARES.	Winning offspring of three years old and upwards sired by the 17 staying sires from the different types of mares.		
		Sprinters.	Intermediates	Stayers.
17 staying Sires RR ×	Sprinting Mares, PP + (PR + I)	17	152	156
	Intermediate Mares, PR.....	1	65	72
	Staying Mares, RR	1	14	54
	Mares which never ran	9	104	114

Assuming all the sprinting mares to be PP in composition, the calculated result for RR sires mated to PP mares is, of course, 325 PR. The appearance of the 156 staying individuals in the offspring clearly demonstrates that a very large number of these

brood mares were really PR in gametic composition, but carried a factor which inhibited R in their own soma. For none of these mares could run more than seven furlongs, yet they produced 156 stayers.

The mating of the RR sires with the PR mares comes close to expectation, that is PR (intermediates) and RR (stayers) in equal numbers. The solitary sprinter from this mating is the colt *Leisure Hour*, by *St. Simon* out of *Love in Idleness*, a mare who with difficulty could get a mile.

In the group RR sires mated to RR mares, which should give RR only, the intrusion of a sprinter needs a little consideration. The colt in question is *Carburton*, by *Carbine* out of *Mrs. Butterwick*. This mare won the Oaks, but, like many other winners of that race, was not really a stayer. Still she must have carried one R factor, else she would not have bred *Phaleron* to *Gallinule* and *Wombwell* to *Isinglass*, both of which won races of twelve furlongs or over. Why, then, did *Carburton* so belie his ancestry ? A reference to Vol. XIX. of *The General Stud Book* gives the answer. *Carburton* was *Mrs. Butterwick's* first living foal. She produced a dead filly to *Morion* in 1895, was barren in 1896, 1897, and 1898, and *Carburton* was foaled in 1899. Here, unmistakably, is an instance of some defective quality in reproduction, or some segregable factor in the germinal plasm leading to weak viability in the offspring. Her next foal, *Greatorex* (1900), also by *Carbine*, was a great improvement on *Carburton*. As a two-year-old he could stay seven furlongs in the best class, but, being

sold for shipment to South Africa, did not run in this country as a three-year-old.

But how are we to account for the fourteen intermediates from RR sires mated to RR mares ? For the purpose of more fully considering the question, we may set them out in detail as in Table IX. :—

TABLE IX.

The Fourteen Intermediates from presumably staying dams × staying sires.	Remarks on dam's performances.
SANDBAG (c),* 8 furlongs, by *Carbine* out of *Sanderling.*	Won one falsely-run race of 2 miles, but could not really stay more than 10 furlongs. Gametic composition PR.
MOUSQUETON (c), 8 furlongs, by *Carbine* out of *Musa.*	Winner of the Oaks, 12 furlongs. Not a stayer. Gametic composition PR.
FRIOSSART (c), 8 furlongs, by *St. Frusquin* out of *Musa.*	Winner of the Oaks, 12 furlongs. Not a stayer. Gametic composition PR.
LARKSPUR (f), 8 furlongs, by *St. Frusquin* out of *Japonica.*	Won at 12 furlongs on an easy course. Gametic composition PR.
PILGRIM'S WAY (f), 8 furlongs, by *St. Frusquin* out of *Canterbury Pilgrim.*	A stayer = RR.
CHAUCER (c), 11 furlongs, by *St. Simon* out of *Canterbury Pilgrim.*	A stayer = RR.
STYMIE (c), 10 furlongs, by *St. Frusquin* out of *Mimi.*	Winner of the Oaks, 12 furlongs. Gametic composition probably RR.
SIMON'S BATH (c), 10 furlongs, by *St. Simon* out of *Mimi.*	Winner of the Oaks, 12 furlongs. Gametic composition probably RR.

* C = colt; f = filly.

The Fourteen Intermediates from presumably staying dams × staying sires.	Remarks on dam's performances.
PLANUDES (c), 8 furlongs, by *St. Simon* out of *Lonely*.	Winner of the Oaks. Not a stayer. Gametic composition PR.
CORNFIELD (f), 11 furlongs, by *Isinglass* out of *Landrail*.	A doubtful stayer = PR.
SANCTUARY (c), 8 furlongs, by *Isinglass* out of *Stagsden*.	Won one race of 14 furlongs at Alexandra Park. Only other performance sixth in a 5 furlong race at Birmingham. *Stagsden* did not stay. Gametic composition PR.
LUCKY JAP (c), 8 furlongs, by *Santoi* out of *Bel Item*.	Won a small race of 12 furlongs in Ireland. Not a stayer = PR.
WISE DUCHESS (f), 11 furlongs, by *Love Wisely* out of *Merry Duchess*.	A stayer. Won at 14 furlongs = RR.
PADDINGTON (c), 10 furlongs, by *Martagon* out of *Padua*. (A roarer and tubed.)	Won at 12 furlongs.

It will be gathered that seven of the above cases were not actually from mares of RR composition, and *Paddington*, being a roarer, may also be put on one side. *Chaucer's* actual position is a little doubtful. In addition to winning the Liverpool Cup (eleven furlongs) twice, he ran a moderate fourth in the Great Yorkshire Handicap, nearly 15 furlongs. *Simon's Bath*, by *St. Simon* out of *Mimi*, was an incorrigible rogue ; no reliance whatever can be placed on his performances. *Cornfield* and *Wise Duchess* are fillies, and could stay eleven furlongs and perhaps

a little more. We have thus eliminated twelve of the exceptions, and only *Pilgrim's Way* and *Stymie* (a bad selling plater), by *St. Frusquin* out of *Mimi*, remain.

One of the standard tests of Mendelian segregation is the behaviour of impure dominants or, in my present terminology, intermediates, when bred *inter se*. As a fair representative sample of intermediates, the following sires have been taken, viz. : *Gallinule* and his half-brother *Pioneer*, *Orvieto* and *Laveno* (own brothers), *Ladas*, *Ayrshire*, *Orme*, *Enthusiast*, *Hackler*, *Raeburn*, *Eager*, *Amphion*, *Despair*, and *Grey Leg*. Here are the results of the matings of these fourteen horses with mares of various compositions as deduced by their performances :—

TABLE X.

	MARES.	Winning offspring of three years old and upwards sired by 14 intermediate sires from the different types of mares.		
		Sprinters.	Intermediates.	Stayers.
14 Intermediate × Sires, PR	Sprinting Mares, PP or (PR + I).	118	109	40
	Intermediate Mares, PR.....	44	88	60
	Staying Mares, RR	3	26	27
	Mares which never ran	75	97	65

The calculated result for PR sires mated with PP mares is sprinters and intermediates in equal numbers. The forty stayers which appear here are obviously the consequence of a percentage of the mares in this mating having an PR composition, but carrying the inhibitory factor I. The presence of the

inhibitory factor would *ex hypothesi* reduce them to sprinters. As, approximately, only half of the PR stallions' gametes carried R, their percentage of staying produce must fall considerably below that of RR stallions in the same mating. The actual result shows that the crossing of the seventeen RR sires with PP and (PR + I) mares gives 48·0 per cent. of stayers (see Table VIII.), whereas the heterozygous PR stallions show 15 per cent. of stayers from the same mating. (See Table X.)

In the next mating, PR sires to PR mares, the result is remarkably close to the typical ratio of 1 : 2 : 1, and affords unmistakable evidence of Mendelian segregation. The expectation is 48 : 96 : 48. The result is 44 : 88 : 60.

The three sprinters from the mating of PR sires with RR mares, just considered, are aberrant. One of these was the gelding *Ashdod*, who, like certain other sons of *Gallinule*, broke blood vessels. Another is *Mailed Fist*, by *Orme* out of *Gantlet*. This mare *Gantlet* won the Park Hill Stakes, Doncaster, nearly fifteen furlongs, but the fillies she beat were, like herself, non-stayers. Next year, as a four-year-old, she ran at five and eight furlongs. She is the dam of *Duke of Westminster*, also by *Orme*, and of *Grey Plume*, by *Grey Leg*, neither of which could stay more than a mile. The third sprinter from PR ×RR is *Excelsior*, by *Eager* out of *Perchance* (by *Persimmon*). *Perchance* won one race only, and is probably an intermediate.

In the last series of matings shown in Table X.,

namely, the mixed sample which never ran, the mares are made up principally of sprinters, PP, and sex limited intermediates, PRI, together with a small percentage of slow intermediates and slow stayers. It is not possible to give any approximate expectation from such a mixed and undetermined parentage. The actual result, as will be seen, is 75 : 97 : 65, which stands out clearly from the ratio of 9 : 104 : 114, which we have seen is the result of crossing the seventeen RR stallions with mares very similar to those mated to the PR sires.

Turning now to the ten sprinting sires at the bottom of Table III., we have a striking contrast to the first seventeen staying sires in the same table, and already considered. The offspring of these ten horses is made up of :—

Sprinters.	Intermediates.	Stayers.
118	50	5

The following table illustrates the actual matings :

TABLE XI.

	MARES.	Winning offspring of three years old and upwards sired by ten sprinting sires from the different types of mares.		
		Sprinters.	Intermediates.	Stayers
10 Sprinting Sires, PP ×	Sprinting Mares, PP or (PR + I)	65	14	1
	Intermediate Mares, PR.....	9	15	—
	Staying Mares, RR	2	4	—
	Mares which never ran	42	17	4

Here the numbers are few, in all only one hundred and seventy-three matings. This, however, is unavoidable, for it is difficult to find a sprinter with even ten winning produce. *Missel Thrush* probably should be included in the above classification, for his percentage of intermediate and stayers is small. He is the first foal of the St. Leger winner, *Throstle*, who so far has been somewhat of a stud failure; but as *Missel Thrush* never ran, and as his brother *Songcraft*, by *Orme*, and his half-brother *Grey Bird*, by *Grey Leg*, could stay a mile, he may be given the benefit of the doubt till his further stud record proves his composition. The sprinter *Sundridge* has been omitted from the list, first because he might have stayed a mile or over, had he been sound in his wind; then, in the second place, his winning produce of three-year-olds and upwards, as yet, only total fourteen, viz.: Seven sprinters, six intermediates, and one stayer—that is if the Derby winner *Sunstar* really is a stayer. In any event, *Sunstar's* dam, *Doris*, is an interesting example of the staying factor being sex limited. *Doris*, like many other good brood mares, was just a five furlong sprinter, but it seems clear that some of her gametes carry R.

Reverting to Table XI., the offspring table of the ten sprinting sires, I would emphasize that the result in the first mating of PP sires with PP mares, allowing for sex limitation of the R factor in some of the mares, is pretty much what one would expect; for, in place of getting the whole eighty offspring as

sprinters, we have sixty-five sprinters, fourteen intermediates, and one pseudo-stayer, to which I shall refer presently.

In the mating of PP sires (sprinters) with PR mares (intermediates) we expect in the offspring, if there are no complications due to inhibiting factors in the mares, equal numbers of sprinters and intermediates. The result, as the Table XI. shows, is to give nine sprinters and fifteen intermediates. This discrepancy in the proportions may be due to the smallness of the numbers or to some of the mares being really RR in composition but carrying an inhibiting factor. Coming next to the mating of sprinting sires PP with staying mares, the number of the offspring is extremely small, and this gives an exaggerated value to the two sprinters from the staying mares *Sacristy* and *Carnatum*. *Sacristy*, although mated repeatedly with *Marco* and other horses of intermediate composition, has never bred anything which could stay. In these circumstances she must really have been an intermediate PR. It is the same with *Carnatum*, for she, too, has never bred a winner at a greater distance than five furlongs.

In dealing with the theoretical considerations arising from the facts, we must remember, as already stated, that the border line between intermediates and stayers is not clearly enough defined. Twelve furlongs does not afford a sufficiently severe test. But, even so, a scrutiny shows that two of the five presumptive stayers in the fourth column of Table XI., produced as the offspring of sprinting sires mated with

different types of mares, were moderate platers whose proper courses were eight to ten furlongs. They ought, therefore, to fall into the intermediate class. The other three were *Trevor*, by *Juggler*, out of *Chevrette*, *Hulcot*, by *Crafton*, out of *Queen of the Riding* (a five-furlong mare by *Galopin*), and *Lawn Sand*, by *Ugly*, out of *Oberia* (who never ran), by *Oberon*. Eight or nine furlongs truly run was sufficiently far for both *Trevor* and *Hulcot*, and this leaves *Lawn Sand*, by *Ugly*, as an apparent exception to the rule that sprinting sires crossed with mares of any composition do not give staying produce. To view this divergence in its proper perspective it is imperative to bear in mind that *Ugly* sired eighty-nine foals from mares of various extractions, but only one of his produce, *Lawn Sand*, won a race of twelve furlongs or over, and only four have won at eight to eleven furlongs. The offspring of *Hazlehatch* numbered 127; but only five were successful over a greater distance than seven furlongs. *Prince Hampton* sired 95 foals of which only three won at a mile or over. The case of *Americus* is still more glaring. His 65 foals give four winners at eight to eleven furlongs, and none over the latter distance.

Scattered throughout the Racing Calendars there are a fair number of sprinting horses which have only sired one or two winners during the course of their lives. When these horses have been mated with non-staying mares, the offspring have been invariably sprinters. Taking this evidence in conjunction with the results given in Table XI., it seems clear that

sprinters breed true, and this irrespective of weight of ancestry, for I need hardly point out that natural selection by the racecourse test is against the sprinting sire. If the pedigree of a sprinter be examined, it will be found that the names of the males which occur in the second and earlier parental generations are those of horses which could stay at least a mile, and usually a greater distance.

The winning stock of these sprinting sires can go fast. Their nerve axons and muscle fibres conduct impulses readily. Co-ordination, which involves the alternate contraction and relaxation of voluntary muscles, and even parts of the same muscle, is as perfect as in the stayer. They have good bone, well-proportioned frames, and the capacity for making acquirements, desirable and undesirable, is not wanting. One essential feature, however, they lack, and can never acquire. They have not the faculty for storing up an excess of oxygen in their muscle fibres. A signal proof of this lies in the fact that inhalation of oxygen has very little effect in increasing a non-stayer's distance, but it has a decided effect in that direction if given to an intermediate, and still more in the case of a stayer. It very frequently happens, if an attempt be made to train and race a sprinter over long courses, that he rapidly looses muscle and becomes slow. This would appear to be the practical outcome of his carrying a muscle slightly deficient in proteins and glycogen, and proportionately increased in muscle fats and water.

Some Hypothetical Considerations.

Reviewing all the circumstances then, it may be gathered that the assumption of the dark and pale red muscle fibres being represented in the germplasm of the thoroughbred by two alternative unit characters requires a little qualification. It would be more correct to say that a number of separate entities are with some regularity correlated to form a unit character, for it seems fairly certain that the various properties which mark off the dark fibre are in the nature of additions to the pale fibre. At some time far back in the phylogeny of the horse, those additions no doubt arose through germinal variation, and were fixed by natural selection. But, as the thoroughbred is a comparatively recent creation, having arisen in the 17th and early part of the 18th centuries from crossing the Arab, Turk, Barb, and the native Galloway, some of his ancestors brought in the phylogenetically newer staying character, and some—there are good grounds for the belief that it was the hybrid Turkish horse—the older character. From the inception of the thoroughbred to the present moment there has been persistent segregation of the two types of muscle. Sprinters and stayers have with constant regularity made their appearance when intermediates have been bred together. And this is true whether we take results in the mass or analyse the offspring of specific parents as I have done in Table XII. (page 90). The crossing of stayers and true sprinters has nearly always given intermediates.

That these intermediate forms should at times

have a bias to one or other of the homozygous characters—that is, tend to become sprinters or stayers—may possibly be due to a variability in the number of chromosomes which carry the phylogenetically later developed staying character. This, of course, is a purely speculative hypothesis. Walker has endeavoured to show that older or racial characters are carried by the cytoplasm of the gamete, and the later ones by the chromosomes.* Let us tentatively apply this theory to the two muscle characters in the race-horse. Underlying, as it were, the factors which produce the dark fibre we should have an ever-present factor for the pale fibre carried by the cytoplasm of the gamete, for it is inconceivable that the presence of the pale fibre could be alternative to its absence, as this would entail that a horse might be born without voluntary muscles. If, however, the entities making up the unit character for the dark fibre were carried by the chromosomes, the presence and absence theory fits in remarkably well. The presence in the chromosomes of these entities forming the unit character of red fibre would be alternative to their absence. Therefore, during the reduction of the chromosomes from twenty-six to thirteen, which takes place prior to the maturation of the gamete, the presence factor would enter some chromosomes, but not others. The greater the number of chromosomes carrying the staying factors of the red fibre, the greater may be the intensity of the character manifested in the soma of

* Walker, C. E., "Hereditary Characters and their Mode of Transmission." 1910.

the filial generation. Latency may thus be the result of a minimum number of the chromosomes carrying the factors for the dark fibre. The gametic formula, RR, would therefore represent a horse in which the R factor was present in a maximum number of chromosomes which can carry the character, and the formula PP, in a strict sense, a horse in which R was not present in any chromosome. The dark fibres, however, would appear to be never entirely absent from the tissues of the thoroughbred, but certain individuals carry so few that they are practically no use to them as aids to stamina.

The epistatic and hypostatic theory is thoroughly in accordance with the conception of the recapitulation process. During foetal life and foalhood the two muscle characters—red and pale—are morphologically indistinguishable. At a year old the difference is still imperceptible, and even at two years of age the physiology of the dark fibre is imperfect. It rarely reaches the full potentiality before the bearer is four years of age, and sometimes not till he is six.

In collecting the data for this paper I have received valuable assistance from my son, H. C. Robertson. I have also to thank the Editor of " The Sportsman " for allowing me to reproduce the time-distance table (Table II.) which originally appeared in one of my communications to that paper.

The statistics for this paper were brought up to June 30th, 1911. Whilst the material was in the printer's hands, the extra data for the latter part of 1911 became available, but too late to be incorporated in the tables. I have gone carefully through the extra figures and find that they make no material alteration in the results, except that the increased

numbers approximate the percentages nearer to expectation. Typical stayers like *Carbine*, *Isinglass*, *Florizel II.*, and *Persimmon* show an increase in the staying and intermediate, but none in the sprinting group. In the intermediate sires there are additions to all three groups. The new material in the case of the ten sprinting stallions consists of two sprinters only.

AUTHOR.

TABLE III.

Table showing the maximum distances over which the stock of 60 representative sires have won on the flat in Great Britain and Ireland. Races run in Ireland of less value than 90 sovereigns excluded. The figures refer only to races won by horses of three years of age and upwards, all two-year-old running being ignored. The calculations are taken out to June 30th, 1911, but the produce of 1908 is omitted.

	STALLION.		Max. Winning Distance in Furlongs.	Gametic Composition.		DISTANCES IN FURLONGS. PP.** SPRINTERS 5	6	7	PR. INTERMEDIATES 8	9	10	11	RR. STAYERS. and upwards 12	13	14	15	16	Total Winners.	% PP	% PR	% RR
1.	*TRENTON* ..... (1881)	*Musket*	Ran in Australia.	RR	Colts	—	1	—	3	—	2	—	7	—	—	—	2	15	6·7	33·3	60·0
					Fillies	—	—	—	2	—	—	—	5	—	—	—	2	9	—	22·2	77·8
		Frailty			Total		1			7				16				24	4·2	29·2	66·6
2.	*CARBINE* ..... (1885)	*Musket*	Ran in Australia.	RR	Colts	2	2	—	7	1	1	3	16	2	1	1	6	42	9·5	28·6	61·9
					Fillies	2	—	—	3	—	3	—	5	—	—	—	2	15	13·3	40·0	46·7
		The Mersey			Total		6			18				33				57	10·5	31·6	57·9
3.	*ISINGLASS* ..... (1890)	*Isonomy*	20	RR	Colts	1	2	1	10	—	4	1	16	2	3	—	5	45	8·9	33·3	57·8
					Fillies	2	1	—	4	2	1	2	9	2	2	—	3	28	10·7	32·1	57·2
		Deadlock			Total		7			24				42				73	9·6	32·9	57·5
4.	*RAVENSBURY* .... (1890)	*Isonomy*	22	RR	Colts	1	1	—	1	—	2	—	6	—	1	1	3	16	12·5	18·8	68·7
					Fillies	—	2	1	4	—	—	1	1	1	—	—	—	10	30·0	50·0	20·0
		Penitent			Total		5			8				13				26	19·2	30·8	50·0
5.	*ST. SIMON** ..... (1881)	*Galopin*	21	RR	Colts	1	5	1	7	1	5	4	14	4	2	1	5	50	14·0	34·0	52·0
					Fillies	—	—	1	3	1	5	2	5	—	—	2	2	21	4·8	52·4	42·8
		St. Angela			Total		8			28				35				71	11·3	39·4	49·3

TABLE III.—*Continued.*

STALLION.		Max. Winning Distance in Furlongs.	Gametic Composition.		DISTANCES IN FURLONGS. PP, SPRINTERS 5 6 7	PR, INTERMEDIATES 8 9 10 11	RR, STAYERS, and upwards 12 13 14 15 16	Total Winners.	% PP	% PR	% RR
6. *FLORIZEL II.* .... (1891)	*St. Simon*	21	RR	Colts	— 1 2	9 — 1 1	15 1 2 1 7	40	7·5	27·5	65·0
				Fillies	2 — —	7 2 4 —	3 2 — — 2	22	9·1	59·1	31·8
	Perdita II.			Total	5	24	33	62	8·1	38·7	53·2
7. *PERSIMMON* .... (1893)	*St. Simon*	20	RR	Colts	— 4 2	5 1 4 1	10 1 3 1 8	40	15·0	27·5	57·5
				Fillies	1 — —	3 1 — 3	6 2 3 1 1	21	4·8	33·3	61·9
	Perdita II.			Total	7	18	36	61	11·5	29·5	59·0
8. *DIAMOND JUBILEE* (1897)	*St. Simon*	15	? RR	Colts	— 1 —	5 1 4 1	1 — 1 — 1	15	6·7	73·3	20·0
				Fillies	2 1 —	— — — —	1 — 1 1 2	8	37·5	—	62·5
	Perdita II.			Total	4	11	8	23	17·4	47·8	34·8
9. *CHILDWICK* ..... (1890)	*St. Simon*	18	RR	Colts	— — —	2 — — 1	4 — 1 — 4	12	—	25·0	75·0
				Fillies	— 1 1	6 — 2 1	1 1 1 — —	14	14·3	64·3	21·4
	Plaisanterie			Total	2	12	12	26	7·6	46·2	46·2
10. *ST. SERF* ..... (1887)	*St. Simon*	10	RR	Colts	— 3 1	15 2 7 6	10 5 1 1 3	54	7·4	55·5	37·1
				Fillies	1 — 2	8 — 4 —	5 1 1 2 1	25	12·0	48·0	40·0
	Feronia			Total	7	42	30	79	8·9	53·1	38·0
11. *ST. FRUSQUIN* .. (1893)	*St. Simon*	10 †	RR	Colts	3 3 3	14 3 9 2	14 3 2 — 7	63	14·3	44·5	41·2
				Fillies	— — 1	10 — 5 1	8 1 2 — 1	29	3·4	55·2	41·4
	Isabel			Total	10	44	38	92	10·9	47·8	41·3

*St. Simon sired 66 winners of three years old and upward prior to 1899. These are not included in the Table.

**PP=sprinters unable to run at true racing rate more than seven furlongs. PR=intermediates which "stay" from eight to eleven furlongs. RR="stayers" which won at twelve furlongs and upwards. † Only beaten by a neck in the Derby. Broke down and did not fulfil his long-distance engagements.

TABLE III.—*Continued.*

12. *QUEEN'S BIRTHDAY* (1887)	*Hagioscope*	16	RR	Colts	— 1 2	6 — 1 2	5 1 1 — 1	20	15·0	45·0	40·0
				Fillies	1 — 1	5 1 — 1	3 1 1 — 1	15	13·3	46·7	40·0
	Matilda			Total	5	16	14	35	14·3	45·7	40·0
13. *SANTOI* ..... (1897)	*Queen's Birthday*	20	RR	Colts	— — 1	4 — 1 2	5 — 1 — 6	20	5·0	35·0	60·0
				Fillies	1 2 —	4 — — —	2 — — — —	9	33·3	44·4	22·3
	Merry Wife			Total	4	11	14	29	13·8	37·9	48·3
14. *MARTAGON* ..... (1887)	*Bend Or*	21	RR	Colts	1 3 1	7 1 3 3	15 — 3 1 7	45	11·1	31·1	57·8
				Fillies	6 3 1	3 — — 1	4 — 1 — —	19	52·6	21·1	26·3
	Tiger Lily			Total	15	18	31	64	23·4	28·1	48·5
15. *BAY RONALD* .... (1893)	*Hampton*	12	? RR	Colts	2 2 —	3 — 2 —	5 1 1 — 3	19	21·0	26·3	52·7
				Fillies	— — 1	2 — — 1	— — 1 — 1	6	16·7	50·0	33·3
	Black Duchess			Total	5	8	12	25	20·0	32·0	48·0
16. *LOVE WISELY* .... (1893)	*Wisdom*	20	RR	Colts	1 4 —	5 1 1 —	7 — — 1 3	23	21·8	30·4	47·8
				Fillies	— 1 —	1 2 — 2	2 — — — 1	9	11·1	55·6	33·3
	Lovelorn			Total	6	12	14	32	18·8	37·5	43·7
17. *COUNT SCHOMBERG* (1892)	*Aughrim*	21	RR	Colts	— 2 —	2 4 1 —	8 — — — 4	21	9·5	33·3	57·2
				Fillies	3 1 1	1 — 1 —	2 1 — — —	10	50·0	20·0	30·0
	Clonavarn			Total	7	9	15	31	22·6	29·0	48·4
18. *MARCO* ..... (1892)	*Barcaldine*	12	? RR	Colts	1 3 —	6 1 2 2	10 1 2 1 3	32	12·5	34·4	53·1
				Fillies	3 6 2	4 3 1 —	— 1 — — —	20	55·0	40·0	5·0
	Novitiate			Total	15	19	18	52	28·8	36·6	34·6
19. *CYLLENE* ..... (1895)	*Bona Vista*	20	? RR	Colts	3 3 1	1 1 5 2	9 1 1 — 2	29	24·1	31·1	44·8
				Fillies	3 1 —	5 — 6 2	1 1 1 — —	20	20·0	65·0	15·0
	Arcadia ..			Total	11	22	16	49	22·5	44·8	32·7

TABLE III.—*Continued.*

STALLION.		Max. Winning Distance in Furlongs.	Gametic Composition.		DISTANCES IN FURLONGS. PP. SPRINTERS 5	6	7	PR. INTERMEDIATES 8	9	10	11	RR. STAYERS. and upwards 12	13	14	15	16	Total Winners.	% PP	% PR	% RR
20. *COLLAR* (1895)	*St. Simon*	12	? RR	Colts	—	1	—	3	—	2	—	6	—	2	1	1	16	6·3	31·2	62·5
				Fillies	—	1	4	3	1	2	1	—	—	—	—	—	12	41·7	58·3	—
	Ornament			Total	6			12				10					28	21·4	42·9	35·7
21. *GALLINULE* (1884)	*Isonomy*	10 ?	PR	Colts	4	9	1	17	1	7	3	15	3	2	2	7	71	19·7	39·5	40·8
				Fillies	10	5	4	11	1	4	—	7	2	—	2	8	54	35·2	29·6	35·2
	Moorhen	*		Total	33			44				48					125	26·4	35·2	38·4
22. *DESMOND* (1896)	*St. Simon*	12 ?	PR	Colts	1	1	2	8	—	2	1	8	1	—	—	5	29	13·8	38·0	48·2
				Fillies	4	1	1	5	—	2	2	2	—	—	—	3	20	30·0	45·0	25·0
	L'Abbesse de Jouarre	*		Total	10			20				19					49	20·4	40·8	38·8
23. *LADAS* (1891)	*Hampton*	12	PR	Colts	6	5	—	7	1	5	—	10	—	1	—	2	37	29·8	35·1	35·1
				Fillies	4	1	—	2	—	1	—	2	1	—	—	1	12	41·7	25·0	33·3
	Illuminata			Total	16			16				17					49	32·6	32·6	34·8
24. *DONOVAN* (1886)	*Galopin*	14·6	PR	Colts	3	3	1	17	—	7	1	4	5	—	—	3	44	15·9	56·8	27·3
				Fillies	4	1	—	2	—	2	—	3	—	—	—	—	12	41·6	33·4	25·0
	Mowerina			Total	12			29				15					56	21·4	51·8	26·8
25. *MATCHMAKER* (1892)	*Donovan*	13	PR	Colts	2	3	—	4	—	5	1	10	—	1	—	—	26	19·2	38·5	42·3
				Fillies	1	5	—	3	—	1	1	—	—	—	—	2	13	46·2	38·4	15·4
	Match Girl			Total	11			15				13					39	28·2	38·5	33·3

* Never won after two years old.

TABLE III.—*Continued.*

26. *AYRSHIRE* ...... (1885)	*Hampton*	12	PR	Colts	12	10	2	13	2	5	5	9	2	1	1	5	67	35·8	37·3	26·9
				Fillies	8	4	—	7	3	2	1	5	—	1	—	1	32	37·5	40·6	21·9
	Atalanta			Total	36			38				25					99	36·4	38·4	25·2
27. *KENDAL* .... (1883)	*Bend Or*	*	PR	Colts	5	6	2	21	1	5	4	12	—	—	1	4	61	21·3	50·8	27·9
				Fillies	6	—	1	6	1	2	2	2	—	—	—	6	26	26·9	42·3	30·8
	Windermere			Total	20			42				25					87	23·0	48·3	28·7
28. *ROYAL HAMPTON* (1882)	*Hampton*	10	PR	Colts	8	1	1	24	2	7	2	14	—	—	—	2	61	16·4	57·4	26·2
				Fillies	6	2	—	8	1	2	2	4	—	—	—	—	25	32·0	52·0	16·0
	Princess			Total	18			48				20					86	21·0	55·8	23·2
29. *LAVENO* .... (1892)	*Bend Or*	10	PR	Colts	3	3	—	8	1	3	2	5	—	—	—	2	27	22·1	51·9	26·0
				Fillies	2	1	—	3	—	2	—	3	—	—	—	2	13	23·0	38·5	38·5
	Napoli			Total	9			19				12					40	22·5	47·5	30·0
30. *ORVIETO* .... (1888)	*Bend Or*	16 †	PR	Colts	3	—	1	11	1	2	1	2	1	1	—	1	24	16·7	62·5	20·8
				Fillies	2	2	—	8	—	2	—	6	—	—	—	—	20	20·0	50·0	30·0
	Napoli			Total	8			25				11					44	18·2	56·8	25·0
31. *ORME* .... (1889)	*Ormonde*	10	PR	Colts	6	6	—	16	2	1	—	9	—	—	1	2	43	27·9	44·2	27·9
				Fillies	4	3	1	6	—	2	—	1	1	—	—	—	18	44·4	44·4	11·2
	Angelica			Total	20			27				14					61	32·8	44·3	22·9
32. *WINKFIELD* ...... (1885)	*Barcaldine*	8	PR	Colts	2	1	2	7	2	4	3	5	—	—	—	6	32	15·6	50·0	34·4
				Fillies	1	1	1	2	1	—	—	1	—	1	—	1	9	33·3	33·4	33·3
	Chaplet			Total	8			19				14					41	19·5	46·4	34·1
33. *AVINGTON* ...... (1890)	*Melton*	8	PR	Colts	—	2	—	5	1	4	3	5	—	—	—	2	22	9·1	59·1	31·8
				Fillies	—	2	—	1	1	—	—	4	1	—	—	2	11	18·2	18·2	63·6
	Annette			Total	4			15				14					33	12·1	45·5	42·4

TABLE III.—*Continued.*

STALLION.		Max. Winning Distance in Furlongs.	Gametic Composition.		DISTANCES IN FURLONGS. PP, SPRINTERS 5	6	7	PR, INTERMEDIATES 8	9	10	11	RR, STAYERS. and upwards 12	13	14	15	16	Total Winners.	% PP	% PR	% RR
34. *ENTHUSIAST* .... (1886)	*Sterling*	8	PR	Colts	2	1	1	6	1	4	1	8	1	—	—	3	28	14·4	42·8	42·8
				Fillies	3	2	1	2	1	3	—	2	—	—	—	—	14	42·8	42·8	14·4
	Cherry Duchess			Total	10			18				14					42	23·8	42·8	33·4
35. *HACKLER* ..... (1887)	*Petrarch*	8	PR	Colts	2	1	—	4	2	3	—	5	1	—	—	1	19	15·8	47·4	36·8
				Fillies	5	3	—	4	1	2	—	4	—	—	—	1	20	40·0	35·0	25·0
	Hackness			Total	11			16				12					39	28·2	41·0	30·8
36. *WOLF'S CRAG* .... (1890)	*Barcaldine*	8	PR	Colts	3	4	2	6	—	5	2	5	4	2	—	2	35	25·8	37·1	37·1
				Fillies	4	3	—	8	—	—	—	—	—	—	—	—	15	46·6	53·4	—
	Lucy Ashton			Total	16			21				13					50	32·0	42·0	26·0
37. *RAEBURN* ..... (1890)	*St. Simon*	8	PR	Colts	6	1	—	10	—	4	2	6	—	—	—	3	32	21·9	50·0	28·1
				Fillies	3	1	—	7	—	3	—	1	—	—	1	—	16	25·0	62·5	12·5
	Mowerina			Total	11			26				11					48	22·9	54·2	22·9
38. *RIGHTAWAY* .... (1887)	*Wisdom*	8	PR	Colts	4	4	1	12	1	4	1	7	—	—	—	2	36	25·0	50·0	25·0
				Fillies	2	1	1	4	—	—	1	1	—	—	—	—	10	40·0	50·0	10·0
	Vanish			Total	13			23				10					46	28·3	50·0	21·7
39. *EAGER* ..... (1894)	*Enthusiast*	8	PR	Colts	—	5	3	7	—	3	1	2	1	1	—	1	24	33·3	45·8	20·9
				Fillies	1	3	—	4	2	—	—	1	—	—	1	—	12	33·3	50·0	16·7
	Greeba			Total	12			17				7					36	33·3	47·3	19·4

* Never ran after two years old. † Did not stay more than ten furlongs. ‡ Probably could not stay more than ten furlongs.

TABLE III.—*Continued.*

40. *AMPHION* (1886)	*Rosebery* / *Suicide*	12 ‡	PR	Colts	4 6 2	11 — 5 2	2 1 — — 1	34		35·3	53·0	11·7
				Fillies	6 4 2	6 — 2 1	— — — — 2	23		52·1	39·2	8·7
				Total	**24**	**27**	**6**	**57**		42·1	47·4	10·5
41. *ST. ANGELO* (1889)	*Galopin* / *Agneta*	8	PR	Colts	6 10 —	11 2 4 3	7 — 1 — 1	45		35·6	44·4	20·0
				Fillies	5 6 —	7 — — 2	— — — — —	20		55·0	45·0	—
				Total	**27**	**29**	**9**	**65**		41·5	44·6	13·9
42. *PIONEER* (1886)	*Galopin* / *Moorhen*	8	PR	Colts	7 1 2	3 — 1 1	3 1 — — —	19		52·6	26·3	21·1
				Fillies	6 1 —	3 2 — 1	1 — — — —	14		50·0	42·9	7·1
				Total	**17**	**11**	**5**	**33**		51·5	33·3	15·2
43. *PERIGORD* (1890)	*St. Simon* / *Effie Deans*	*	PR	Colts	3 2 —	7 — — 3	4 2 — — —	21		23·8	47·6	28·6
				Fillies	3 2 —	6 — — 1	— — — — —	12		41·7	58·3	—
				Total	**10**	**17**	**6**	**33**		30·2	51·6	18·2
44. *LESTERLIN* (1892)	*Gallinule* / *Miliora*	8	PR	Colts	2 1 1	8 1 2 —	3 — — — —	18		22·2	61·1	16·7
				Fillies	5 1 —	3 — — —	— — — — 1	10		60·0	30·0	10·0
				Total	**10**	**14**	**4**	**28**		35·8	50·0	14·2
45. *GALLOPING LAD* (1893)	*Galopin* / *Braw Lass*	Never ran.	PR	Colts	5 4 1	5 — — —	7 1 1 — 1	25		40·0	20·0	40·0
				Fillies	6 2 —	1 — — —	1 — — — —	10		80·0	10·0	10·0
				Total	**18**	**6**	**11**	**35**		51·4	17·2	31·4
46. *OCEAN WAVE* (1883)	*See Saw* / *Par Excellence*	†	PR	Colts	2 1 2	4 — 3 1	2 — — — —	15		33·3	53·4	13·3
				Fillies	4 1 —	8 — 1 1	1 — — — 3	19		26·3	52·6	21·1
				Total	**10**	**18**	**6**	**34**		29·4	53·0	17·6
47. *DESPAIR* (1879)	*See Saw* / *Peine de Cœur*	8	PR	Colts	6 4 5	17 — — 3	4 — — — 2	41		36·6	48·8	14·6
				Fillies	9 2 —	4 — — —	— — — — 1	16		68·8	25·0	6·2
				Total	**26**	**24**	**7**	**57**		45·6	42·1	12·3

TABLE III.—*Continued.*

STALLION.		Max. Winning Distance in Furlongs.	Gamelic Composition.		DISTANCES IN FURLONGS. PP, SPRINTERS 5 6 7	PR, INTERMEDIATES. 8 9 10 11	RR, STAYERS, and upwards. 12 13 14 15 16	Total Winners.	% PP	% PR	% RR
48. *TARPORLEY* ..... (1892)	*St. Simon*	†	PR	Colts	7 3 —	7 — 1 2	1 — 1 — —	22	45·5	45·4	9·1
				Fillies	2 2 —	2 — 1 2	2 — — — 2	13	30·8	38·4	30·8
	Ruth			Total	14	15	6	35	40·0	42·8	17·2
49. *MISSEL THRUSH* (1897)	*Orme*	Never ran.	? PR	Colts	3 5 —	1 — 1 1	3 — — — —	14	57·2	21·4	21·4
				Fillies	5 3 1	1 — — —	— — — — —	10	90·0	10·0	—
	Throstle			Total	17	4	3	24	70·8	16·7	12·5
50. *GREY LEG* ..... (1891)	*Pepper and Salt*	10	PR	Colts	— 4 1	7 — 1 2	1 — — — —	16	31·3	62·5	6·2
				Fillies	— 1 1	2 — — —	— 1 — — 1	6	33·3	33·4	33·3
	Quetta			Total	7	12	3	22	31·8	54·6	13·6
51. *JUGGLER* ..... (1885)	*Touchet*	8 ‡	PP	Colts	5 2 —	7 — 2 —	1 — — — —	17	41·2	53·0	5·8
				Fillies	5 5 2	5 — 2 —	— — — — —	19	63·2	36·8	—
	Enchantress			Total	19	16	1	36	52·8	44·4	2·8
52. *CRAFTON* ..... (1882)	*Kisber*	8 ‡	PP	Colts	4 3 3	3 1 — 1	1 — — — —	16	—	—	—
				Fillies	2 2 —	3 1 1 —	— — — — —	9	—	—	—
	Chopette			Total	14	10	1	25	56·0	40·0	4·0
53. *PRINCE HAMPTON* (1888)	*Royal Hampton*	8 ‡	PP	Colts	4 1 2	— — 1 —	— — — — —	8	—	—	—
				Fillies	3 3 1	— — — —	1 1 — — —	9	—	—	—
	Pibroch			Total	14	1	2	17	82·4	5·8	11·8
54. *UGLY*..... (1892)	*Minting*	6	PP	Colts	3 — 2	2 — 1 —	1 — — — —	9	—	—	—
				Fillies	3 1 1	1 — — —	— — — — —	6	—	—	—
	Wee Agnes			Total	10	4	1	15	66·6	26·7	6·7

* Never won after two years old. † Never ran after two years old. ‡ Could not stay more than six furlongs.

TABLE III.—*Continued.*

55. Imp. from U.S.A. *AMERICUS* ..... (1892)	*Emperor of Norfolk*	6	PP	Colts	2 5 1	1 — 1 —	— — — — —	10		—	—	—
				Fillies	4 1 —	1 — 1 —	— — — — —	7		—	—	—
	Clara D			Total	13	4	—	17		76·5	23·5	—
56. *HAZLEHATCH* .... (1885)	*Hermit*	‡ 8	PP	Colts	5 5 1	1 — — —	— — — — —	12		—	—	—
				Fillies	2 3 —	1 — 3 —	— — — — —	9		—	—	—
	Hazledean			Total	16	5	—	21		76·2	23·8	—
57. *NECROMANCER* .. (1882)	*Touchet*	5	PP	Colts	2 2 —	1 — 1 —	— — — — —	6		—	—	—
				Fillies	5 4 —	1 — — —	— — — — —	10		—	—	—
	Enchantress			Total	13	3	—	16		81·3	18·7	—
58. *DOG ROSE* ..... (1883)	*See Saw*	6	PP	Colts	2 1 —	2 — — —	— — — — —	5		—	—	—
				Fillies	2 1 —	1 — 1 —	— — — — —	5		—	—	—
	Hedge Rose			Total	6	4	—	10		60·0	40·0	—
59. *BENTWORTH* .... (1892)	*Despair*	‡ 8	PP	Colts	4 2 1	1 — 1 —	— — — — —	9		—	—	—
				Fillies	2 — —	— — — —	— — — — —	2		—	—	—
	Scampavia			Total	9	2	—	11		81·8	18·2	—
60. *NOBLE CHIEFTAIN* (1885)	*Fitz James*	6	PP	Colts	2 — —	— — — 1	— — — — —	3		—	—	—
				Fillies	1 1 —	— — — —	— — — — —	2		—	—	—
	Village Belle			Total	4	1	—	5		80·0	20·0	—

‡ Could not stay more than six furlongs.

TABLE XII.

Illustrating the segregation of the staying and non-staying characters in the germplasm of intermediate sires, the offspring in each instance being own brothers or own sisters. The figures following the names of the offspring denote the distances in furlongs which they could stay.

Sires (PR)	Dams (PR)	Offspring.		
		Sprinters.	Intermediates.	Stayers.
Amphion	*Vetch*	*Amphinome* (F) .. 5	*Ampelion* (C) .. 8	*Miss Lettice* (F) .. 16
„	*Albertine*	*Opera* (F) 5	*Sonatina* (F) .. 8	—
„	„	—	*Sonatura* (F) 10	—
„	*Miss Hoyden* ..	—	*Lally* (C) 11	*The Nut* (C) .. 13
Despair	*Lalage* *	*Anchoret* (C) .. 5	—	*Designer* (C) 12
„	„	*Hope On* (C) .. 5	—	—
„	„	*Never Despair* (F) .. 5	—	—
„	*Flower of Teviot* ..	*Branksome* (C) .. 5	*Wild Despair* (F) .. 10	*Teviot II.* (C) .. 12
„	„ ..	—	—	*Margaret of Branksome* (F) 16
Deuce of Clubs ..	*Sweet Mart*	*Martha II.* (F) .. 5	*Martha III.* (F) .. 10	—
„	„	*Martha IV.* (F) .. 5	*Marthus* (C) 11	—
„	„	—	*Marthus II.* (C) .. 8	—
„	*Daisy Wreath* † ..	*Double Daisy* (F) .. 5	*The World's Verdict* (F) 8	—
„	„ ..	*Deuce of a Daisy* (F) 5	—	—
Enthusiast	*Greeba* *	*Anxious* (C) 5	*Eager* (C) 8	—
„	„	—	*Mormon* (C) 10	—

TABLE XII.—*Continued.*

Gallinule	*Admiration*	*Miranda* (F) .. 5	*Adula* (F) 10	*Pretty Polly* (F) .. 18
„	„	*Cock-a-hoop* (C) .. 5	*Admiral Hawke* (C) 10	*Adora* (F) 12
„	*Concussion*	—	*Dabchick* (F) 8	*Water-chute* (C) .. 12
„	„	—	*Sirenia* (F) 10	*Hammerkop* (F) .. 22
„	*Rose d'Amour* ..	*Cock-a-hoop* (C) .. 5	*Goosander* (F) .. 11	—
„	*Moira*	*Conquering Hero* (C) 6	*General Peace* (C) .. 8	*Oppressor* (C) 16
„	„	*Glencairn Maid* (F) .. 6	*General Cronje* (C) .. 11	—
„	„	—	*Christian De Wet* (C) 8	—
„ ..	„	—	*Carita* (F) 8	—
„	*Excellenza*	*Glasgerion* (C) .. 5	—	*Viceroy* (C) 16
„	*La Joie* *	*Happy Bird* (C) .. 5	*Jolly Bird* (C) .. 8	—
„	„	*Laughing Gull* (C) .. 6	—	—
Hackler	*Lady Gough*	*Filassier* (C) .. 5	—	*Ebor* (C) 12
„	„	*Lalla Rookh* (F) .. 5	—	*Lady Hackler* (F) .. 12
Kendal	*Chrysalis*	*Sir Michael* (C) .. 6	*Lakeland* (C) .. 10	*Laodamia* (F) .. 16
„	„	*Kentish Glory* (C) .. 6	—	—
„	„	*Lady Charity* (F) .. 5	—	—
Marco	*Hampton Lass* ..	*Morna* (F) 6	*Marco Hampton* (C) 10	*Marcobrunner* (C) .. 16
„	„ ..	*Mark Antony* (C) .. 6	—	—
„	*Improvement*	*Amendment* (C) .. 5	—	*Mark Tapley* (C) .. 12
Master Kildare ..	*Violet Melrose* ..	*Seaton* (C) 6	—	*Melton* (C) 15
„ ..	„ ..	*Rhoda* (F) 5	—	—
„ ..	„ ..	*Miss Astley* (F) .. 5	—	—
Orme	*Vampire*	*Vane* (F) 5	*Flying Leap* (C) .. 8	*Flying Fox* (C) .. 15
„	„	*Wetaria* (F) 5	*Vamose* (C) 8	*Flying Lemur* (C) .. 12

* Gametic composition, probably PRI. † Gametic composition, probably PP.

TABLE XII.—*Continued.*

Sires (PR)	Dams (PR)	Offspring. Sprinters.	Intermediates.	Stayers.
Orvicto	*Ishbel*	—	*Brindisi* (C) 8	*Orbel* (C) 16
„	*Coiffure*	*Hairdresser* (F) .. 5	—	*Saroth* (F) 12
„	*Dorothy Draggletail* ..	—	*Donzella* (F) 8	*Miss Taylor* (F) .. 12
„	„ ..	—	*Giglio* (C) 8	—
Prism	*Speculum Mare* ..	*Chromatic* (C) .. 5	*Euclid* (C) 8	—
„	„ ..	*Proposition* (F) .. 5	*Phenomenon* (C) .. 8	—
Raeburn	*Last Link II.* ..	—	*Darwinian* (C) .. 8	*Torch* (C) 16
„	„	—	*Lady Thea* (F) .. 8	—
„	*Tibbie Shiels*	*Wedderburn* (C) .. 5	*Hepburn* (C) .. 10	—
„	*Calliope* *	*Hoppner* (C) .. 5	*Romney* (C) 8	—
Rightaway	*Sister Lumley* * ..	*Clatford* (C) 6	—	*Andover* (C) 12
„	*The Warren Belle* ..	—	*Bridle Path* (C) .. 8	*Cottontail* (C) 12
„	*Theale*	—	*Jettison* (C) 8	*Throwaway* (C) .. 20
„	„	—	—	*Chuckaway* (C) .. 12
Royal Hampton ..	*Siffleuse*	*Bosco* (C) 5	*Royal Whistle* (C) .. 10	—
„ ..	„	—	*Royal Summons* (F) 10	—
St. Angelo	*Intruder*	*Archangel* (C) .. 6	*Intruder Colt* .. 9	—
„	*Mesopotamia*	*Poppits* (F) 6	*Potamia* (F) 10	—
„	*Chimera*	*Visionary* (C) .. 6	—	*Thunderbolt* (C) .. 12
St. Florian	*Morganette*	*Galteebeg* (C) .. 5	—	*Ard Patrick* (C) .. 13
„	„	*Galtee Queen* (F) .. 5	—	—

* Gametic composition, probably PRI.

Some Sociological Considerations

Arising out of Mr. Robertson's Article on the Heredity of Stamina in the Thoroughbred Horse.

By GEORGE PERCIVAL MUDGE.

THE investigation of the question of stamina in racehorses by means of exact experiment along Mendelian lines must be preceded by an adequate consideration of the facts which are known to men in every-day contact with the more concrete problems of inheritance in horses. The value of these considerations is very much enhanced when the practical men themselves become interested in the theoretical aspect of the problem and in the Mendelian principles of inheritance. All recent enquiry shows that hidden away as it were in the records kept by practical men, or in the common experiences of life, there are undoubted evidences of segregation, both somatic and gametic. But like many other familiar things they have been passed over and ignored because they were familiar and their place in the scheme of Nature was not adequately recognised in the absence of an unifying and embracing theory.

For reasons such as these and for others, the article which is published on "The Heredity of Racing Stamina in the Thoroughbred Horse," by Mr. J. B. Robertson, in this number of the *Mendel Journal*, and the article on "Inheritance in Coat Colour," by Mr. Robert Bunsow, which was published in the

last number, carry with them a large measure of utility to those who propose actually working along Mendelian lines and will be of use to others interested in the general problems, as showing how important and widespread are the evidences of segregation in the mechanism of inheritance.

I shall in the few remarks I am about to make, endeavour to show the suggestiveness of some of the facts in Mr. Robertson's paper. They have not only a bearing on horses but on sociological problems that touch human life at many points.

In considering Mr. Robertson's figures on page 55, which show that horses and colts have on the whole a greater staying power than mares and fillies, it is desirable to bear in mind that more horses from three years of age upwards are in training for a race than mares, in the proportion of 1 mare : 1·6 horses.† But the proportion of mares to horses which actually start in a race is as 1 : 2. This specific fact does not of course invalidate the more general fact, as manifested by the figures shown on page 55, that the horses win more races than the mares. For notwithstanding the smaller numbers entering a race, if mares had more staying powers than horses, it should still be manifested in a higher percentage of

† This fact and the following explanation were kindly supplied to me by Mr. Robertson upon my enquiry. Mr. Robertson tells me that "The reason there are fewer mares than horses in training at three years old and upwards and still fewer which start in a race, is because during training a large number of mares are found to be useless for racing after two years of age; the principal factor determining their uselessness is want of staying power."

the winning records.* Mr. Robertson's conclusion that the mares, in some cases, carry some inherent factor which inhibits their staying powers, therefore seems to be provisionally justified. I think the further facts which he has so clearly brought out and which show a continual decrease in the proportion of winning mares as the severity of the test (length of the race) increases, is strongly in favour of his view.† Thus in a race of five to seven furlongs in length the ratio of winning mares and horses, though slightly in favour of the horse, is nearly equal! But in more severe races of twelve furlongs and upwards, the winning horses are nearly three times more numerous than the mares. That is an extremely interesting physiological fact. I do not know that in any problem turning about the question of sex, any more significant fact could be found. For it seems to demonstrate the important general deduction that the more severe the test of staying power becomes, the more clearly the males and females are differentiated from each other.

Though this deduction is derived from a series of very accurate experiments with horses—for the racecourse is virtually a place where a supreme experiment takes place—it is probably true of human beings. It would be of great interest to put the

* It is additionally necessary to bear in mind the fact, as pointed out to me by Mr. Robertson, that if the mares were good enough, there are a sufficient number in training to win every race run. But while compared to horses they are but of little use as stayers, as sprinters they seem to excel. For in purely sprint races (five furlongs length) they gain a greater number of victories than their proportion to horses entering the race with them warrants.

† See p. 56.

matter to experimental test in children and adults of both sexes. The result might serve to remind us that beneath our civilisation lie the more enduring factors upon which our existence ultimately depends. It would answer for us the extremely important and interesting question as to whether the more or less apparent equality with men which is seemingly attained by women under the refined conditions of civilisation would manifest itself as a reality under sterner circumstances and under tests of endurance more severe than civilisation applies.

There are some facts which enable us to reach some idea as to the nature of the ultimate answer. I am indebted to a colleague, Miss Winifred Cullis, who kindly called my attention to the following data from Professor Thomas' book on "Sex and Society." At Vassar, a Woman's college in New York, there is held an annual field day. The results of the first field day of the Vassar College Athletic Association, held on November 9th, 1895, were compared with those of Yale, a Man's college, in the same year. They came out as follows:—

	Yale.	Vassar.
100 yards	$10\frac{2}{5}$ sec.	$15\frac{1}{4}$ sec.
220 yards dash	$22\frac{3}{5}$ sec.	$36\frac{1}{4}$ sec.
Running broad jump ..	23ft.	11ft. 5in.
,, high jump ..	5ft. 9in.	4ft.

This gives the results very much in favour of the men. But since 1895 the women seem to have improved, or more strictly speaking, the women who took part in the sports of 1904 were superior to those

of 1895. For in 1904 the results at Vassar were as follows :—

100 yards	.. ..	13 sec.
Running broad jump	..	14ft. 6½in.
,, high ,,	..	4ft. 2½in.

But though these results are an improvement on the earlier attempt they still fall very far short of the men's attainments.

On the other hand, Miss Cullis tells me that in Miss Helen B. Thompson's book on "The Mental Traits of Sex," a series of measurements show that women are better able than men to form new co-ordinate movements and to co-ordinate more rapidly to unforeseen stimuli. I called Mr. Robertson's attention to these results, and he says that, so far as the latter fact is concerned, it is much the same with mares when contrasted to horses. "If anyone passing into a mare's stall should happen to unexpectedly touch her, she will have kicked the intruder before he has had time to get away. But a horse is slower and there is time to get out of harm's way." Of course, so far as woman is concerned, this quicker response to unforeseen stimuli has been generally recognised for centuries, and has found expression in the popular saying "that woman's nervous mechanism is finer and more delicate than man's." It is one of the attributes indeed which give to her a large measure of her charm in the ordinary intercourse of life.

Another fact of extreme interest in Mr. Robertson's paper, and which is also applicable to human society,

is the difference in the physiological energy and endurance between the red and pale varieties of muscle fibre. Mr. Robertson has explained that the red fibre in racehorses has a greater staying power than the pale on account of its innate capacity to store up within its own substance a greater reserve of oxygen. But from the social aspect of the matter, in which I am now chiefly concerned, this fact is valuable as emphasizing that inherent physiological differences do exist between different sorts of muscle fibres. We may take two horses which in form and other external attributes appear very similar, but yet their performances will be very dissimilar. And that dissimilarity is due to inherent causes. No amount of environmental influence, in the way of feeding, training, and external stimulation, will ever convert the pale-fibred "sprinter" into the red-fibred "stayer." Indeed—and from our view this is a crucial social fact—the more we endeavour by environmental influences to change the "sprinter" into the "stayer" the worse we make him. For in Mr. Robertson's words: "It very frequently happens, if an attempt be made to train and race a sprinter over long courses, that he rapidly loses muscle and becomes slow."* In the light of this fact, perhaps we have a more rational explanation of the dullness and stupidity often shown by children in elementary national schools, than that other explanation so often urged by emotional sentimentalists, that these mental defects are due to want of proper food.

* Page 76.

They are more probably due to physiological deficiency in the attributes of the nerve cells. For who among us have not seen children bright and alert even when hungry, and others dull and inert even though well fed. That there is a difference in the quality as apart from the mass of the muscle fibre and of nervous energy in different human subjects can be seen at any swimming contest. I have seen some of the finest looking specimens of manhood, so far as external form and mass was concerned, break down half way through a race, while in the same contest a lean, sparse figure has glided in easily to the winning post. It is not only among adults but also among boys of ten to fourteen years of age that such differences may also be observable.

It was biological facts of this kind that several years ago caused me to depart from an adherence to the prevailing social sentiment, and to enquire whether it was not a more statesmanlike conception to frame a social policy which should leave every individual to find his own level by the spontaneous exercise of those qualities which Nature has given to him. We cannot convert human "sprinters" into human "stayers" by training or racing, however costly we make the process. We but spoil the "sprinter," as Mr. Robertson has shown in horses, by the process. Not even the inhalation of oxygen has any appreciable effect in increasing a "sprinter's" staying powers.*

Here, again, we meet another significant fact.

* See p. 76.

When oxygen is inhaled by a "stayer," that is, by a horse inherently capable of storing it up, the staying powers are increased.* In other words, if we desire to express it as a social analogy, an environmental influence may improve the powers of an organism which naturally possesses an inherent mechanism for responding to it and is capable of utilising its stimuli. In the light of such a consideration, perhaps it would be well to consider the advisability of no longer wasting millions of money annually upon the elementary education of social classes incapable from inherent defects of benefitting by it, but of spending instead a few hundred thousands upon the organisation and equipment of the higher education of our more cultured and successful classes, from among whom the leaders of the nation ought naturally to be chosen. In other words, train and oxygenate the "stayers," and leave the "sprinters" to do such sprinting as they can. The one can benefit by training; the other not only does not benefit, but is harmfully affected by it. "What is one man's food is another man's poison," is as true of social classes as it is of individuals. What is a stimulus bringing brightness and gladness to one, is an inhibition bringing anguish and hopelessness to another. In that fact lies, in large measure, the explanation of human happiness and misery and the futility of the sentimentalist's hopes.

Another point of sociological interest in Mr. Robertson's paper lies in the consideration which he

*See p. 76.

has adduced, that previous to the introduction of more severe tests of endurance on the racecourse, in the year 1899, it was difficult to pick out a good staying horse from his non-staying fellows. At that time not many races were run of a greater distance than a mile. This difficulty of discrimination did not arise because the good staying horses were not there, but because the selective tests were so easy that most racehorses with no real staying powers could stay them out. It was only when the races were lengthened to a mile and half or more, and the pace throughout had to be run at racing rate, that it became easy to make discriminations between the horses, and to classify them as " stayers " and " non-stayers."

Now this fact has an important and significant bearing on civilisation as it is manifested in the social life of Western Europe. Just as in the racing period prior to 1899, good " stayers " among racing horses were virtually lost to the racing world because they were submerged in a multitude of " intermediates " and " sprinters," so in human life, every step in the direction of softening the environment—such as free education and the like—does not make better men, it only serves to label the inherently vigorous and capable with the same label as the inert and incapable. The difference between the two classes is very soon shown when exceptional circumstances momentarily raise the selective test. It is not, as a rule, the men who took highest marks in the Army examinations who have proved the most

competent commanders of men and the most intrepid of soldiers. And at the other end of the social scale, one chooses one's domestic servants not because the London County Council gave them Domestic Scholarships and had the poor girls lectured to on subjects they never understood, but because they have muscle, good temper, capacity to work, honesty, and a power of happy adaptation. We are aware that there are but few servants with these desirable qualifications, but the moment the environment is softened and conditions are rendered so easy that scholarships are not only given away in thousands, but are accompanied with a prayer to the recipients to be kind enough to accept them,* we submerge our really vigorous and competent girls beneath the army of "slackers," who only accept scholarships because the life it confers upon them is easier than the life of the world.

Another fact of social significance in Mr. Robertson's article is that every horse has his course, beyond which he cannot stay. It is not training that makes a "stayer." Whether a horse is able to win over a long or short course is determined by bodily qualities brought into his constitution by the gametic factors of his parents. This is a fact equally true of human life. No sane person with adequate knowledge of the facts doubts it, but every politician and sentimentalist and social reformer systematically ignore

* I have had many occasions to see a good deal of this scholarship system as undertaken by the London County Council, and my statement is virtually correct and there is very little exaggeration in the metaphorical allusion to prayers made to the recipients.

it. It is not education that gives the capacity for acquiring strength or knowledge. Our statesmen of the year 1870, and before and since, would have been more of statesmen had they first enquired whether the masses possessed the inborn capacity of acquiring and retaining knowledge, before they thrust compulsory and pauperised education upon them. The inborn instincts of the masses were, indeed, in this matter truer than the artificial aspirations of their social superiors, for at the time they bitterly resented —and in my opinion rightly so—the compulsory interference with their legitimate liberties and the rightful privacy of their lives.

HEREDITY IN GOATS.

By C. J. DAVIES.

In all the literature dealing with modern heredity only the slightest allusion is made to goats. It might have been thought that some of the obvious differentiating characters possessed by domesticated animals of the genus *Capra* would have been seized upon to demonstrate the working of familiar principles or to prove exceptions to accepted rules. Yet this is so far from being the case that we find heredity in goats either being taken for granted as analogous to that in other forms, or else being described as baffling.

In the early days of Mendelism it was tentatively assumed that the inheritance of horns in goats followed the same course as in cattle. The following paragraph on page 133 of Report 1 to the Evolution Committee of the Royal Society (1901) expresses the view taken at the time :—

"The fact that the hornless breeds of goats still give some horned offspring is probably referable to the same cause, namely, promiscuous selection. The point is, of course, not certain, but from the analogy of cattle (page 140) we may anticipate that the hornless form is dominant. In the polled breeds of cattle, which are never *promiscuously* selected, the polled character has naturally been easily fixed pure, but in goats selection among the *ewes* has been probably to a large extent promiscuous."

On page 140 of the Report, the polled character is given as dominant to the horned character in cattle, and in, it is added, "probably goats" also.

No further allusion is made in the chief technical publications to heredity in goats until 1909, when Professor Bateson again briefly alludes to them in his book on "Mendel's Principles of Heredity." By this time additional evidence rendered a change of view necessary, and the author says, on page 32: "In sheep the inheritance of horns is sex-limited, and from evidence given me by Mr. E. P. Boys-Smith I suspect that this is true in the case of goats also." Further on in the same volume (page 170), when describing the phenomena connected with the inheritance of horns in sheep and cattle, a footnote is inserted by the author to the following effect:—

"As to the descent in goats I have no thoroughly adequate evidence. The Rev. E. P. Boys-Smith has kindly given me particulars of many matings which he has made, but the details are complex and I have not been able to extract a consistent scheme from them. There is probably some intricacy due to gametic coupling comparable with that described in the next section, or perhaps to sex-limitation."

In consequence of the unsatisfactory results hitherto attained in this bye-path of knowledge, the writer has recently been endeavouring, by a consultation of the excellently-arranged and carefully-kept Herd Books, issued by the British Goat Society, to

collect more evidence upon the subject of heredity in goats. These Herd Books date back to 1875, and comprise the entries of something like one thousand seven hundred goats. Of this number it has been possible to discover the horn characters of the parents of one thousand three hundred and seventy individuals ; and it has also been found possible to analyse still further the breeding of seven hundred and one goats. Although the results are not perhaps entirely satisfactory, yet they conclusively clear up more than one debatable point ; and they will, it is hoped, help to throw a little light on the elusive problem of the inheritance of horns in goats.

First of all, the twenty-four matings of a polled animal bred from polled parents, with a horned animal bred from horned parents give equal numbers of polled and horned offspring (6 horned males, 6 horned females, 3 polled males, 9 polled females).

If inheritance had followed the same lines as that in cattle, this mating would have given all polled offspring. If, on the other hand, it had been analogous to the phenomena observed in sheep, all the male kids would have been horned and all the females polled ; which is equally not the case. So we are apparently confronted at the outset with the fact that there is no dominance (sexual or otherwise) of either character, and that the horned or polled condition is equally likely to be inherited by cross-bred goats of either sex.

In cattle the horned individuals of the F_2 generation are pure (namely, breed true). In sheep only

the F_2 polled males and F_2 horned females breed true to their respective characters. In goats a pair of horned animals descended from four parents all of which were also horned breed true to the horn character without exception in the few cases of this precise mating which the writer has been able to collect; but practically all matings of two polled individuals throw a proportion (1 : 3) of horned progeny whatever the precise germinal constitution of the parents may be.

The following tables give full details of the data collected from which readers can draw their own conclusions :—

THE BREEDING OF 1,370 GOATS.

Both Parents Horned.	1 Polled × 1 Horned Parent.	Both Parents Polled.	Offspring.
55	73	39	167 Horned he-goats.
63	151	107	321 Horned she-goats.
14	116	222	352 Polled he-goats.
18	231	281	530 Polled she-goats.

Approximate percentages :—

488 Horned Goats are bred :—

24 per cent. from 2 horned parents.

46 per cent. from 1 polled 1 horned parent.

30 per cent. from 2 polled parents.

882 Polled Goats are bred :—

3·50 per cent. from 2 horned parents.

39·25 per cent. from 1 polled 1 horned parent.

57·25 per cent. from 2 polled parents.

ANALYSIS OF THE BREEDING OF 701 GOATS OF ALL VARIETIES, PURE AND CROSSBRED, IN RESPECT OF THE HORN CHARACTER.

Characters of Parents and Grandparents.			Horned Males.	Horned Females.	Polled Males.	Polled Females.	Horned (both sexes).	Polled (both sexes).
Grandparents.	Parents.							
(P P × H H)	P × P	Parents all polled.	3	2	5	9	5	14
(P P × P P)	P × P		6	25	50	47	31	97
(P P × P H)	P × P		11	28	54	83	39	137
(P H × P H)	P × P		4	7	18	24	11	42
(H H × H H)	P × P		—	—	1	—	—	1
(P H × H H)	P × P		2	3	6	8	5	14
(P P × H H)	H × H	Parents all horned.	1	1	—	—	2	—
(H H × H H)	H × H		—	8	—	—	8	—
(H H × P H)	H × H		4	8	2	5	12	7
(H P × H P)	H × H		3	3	1	1	6	2
(P P × P P)	H × H		1	—	1	4	1	5
(H P × P P)	H × H		1	3	3	5	4	8
(P P × H H)	P × H	1 parent polled × 1 parent horned.	6	6	3	9	12	12
(P H × H H)	P × H		2	14	9	16	16	25
(P H × P P)	P × H		9	25	24	38	34	62
(P P × P P)	P × H		5	13	9	23	18	32
(H H × H H)	P × H		1	3	—	3	4	3
(H P × H P)	P × H		7	11	7	7	18	14

Now, the foregoing particulars are drawn from all the entries in the Herd Book, irrespective of variety. The basis upon which the modern pedigree goat of these islands has been built up is presumably the native English, Scotch, or Irish animal, which is naturally horned in both sexes. The British goat

ANGLO-NUBIAN GOATS.

Illustration No. 1.—Upper figure is that of Miss Mortimer's polled Anglo-Nubian she-goat, *Wigmore Topsy*. It shows English rather than Nubian characteristics.

The lower figure is that of Mrs. Taylor Marsh's polled Anglo-Nubian he-goat, *Scriventon Budget*. It shows Nubian characteristics.

has, however, for many years been continually crossed with imported animals of a totally different type which are known to the fancy as "Nubians," and from which a so-call Anglo-Nubianed breed has been evolved. These "Nubian" or Eastern goats differ so markedly in many respects from the British and Continental types of common goats that they may have originated from a distinct wild species. If this was actually the case, it is not impossible that a complication might have been introduced into the heredity of the horn character in their descendants. Many of the imported Oriental he-goats possess horns, but the shape and carriage of these appendages differ totally from that familiar to English goat owners. They are usually small with a downward and outward curve or corkscrew-like twist difficult to describe. It should be added that although the term Anglo-Nubian has always been applied to goats which have been crossed with the Nubian, the cross may be so remote as to be negligible for all practicable purposes. In other words, an Anglo-Nubian goat may be almost indistinguishable from a pure English, as shown in the illustration No. 1, upper figure, or a pure "Nubian," as seen in the lower figure, or it may possess characters peculiar to both races.

As a check on possible complications due to complex crosses, a separate analysis has been made of horn inheritance in all those goats belonging to the Toggenburg section of the Herd Book. When horned at all the Toggenburg goat has scimitar-shaped horns like those carried by our British breeds,

and there can be little doubt that it is, in origin, a descendant of the same primeval stock.

Pure Toggenburgs are rare in this country, hence the number of pedigrees available for separate analysis is few. The results, however, are not without interest when compared with the figures obtained for all breeds in the Herd Book.

THE BREEDING OF 164 PURE TOGGENBURG GOATS.

Parents.			Offspring.
Both horned.	One polled 1 horned.	Both polled.	
—	2	3	5 Horned males.
—	2	24	26 Horned females.
—	12	60	72 Polled males
1	11	49	61 Polled females.

Approximate percentages :—

31 Horned Goats are bred :—

None from 2 horned parents.

About 13 per cent. from 1 polled and 1 horned parent.

About 87 per cent. from 2 polled parents.

133 Polled Goats are bred :—

About ·07 per cent. from 2 horned parents.

About 17 per cent. from 1 polled and 1 horned parent.

About 82 per cent. from 2 polled parents.

Another pair of characters in goats which might have been expected to yield interesting results when analysed by Mendelian methods is long coat and short

TOGGENBURG GOATS.

Illustration No. 2.—Upper figure shows Dr. Clutterbuck's short-haired horned Toggenburg she-goat, *Withdean Babette.*

The lower figure shows Mr. W. A. Wilcox's long-haired polled Toggenburg he-goat, *Champion Le Castor.*

ANALYSIS OF THE BREEDING OF 82 PURE TOGGENBURG GOATS.

Characters of Parents and Grandparents.		Horned Males.	Horned Females.	Polled Males.	Polled Females.	Horned (both sexes).	Polled (both sexes).
Grandparents.	Parents.						
(P P × H H)	P × P	—	—	—	—	—	—
(P P × P P)	P × P	1	5	22	14	6	36
(P P × P H)	P × P	—	4	9	8	4	17
(P H × P H)	P × P	—	—	—	—	—	—
(H H × H H)	P × P	—	—	—	—	—	—
(P H × P H)	P × P	—	—	—	—	—	—
(P P × H H)	H × H	—	—	—	—	—	—
(H H × H H)	H × H	—	—	—	—	—	—
(H H × P H)	H × H	—	—	—	—	—	—
(H P × H P)	H × H	—	—	—	—	—	—
(H P × P P)	H × H	—	—	—	1	—	1
(P P × P P)	H × H	—	—	—	—	—	—
(P P × H H)	P × H	—	—	—	—	—	—
(P H × P P)	P × H	—	—	—	—	—	—
(P H × P P)	P × H	—	1	2	2	1	4
(P P × P P)	P × H	2	—	7	3	2	10
(H H × H H)	P × H	—	—	—	—	—	—
(H P × H P)	P × H	—	—	—	1	—	1

coat. In rabbits and guinea pigs long hair is recessive to short hair, but in the F_2 generation segregation is thought to be imperfect, and a type with an intermediate length of hair is produced in addition to the two pure parent forms.

In goats the hair character is only stated in the first seven hundred entries in the Herd Book, and a distinction is made between long and medium haired individuals. Among the seven hundred there seem only to be twenty-five medium or long-haired

animals. These are bred as shown in the following table :—

Parents.	Number of Medium or Long Haired Offspring.
Both short haired	5
Both long or medium haired....	6
One long × one short.....	3
One long × other unknown	5
One short × other unknown	3
Both of unknown character	3

From this unsatisfactorily brief analysis one learns that two short-haired goats may throw long-haired animals ; but two long hairs have not in the cases investigated thrown short hairs. It should be added that recent conversations with breeders have led the writer to gather that in some of the Swiss breeds the pure-bred males are sometimes long-haired in the Summer and Winter coats, while the females are always short-haired or at most have merely a dorsal fringe of long hair in the Winter coat. The accompanying photographs of pairs of pure Toggenburgs and Saanens show animals with this sexual differentiation.

When Oriental blood was introduced into British stock, among other things there was also introduced the totally pendulous or " drop " ear. In view of the originally erroneous conclusions arrived at from the analysis of insufficient data, it is unsafe to be too sure that the very scanty details available on the heredity

SAANEN GOATS.

Illustration No. 3.—The upper figure shows Mr. H. E. Hughes' short-haired polled Saanen she-goat, *Broxbourne Venus*.

The lower figure is that of Mr. H. E. Hughes' long-haired Saanen he-goat, *Broxbourne Adversal*.

of "prick" and "drop" ears point to any definite conclusion. In the "Book of the Goat" it is stated that the progeny of Oriental lop-eared goats crossed with native prick-eared goats, were always popular because of the short, sleek coat, small horns, rich black-and-tan colour, "and drooping ears which characterised the progeny of this combination." This suggests dominance of the pendant ears, as do certain photographs, records, and results of observation. The point is one which is worthy of closer investigation by those actually acquainted with the characteristics of all the well-known goats of the last thirty years.

The heredity of the tassel-like appendages which are found on the throats of many individuals of all breeds of goats is another point which merits a detailed investigation.

A FAMILY OF DEGENERATES.

By Dr. W. J. RUTHERFURD.

THE following extraordinary family history affords food for thought. The common ancestor, a working-man of whose antecedents nothing is now known, married twice, and his descendants by each marriage have proved themselves so markedly abnormal that it would seem highly probable he must have been the common source of the tainted stock.

By the first marriage he had six children. The oldest, a son, fortunately did not marry. The second child, a daughter, married, and had six children, and in a family epidemic of diphtheria* no less than four of them died. The next, also a daughter, died of apoplexy when sixty-two years old. The fourth was a son, who married, and had six children, the two oldest of whom have families of their own. This man got hold of a dynamite cartridge one day, and thought it would be "a nice sort of toy" to give to one of his children when he got home; so he gave it to his eleven-year-old son, who thereupon "played with it" with fatal results. The next son in this first family died of apoplexy, leaving one daughter, who is now about fourteen years old. The last of the six children was a daughter, who died in childbirth, probably, as the sequel

* An illness the poison of which exerts a specially damaging effect upon the nervous structures, especially if these are already not of the most robust.

would seem to indicate, before the latent abnormal tendencies of her stock had had time to show themselves. This woman had four daughters; the oldest died of apoplexy at the comparatively early age of thirty-four, and has left one daughter; the youngest developed, when getting on in years, attacks of what may have been either the *petit mal* or hystero-epilepsy. She is married, and has been pregnant eight times; the first four pregnancies resulted in live-born children, who are now aged from six to nineteen years, then came two still-births, then a girl who had a series of fits "when she was cutting her teeth"; and her last pregnancy again resulted in a stillborn child.

The progeny resulting from the second marriage of the male ancestor of this stock now come to be considered. Happily, there was but one child—a woman now aged sixty-four years. At the age of nineteen she developed a double rupture; later in life she began to be subject to fits; and for the last twenty years she has been afflicted with paralysis agitans. She married a drunken and immoral husband, who, however, rose to the position of being an employer of labour in one of our manufacturing towns. When nearly seventy years old he died of apoplexy, receiving posthumous eulogies from the local press.

This evilly-matched pair had no fewer than eleven children, whom we must consider in detail. Their oldest daughter, now aged forty-four years, as a girl and young woman showed most pronounced

homicidal impulses, always directed against babies or quite young children; she was kleptomaniac, had extraordinary outbursts of destructiveness, and at the age of twenty-eight became epileptic. None of these many negative advantages, singly or combined, was sufficient to prevent her obtaining a husband; and on his death she married again; but fate has been kind, and she has had no children.

The second daughter deserves special consideration, and I shall deal with her later.

The third had the good fortune to die in fits in infancy. Of number four there is little to record, except that, coming of such a stock, he did not refrain from either marriage or parenthood. The fifth child was an epileptic, and died of apoplexy at the quite abnormally early age of twenty-four. Number six died of fits in infancy. The seventh had fits in infancy, has been neurasthenic for years, is married, and has three children. The next child died of fits at the age of three. The ninth child was prematurely born, and only lived for three days. The tenth is subject to some sort of fainting seizures; she is married; her first child was still-born, and the only other child died in convulsions when but three days old. The last child in this family, *i.e.*, the eleventh, was prematurely born, became early convulsed, and died probably in consequence of the fits when nine hours old. He was polydactylous, having an accessory little finger on each hand.

The second child of this family (*vide ut supra*)

was an epileptic, and either in consequence of this or of her general unstable heredity, was a drunkard, was unveracious to a degree, and was a thief. At one time in her life she seems to have been distinctly insane; but she was not incarcerated, as she soon "got all right again"—as people term it in their ignorance of the fact that insanity is far more than a mere matter of a transitory train of more or less striking symptoms. At the age of thirty-six years she developed ataxic paraplegia; and when forty-two years of age had an attack of pneumonia, and after lingering on for over a month she died. On her deathbed, in a practically furnitureless house to which she had obtained entry by guile, and of which she paid no rent, and deserted by her paramour, she took in as a lodger a young married prostitute, who became a companion to her fifteen-year-old daughter, the two becoming inseparables except when temporarily estranged by violent outbursts of quarrelling. This woman had married a drunken and immoral husband, and during her married life had nine pregnancies. Her first child was prematurely born, took fits in infancy, and soon died. The next two pregnancies resulted in abortions. Then came another prematurely-born infant, which also developed fits and died. She then had another abortion, and then came still another premature child, about which there is the same story of convulsions leading on to a fatal issue. Then came a daughter, born at full time, being subject to attacks of laryngismus for years, and at the age

of fifteen presenting a suppurative nasal sinusitis of long standing: this is distinctly worthy of remark, as it is established beyond dispute that one of the common predisposing causes of the low forms of inflammation that lead on to a chronic sinusitis is the existence of some anatomical peculiarity in these situations preventing proper drainage of the normal secretions. The next two children were prematurely born, and both died with fits while still infants. Becoming tired of her husband's behaviour she divorced him, and for years thereafter occupied herself in paying on the instalment plan the lawyer who had put this matter through for her. After getting rid of her husband she took up with a half-bred Irish navvy, who, like herself, was a thief; and she bore to him two illegitimate children. The first of these had a gross malformation of the cerebrospinal axis of the type that is known technically as *spina bifida*, and untechnically as "a hole in the back": it only lived four weeks, and mainly spent its brief life in having fits. The last child appeared a fairly normal infant, but at the age of seventeen months it took a cold, and died almost before her relatives were aware that the child was ill.*

It must be acknowledged that there is always room for doubt whether apoplexy does not imply a vascular disorder rather than a nervous one. Though dependent upon degeneracy of the blood-

* The story is suggestive of pneumonia, the same disease as her mother had died of.

vessels, it is questionable whether in a stock such as we have here to deal with, where, among many and varied nervous disorders, including gross physical defect, women—who, as a general rule, have a certain exemption from apoplexy—suffered from fatal attacks at the age of thirty-four and even twenty-four years, there may not have been, in addition to the weakness of the vessel-wall, a contributory weakness in the supporting structures, such as the nerve fibres and the neurogliar tissue. Yet it is but fair to admit that in one of the best-known of modern medical text-books, written, however, by one who makes no claim to be regarded as an authority on the subject of heredity, it is asserted that "heredity influences cerebral hæmorrhage entirely through the arteries."*

To a family like this the expression of Professor Raymond, of Paris, is particularly applicable where he refers to certain "nervous diseases . . showing the bad quality of certain parts of the nervous system as a hereditary malformation, total or partial, of the cerebro-spinal axis."† It would seem that such nervous diseases and conditions are due to a primary constitutional vulnerability of the cerebro-spinal axis, and that this defective condition of the central nervous system is invariably transmitted to few or many of the descendants. In considering this pedigree it must be remembered that it contains individuals of whom nothing is known. They are

* Osler, *Principles and Practice of Medicine*, ed. 6, 1906, p. 966.

† *British Medical Journal*, July 4th, 1908, p. 7.

indicated as normal in the chart simply because they were not known to be abnormal. What they really were, or what they would become later in life, is a point of the greatest interest which I am unable to answer. As Clouston* has put it, "the weak and troublesome point of all studies of heredity is that they cannot be regarded as complete till all the subjects of them are dead;" the obvious reason for this being that at any time during life it is possible for hitherto latent tendencies to display themselves, or (within certain limits) for more children to be born who may or may not have further manifestations of the conditions under observation. A very striking example of this has come under my own notice in the case of a man who had been epileptic from his youth, and who had a nephew—out of the line of direct descent, be it noted—who only became epileptic at the age of fifty-seven: this condition (and I can vouch for it, as I examined him myself, and had him under observation for months) being a true epilepsy, and not traumatic or due to syphilis. Now, in this particular case, if this second person had died before reaching the age of fifty-seven years, none of his epileptic manifestations would ever have been displayed, and the uncle might then have remained the only member of the family group who was known to have displayed any of the phenomena of that disease.

The sociological importance of a knowledge of

* *Clinical Lectures on Mental Disease*, ed. 6, 1904, p. 620.

the existence of such hopelessly defective strains in the population can hardly be over-estimated. It is truly unfortunate that they should exist, and not only so, but that they should increase the way they do: but it will always be making the best of a bad job—to use a phrase that is more blunt than elegant—if we can avail ourselves of the knowledge of such infamies in preparing for the fight which must inevitably come sooner or later against such sources of weakness and degeneracy. It will not do to forget that there are many stocks whose nature, in the words of the old writer of the Wisdom of Solomon in the Jewish Apocrypha, by birth is evil and their wickedness inborn, whose manner of thought can in no wise ever be changed, for they are "a seed accursed from the beginning."*

The idea that production of a clean bill of health, either in the shape of a doctor's certificate or of an insurance policy, should be made an essential condition as a preliminary to marriage would, if attempted, defeat its own ends. That this is so cannot be too widely understood before the time comes when some government—as governments have such a habit of doing—begins to interfere blindly, as may perhaps happen any day in these over-legislated times. No attempt to put difficulties in the way of marriage will permanently affect the birth-rate except among the more decent and self-respecting sections of the community. The others will remain as they were, and might very easily be encouraged by such legisla-

* *Wisdom of Solomon*, R.V., ch. 12, vv. 10-11.

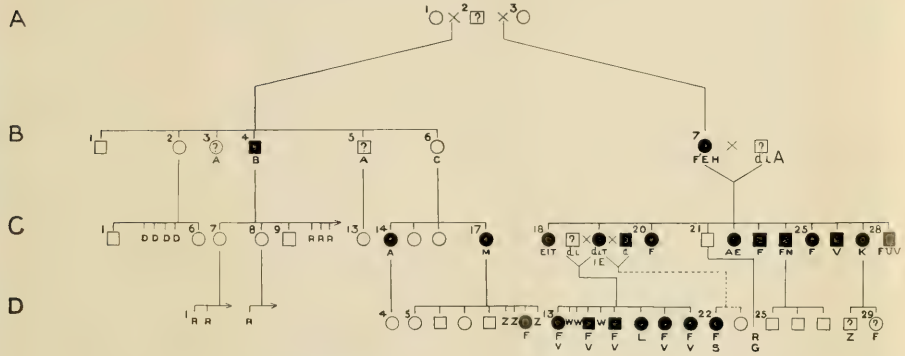
tion to accentuate their social misdeeds. Everyone who has a first-hand knowledge of the conditions of dysgeny that exist in the ranks of the thriftless and degenerate is aware of the wholesale disregard shown by such for the marriage bond and the rifeness of illegitimacy even under existing conditions. To debar legal marriage to these, to impose extra formalities, especially if they tended to be either of an irksome or of an expensive nature, would only serve to place a premium on promiscuity and on illegitimacy, not to promote eugenics, or to hinder the perpetuation of what the nation can well be without.

Other alternatives there are, of which perhaps the most widely advocated at the present time is wholesale segregation. To round up the hopelessly debased, the insane, and all the other representatives of undesirable stocks, and then to allow them to lead happy, useless lives, sheltered and comforted till a long-deferred period is at last put to their protracted senility by the slow decay of Nature, would, if attempted on any but that scale at present available to amiable and amateur private philanthropy (as is already attempted in some parts of the country) lead to the imposition of such an intolerable burden of taxation upon the remaining portion of the community as would speedily render the attempt unworkable. Any attempt to enforce the adoption of segregation on a scale such as would alone be of practical utility could otherwise only end in an intolerable national burden.

Still another idea finds adherents ; namely, that the unfit should be sterilised, so that in the course of, at the most, a very few generations the inherited strains of unfitness would tend automatically to become extinct. Apart from the oppressive tyranny with which this would arm officialdom, grave objection can be taken to such an idea. In practice it might work fairly well in the case of the male degenerate, but it would be another story altogether with the female. Little imagination is required to foresee that, as segregation and effective mutilation are alternatives, and as any combination of alternatives can only result in excess of the most wasteful and extravagant form, such a course would at once result in letting loose on the community hordes of utterly reckless characters whose instincts would lead them to spend lives of depraved abandon in the realisation of having been emancipated from those physiological consequences which, to a certain extent, act at the present time as deterrents to the depraved of both sexes.

Such is some of the criticism that can be directed against suggestions already put forward for the remedy of a danger which, though always present and never inactive, has, in these latter days of increased populations, of added facilities for migration whereby stocks may become disseminated in all directions, and of the increasing struggle for existence, assumed an importance that can hardly be over-estimated, and the menace of which becomes ever greater as time goes on. It remains to be seen

PEDIGREE OF A FAMILY OF DEGENERATES



whether some other remedy can be found for the evil, whether we will be driven to fall back on a combination of these glaringly faulty remedies adjusted to a sexual basis, or whether the field in which the enemy is sowing tares will, after all, just have to be left to itself.

EXPLANATION OF SYMBOLS USED IN PEDIGREE OF A DEGENERATE FAMILY.

The dotted line indicates illegitimate descent.

A = apoplexy.
B = designates the man who gave his child a live cartridge to play with.
C = died in childbirth.
D = died of diphtheria.
d = of drunken habits.
E = suffered from epilepsy.
F = had fits in infancy or childhood.
F^1 = subject to fits after twenty years of age.
G = details not known. As a matter of fact the child was not yet born when this table was made up.
H = hernia (*or rupture*).
I = insane.
i = immoral.
K = syncopal attacks.
L = laryngismus stridulus, &c.
M = *petit mal?*
N = neurasthenia.
P = paralysis agitans.
R = no details known.
S = spina bifida.
T = thief.
U = polydactyly.
V = prematurely born.
W = abortion.
Z = stillborn.

Some Facts of Inheritance and their Bearing on the Advocated Issue of "Marriage Certificates."

By THE EDITOR.

THE article by Dr. Rutherfurd on a "Family of Degenerates" is of great interest academically and sociologically. The title of the article is that given to it by its author. It is clear, if we carefully read what he writes of this family, that Dr. Rutherfurd was at the time when he made his enquiries, impressed with the degenerate nature of the people whom he was professionally attending. And with a commendable enthusiasm for recording facts in order to advance our knowledge of inheritance, he framed the pedigree and drew up the notes, which are now published in this Journal. It is a thankless and tedious task which only men of indomitable courage and great energy ever undertake.

When Dr. Rutherfurd sent his article to us for publication, an examination of the pedigree accompanying it revealed the fact that he had left all the symbols open; that is, he had not "blacked in" the symbols that are meant to indicate the degenerate individuals recorded there. He later wrote, while the proofs were passing through his hands, and suggested to us that it would be a

desirable thing to indicate the degenerates in solid-black symbols. As we have had some previous experience of trying to indicate in a *precise* way who are, and who are not degenerate individuals in a stock, we felt some interest in seeing how Dr. Rutherfurd would acquit himself of the responsibility he had thus imposed upon his own judgement. His answer did not surprise us. He wrote after due consideration, to say that, "when one comes to analyse over-closely it becomes more and more difficult to say definitely what is normal and what is not. It is a matter of definition. What do we mean by a degenerate?" Dr. Rutherfurd therefore declined to accept the responsibility of marking any of the individuals on the pedigree chart as degenerate, and concluded his letter by saying: "I think, therefore, that I shall leave it to your discretion to decide which symbols should and which should not be blacked in." We accordingly undertook the task, and we are alone responsible if any error of judgement has been committed. But having accepted this responsibility we feel it due to many considerations to justify ourselves by a statement of the reasons which impelled us to mark some members as degenerate, and to formulate the doubts which caused us to leave others unmarked.

But before doing so there is one aspect of this matter having a sociological significance which we desire to discuss. There are certain very well-intentioned people, prompted by the highest and most generous motives, who believe that the foundation of

a sound nation is to be sought in the creation of an army of medical officials, who are to decree who shall marry, and are to weigh us all in the scales of Hygeia, and to label us " fit " or " unfit," " degenerate " or " normal," as they think right according to official judgement. All this is to be done by Act of Parliament. Such schemes are very pretty on paper, and very plausible when discussed over a round table after dinner. But it is in the working of them, when we come into contact with all sorts and degrees of realities, that their latent and inherent dangers, impossibilities, imperfections, incongruities, and absurdities become painfully and obtrusively patent. Here, in Dr. Rutherfurd's pedigree, we have a concrete case. Dr. Rutherfurd, who personally knows some of the people recorded in it, finds it difficult to say who are normal or degenerate. He prefers, therefore, not to indicate them in any *precise* way. But when a self-satisfied body of medical officials in the discharge of their statutory functions, have to say whether a given person shall or shall not marry, whether this man, woman, or child is or is not sound, they have got to be *specific*. No vague general principles, floating nebulous in an optimistic and otiose atmosphere, will suffice. Persons are very concrete things, and their passions and desires are persistently imperative. It is unsafe and unstatesmanlike to dismiss either by definition or statute, laboriously formulated amid many conflicting doubts, impossible of certain interpretation by the wisest judges, and incapable of general enforcement upon the people.

The Sansculottes of Paris and their leaders attempted it in connection with other problems, and deservedly lost their heads on the guillotine in consequence. To play with the subtle and yet powerful promptings of men is to invite a conflagration in which decrees shall be rightly burned and their authors justly destroyed. All the suggestions put forward in advocacy of "marriage certificates" seem to those who are acquainted both with the course of History and the facts of Heredity to be untenable in even the smallest measure. The pedigree of Dr. Rutherfurd affords a concrete example of the difficulties which beset the practical application of all such schemes.

Let us take, for instance, the individual No. 8, in the B. Generation, and ask ourselves the question: Is he degenerate or normal? Is his degeneracy physical, intellectual, or moral, or all three? If he is only physically degenerate, but mentally and morally normal, shall we incarcerate him and forbid him marriage? If he is morally as well as physically degenerate, but mentally quite capable, what shall we do with him? Let us enquire, then, into the facts concerning him. He was drunken and immoral, and at seventy years of age died of apoplexy. He manifested the sort of reckless deeds of which he was capable by marrying a woman who suffered from double hernia, was epileptic, and ultimately developed paralysis agitans. Such a woman undoubtedly had undesirable family connections, and he must, or should, have known of them. Was he civically degenerate because of his consistent immorality and

drunkenness and of his stupidity in the choice of his partner ? Was he physically fit because he was capable of living until seventy years of age, in spite of his vicious habits of life ? Should we let him marry because of his physical robustness, and decree his celibacy because of his moral delinquencies ? These are tough questions for a board of medical marriage assessors, calculated truly to shake their self-complacency ! And when we further learn that this man was " an employer of labour in an industrial town, and received posthumous eulogies from the local Press," we shall find it hard to say whether he or his degenerate offspring shall or shall not be forbidden to marry. And thus in imagination, postulating that we are living in a degenerate and helotised England, which tamely tolerates a " Marriage Board," and that we are the Chairman of that Board, and have a casting vote, we decide to regard this man as not degenerate. We simply mark him with a query.

The next case that comes before us for consideration is No. 19 of the C. Generation. She desires to marry the man shown to the left of her on the pedigree. We find that he, too, like the man whom we have just discussed, is drunken and immoral. Is he to be " blacked in " or left blank on the official chart ? He certainly is not a nice person. But we have no information of his family. No. 8 of the B. Generation, who was drunk and immoral, we decided to leave alone. Can we consistently condemn this man of the C. Generation on account of the same vices ? And if we do, and plead that we left No. 8 B. alone because

he was an employer of labour, and therefore useful to the community, will there not in these days of highly placed, highly paid, and extremely unscrupulous demagogues, go forth the cry of class decisions and of class laws ? Will not the reputation of science suffer because of the incongruities and absurdities of those who frame decisions in her name ?

Let us take another individual, No. 4 B. Generation. There is nothing recorded against him except that he took home a dynamite cartridge and thought it would be a nice toy for his eleven-year-old son. The boy was killed, of course. Suppose this man had done some silly deed like this before marriage, and he asked permission of a " Marriage Board " to marry ? Are we to regard him as degenerate or not ? We have marked him as degenerate on the pedigree, because any person who lives in this community and does not learn that dynamite is dangerous, is as much an irresponsible person as one who repeatedly puts his fingers in the fire or commits incendiarism. But if mental traits are hereditarily transmitted, how are we to regard his six children and three grandchildren, when their turn arrives to apply for marriage certificates ? It is almost impossible to say, until the deed is done, who among the descendants will become responsible and who irresponsible. And if officials, driven by statutory obligation to say " Nay " or " Yea," grant a certificate of marriage to a descendant who later gives a dynamite cartridge to one of his children, and refuses it to a descendant who turns out quite responsible for his

deeds, surely the reputation of that Board as an aggregation of funny men will have been made in history !

Next consider the individuals Nos. 3 and 5 in the B. Generation. They both died of apoplexy, the former at sixty-two years of age, and the latter at an age not given. Are either of these members to be regarded as physically degenerate ? Quite possible. But who is to prophecy when such men as these are only twenty-three years of age and desire to marry, that they are going to die at sixty years of age of nervous or arterial lesions, or a combination of both ? And if we seek safety in a knowledge of the ancestry, and say "let that guide us," and then the individuals No. 14 or No. 22, C. Generation, come to us asking for a certificate of marriage, and upon an examination of the pedigree we find in the left-hand branch of the family that one of the ancestors died of apoplexy at sixty-two, and in the right-hand branch that another died of the same disease at seventy years of age, we might be content to argue that if the apoplectic ancestors lived to such ages before dying of a congenital disease, there is not much to be alarmed at in the application for marriage of Nos. 14 and 22 C. And consequently we grant their request, only to find that our surmises are falsified by No. 14 dying of apoplexy at the early age of thirty-four years of age, and No. 22 dying of the same disease at twenty-four ! Verily, we had better not meddle with these things ! We can promulgate the facts of inheritance and then let the

inherently wise, spontaneously and voluntarily benefit by such knowledge, and at the same time we can leave the congenital fools to bear the consequences and pay the penalties of their own follies. There is, it seems to us, after long and anxious deliberation over this question, no other rational and safe path.

When we deal with two persons like Nos. 18 and 19 in the C. Generation, we have little doubt as to their degeneracy and anti-social attributes. Both were epileptic, insane, and thieves. When *in virtue of their criminal deeds they appear* at a Criminal Court, we should certainly advocate the right of the State to enquire into their family history, and finding it to be what it is, to condemn them to life-long segregation, under humane conditions, from the community. But to do that is one thing. To create an army of officials who are *to go forth and hunt* for degenerates and to label every individual in the community with an official ticket, is quite another. The one thing lends itself to the minimum possibilities of abuse, the other to the maximum growth of an intolerable tyranny.

With regard to the remaining members (*i.e.* Nos. 20, 22—28, C., and 13—22, D.), whom we have blacked-in as degenerate, there is little room, probably, for difference of opinion, bearing in mind not only the individual attributes, but also the family history. For the individuals have already manifested the inherent defects of their race. We will urge nothing at this point against the wickedness of deliberately endeavouring to preserve the defective

individuals of the D. Generation, by conscious efforts to reduce infantile mortality. That we have already urged in connection with another case on pages 191 and 205, and the considerations we pleaded there are equally applicable here.

HEREDITY OR ENVIRONMENT: WHICH MAKES THE MAN?

By LOUIS COBBETT, M.D.

(Lecturer in Pathology, Cambridge University.)

No one doubts that man comes into the world with definite inborn potentialities. He will be tall or short, dark or fair, quick tempered or placid whatever happens; nay more, in many subtle and minute points he will resemble his parents. But then, on the other hand, he is an imitative creature, is much influenced by his companions, is moulded by circumstances. Both heredity and environment then admittedly go to the making of man. But in what proportions? That is the question. How much of the finished article is due to the quality of the raw material? How much to the processes to which it has been subjected in the workshop? That is the point on which men differ.

Mr. Mudge has replied to my "criticism" of his "Plea for the Operation of a More Virile Sentiment in Human Affairs" at some length. From this I am led to suspect that I did not make my meaning so clear as I should have liked. And I feel constrained to beg for space to express my views more fully.

My short "criticism" was intended only to discuss the degree of permanence of changes produced in a human strain by alteration of its envi-

ronment, and did not attempt to deal with the general proposition raised by Mr. Mudge in his first paper, with much of which, indeed, I am in sympathy. I, too, deplore the fact that the criminal and degenerated strains in our race are allowed to perpetuate themselves and to multiply. But, then, I do not think that all the children of our slums are degenerate, but believe that many—probably the great majority—will grow into useful men and women if given a fair chance. Herein, I suppose, we differ. Human nature appears to Mr. Mudge less plastic than it does to me. And, consequently, I attach more importance to environment than he is willing to allow. And, yet, he too admits the influence of environment, for evil if not for good, when in his "Rejoinder" he claims that the children of the island, to which was sent the scouring of the Glasgow slums, became contaminated by the examples set by the latter. Again, I differ from him in thinking that we do not err in trying to cure the sick and to stamp out disease, for I am not convinced that there is any correlation in general between susceptibility to a given disease and other human weakness, excepting, perhaps, in such diseases as epilepsy and some kinds of insanity, and such as are due to intemperance.

My first paper, as I have already said, was intended to deal only with a single point raised by Mr. Mudge, namely, the degree of *permanence* of any improvement wrought in a given strain of human beings by purposely altering the environment of a

single generation. Mr. Mudge held that, do what we will to improve the health of mind or body, we can but influence the individual, when he dies the results of our efforts will perish also, for they cannot be transmitted to his progeny. With this proposition I could not agree, and I sought to show that the new environment might be handed on, like money or other property, and so continue to influence each succeeding generation in its turn. And as evidence of this I took two examples: A negative one, showing how an inheritable environment might continue to suppress an instinct, even if it did not ultimately destroy it; and a positive one, showing how the inherited environment might perpetuate an acquired accomplishment.

Mr. Mudge is not quite fair in challenging me to show that every inhabitant of this country possesses cannibal instincts. I really do not pretend to know whether he does or not. What I wrote was, "What does it matter if he does?" I assumed, perhaps rather rashly, that at some remote period our ancestors were cannibals; and I took it for granted that so thorough-going a Weismannist as Mr. Mudge would not admit that the cannibal instinct could be destroyed by civilisation. Since I did not see how else could it be eradicated, it must, I argued, if it ever existed, still exist. But it lies now deep down in our nature, altogether latent, and as good as extinct; and that because we inherit an environment, which we call civilisation, in which such desires are never awakened.

I myself think that it is more probable that this and many other primitive instincts have died out from disuse, for I am not yet altogether converted to Weismann's doctrine. But surely Mr. Mudge cannot accept such an explanation. Perhaps he would deny that our ancestors were cannibals; but even so, besides cannibalism there are many traits of character in primitive man which would have served the purpose of my argument equally well. To me it seems that man tends to progress in many, if not in all, ways, in the right direction. We differ essentially from our ancestors chiefly because we inherit an evolving environment which we are constantly moulding, and which moulds us in its turn.

I next attempted in my "Criticism" to show that an acquired accomplishment may be transmitted by means of an inherited environment. We do not inherit language through our germ cells; we learn it. The constant practice of speech by countless generations of man may, perhaps, have conferred upon us no advantage inheritable in the biological sense. Yet, because our forefathers have slowly and laboriously developed the art of talking, and our parents practice it, we pick it up in our childhood, almost unconsciously, through the example of those with whom we associate. And thus we inherit our language almost as effectively as if it was one of the unit characters embodied in our chromosomes.

Mr. Mudge, in his "Rejoinder," does not attack the general principle of the doctrine of the trans-

missibility of characters acquired as the consequence of a change of environment, by means of the change they produce in their turn on the inheritable environment of the race; but rather he proceeds to deny—what he seemed to have admitted in his first paper—that it is not possible by a change of environment to improve even the generation directly affected. This he does by describing at some length a disastrous "experiment" carried on for forty years by the Glasgow Parish Council—an experiment which he supposes is in accordance with my principles.

In this experiment, children were taken from the slums of Glasgow and transported to a certain island on the west of Scotland, and there, amid the sea breezes and scenic displays of that beautiful and romantic coast, were boarded out among the honest crofters, in the hope that they would become good citizens. But these hopes were doomed to disappointment. The children, for the most part, grew up to be very much the same kind of people they would have been had they remained at home. They, in fact, became the terror of the neighbourhood. They would congregate in bands at night time, link arms, and rush through the streets in serried ranks, shouting, whistling, and gesticulating, driving all before them. Where there were maid servants they would collect in groups and "*indulge in language which is not of the Highlands, but of the Glasgow slums.*" In a certain island, where at one time only ordinary human weakness prevailed, was "*heard*

the obscene language and suggestive songs of the slums." And so on ; the italics are mine. "What is bred in the bone will come out in the flesh," says Mr. Mudge. Thus, he would seem to ask us to believe that obscene language and suggestive songs are biologically inheritable, instead of being transmitted by bad example from bad companions.

The facts, as stated by Mr. Mudge, seem to me to show that the experiment failed, not because the Parish Council attempted the impossible by trying to improve certain individuals of a slum population by changing their environment, but *because it did not effectively change the environment.* For it is clear that the children took with them, not only the instincts of the slums, but its habits and customs. Mr. Mudge, indeed, tells us that " the children were sent into the island when quite young—some little more than infants ;" but some at least of them must have been old enough to have already learnt the language and songs of their native place, unless indeed they were allowed to revisit their first home from time to time, which should not have been allowed. Again, the Parish Council seemed to have failed through not making a proper selection. Surely some of the children to whom special reference is made might have been excluded with due care. But the most important point seems to me to be that they "swamped the place with an importation so great that seventy or eighty per cent. of the school population was made up of the imported element. In a word, the old and good environment

is swamped by the new and imported vicious one! And this by the people who believe in the influence of environment! Was ever so absurd a *reductio ad absurdum* of any doctrine before?" Thus, Mr. Mudge, and I thoroughly agree with him. The experiment was a thoroughly bad experiment. But how, then, can Mr. Mudge believe that it will convince any one of the uselessness of trying to reform human beings by transplanting them to a new environment, when he himself admits that in the experiment *the vicious environment was imported along with the children?*

May I remark, in passing, that Mr. Mudge seems to have rather a confused notion of the kind of environment which social reformers believe to be capable of improving human character. If, indeed, there be a school of Blue Sky Philanthropists, let me dissociate myself from them at once. It is not by "romantic scenery" nor by "sea breezes," good things though these be, that human nature is to be drawn fundamentally out of its course, but rather by the example of well-ordered lives. In a word, it is the human environment, and not the scenic one, which counts.

Mr. Mudge does not move me from my faith in environment by his tale of the three "half-witted" brothers who wrecked such disaster upon their unfortunate associates, for I recognise these as degenerates whom it is useless to try to reclaim. Such, as soon as they have given clear evidence of their nature, I should like to see segregated and

prevented from reproducing their kind. But it by no means follows that because we can do nothing to reform the true degenerates that it is hopeless to try and improve the average slum child. We might as justly argue that it is useless to send children to school because certain acephalic idiots are incapable of learning anything.

I do not believe that all slum children are degenerate; no, not even a large proportion of them—I share the Bishop of Stepney's liking for the independent street Arab who sells newspapers in our streets—and I believe that the majority are of the right stuff, but prevented from becoming good men and women by their miserable circumstances. What they most want is an environment filled with decent people, whether the scenery be romantic or otherwise.

I feel strongly about this; and I object to cold water being thrown indiscriminately on the efforts of social reformers. By all means let us have criticism, for of course mistakes are made, but the criticism to be helpful must not be merely destructive. The people of the slums are with us, and we have got to make the best of them we can. It is no use saying it is hopeless, for the only alternative, destruction, is not practicable. The limitation of procreation among the undesirable classes, excepting those who can be legally certified as criminal or degenerate, is not likely to become a matter of practical politics for many years. By all means, let public opinion be educated to accept the

sterilisation of the unfit; but, in the meantime, reformers must go on the old lines, guided by an ever-increasing experience to avoid old errors, and becoming bolder as success makes them feel their grasp of the problem gradually tightening.

A Plea for the Operation of a More Virile Sentiment in Human Affairs.

A Second Rejoinder.

By GEORGE PERCIVAL MUDGE.

Dr. Louis Cobbett, in his second reply, entitled "Heredity or Environment," to my "Plea for the Operation of a More Virile Sentiment in Human Affairs," asserts "that both heredity and environment admittedly go to the making of man." He then asks the crucial questions: "But in what proportions? How much of the finished article is due to the quality of the raw material?" In reply to these questions of Dr. Cobbett, I shall argue the thesis that in a sense, what a man *as an individual* is, does not depend upon his environment at all, but *wholly* upon his innate constitution. Dr. Cobbett and others who adopt his attitude, have apparently overlooked the essential fact, that the *capacity to respond* to the environment is as much an inborn character of an individual, as the colour of his hair or the possession of limbs instead of fins. Some men possess the capacity of responding, others do not; some have it in great measure, others in less measure. All the sentimental agitation that is fanning a great wave of unmerited emotion over the surface of an ill-instructed

and hyper-sensitive democracy, is wholly on behalf of an insignificant minority of defective and undesirable citizens who are devoid of this capacity to respond to a social environment which is sufficiently healthy and beautiful. We are living on the earth and not beyond its pale, and we must render unto the world the things that are of it. We can never have an environment that will be worthy of Paradise. It is sufficient for all purposes, that commingled with some features which we all regret but are powerless to avoid, there is in normal human life, much that is beautiful, inspiring, and noble. Civilisation has always been a mixture of good and evil, and it can never be anything else, until the day when Man can satisfy his hunger by noble thoughts and maintain his race without passion. Into this mixed environment, which is the same for all, men of varying innate potentialities are born. In virtue of their congenital attributes some respond in greater degree to the nobler influences of that environment, and others mainly or completely respond to its baser calls. If men possess certain inherent attributes they will *spontaneously* seek the appropriate environment to which those attributes are attuned. If they are, however, devoid of the requisite inborn qualities it is idle to *take* the environment to them. If Mahomet loves the mountain he will go to it. If he does not, it is folly to attempt to take it to him. In these two antithetic facts, is the one guiding *principle* of a rational social policy.

Nothing is farther from my desire than to appear dogmatic, but I cannot help thinking that in this central fact—that responsiveness to the environment is itself an inherent quality—is to be found the essence of the problem. Civilisation has not made men, it is men who have made civilisation. In the past as now, there were varying types of men. In Neolithic times there were the artists and the hunters, and doubtless there were men who were both. In the Lake dwellings there were men gifted with the attributes of thrift and who harvested in Autumn a store of apples and corn for the coming Winter, and there were thriftless men who were merry in the over-plentitude of the bountiful season and deservedly died in the barrenness of the unproductive months. They were all exposed to the same environment, but they were not all the same in their behaviour.

In my original article* I considered a very simple illustration in order that the essential relationship between inherent qualities and environment might be discussed, uncomplicated by unessential considerations. I described the behaviour of two plants, the Summer Savory and the Flax, when both were removed from an old to a new environment. These plants grow naturally under similar conditions in the Tyrolese valleys and elsewhere. But when they are removed to a high altitude on the mountains, the Summer Savory

*A Plea for the Operation of a More Virile Sentiment. Mendel Journal. Vol. I., No. 1, Oct., 1909.

responds to its altered conditions and lives, the Flax cannot respond and perishes. The new environment is common to both plants, but the difference in the behaviour of the two organisms is the manifestation of inborn constitutional divergencies, which the environment cannot alter nor even modify in the smallest degree. Since the environment *per se* cannot make the Flax respond, it follows that it is equally incapable of making the Summer Savory respond, and the response which this plant does manifest under certain changed conditions is therefore not environmental in *origin*, but is inborn in the organism.

Let us pass from the example of plants in the Tyrolese valleys to Man under social conditions. The same conclusion confronts us. In Knightsbridge, fifteen years ago, there was a Church wedged in between two public-houses. Both the latter were small and dirty; their atmosphere was heavy with the smell of beer and smoke; the floors were begrimed with dirt which cannot be described; torn papers, spillings of beer, discoloured sawdust, and disused cigarettes were commingled in an untidy medley. To anyone with any degree of inborn refinement, the surroundings must have been repellent. On Sundays, as well as on other days, the company within was of the type incident to that class of house. The speech was coarse, loud, gesticulating, obscene, and ribald; there was no order or priority of utterance and the words of each person were drowned in the prevailing din created by the

speech of all. For these "privileges," everyone who desired them had to pay. In the Church, on the other hand, was refinement of speech, habits of culture, quietness of atmosphere, an ordered service, mollient music, an inspiring Choir, and an endeavour to take the minds of men above the level of the bestial topics which constituted the mental staple of the visitors to the houses on either side. These things were free to whomsoever chose to have them. Will Dr. Cobbett tell us who commanded that some men should go to the Church, which was free, and that other men should congregate in the public-houses, which demanded a price of all who entered? Was it Parliament, or the Priest, or the Publican? If it was not by the edict of Man, will he tell us in virtue of what social influences it was that some chose public-houses and others the Church? The one costly and the other free! Can the experiences of mankind as expressed in its proverbs better answer the question than the sentimentalists and the "reformers" who have enshrined their hopes on the altar of futility? When, from the traditional experiences of life, the old proverb was formulated: "That birds of a feather flock together," did it then, and does it now, express the central fact in this problem of social life? Does it explain why some men love the public-house with its attendant features and others prefer the culture of the Church? Dr. Cobbett cannot contend that people went to the public-house because it was easier and cheaper, for the reverse is the truth. The Church stands with

extended arms and almost prays to every passer-by to enter its sanctuary. The public-house has no missionaries travelling throughout the land, pleading to men to enter and drink of its beer, to partake of its coarse talk, and to inhale its noisome atmosphere. The Church has sent forth its apostles into every hamlet in the land, pleading with men to desert the one path and to follow the other. But in spite of it all the public-house has still its votaries. We may destroy the drinking-house but its votaries will not therefore go to Church, because their inborn desires will soon convert their own homes into a small type of public-house.

These considerations lead us to a corollary. Neither the Church nor the public-house *create* their votaries. Both are merely centres whither men with different inherent aspirations and tastes do congregate. It is not the Church which makes people good, but it is the assembling of the "good people" which constitutes the Church. The inborn aspirations came first, the Church afterwards. Similarly with the public-house. It does not produce the qualities which make drunkards and loungers ; it simply does what the Church does, and offers facilities for gratifying certain inborn desires. It satisfies very different desires, of course. The safety of the Church, therefore, like that of the nation, depends not on its missionary efforts but on the nature of the citizens whose birth it encourages. The Roman Catholic Church, which is making great attempts in this country to obtain adherents among the children of the masses, is indeed

but laying the foundations of its discredit and its destruction. It imagines—if we may judge its beliefs by its deeds—that it can make worthy men out of the children of unworthy races. It will achieve for itself what modern social sentiment is fast accomplishing for the nation as a whole: the ultimate disruption of Society composed of civically defective materials.

But let us further illustrate the untenability of Dr. Cobbett's contention. He implies that in some way the environment *per se* has the power to mould men and their deeds.* I have attempted to show that any apparent influence exerted by the environment is in reality due to an inborn capacity in the individual to respond to it. Some have a capacity to respond to one sort of environmental stimulus and some to another. Some cannot respond at all. Let us advance that line of argument deeper into social life. Churches and public-houses are old institutions. Music-halls, cinematograph shows, and County Council Scholarships are the growth of more modern times. At all times of the year London and other cities afford certain inducements and attractions to people of all ages. Among these we may mention two contrasted forms. They are as different as the Church and the public-house, but in a rather different manner. The contrast, however, is the same in this respect, that whereas everyone who enters a music-hall has to pay for admission, anyone who desires to enter an evening class for "self-improvement" in

* By men is implied individuals, not races.

a wide range of subjects, from the science of Biology down to the simple art of handling a carpet broom and a dust-pan, or accepting a tip as a waiter,† can do so for nothing.‡ Indeed, the aspiring "scholar" will even be paid to join a class by a pleading and anxious County Council, very dubious as to the existence of any real desire upon the part of the community for the intellectual wares it desires to scatter broadcast, and very indifferent to the waste of public money virtually entrusted to its care. To take a concrete example, we have then, let us say, in London, existing side by side, music-halls and evening classes. Let us pass by any of the former at six in the evening, and long files of persons, stretching down the main street and round into the bye-streets will be seen. It is a common sight occurring every night. When we pass to the evening classes there will be found no long files in the streets. The classes are small; in some cases so small that there are more teachers than scholars. This dearth of pupils is in spite of what has now almost become a custom of paying any student who cares to ask for it, five pounds or more a session, ostensibly to pay travelling expenses. Even of the relatively small number of students who attend the classes, a certain deduction must be made of those who have no serious intention of studying, and who go for various reasons. Some of the women students attending evening classes

† The London County Council, not satisfied with having an Education Rate nearly 2s. in the pound, or with the national expenditure of £25,000,000 a year on education, from the rates and taxes, has recently decided to waste more public money on classes for the training of waiters.

‡ In some cases the fees are merely nominal.

conducted by men teachers and containing men students, have much more interest in matrimonial possibilities than in academic considerations. The ratepayers are compelled to pay for matrimonial potentialities, and not for educational kinetics. Some students merely want to while away an evening or two in the week. They may think, for instance, that a class in Elocution will be a delightful way of passing time and making acquaintances with a subsequent view of consolidating the friendship on the altar. How serious the intentions of some students are, is well exemplified by a case for the accuracy of which I can vouch, when a few years ago, two prospective and obviously not earnest women students joined a class in Elocution only to find that it was full. With significantly spontaneous readiness they at once transferred their "studies" to a subject of wholly different nature, Biology. The teacher of this latter class, naturally curious as to such a transference, enquired why they had joined his class. They replied "that they first joined the class in Elocution because they wanted something to do, but finding all the classes in that subject full, they thought Biology would be just as good as any other class!" They were asked if they had any idea as to what kind of subject Biology was, and they replied: "Not in the least." Needless to say their "studies" were not marked by diligence or enthusiasm, and if they manifested any attention at all in class, it was not to the work in hand, but to an object more easily attained at other places of

entertainment. But I am not now concerned with the defects of this rate-endowed and tax-endowed system of education, in which certain people may play loose with the money of other citizens. It is sufficient to show that comparatively insignificant as the attendance at these classes is, that even then "all is not grist which comes to the mill."

It will suffice to notice that many students attend evening and other classes, provided for them out of public money, for other reasons than a burning desire for knowledge or culture. But there remains what is for us the cardinal fact, that the "educational" classes are sparsely attended and the music-halls are full. Why? The "educational environment" is open to all. Indeed, under the ægis of County Councils and such like bodies, it pleads with open coffers in the form of so-called "Scholarships," to whomsoever will to come. Many are called, but few respond. And of that few more are moved by aspiration than by inspiration, and will later vent the consequences of their incapacity and the disappointments of their chequered aspirations upon a community which foolishly encouraged them in the attempt to traverse the plains of Cirrha and the hills of Cithæron.

They will eventually turn upon the community which fed and educated them and become the demagogues of street corners or even of more eminent positions. That is the inevitable result of attempting to force a higher type of environment upon a lower type of mind. I do not of course deny that a few of

the students who attend evening classes do so with a genuine desire to acquire knowledge. Neither is it denied that a few possess an inherent capacity which enables them to reap the educational benefits that are forthcoming. But these students would in any case have obtained an education fitted to their merits and have been able to pay for it in the ordinary way. They belong to cultured Middle-Class families and for them rate-aided classes are entirely unwarranted. All that is achieved by this modern sentiment which sets out to provide educational facilities for the masses, in much the same way that it has before in history provided circuses for them, is the destruction of many Secondary Schools by a scarce veiled competition with ratepayers' money, which prefer not to pauperise their pupils by accepting doles from the rates or the State.

University College School, London, has learned the bitter lesson that environment cannot alter the inherent defects of the children of the masses. It cannot even do it when potential men are at the plastic stage of boys. It made a generous experiment but the processes of Nature have nothing to do with noble aspirations nor with false hopes, and the "Hammer of Thor" was fast falling upon its mistaken intentions and futile aims. In considerate language that had to be read between the lines, the Council of this School had to admit the failure of the attempt to convert by environmental influences, the morally defective instincts and limited intellectual capacities of the children from the elementary schools. And

with this case and others which I could mention before us, Dr. Cobbett's plea that the "street arab" only "needs a good environment and the influence of decent people" to make him a desirable citizen, falls hopelessly to the ground. In the light of facts it is untenable; the voice of tradition has long since condemned it.

But University College School does not stand alone in its experience. I have made enquiries of teachers at Grammar and other Secondary Schools, and from all my informants come the same report, variously expressed. I am informed that these boys have no idea of the use of freedom and never learn its meaning. The average fee-paying boy of the Secondary School may be allowed his freedom and will not turn it into licence. That is not the case with the boys sent up at an early age from the Elementary Schools. They need to be continually supervised. They are a constant menace to the morale and morality of the School. They are incapable of appreciating the beauties of literature, and some of the finest passages in the English Classics are parodied in ribald and revolting language. Even Tennyson's "Charge of the Light Brigade" has not escaped their prurient minds, and I was once shown by a Secondary School teacher a most repulsive parody upon it. The boy who wrote it was caught reading it out to the whole class of fee-paying students at that school. This is not an accident due to carelessness, for it occurred in a Secondary School where even undue care is taken to assimilate the

County Council scholars of the Elementary Schools with the fee-paying students; where they are, in a sense, treated with special favour, and every effort that the circumstances of a misguided sentiment will allow is made to fulfil a futile hope. But it is in vain. Indeed, the longer the elementary scholars are at a Secondary School, the wider and more apparent become the intellectual, moral, and physical divergencies that mark them off from the Middle-Class fee-paying students. They are in human life the homologues of the "sprinters" among horses, which, the more they are trained and oxygenated, the worse become their running powers.* It is not only Secondary School teachers who say that the Scholarships awarded in London and elsewhere to Elementary School children, are wasted alike financially and in intellectual and moral results, but teachers at the Elementary Schools tell me the same thing. They know these children when they leave their school to go to the Secondary School, and in many cases they are able to follow their subsequent career when they come down from the Secondary School to enter life. What do they become? After they have received a secondary education which costs the ratepayers of their district about one hundred pounds a year, not counting the whole cost of officials, and having remained two or three years at the Secondary School, they become milk-boys, errand-boys, and so on! They will never adopt anything else than a milk-

* See Mr. Robertson's article in this Journal, on the "Inheritance of Staying Powers in Thoroughbred Horses," p. 76; and the Editorial Article appended to it: "Some Sociological Considerations, &c.," p. 98-100.

man's calling or some similar occupation. And as a head-master of one of London's Elementary Church Schools recently expressed it to me: "The County Council Scholarships are an absolute waste of public money and the boys are not benefited by them at all. Far better," he said, "not to have made them discontented with a lot from which it is impossible for them to escape." After the community has made this expensive effort to raise them to a higher standard, they sink again to the level in which they were born and to the station to which their congenital attributes inexorably anchor them.*

Unconscious of their own defects, noting only that men with other modes of thinking and other desires of living belong to another kinship in feather, and remembering that once they were artificially forced into that kinship, but omitting to see that they have failed to remain within it, for lack of the requisite attributes, we need not be surprised that they go back again to their own class with an embittered spirit, only to wage war on the community which kindly but mistakenly endeavoured to force them out of their natural position. "*Qui delicatè a pueritia nutrit servum suum, postea sentiet eum contumacem*" is an aphorism worth remembering in public as in private life."†

* The letter of Mr. Bourne Benson, Chairman of the Council of University College School, to *The Times*, of March, 1911, conveys the same information. He says the scholars "are compelled in the end to earn their living in some humble employment of lower *status* and of less remuneration than the work they would have been likely to obtain, if they had devoted the same ability and the same time, at less than half the expense, to learning a trade."

† "He that delicately bringeth up a servant from a child shall find him insolent at the last." Francis Bacon. The Advancement of Learning. Second Book.

Dr. Cobbett says that the "street arabs" of the slums only need the influence of "decent people" to make them respectable and useful citizens. I suppose Dr. Cobbett will assent to the proposition that what is true of "street arabs" must be truer still, or at least as true, of more successful people. For if he does not accept that proposition, then he is asking us to believe that the works of social evolution have been cast out of her mould upside down, and that the good, noble, and responsive people have sunk into the slums, while the evil, vicious, and irresponsive people have risen to the higher civic strata of Society. Let us see, then, whether the influence of "decent people" is real or imaginary. The train of ideas underlying the term "decent people" may of course be applied to many things. We may consider it in relation to good taste. If we pass through the very respectable portions of any of the suburbs around London during the Summer months and on Sundays, we shall occasionally hear coming through the open windows of some even of the larger houses, evidently well furnished and with well-kept gardens, the discordant and metallic vibrations of the gramophone. These spread far and wide, and gratuitously impinge upon the hearing of everyone in the neighbourhood, whether they care for such entertainment or not. Now, no stretch of the imagination upon the part of people with good taste can possibly regard these vibrations as beautiful or inspiring. They are not melodious; they are merely pulsating volumes of clashing noises. In some cases the songs which have been chosen for emission by the gramo-

phone are not of the classical order ; they convey no lofty thoughts, they do not appeal to the higher side of man, and if they have any meaning at all, it is that of a suggestive lewdness. The bad taste which it all implies is accentuated by the fact that these displays occur on a Sunday morning. It is no extenuation to urge that if certain people do not care to go to church they are free to disturb the peace of the Sabbath which other citizens hold sacred. They have no more right to do it than has the Salvation Army, with its nondescript and brazen band of civic and moral weaklings, to render Sunday afternoon hideous to eye and ear alike. The very fact that the feelings of others have been in this blatant manner disregarded is simply another form of bad taste superimposed upon the first. Will Dr. Cobbett tell us why these people manifest such bad taste ? He cannot plead in these cases the misery of their lot, the poverty of their lives, or the absence of decent people among their acquaintances. None of these things explain it. It is not Parliament, nor the Priest, nor ignorance, nor the absence of all accessories which relate to culture, nor a vicious social environment. It is the inherent obtuseness in regard to matters of taste of the persons concerned. Their lack of good taste is inborn. A momentary look at their faces and deportment is by itself sufficient evidence, for from the hungry lion and the wolf up to Man, Nature frequently implants her meaning on the physiognomy and the motions of her higher forms of life.

To make any artificial effort—whether compulsory through the State or voluntary through " Waif and Stray Societies "—to raise "street arabs" from their natural position to higher social or civic rungs would, if even it could be successful in a sense, simply result in swarming the ranks of the Middle Classes with a larger number of unculturable Philistines of the " gramophone order." We do not by such means save the street arab ; we do not convert him ; we simply lower the general civic standard and the degree of culture of a higher social class by diluting it with grosser waters. We have only to recall a certain notorious and uncultured politician and the general run of his platform companions, and to carefully consider their speech and other performances, in order to perceive how little contact with " decent people " can influence inborn uncouthness, and to instinctively recognise how grave is the danger to the continued social stability and refinement of the social class into which these persons have been intruded, by the incited passions of Democracy. Just as dead and worthless leaves, through no merit of their own, are elevated by the Autumn winds, so under a democratic regime, in which from the very nature of things the voice of the uncultured masses is most heard, the politicians who are most successful in arousing the acclamations of the mob are not the men of culture, but the men of inborn uliginous and zanic instincts. Not even years of contact with cultured men, either in the House of Commons or in social life, serves to convert the uncouth buffoon into anything else than what he was

born, or to modify in the least the traits of his race. So long as these very patent facts obtrude themselves upon our notice, it is vain to contend that the undesirable traits of "street arabs" or of other persons are due to the absence of an environment composed of decent people. That otiose hypothesis fails as completely as that of the "Blue Sky Philanthropists," the moment we look impartially and dispassionately at the hard and ugly facts of life.

The mention of the "Blue Sky Philanthropists" recalls to me the objection which Dr. Cobbett associated with this, his own term. He says it is not by romantic scenery, nor by sea breezes that human nature is to be fundamentally altered. Dr. Cobbett asserts that social reformers do not believe this sort of environment can improve human character. Well, that again depends upon the person. An introspective examination by any one will show how much or how little it influences us. I must confess I have been at times deeply stirred by a glorious sunset, and subdued to an emotion of intense awe by some mighty and silent landscape. More than once when I have been alone on an angry sea in a tempest of wind, I have learned lessons which have so deeply impressed me that they have permanently moulded one line of my thought. The chief of these lessons King Canute is traditionally reputed to have learned. The tide, which neither rises nor recedes until its appointed hour, is but symbolic of the processes of Nature, which are pitiless, inexorable, and unrelenting. Nature cares nothing, neither for our regrets, nor hopes, nor

aspirations, nor futile schemes, nor castles in the air, nor roseate delusions, nor generous intentions; our lives may be young and with all their promise yet before us; we may have work we want to finish, and a cause we desire to advance before we die. But what of these things? Does Nature heed them and our vanities? We come face to face with her angry mood in a cockle-shell of a boat, thinking, like social reformers and the courtiers of King Canute, that the laws of Nature will cease operating to save us from the consequences of our own folly. Nature does nothing of the sort; on the bark of our own venturesomeness, while yet we recall the lives that are precious to us and the work we have not finished, the wind increases in intensity, the billows break more angrily as our craft drifts nearer to the rocks upon its lee. The rocks neither melt nor soften for us; they are adamantine and enduring. One thing alone can save us; and that is to intimately know the forces with which we are dealing, the capacities of our craft, and the degree of reliance we can place upon ourselves. For there are no social reformers, nor County Councils, nor Medical Inspectors, nor vote-catching Chancellors who think they can save incompetent or cowardly navigators. It is then we learn that political buffoons playing before an electorate of merry-andrews, are fools if they believe that Nature will turn aside her relentless processes because of their zanic antics; and, if they do not believe it, it is then we recognise the deep pathos of the imminent tragedy which their

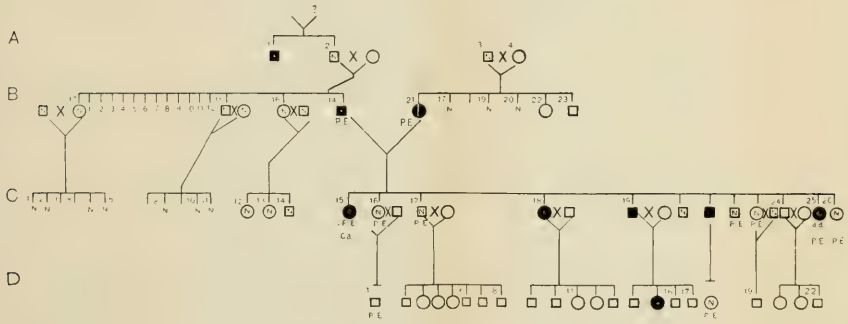
roguery will inevitably impose upon their dupes and their countrymen.

I wish every honest social reformer would for once in his life go out alone in an open sailing boat, when the wind is blowing a gale and is against a Spring-tide at half level, and the coast is on his lee. He would, if he came back safely, have more respect for those processes of Nature which in his optimistic propaganda on terra firma he so completely ignores. But if he has no other equipment than noble intentions and laudable aspirations, he had better remain on the shore until he has substituted for them a stern determination to face the waves, winds, tides, and rocks as realities from which there is no escape.

But Dr. Cobbett is mistaken in believing that there are no "Sky Blue Philanthropists." They are everywhere. Ruskin was among them. In the chapter on the "Deteriorative Power of Conventional Art" in "The Two Paths," he contrasted the influence of the "Gilded Temples" of Indian cities upon Indian Art, and of the Craig Ellachie at the western foot of the Grampians upon the Scotch character. "The Art of India," he wrote: "never represents a natural fact. Over the whole spectacle of creation the Indian people have thrown a veil in which there is no rent. For them no star peeps through the blanket of the dark—for them neither their heaven shines nor their mountains rise—for them the flowers do not blossom—for them the creatures of field and forest do not live. They lie bound in the dungeon of their own corruption, en-

compassed only by doleful phantoms, or by spectral vacancy." On the other hand, he describes the influence of Scottish scenery upon its people in these words: "You will find, upon reflection, that all the highest points of the Scottish character are connected with impressions derived straight from the natural scenery of their country." Can "Sky Blue Philanthropy" express itself in clearer language than that? In a sense, Ruskin was right. It is the inherent desires of the Indian people which have created their special type of art, of which their temples are but symbols. It is the inborn emotional character of the Highlander that he loves to contemplate the majestic and rugged hills of his native country, and to find his thoughts expressed by the art of his country in the ballads of Burns and the novels of Scott. That is the only real "connection" between scenery and human impressions. But that is quite a different thing to supposing that the vision of Craig Ellachie can convert the congenital race of "street arabs" of the slums of Glasgow into respectable and useful citizens. That piece of extreme social folly was reserved for the Glasgow Parish Council and for potential officials who sought some plausible pretext to create new salaried appointments.

PEDIGREE OF LENTICULAR CATARACT



Some Cases of Inheritance of Human Degeneracies.

By Dr. W. J. RUTHERFURD.

To no one more than to the medical practitioner do opportunities of tracing the hereditary origin of pathological defects or physical abnormalities in human subjects present themselves. Almost every week some case comes to his professional notice, which if patiently traced would reveal an example of the inexorableness and inevitableness of the hereditary process. Behind the old adage "that what is bred in the bone comes out in the flesh" lies the eternal truth of inheritance. That this truth has been so long ignored in practical affairs is no doubt due to the existence of a belief in the kindly influence of environment which is based on a misconception of the facts. No favourable environment acting on "flesh" can change bad "bone" into good bone.

But while a medical practitioner may have many chances of observing the hereditary mechanism at work, it is seldom that he can find the time to pursue such observations with that exactitude which scientific treatment demands, and with that detail without which general conclusions can be but tentative at the best.

A few years ago several interesting cases presented themselves to my notice, and the current circumstances enabled me to make enquiries into the family histories of the patients concerned. I was able to personally diagnose the diseases and to observe the conditions, in many cases, among individuals in two or three of the several generations whose history I was able by enquiry of the patients and their relatives to record.

An Instance of the Inheritance of Lenticular Cataract from Both Parents.

The occurrence in a family group of a transmissible defect of which it can be definitely asserted that the condition is shared by *both* parents is admittedly rare. When it occurs in the human race it is sufficiently important from a biological standpoint to render it advisable

to place such a case on record, so that the details may be accessible to workers in the comparatively new field of experimental heredity.

In the family here recorded (using the word family in its colloquial and more restricted sense) lenticular cataract occurred in childhood or adolescence in five out of twelve persons (C, 15-26). Another member of the family died many years ago while still an infant, and as naturally no information is available as to him, he cannot be included in the present calculation. Of the five affected members of this family in generation C, I examined the eyes of two. In addition I examined the eyes of the two affected parents in generation B. Of the six living unaffected individuals I was able to examine five at one time or another. A seventh member died at the age of twenty-four years. The presence or absence of the defect in the persons I was able to examine may therefore be taken as definitely settled. All members so examined will be found indicated in the explanatory description attached to the chart. Three of the affected individuals (Nos. 18, 19, and 21, generation C) in this family were married and had children, the offspring, ten in number, being aged from about seven years to twenty. One of these, No. 15, D generation, as will be seen later, seems to have inherited the affection under consideration. Five of the unaffected members of the family group are married. One has no family ; the children of the others, twelve in number and aged from a twelvemonth to twenty-two years, seem all to have escaped the condition up to the time when these details were gathered.

An investigation into the family histories of the parents of these five affected persons (C generation) revealed very little. In the mother the cataract had apparently arisen *de novo*.* The father seemed definitely to owe the origin of his condition to the ancestral germ-plasm, as, although his own immediate parent was unaffected, the somatic stigma had displayed itself in one of the members of that generation. The nine cases in the four generations are briefly described below.

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* See Editorial note, p. 184.

This is one of the very few cases where both parents [*i.e.*, Nos. 14 and 21, B generation] suffer from the same transmissible disease.*

The Facts of the Pedigree.

Generation A.

No. 1. The uncle of B generation. An old man, now dead. His symptoms ensued when he was more than 80 years old and progressed to total blindness. It was graphically described to me how when he wanted to know the time, he would grope his way from his chair to the corner where the clock stood and feel over its face to find the position of the hands.

Nos. 2, 3, and their marital partners. No history of lenticular cataract or blindness.

Generation B.

Nos. 1 to 12. No history to record. Are said to have been normal.

No. 13. Married. Had six children. Mother, father, and children not affected. Her vision remained quite good up to 78 years of age.

No. 14. The nephew of No. 1, A, and father of Nos. 15 to 26 in generation C. When I first saw him within a few months of his death from apoplexy at the age of 76 he disclaimed any eye affection except a long-standing and steadily progressive presbyopia for which he was wearing glasses. Examination, however, revealed the presence of unripe cataract in both eyes.

Nos. 15 and 16. Both married normal and unrelated partners and had normal children.

Nos. 17 to 20. As far as known there is no evidence of lenticular cataract or of blindness.

No. 21. The wife of No. 14, B. She is not related by blood to her husband. Age 70 years. Cataract in the left eye for the past ten years. There was no history of traumatism to the eye. Her cataract has apparently, as already suggested, arisen *de novo*, as no indications

* Compare Mr. E. Nettleship, "Three New Pedigrees of Senile Cataract," *Ophthalmological Society's Transactions*, vol. xxviii., 1908, p. 224, where he describes the first instance known to him of the same heritable *eye* disease present in *both* parents.—[Editor.]

of it appear to have shown themselves in either of her parents, Nos. 3 and 4, A.

Generation C.

Nos. 1 to 14, 20, and 24 were almost certainly normal. I was able to examine Nos. 16, 17, 22, 23, and 26. They were quite normal.

No. 15. Affected daughter of affected parents Nos. 14 and 21, B. She died of pneumonia at the age of 50. Had double cataract as a child. One eye had been operated on successfully when she was twenty-four years of age. The other eye was operated on twenty-one years afterwards, with the result that sepsis set in, and enucleation became necessary. At the age of forty-eight she suffered from mammary carcinoma, which was extirpated a year later. It was probably recurrent in the dorsal vertebræ before death. There was no post-mortem examination.

No. 18. Sister of No. 15, C. Cataract in both eyes since childhood. Operated upon at the age of forty years.

No. 19. Brother of Nos. 15 and 18. Father of No. 15, D. Had double cataract since a child at school. Operated on at thirty-five years of age.

No. 21. The seventh child. He lives abroad and is inaccessible. Had double cataract since childhood. Was operated upon when twenty-four years of age, at about the same time as most of the other affected members of the family.

No. 25. The twelfth child had double cataract. The condition does not seem to have appeared till she was eighteen years old. After the cataract had been present for five years the right eye was operated on. In the spring of 1909 she died, at the age of twenty-eight years, of Addison's disease.

Note.—In the family Nos. 15 to 26, C, there was a child who died in infancy, and he is not included in the Chart, as no information is available concerning him.

Generation D.

All the members of this generation are yet quite young. The defect may appear in them at a later age.

But so far only one individual, No. 15, manifests any impairment of vision. Her father, No. 19, C, suffers from the defect, but not her mother. She is only eighteen years of age, and for some years back has progressively been becoming blind; her relations recognise the condition as being the same as their own. No opportunity for examination of this particular member presented itself.

General Considerations.

This clanship illustrates what Nettleship and others have called "anticipation" in the appearance of the cataract. The period at which it first becomes evident is progressively earlier in the more recent generations. Thus in the A generation it seems to have appeared when the patient was over eighty years of age. In the B generation it manifested itself at about seventy-six years of age. In the C generation it showed itself at ages varying from childhood up to the age of eighteen. In the latest generation the symptoms have also put in their appearance during adolescence.

No deductions will here be drawn from the frequency of the condition either in the entire stock or in the family where both parents were affected (seven unaffected to five affected, and one doubtful), but it is noteworthy that in the instance recorded by Nettleship,* of a cataractigenous family where the condition was evident in both parents, there was a similar occurrence of several individuals free from the organic defect among the offspring.

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* *Loco citato.*

Chart indicating the Family Incidence of Lenticular Cataract.

The square symbols indicate males, and round symbols females.

Where there are numbers but no symbols it means that the sex of the individuals indicated was not ascertained.

The solid black symbols indicate the affected persons.

N within the symbols indicates normality of the individual so far as cataract is concerned. Without a symbol it means that, as far as I could ascertain, none of the persons indicated had suffered from blindness in any form. In many of these cases I merely ascertained their number but did not obtain other details, such as their sex.

When no letter is within the open symbol it means that no definite information as to presence or absence of cataract was forthcoming.

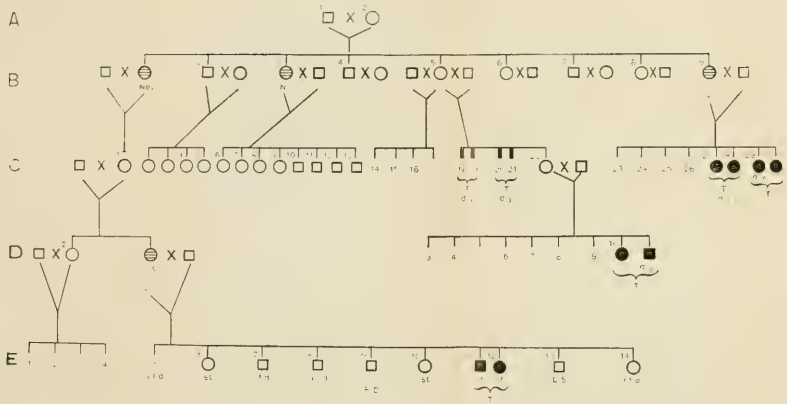
The numbers to the top left corner of each symbol indicate the relative ages of the individuals and also serve as the reference number. Among the affected, Nos. 14 and 21 in the B generation, and Nos. 15 and 25 in the C generation, were personally examined by me and their condition accurately diagnosed. Among the unaffected I similarly examined the following: In the C generation, Nos. 16, 17, 22, 23, and 26. In the D generation, Nos. 1 and 18. Others were seen at various times and either refused to allow their eyes being examined or else no suitable opportunity for so doing presented itself.

P.E. beneath the symbol means that I personally examined the eyes of that individual.

a d = Addison's disease (died of).

C a = Cancer of breast, recurring later in dorsal vertebræ.

PEDIGREE OF TWINS. FAMILY A



The Occurrence of Twins in Successive Generations.

By Dr. W. J. RUTHERFURD.

Pedigree A.

The following Pedigree shows a decided tendency to the production of twins in three succeeding generations. It is but one of several of a similar nature which I have been able to obtain.*

In this Pedigree are twelve twin children, of whom eight died in infancy or early childhood. This is a heavy mortality and is doubtless significant of an inborn feebleness of constitution. A glance at the Pedigree and at the statement of facts will at once show that the greater part of the stock here represented is to some extent degenerate. Stillbirths, death with fits in infancy and other evidences of nervous instability such as laryngismus stridulus, taken together with vesical calculi and nephritis so frequently recurring, is indicative of a sub-normal race.

The person from whom the details of this clanship were obtained realises that among her relations the first-born children are not twins, but that these tend to be produced towards the end of the family. There may or may not be significance attaching to this, but that is a question I propose discussing later on.

The Facts of the Pedigree.

Generation A.

Nos. 1 and 2. Nothing of importance is known of these two people.

Generation B.

No. 1. A daughter of Generation A. Died of nephritis. Had only one child.

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* Dr. Rutherfurd tells me that in several instances, in both Pedigrees A and B, he has had to rely upon the oral tradition of the family for his information.—[Editor.]

Nos. 2, 4, 6, 7, and 8. Nothing is known of them, except that they did not die of nephritis. Their sex, and the families of some of them are shown in the pedigree.

No. 7 had no family ; Nos. 6 and 8 had families, but no information was forthcoming as to them.

No. 3. Died of nephritis. Female.

No. 5. Married twice. Had twins twice (*i.e.*, Nos. 18—19, 20—21, Generation C) by her second husband. Seemingly did not die of nephritis.

No. 9. Eldest daughter of Generation A. Suffered from vesical calculi and died of nephritis.

Generation C.

No. 1. The only child of No. 1, Generation B. Not known to have died of nephritis.

Nos. 2—17 and 23—26. Not known to have died of nephritis or to have had vesical calculi.

Nos. 18—19 and 20—21 were both sets of twins. All four died young.

Nos. 27—28, and 29—30 were twins. Of these Nos. 27, 28, and 29 died young. The other was still living in 1911 and was then twenty-six years of age.

Generation D.

No. 1. Grand-daughter of No. 1, B Generation, who died of nephritis. The present subject suffered from vesical calculi, incontinence of urine, and varicose veins. She is about thirty-eight years of age.

Nos. 2 and 3—9. No details known. Not known to have suffered from nephritis or vesical calculi.

Nos. 10 and 11. Twin children. No. 11 died young. No. 10 was still living in 1911 and was then nine years old.

Generation E.

Nos. 1—4. No details known. Not known to have died of or suffered from nephritis or vesical calculi.

Nos. 5, 7, 8, and 14 died in infancy from fits. No. 8, in addition, had a malformed cranium. Nos. 6 and 10 were stillborn.

No. 9. Had fits in infancy, and has suffered from Raynaud's disease.

Nos. 11 and 12 were twins. There were two afterbirths. Both had fits in infancy. Both living in 1911, and then nine years of age.

No. 13. Suffered from laryngismus stridulus.

Explanation of Pedigree A.

Occurrence of Twins in Successive Generations.

The symbols and numbers have the same significance as in the Pedigree for Lenticular Cataract (p. 172). The solid black symbols indicate twins, where the sex is known. Where tradition was defective, as in Nos. 18, 19, 20, and 21, C Generation, the thickened fraternal (vertical) lines have the same meaning.

Symbols with cross hatching indicate persons who have had nephritis or vesical calculi or both.

d.y. = died young.
i.f. = suffered from fits in infancy.
i.f.d. = died from fits in infancy.
I.U. = incontinence of urine.
L.S. = suffered from laryngismus stridulus.
Ne. = died of nephritis.
R.D. = suffered from Raynaud's disease.
st. = stillborn.
T. = twins.
V.C. = suffered from vesical calculi.
V.V. = suffered from varicose veins.

Pedigree B.

Occurrence of Twins in Successive Generations.

In this clanship of thirteen families, twins have occurred in no less than six, eight sets of twins having been born to these six families. In five of the families twins occurred only once, but in the other family of the twin-bearing group twins were born thrice.

Unlike the case shown in Pedigree A, there is in this one no uniform tendency to the production of twins only towards the end of the family. In one case only of the six is this shown, *i.e.*, in Nos. 64 and 65, Generation D. In the other families they occur indifferently. Nos. 18 and 19, 27 and 28, Generation D occur near the beginning of the family in each case, appearing

in both families at the third birth. Nos. 13—14, Generation C, occur in the middle of a large family, and Nos. 27—28, and 31—32, and 37—38 of the same generation, were pretty uniformly arranged throughout the household of twenty-one children.

In this Pedigree, too, there is a further difference as compared with Pedigree A. In the present case, the twin-bearing capacity seems evidently capable of transmission through the male as well as through the female, and not exclusively through the latter as in Pedigree A. From the construction of the Chart it seems clear that the quality of producing twins came from Generation B, though no member of that generation was one of twins. And this generation must equally have inherited it from one or other parent in Generation A, concerning which little is known. But in Generation B, No. 4, a male transmitted the twin-bearing capacity which is manifested in Generation C, in Nos. 27—28, 31—32, and 37—38. In the case of all the other members of B Generation, however, the quality was transmitted by the female. And in Generation C it was similarly transmitted by females.

The Facts of the Pedigree.

Generation A.

Nos. 1 and 2. The common ancestors of the thirteen families shown in this pedigree.

Generation B.

Nos. 1—4. Nothing of importance was ascertained. This is an interesting family group, as every member of it has transmitted what has been described above as the quality of producing twins.

Generation C.

Nos. 1—8. Of these children, the two at the last birth but one were female twins.

No. 9. A male child. Was burnt to death in childhood. This is merely recorded here as presumptive evidence of parental neglect.

Nos. 10, 12, and 16. A male and two females respectively. Nothing of importance at present known.

No. 11. A female. Became insane, and is in an asylum. She had an illegitimate child, *i.e.*, No. 2 of D Generation, before marriage. This child is an epileptic.

Nos. 13 and 14 were female twins. No. 13 is eccentric.

No. 15. A female ; also eccentric.

Nos. 17—43. Of most of these there is nothing at present worthy of record. The chief features of importance are two :—

First : Nos. 27—28, 31—32, and 37—38 were twins. Nos. 27—28 were female twins ; Nos. 37—38 were male twins, and Nos. 31—32 were dissimilar in sex.

Second : Of the huge family (Nos. 23—43) of twenty-one members, only two are alive, *i.e.*, one daughter in the first pair of twins, and one other child, most of them having died early.

Generation D.

No. 1. Nothing of importance as yet to record.

No. 2. An epileptic and illegitimate child.

Nos. 3—15. Of the twelve children of this family, ten are now dead and several were stillborn.

Nos. 16—24. Nothing to record, except that Nos. 18 and 19 were female twins.

Nos. 25—33. Nothing of importance to record, except that Nos. 27 and 28 were twins of dissimilar sex. Both were stillborn. Nos. 29 and 32 were also stillborn.

Nos. 34—48. Nothing to record from the point of view of the subject under present consideration.

Nos. 49—59. Of these eleven children four are dead.

Nos. 60—65. Nothing to record except that Nos. 64 and 65 were female twins.

Explanation of Pedigree B.

The symbols and numbers have the same significance as in the Pedigree for Lenticular Cataract (p. 172), and in Pedigree A for Twins (p. 175). The cross-hatched symbols, however, have here a different significance. They indicate insanity or epilepsy.

Ec. = eccentric.

Ep. = epileptic.

D. = dead. The figure preceding the D indicates the number dead.
Il. = illegitimate.
T. = twins.
Sev. st. = several stillborn,
st. = stillborn.

Pedigree C.

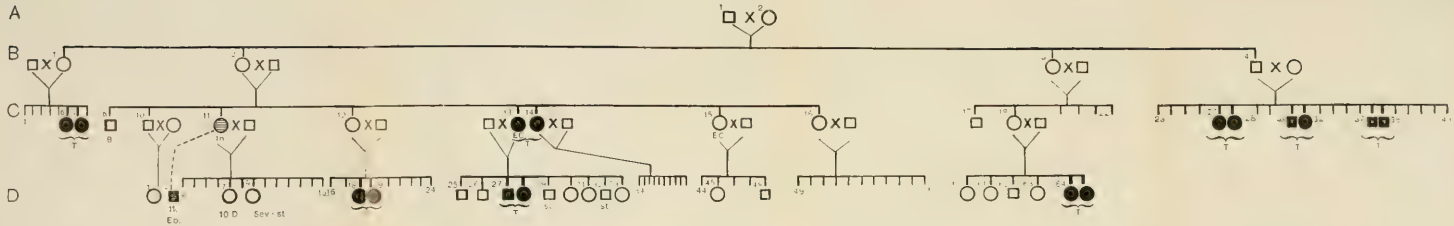
In Pedigree C we have the results of a study of the inter-relationships of four twin-bearing stocks, among which twin births may be seen to have occurred at least ten times.

A reference to the chart will show how a man (B. 5), whose sister (B. 7) was grandmother of homosexual male twins (D. 66 and 67), marries a woman (B. 15) two of whose nieces (C. 20, 22) had each given birth to twins (D. 75, 76 ; and 77, 78). Of this union there was an only child, a daughter (C. 8), who in her turn married into a didymogenous stock, her husband (C. 7) having a brother (C. 2) in whose family there were twins (D. 17 and 18), and two sisters (C. 4 and 6) who were each parents of twin children (D. 25, 26 ; and 31, 32). This second pair had ten children (D. 41–50), and their youngest child—by some curious instinct or fatality—again married into a twin-bearing stock, her husband's uncle (C. 10) being a father of twins (D. 63, 64). One can hardly be surprised at learning that on the normal termination of her second pregnancy, in July of 1910, she was delivered of twin children (E. 6, 7).

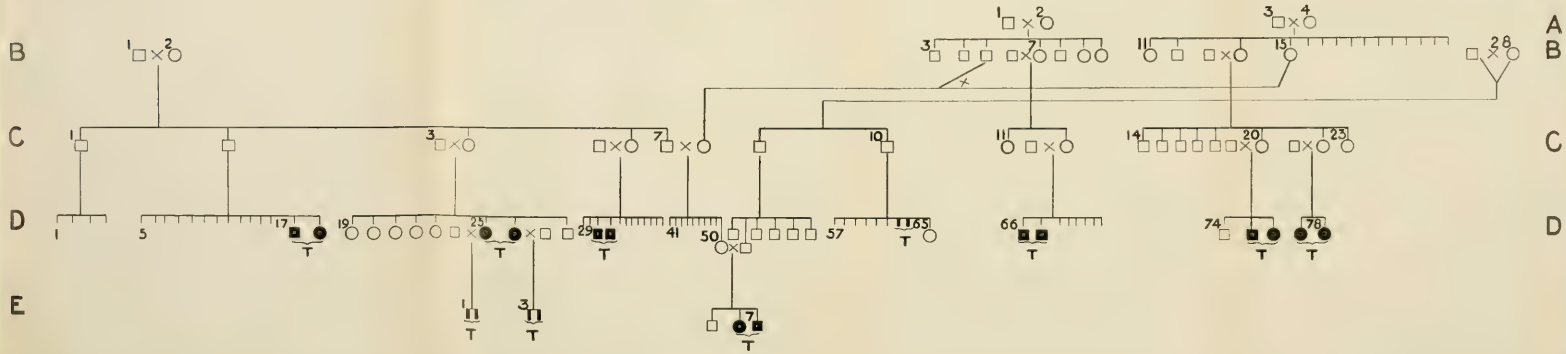
It is not necessary in this case to go over the individual members of these families with the same detail as has been given to pedigrees A and B of this series, for a glance at the accompanying chart is really all that is required to place one in possession of the main facts of the case. The symbols, letters, and numbers have the same significance as in the other pedigrees.

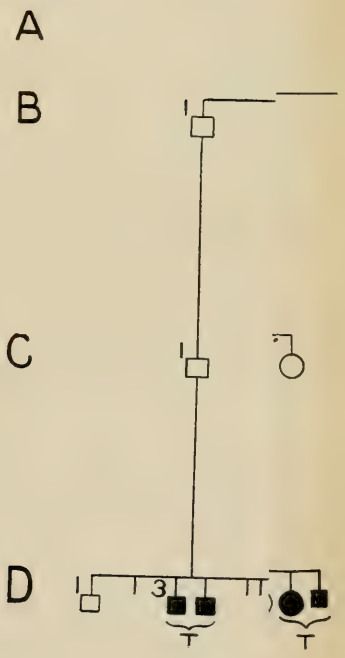
It will be seen, however, that in many instances the sex-symbols are not filled in for this case. But where in the compiling of a pedigree of this sort too minute and particular enquiry is made as to non-essentials, it is at a risk of wearying or irritating the various persons who have to be interviewed and questioned with a view to arriving at the facts, a result which in my own experience has more

PEDIGREE OF TWINS. FAMILY B

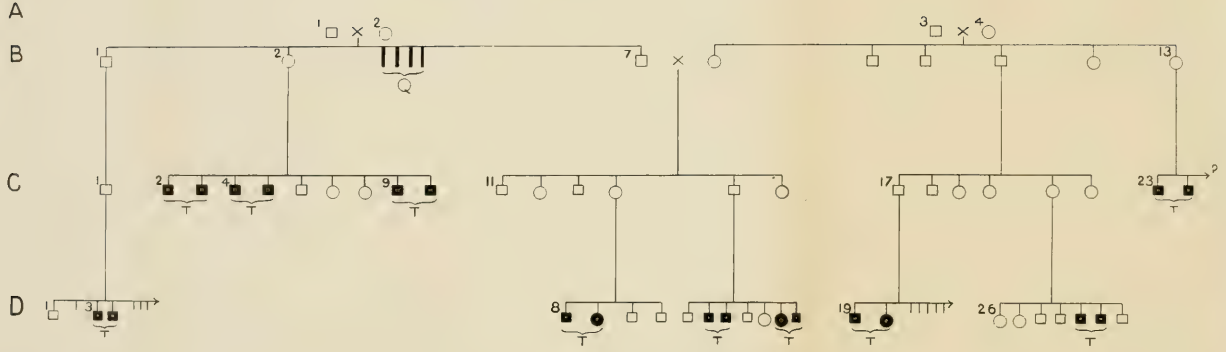


PEDIGREE OF TWINS. FAMILY C




A
B
C
D
1
1
1
3

PEDIGREE OF TWINS. FAMILY D



than once ended in a refusal to supply any details at all. Even when information as to sex has been obtained, it has not always been considered worth recording here ; for in the interests of accuracy not only should this be given in the diagram, but the chart should at the same time display the exact position in which the individual comes in the sequence of births, so far, of course, as the statements of the relatives, or whoever else the details are being gathered from, may be relied on in the matter.

Pedigree D.

In this pedigree may be found the details of the descendants of two allied stocks (A. 1 and 2, 3 and 4), each of which shows a strong tendency to plural births.

In the family of A. 1 and 2, there were quadruplets in Generation B. among seven persons* ; in Generation C. (omitting the offspring of No. 7 B.) there were three pairs of twins among ten individuals, and in Generation D. twins occurred once among some seven children. Among the grandchildren and great-grandchildren of A. 3 and 4 (omitting the descendants of B. 8) twins occurred once and twice respectively out of eight and fourteen children.

The branch produced by intermarriage of these two families, and which begins with Generation B, 7 and 8, gave birth, out of a total of seventeen descendants, to three pairs of twins, all of whom appear in Generation D.

To put it in another way, in the common stem and among its collaterals on both sides of the house, there have been no fewer than eleven plural births in these three generations.

Explanation of Pedigree D.

The symbols, letters, and numbers have the same significance as in the other Pedigrees of Twins, but Q. here means quadruplets.

General Remarks on the Four Pedigree Charts.

Each of the four families, the details of which are displayed on the accompanying charts (A—D), is selected as giving a different aspect of the subject with regard to

* The quadruples did not live.

the inheritance of the twinning tendency. In the first pedigree (A.) it will be seen how, in the family stock dealt with, the twin births tended without exception to be produced at or towards the end of the family. This is certainly the case in some clanships, twins being borne by multiparæ whose fecundity is tending towards exhaustion;* but in others this is not so, and taking all cases together, the incidence is pretty much as might be expected were the liability to twinning spread equally over all the births, the divergencies met with in any given series being, on the whole, such as might reasonably be expected to occur in random samples of small size.

Apart from statistics it may be stated in a general way that twins tend to be born when the mother is advanced in years, and that where it occurs otherwise there will probably be found to have been a strong hereditary tendency to twinning either among the women's own relations or among those of the father of the children.

In the thirteen households dealt with in pedigree B. there are six families in which twins are present, and such are seen to have occurred altogether eight times in these six families. In one of these families the tendency to the plural births has been supplied through the male parent: probably here in each instance the twins arose from a single ovum, the male germ-cell having in it some property capable of inducing want of cohesion between the two cells which result from the first division of the fertilised ovum, these two separated cells then going on in independent development to the formation from each of a new individual†. From the identity of the germ-plasm from which they thus arose these twins were of the same sex in two of the three cases in this family, though it is certainly a

* Another way of looking at the question is to consider the relative liability to twinning at the first birth in the case of women at different ages, and it would appear that the more elderly a woman is the greater is the liability for her to begin by bearing twins. "Prinzing gives the percentage of twin labours in elderly primiparæ as 4·14 per cent. The percentage is 2·96 in primiparæ under 20, 3·54 in women from 20 to 25, and 3·9 in women from 25 to 30."—*Brit. Med. Journ.*, April 20, 1912, p. 912.

† In connection with certain of the details on this chart it may be remarked that, as I have elsewhere pointed out (*Glasgow Medical Journal*, June, 1910), there are reasons for believing that the tendency to twin births is much more marked in families in whose composition there runs a strongly inherited neuropathic strain than it is among the general population.

difficulty that in the third case the sexes were dissimilar. "In twin-bearing women the ovaries, according to Hellin, contain an unusually large number of ovisacs, a persistence, in fact, of the fœtal character of the glands."* Olshausen,† on the other hand, considers that the hereditary tendency to twin pregnancies depends more probably on an unusual number of the follicles in one ovary coming to maturity at the same time than on an abnormal redundancy of ova occurring in the same follicle, which is still another of the suggestions that have been made. Obviously none of these reasons can hold where the twinning tendency in a family comes *via* the father of the children, as it has done in this case.

In the third chart (pedigree C.) are to be seen four distinct, and so far as is known, otherwise unrelated twinning stocks, which have intermarried so that in the fourth generation of descent there is a household of three children into whose composition there may be supposed to enter the potentialities of all these four stocks. This affinity of like for like in the selection of a mate has been dignified by the name of assortative mating, and has hitherto been recognised as occurring mainly in the case of degenerative conditions; persons coming from alcoholic, tuberculous, epileptic, insane and otherwise neuropathic stocks seeming often to have a strange perversity for marrying into a family where a similar taint exists. What the fundamental instinctive bias underlying this phenomenon may be it is difficult to say, but such a selection in mating is certainly not of a conscious nature. It is of interest to realise that the same tendency may exist in the case of such states as we are at present considering, which do not fall into the category of the degeneracies.

A further illustration of the tendency to assortative mating where the tendency to didymogeny exists is given in the last chart of this series (pedigree D). This, however, is in its way one of the most remarkable charts in my collection; the tendency to plural births here being so strong that one woman had borne twins thrice in her six pregnancies, while another, married to a man from

* Cited by Ballantyne, in Allbutt and Playfair's *System of Gynæcology*, 1896, p. 66.

†*Zentralblatt für Gynäk.*, No. 4, 1905.

a twin-bearing stock and becoming pregnant five times, gave birth to twins twice. It would almost look as if, in a stock like this (and especially in one produced by a union of two such families as are here shown), the twinning tendency might be capable—even in the human subject—of segregation as a definite unit-character. This, however, is but conjecture.

Some Mendelian Considerations arising from the Pedigree on Lenticular Cataract

BY THE EDITOR.

Two features seem clear in the Pedigree of Lenticular Cataract described by Dr. Rutherfurd. First, it is not a case of *simple* Mendelian inheritance. Second, the segregation of affected and non-affected members is quite clean. Dr. Rutherfurd made an examination of the eyes of some of the unaffected persons and though they were of the age, or in some cases older, than other members of the same generation in which the defect had appeared, they themselves manifested no sign of cataract. In as far therefore as we have this clean segregation of affected and non-affected individuals the pedigree exemplifies a Mendelian mode of inheritance.

We may next consider the complexities it presents from the Mendelian standpoint. In the first place, the affected persons produce an offspring containing both affected and non-affected. This is the case whether both parents are affected, as Nos. 14 and 21 in generation B, or whether one parent only presents the defect as with No. 19 and his wife in the C generation. Now, this being the case, on a simple Mendelian basis we should regard the normal condition as being recessive and the defective as a dominant character. But then the normal parents cannot, in a simple Mendelian case, produce affected offspring. The fact that this has occurred in a son of No. 2, A generation, and in a daughter of Nos. 3 and 4 of the same generation, all of them being normal parents, clearly shows this case to be complicated by the presence of two or more congenital factors.

Something of a very similar nature is shown in several of Mr. Nettleship's pedigrees of cases of retinitis pigmentosa. There normal parents produce some affected offspring, and affected parents also produce a mixed offspring of affected and unaffected members. Many

of Mr. Nettleship's cases are interesting and suggestive, for they show that the abnormal condition may skip a generation.* That is probably the meaning, in the pedigree described by Dr. Rutherfurd, of the appearance of the affected individual No. 21, in generation B, who was the daughter of two unaffected parents. Had we known her grandparents' history, it is probable there would have been evidence of cataract forthcoming. A glance at the history of No. 14 in the B generation renders this consideration almost a certainty. No. 14 himself is affected, but both his parents were normal. Nothing is known of his grandparents. But his uncle No. 1, A, was an affected person. There is thus clear history of the existence of the disease in the family, and it may be regarded as certain that if more were known of the history of Nos. 3 and 4, A, the existence of cataract would be manifested somewhere among the members of their family. We can therefore hardly regard No. 21 B, as manifesting a mutation, since it is probable that her defect has not arisen *de novo*, as Dr. Rutherfurd postulates.

There is another point of interest in Mr. Nettleship's pedigrees. In a relatively large number of cases the disease has appeared in the offspring of consanguineous marriages. The disease, however, can always be traced back to an ancestral stock. The relatively frequent appearance of the disease in cousin marriages suggests that the disease may be due to the presence of not one but of several factors. The association of these factors in the same individuals is, of course, more likely to occur in the cousin marriages of an *affected stock* than in marriages among individuals selected at random from the general population. If the congenital diseases, retinitis pigmentosa and lenticular cataract, are due to the presence in the same individual of two or more factors, the presence only of any one of these factors being insufficient to cause the disease, then we have an explanation of unaffected pairs of parents, as well as of pairs in which one or both parents are affected, producing a mixed offspring of abnormal and normal

* See Royal London Ophthalmic Hospital Reports, vol. xvii., part i., 1907, pages 19 and 20, Figs. 16, 16a, 19, and 20.

individuals. On that basis the phenomena can be brought into line with Mendelian principles. The existence of characters which depend upon the simultaneous operation of two or more factors is not an idle hypothesis. It is a practically demonstrated fact in the coat colour of animals and the petal colour of flowers. In peas, for instance, we know of actual cases where coloured parents will produce white-flowered members among their offspring in addition to coloured individuals. There we have the analogy of the appearance of a mixed offspring in the progeny of parents, one or both of whom are affected with lenticular cataract or retinitis pigmentosa. On the other hand, we can seek the analogy for the appearance of affected members from parents unaffected with either of these diseases, in the crossing of the white races of *Emily Henderson* sweet peas, which give coloured individuals among their offspring. But it has been shown that though these two races are identical in having white flowers, they are really two different races. One is characterised by having round pollen-grains and the other by oval pollen-grains. If the members of the oval-pollened race are self-fertilised or fertilised *inter se*, only white-flowered offspring result. And similarly for the round-pollened race. But if the round-pollened white race be crossed with the oval-pollened white race, some coloured members are produced among their offspring.

Now if we knew nothing of the existence of characters dependant upon the simultaneous presence in the body of the same individual of two determining factors, the production of coloured-flowered plants in the offspring of two white parents could not be interpreted on Mendelian lines. There is no reason to doubt that many human qualities and characters depend upon the co-ordinate action and simultaneous presence of several factors. Eliminate one only of the many factors required for the normal manifestation of such a human character and abnormality of greater or less degree will be exhibited. It matters little how that elimination may be brought about ; it may be gametic or zygotic in origin. Now it is a significant fact that many of these family histories of retinitis pigmentosa and lenticular cataract belong to

stocks which manifest more or less obvious degeneracy. Mr. Nettleship regards retinitis pigmentosa as itself a degeneracy.* The degeneracy in the stock may take various forms. It may not always be retinitis pigmentosa or lenticular cataract. We may have three or four generations without these specific diseases showing themselves, and then apparently quite suddenly they appear. But an examination of many of the pedigrees of retinitis pigmentosa reveals the existence of other degeneracies, such as deafness of various degrees, and when complete associated with dumbness ; of congenital idiocy, many varieties of mental deficiency, insanity, epilepsy, and progressive paralysis ; of polydactylism, remains of the hyaloid artery, congenital cataract, conical cornea, and coloboma iridis.†

What is the meaning of this association between certain eye defects and these degenerate traits? It is too common to permit us to dismiss it as an accidental or coincidental phenomenon. We believe the ultimate explanation will be found in somatic segregation. Gametes may carry into the zygote the same group of factors and yet the zygotic manifestation may be very different in different cases.

Let us take for instance the case of deafness. What is deafness ? It may be produced by a variety of causes. We may leave out of consideration the cases of middle-ear disease and confine ourselves to physiological defects or structural imperfections of the internal ear and of the auditory centre in the brain. Now it is conceivable that deafness may be produced in one of two ways. In the first, some essential group of nerve cells in the brain or in the auditory epithelium may be absent. In the second place, these nerve cells may all be present and yet their physiological activities may be inhibited by the presence of some inhibiting factor. Let us deal with the second consideration. Suppose in some cases, that in the course of somatic cell divisions, this inhibitory factor became part of the nuclear contents of the cells of the auditory centre in the brain, or of the cells of the auditory epithelium. The individual would then be deaf. Suppose, however, that in the course of these somatic cell

* Op. cit. p. 342. † Op. cit. p. 343.

divisions, the inhibitory factor fell not into the nerve cells of the auditory centre, but into those of the cerebral cortex, inducing inhibition either of development or of physiological activity of those nerve cells which give us our intelligence, then the individual would be not deaf, but an idiot, a lunatic, or a mental defective, or would manifest some other form of mental degeneracy. But suppose further that it fell not into either of these groups of nerve cells, but into the connective tissue cells of the retinal epithelium, then we should have retinitis pigmentosa.*

Of this disease, of cataract, of deafness due to nervous lesions, and of insanity a general proposition may be made. The existence of some definite chemical compound can be conceived as setting up the specific changes in the various tissues concerned. We know that locomotor ataxy is due to degenerations of the nerve tissues of the spinal cord and is followed by an overgrowth of connective tissue, set up by the specific poison produced from the body fluids by the presence of the syphilis spirochæte. There is nothing improbable in supposing that the overgrowth of the connective tissue of the retina, by which its perceptive nerve cells become disorganised, is similarly produced by the formation of some subtle chemical compound. The elemental factors of this compound may be introduced separately by the parental gametes. If the gametes of one parent carry only one factor of the compound and the gametes of the other parent are normal, that is, carries none of the factors, then the individual born of such parents will be apparently quite normal as regards this disease, yet all the while carrying one of the pathological factors which in union with the complementary factors will result in a manifestation of the disease. But if this apparently normal person marries another apparently normal person whose gametes, however, carry the complemental factor or factors,

* Of course in this case, the factor need not be an inhibiting factor, it might be an exciting factor stimulating overgrowth of the retinal tissue. Or we may suppose that the natural growth of connective tissue is itself inhibited under normal circumstances, at an appropriate time. In this case the factor causing retinitis pigmentosa may be an inhibiting factor which inhibits the normal inhibiting factor of growth.

then from two apparently healthy people, there may thus be produced some affected offspring.

The same considerations apply to cataract and to the degeneracies sometimes associated with these abnormalities. They are not simple attributes. They are doubtless characters of a complex nature. Until we are able to determine precisely the nature and number of the factors which when present simultaneously in the lens of the eye, produce cataract, we cannot predict the precise mode of descent of the abnormality, nor indicate the percentage of offspring in whom it should be present or absent. But that we have clean segregation of the normal from the abnormal, as shown by Dr. Rutherfurd's observations, is as far as it goes, very good, if incomplete, evidence of Mendelian inheritance.

The question of "anticipation," as Mr. Nettleship and others have called it, and which Dr. Rutherfurd describes in the paragraph entitled "General Considerations," is of great interest and deserves fuller consideration in the light of recent Mendelian research. We do not, however, now propose to further consider it.

Some Sociological Considerations Bearing on Infantile Mortality:

A Study of Pedigrees by the Editor.

THERE are five points of interest in Dr. Rutherfurd's Pedigree of "Twins" (A). A glance down the lines of generations shows at once that so far as this clanship is concerned, the inherent tendency to beget twins is transmitted by the females.* It was evidently present in several of the females of Generation B. For instance, Nos. 5 and 9 both had twins among their own children. No. 1 had only one child, and so, although the tendency to twinning may have been carried by her, there was no chance of its manifesting itself. But that she did probably inherit the character is shown by the presence of twins in her great-grandchildren, *i.e.*, Nos. 11 and 12 in the E Generation.

Coming back to No. 5 in the B Generation, we notice, as already alluded to, that twins appeared among her own children. There were two pairs, Nos. 18 and 19, 20 and 21, in the C Generation. But one of her daughters (No. 22 C), not herself a member of twins, had nine children, and among them a pair of twins.

The second point of interest turns around the question: Is there any evidence of the segregation of the twin-producing capacity from the absence of it? The part of the Pedigree which apparently manifests the existence of this segregation is with the woman No. 3, B Generation. She had eight children and none of them twins. But we are bound to ask ourselves: Would she have had twins had her family been larger? As Dr. Rutherfurd has pointed out, there is an apparent tendency shown

* In several others of Dr. Rutherfurd's Pedigrees, the twin-producing quality is sometimes transmitted by the male. (See Pedigree B. No. 4. B Generation.)

in some, but not all,* of the charts in his collection for twins to be produced in the later part of married life, and the probability therefore exists that had this woman had any further children some of them might have been twins. A glance at the other twin-bearing fraternities in this Chart enables us to consider this probability. Among the children of Nos. 5 and 9, B Generation, and of No. 1, D Generation, the twins were produced at the fifth, fifth and seventh births respectively. This clearly suggests that the woman No. 3, B. Generation, who had eight children, and none of them twins, did not carry the twin-producing capacity, and therefore represents in respect of this quality a Mendelian segregation from a twin-producing stock. The evidence is not, of course, conclusive. But it is certainly significant.

The third feature of interest in this Pedigree is that although, as I am informed by my medical colleagues at the London Hospital, nephritis "is more common among men than among women, on account of the greater exposure to which they are subjected," yet if we trace the descent of it through the members of the Pedigree it will be noticed that only females suffered from this disease or from one of its manifestations, vesical calculi. Now we may doubt whether mere exposure is sufficient by itself to produce nephritis. Doubtless if the inherent tendency to contract it is there, exposure will hasten or determine its manifestation. But in this clanship the evidence shows a definite hereditary tendency to acquire the disease, which seems present in some and absent in others. There is no reason, for instance, to believe that the woman No. 1, C Generation, was less exposed in her life to conditions supposed to be favourable to the production of nephritis than her mother and one of her daughters, who both contracted it. She herself apparently never suffered from the disease, but nevertheless transmitted one of its manifestations to one of her daughters.

The fourth feature of interest is the large proportion of females which this clanship produces. Out of a total of thirty-nine people whose sex has been ascertained,

* See for instance Pedigree B, where the earlier children are sometimes twins.

twenty-six are females and thirteen males. But there are twenty-four individuals in the chart with unknown sex, and it is probable that they would have redressed the balance had their sex been known. We cannot, therefore, conclude that this clanship had a tendency to produce a large proportion of females. Roughly speaking, in normal cases the sexes are about equal in numbers.*

The fifth feature of interest has an important sociological bearing. There are a great number of people, mainly medical men in Government or parochial official positions, or who contemplate occupying such positions, who are making a great outcry concerning the rate of infantile mortality among the lower social classes. They urge the creation of more officials and the spending of public money in order to minimise or avert this mortality. But in their pleas they never allude to the primary and crucial consideration, as to whether the *race* which they desire to save is, from the civic and ethical standpoints, worth the outlay of a sixpence. From the economic standpoint it can be shown that many of the people whom they would save by the outlay of thousands of pounds are not worth five pounds apiece. But we are not concerned with the economic but the biological worth of these races. Now let us glance at this Pedigree, and bear in mind that it is by no means as bad in the biological defectiveness which it manifests as many of the Pedigrees of this class of citizens with which we are acquainted. We have seen far worse in our workhouse investigations. Notwithstanding that it is a better pedigree than some, nevertheless, it everywhere manifests signs of low vitality. Of the twelve twins, as Dr. Rutherfurd has pointed out, only four are alive. The others died young. Of the ten children in the present generation, four died in fits in infancy, two were stillborn, one has Raynaud's disease, and one suffers from laryngismus stridulus. Now Raynaud's disease and laryngismus are both diseases indicative of nerve degenerations or abnormalities.

From the standpoint of national welfare and virility is it common-sense, or is it even humanitarianism, to go out of our way to save the offspring of such a stock, and

* See, however, Editorial Note, page 193.

thereby to multiply human weakness and defectiveness and *increase the sum total* of human misery, which is their correlative outcome ? I plead that it is nobler and sounder statesmanship to leave things alone.

We see again in Dr. Rutherfurd's Pedigree B the same fact of sociological significance obtruded on our notice that a high rate of infantile mortality is the expression of a weak, defective, and lowly viable stock.* Let us glance at the family Nos. 25—33, in Generation D. There were nine children, and of these four,† or nearly fifty per cent., had such a weak hold on life that they were stillborn. No doubt some sentimental people would say, "Poor children ; their mother had a hard time ! It was her struggle with adversity that weakened her constitution and destroyed the viability of her seed." That, too, might be pleaded for the illegitimate, epileptic child, No. 2, D Generation.

Such contentions were doubtless very plausible thirty or twenty years ago, but they have no validity now. Our knowledge of inheritance is too clear to justify such pleas. For, quite apart from the fact that stillborn children are born of mothers, as in the family shown in Pedigree Chart, No. II., Nos. 2—7, Generation F_1, p. 21, *Mendel Journal*, No. 1, October, 1909, who live a normal, healthy, country life, and are of well-to-do Middle-Class standing, there is the obvious record of the unstable nature of the stock we are considering. For the mother, No. 13, Generation C, was eccentric, her sister, No. 11, was insane, and another sister, No. 15, was eccentric.

Almost everywhere through the Pedigree we see the same manifestation of low viability. There is the family Nos. 23—43, of twenty-one members, in Generation C. Of these only two are alive ! And yet, had this been a vigorous stock, the majority of them should now be in the prime of life.

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* In answer to my inquiry, Dr. Rutherfurd informs me that the social class of all the persons in his pedigrees was low. They were navvies, coal-miners, cotton-weavers, scavengers, old age pensioners, &c. This has an obvious significance when read with the facts of the pedigree.—[Editor.]

† Only two are indicated on the chart, but that is the artist's error in copying the original. The twins 27 and 28 were also both stillborn. See the "Facts of the Pedigree," page 177.

Again, we note in the family Nos. 3—15, Generation D, there were thirteen children, but ten are dead and several were stillborn. The mother is insane. Before her marriage she had an illegitimate child, which is epileptic.

In family Nos. 49—59, Generation D, there were eleven children, but four are dead. A glance through this Pedigree and through the preceding Pedigree A will reveal at once the large number of gaps where Dr. Rutherfurd was unable to obtain any information as to the sex of the members concerned. In the present Pedigree there is a total of one hundred and eight individuals, and of these sixty-five are of unknown sex. Everyone who is familiar with Pedigree collecting and construction knows that this absence of specific information as to sex is an indication that the children died early, and their sex is forgotten by the family which knew them.*

Now, considering all these facts together, it is clear there is shown in this Chart a clanship of defective and weak families.† There is no inherent vigour in the majority of the individuals of which these families consist. They never can, under any circumstances, make desirable, useful, or beautiful citizens. Where, then, we are impelled once more to ask, is the common-sense or the humanitarianism of making conscious and extremely expensive efforts to lower the death-rate of the infants of a stock like this? Do we want to rear a nation of miserable families, so utterly non-viable that half its members cannot even be born alive? And yet that is the path of national degradation and decline along which an army of sentimentalists and of medical officials seeking to justify their salaried positions are attempting to lead this nation.

There is yet one other point of apparently sinister significance which should not be overlooked. It is a well-known fact that although on an average there are an equal number of males and females born, yet more male infants die than females. That leaves us, even under

* Dr. Rutherfurd tells me that is the true interpretation of most of the blanks in his charts. But in some cases the patience of his informants became exhausted, and he had to content himself with getting the essential facts and letting the sex of *unaffected* persons go.—[Editor.]

† See also "Coroner and a Biological Fact," p. 205.

normal conditions, with a population having an excess of females. In our present population of about forty millions the females exceed the males by about one and a half millions. That is not a very great excess from the standpoint of proportion.

But when we deal not with a normal population, but with such an abnormal class like that shown in Dr. Rutherfurd's Pedigrees A and B, as already noted, there seems, indeed, to be a very great excess of females. In Pedigree A there is a total of twenty-six females, thirteen males, and twenty-four of unknown sex. In Pedigree B there are twenty-eight females, sixteen males, and sixty-five of unknown sex. In the two families, then, is a total of one hundred and seventy-one individuals, of which eighty-nine are of unknown sex, fifty-four are females, and twenty-eight are males. Now if these proportions are indicative of some peculiarity correlated with a defective stock, it would seem that through our sentimentality we are going to rear a nation so constituted that there will be two women to each man! If this is to be the outcome of an artificially-reduced infantile mortality, we shall eventually have a nation of cripples and two-thirds of them females!

But is the premiss based on these two pedigrees applicable to other degenerate stocks? Do other races, showing some other type of degeneracy, also produce this great excess of females? I do not believe it will be found a rule applicable generally, for in two other of Dr. Rutherfurd's charts of twins, namely, C and D, there are respectively twenty-one females and thirty-seven males, and twenty-seven females and thirty-one males. Here we have an excess of males surviving. But we can examine the question from the facts given by other types of degenerate stocks. During the best part of the year 1910 and the latter part of 1909, I was in charge of a small Committee of Workers, organised by the Eugenics Education Society, and we were occupied in hunting through workhouse records and in interviewing paupers and their relatives. By dint of hard work and patience we got together a large pedigree going through six generations.* The portion of the stock which I was able personally to examine

* Eugenics Education Review, Nov., 1910, Vol. II., No. 3, page 186.

was defective in many civic qualities, and to some extent in physical qualities. Leaving out a few people whom we cannot regard as produced *within* the stock and a few whose sex we could not ascertain, there were in this pedigree of paupers, sixty-nine males and fifty-eight females. But it is to be noted that this stock did not show any heavy rate of infantile mortality. In that respect it approximated pretty nearly to the normal of the nation as a whole. Another pauper pedigree* which I have examined, in which five children died in infancy and three were stillborn, gave among the survivors ten males and eight females. Here we have a manifestation of low viability, and yet the males and not the females are in excess. In another pedigree of paupers, published by Mr. E. S. Lidbetter,† which contains a total of one hundred and eleven persons,‡ forty-five are males and thirty-two are females. That the stock is of low viability is shown by the fact that more than twenty per cent. died in infancy,‖ and of those who remained seventeen died of some tubercular disease or from fits, or were lunatics or imbeciles. This by no means represents the worst of the stock, for I have left out many who died in the infirmary from diseases not specified on the chart.

But from the standpoint of our present inquiry the general fact seems obvious that a degenerate stock does not always—as in Dr. Rutherfurd's two pedigrees—beget or preserve an excess of females. Indeed, the workhouse pedigrees and two of his unpublished charts seem to give an excess of males.

The subject deserves further inquiry, but it is not within the scope of our present purpose to pursue it further.

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* Eugenics Education Review, Nov., 1910, Vol. II., No. 3, page 218.

† Op. cit., page 228.

‡ This includes twenty-one who died in infancy, and therefore mostly of unremembered sex, and of persons introduced by marriage, neither of whom I have reckoned in my calculation. Introductions by marriage do not count as having been produced *within* the stock.

‖ Seventeen persons introduced by marriage have to be deducted from the total of 111.

MISCELLANEA.

Proposed Tests for Inheritance of Mental Qualities According to Mendel's Law.

By A. F.

While certain cases of human inheritance of physical qualities according to Mendel's law have been worked out (*e.g.*, the colour of eyes and the "Drinkwater" hands), the question arises:—When we desire to investigate the inheritance of mental qualities, what precise mental characteristic would best be selected for such investigation? It is clear that it must be a quality (*a*) which is distinctly either absent or present; (*b*) which can in no case be essentially modified by the environment (including thereunder all forms of training and education.)*

It is not easy to find qualities which pass both these tests. Facility in languages and music depend too largely on early opportunities and surroundings, while facility in mathematics admits of degree to such an extent that it might be equally hard to concede or refuse to attribute it to an individual. Other mental qualities are too complex or too transient in their manifestations. I suggest, however, that there is a test which might easily be applied, viz., the capacity, which in some families seems to be congenital, of dreaming certain kinds or types of dreams. Some people frequently dream that they fly, skimming along the ground—this is not universal, but well known in some families; so is the dream of insufficient clothing (*vide* Du Maurier's drawings in *Punch*); or, better still, perhaps the dream (scarcely a dream, rather a sleepy distress) of things growing large and small which has been admirably described by Robert Louis Stevenson. No one who knows the sensation could fail to recognise this description; but there are people to whom it seems to be quite unknown. In fact, in a family with which I am acquainted where the dream is well known among the children, no relation on the mother's side seems to know it, though they are rather vivid dreamers;

* It is important to remember that education cannot modify a character which is not present. Or, if present, it cannot modify it much, unless it possesses an innate degree of responsiveness to educational influences. And what our correspondent is now urging is, we take it, the necessity of discriminating between two persons both of the same inherent capacity, but of whom one has had that capacity made the most of by favourable educational influences and the other has not.—[Editor.]

the father, however, knows it. In support of this suggestion, I may add that it seems generally held that second sight goes in families; perhaps that, too, might be a good quality on which to test the rules of mental inheritance.

A Note on Yellow Dun Horses and the Relation of this Colour to Chestnut.

By C. J. DAVIES.

I HAVE been much interested in the article on the colour of horses in the *Mendel Journal*. There are one or two points in connection with this subject which I have often thought needed closer investigation.

It is generally assumed and has, indeed, been stated by at least one writer (viz., Professor Wilson in "The Inheritance of Coat Colour in Horses," Scientific Proceedings of Royal Dublin Society, 1910), that chestnut is recessive to all other colours, including dun.*

Some years ago in the neighbourhood in which I then lived were two yellow-dun pony mares, in different ownerships. By yellow-dun I mean a yellow body colour with black mane, tail, and dorsal stripe, black on knees, hocks, front of legs and fetlocks, a slight shoulder stripe, and slightly barred legs.

The antecedents of the ponies were unknown. One pony was fourteen hands high. The other was about thirteen hands high, and was of a different type, suggesting a Mongolian pony in some respects. Both were probably foreigners.

The smaller pony bred several bay and brown foals by bay and brown sires. The larger pony bred three chestnut foals (without black points) in three successive years (I think 1891, 1892, 1893, or thereabouts) to the chestnut hackney *Troubadour*, then located in the Taunton district.†

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* Mr. C. C. Hurst arrived at the conclusion that chestnut was recessive to bay and brown as early as 1905, but he did investigate the behaviour of the dun coat. Mr. Hurst's inquiry was the first which showed that coat colour in horses does behave in accordance with Mendelian principles. Since then (1912) Professor Wilson has examined the behaviour of the dun coat.—[EDITOR].

† This observation suggests that yellow is recessive to chestnut, but the absence of the black points in the chestnut offspring which are present in the yellow dun mare, indicate the existence of some complication, and we think it lends some support to Mr. Davies' suggestion as to the relationship between dun and brown which he describes in the next paragragh.—[EDITOR.]

Some years ago (possibly about 1901) there was a famous dun hackney harness horse called *Dusty Miller*. His owner tells me that in colour he was what he should term a "mealy or biscuit dun, but he had *not* the zebra black stripe down his back."

It appears that *Dusty Miller* was sired by *Dunegelt*, a chestnut bred from chestnut parents, and his dam was a brown thoroughbred mare sired by a chestnut. I have no particulars of the colour of the g-dam.

It has occurred to me that dun and dark brown may be intimately connected, and that brown may be of the nature of a pattern covering dun. To try and put this more plainly: When the brown factor is absent the horse is a dun; when it is present the horse is a brown, the dun only showing on muzzle, flank, inside of thighs, &c., to which parts the darker pattern fails to extend.

I think there may be a difference in the colour of the foal coat of browns. By dark brown I mean a very dark colour with a yellow mark on the flank and possibly yellow on the muzzle. I have known several foals, the progeny of two dark coloured parents which were dark brown in the foal coat. Exmoor and other hill pony foals are usually dark brown when the parents are this colour. On the other hand a bay foal sometimes moults into a brown, and I believe this is sometimes, if not always, the case when one parent is a brown and the other bay. Such browns, however, are in my experience of a nondescript dark bay or light brown colour and not a rich seal brown, so perhaps they should in reality be classified as bays. Until some definite distinction is made between bay and brown it is impossible to analyse results. Possibly the colour of the foal coat may help to form a distinction.

These remarks are not, I fear, very enlightening; my excuse for making them is that it is often assumed that black points are dominant in a cross; and the relationship of duns and chestnuts seems to need closer investigation, as in one case at anyrate this does not seem to have been the case.

Sex Limitation in the Inheritance of Eye-Colour in Flies*

Drosophila is a fly of common occurrence during the Summer. It deposits its eggs in fruit and various culinary plants. The eyes of the wild fly are dull red. It has been shown experimentally by Professor T. H. Morgan, of the Zoological Laboratory, Columbia University, that the colour is due to a mixture of three pigments, namely, red, pink, and orange. Flies are known which, instead of having the dull-red colour of the species, have either a vermilion, a pink, or orange colour. The experimental evidence leads us to believe that these colours are produced by the loss, from the germ-cells, of one of the three factors which make up the dull red of the species. Vermilion, for instance, is the colour which results when the pink factor is lost; in other words, it is the colour due to the presence of the two factors producing red and orange. Similarly, the pink eye is due to loss of red, and is the colour manifested when only pink and orange are present. The orange eye is due to the loss of both red and pink.

It follows from this, that if a female fly with vermilion eyes be crossed with a male having pink eyes, we should expect that all the first generation would have the dull-red eyes of the species. If these, then, be interbred, we should further expect that the second generation would produce some flies with orange eyes, together with some whose eyes will be vermilion, some red, and some pink.

It is, however, an apparently strange fact, which has been experimentally shown by Professor Morgan, that the first generation consists not of flies all having red eyes, but of two sex groups. Of these, all the males have vermilion eyes and all the females red eyes. We are dealing with a definite and obvious sex-limited character—that is to say, that the character of sex is in some way definitely bound up with the presence of another character. This character is pinkness, for as we have seen, the vermilion colour is red *minus* pink. In other words, the males of the first hybrid generation whose eyes are all vermilion-coloured lack pinkness, while the females all having the red eyes therefore possess it. This is an extremely interesting fact because it shows two things quite incontestably. First, that qualities do not depend upon environment, because these flies are reared under the same external conditions. Second, that sex may carry with it an association with other qualities.

In the case just described the vermilion-eyed female was

* Further information of this complex subject will be found in the "Journal of Experimental Zoology." Vol. XI., 1911.

mated to the pink-eyed male. But if the reciprocal cross be made, namely, " pink female " by " vermilion male," *all* the first generation, both males and females, are red-eyed.

If vermilion-eyed females be mated to pink-eyed males, the F_1 generation has all its females red-eyed and all its males vermilion. The second hybrid (F_2) generation bred *inter se* from F_1 consists of red-eyed males and females, vermilion-eyed males and females, pink-eyed males and females, and orange-eyed males and females. But when in the second or reciprocal cross, the vermilion-eyed males are mated to their pink-eyed sisters, the F_1 generation are all red-eyed. The F_2 generation derived from F_1 crossed *inter se*, consists of red-eyed and pink-eyed males and females, and of vermilion-eyed and orange-eyed males. That is, the females do not manifest either the vermilion and orange eye colour; these are the two eye colours which are devoid of the pink factor. The females in this experiment are therefore never devoid of the pink factor. Thus in the F_2 generation of this reciprocal cross we see again the intimate relationship of femaleness and pinkness. Some of the males are without pinkness, the females not.

The same female sex relationship with pinkness is shown in another way. In certain kinds of crosses, the offspring give only half as many pink males as females, and twice as many orange males as females.

Professor Morgan endeavours to explain the association of femaleness with pinkness, by assuming that the factor which determines the manifestation of the pink eye colour, is carried only in the same chromosome that also carries the factor which determines the female sex.

The Form of Nose and its Segregative Inheritance.

The form of a nose doubtless depends upon many factors. But chief among them we may suppose are the length, breadth, and angle of inclination of the nasal bones; the form, length, breadth, and thickness of the nasal septum, and the degree of development of the turbinal bones. The segregation and persistence in families of a definite type of nose-form is a subject well worth further study. The inheritance of this character from the Mendelian standpoint has not yet been adequately studied.

We are able in the accompanying photograph to show what appears to be an undoubted transmission of a very prominent form of nose from a grandmother to a grandson. The grandmother (on the right of the photograph, who is now over eighty years of age) was the wife of a gipsy and she herself came of gipsy stock. She and her husband eventually settled in a small village

ENGLISH *v.* GIPSY.

SEGREGATION OF NOSE.

in the West of England, but continued the gipsy habit of travelling in a caravan with tinker's goods and basket work. They had six children, namely, two sons and four daughters. Of the two sons, one was fair in complexion and had the "wild ways and habits of the gipsy." The other was dark in complexion and married an English country-woman of the district in which his parents had settled. She was of fair complexion. They are shown, as husband and wife, in the left-hand corner of the photograph. They have had four children, namely, three girls (shown in the centre of the photo-graph) and one son (shown standing by the right of his gipsy grandmother, in the left corner of the group).

The gipsy grandmother has a very prominent type of nose. It is characterised by three chief features : First, the broad base on which the external narial apertures are lodged ; second, the marked convexity of the contour of the bridge ; third, the well-defined or sharp angularity of the general form. Her son's nose differs from hers in all three of these points. His wife's nose is of the more rounded type and differs very widely from that of the gipsy grandmother (her mother-in-law). The three girl children of these two parents clearly do not possess a nose like that of their grandmother. The two younger daughters appear to resemble their mother, while the oldest appears to be an inter-mediate between her mother and father. So far then there is no feature of any special interest.

But it is otherwise when we come to deal with the nose of the son (grandson of the old gipsy woman). For it resembles her's in all three of the marked features which give to her nose its distinctive and prominent form. The convexity of the bridge is, perhaps, not quite so pronounced, but then he is still young, and this is a feature likely to become accentuated with age.

Two features of Mendelian interest are shown in this group consisting of a grandmother, two parents, and four grandchildren. First, there is a hereditary transmission of nose type from grand-mother to grandson. Second, there is a clean segregation of the nose type manifested by the brother, from the contrasted nose type or types exemplified by his three sisters.

In the absence of precise information concerning the form of nose of the gipsy grandmother's husband, and of their five other children, and of the brothers and sisters of the grandmother, it is difficult to formulate a scheme showing a definite Mendelian inheritance in this case. But the two features alluded to in the preceding paragraph are strongly suggestive of inheritance according to Mendelian principles.

We are indebted to Mrs. Rose Haig Thomas for the facts of this case, and for the photograph of the group. The responsi-bility for the analysis and interpretation of the facts rests with us.

Natural or Instinctive Eugenics.

When one considers the errors committed by beings who boast the possession of rational powers, especially the misdeeds of politicians and social reformers, involuntarily one sometimes wishes that men were less like themselves and more like ants. When we contemplate the great army of foolish people for whose preservation an even greater army of officials is being created at immense national expense, the mind of one reared in Victorian days —when the biblical instructions of the Old Testament played a larger part in national life than they do to-day—quite naturally recalls the exhortation in Proverbs vi. : "Go to the ant, thou sluggard ; consider her ways and be wise : Which having no guide, overseer, or ruler, Provideth her meat in the Summer, and gathereth her food in the harvest. . . . . Yet a little sleep, a little slumber, a little folding of the hands to sleep : So shall thy poverty come as one that travelleth."

The whole trend of our unbiological legislation and of the social sentiment of a few fearsome or designing persons whose clamant utterances are altogether out of proportion to their importance, either in number or in merit, is to rear and perpetuate a race of sluggards, who can achieve in life nothing more than a "little folding of the hands," whose existence is one "long slumber," and who can neither "gather food in the harvest nor provide meat in the Summer."

So at last, awakened to the calamity awaiting the nation, some citizens are endeavouring to *educate* the community to a sense of its responsibility and to direct its policy along sounder biological lines. Under prevailing circumstances this is, of course, a wise and desirable course, always providing that the attempt to educate does not extend beyond its legitimate limits. For if it is intended by legislation, to *enforce* eugenic education upon persons whose intellects and general capacities are incapable of appreciating it, disaster, and not triumph, lies ahead for the gospel of Eugenics. Any attempt to issue "marriage certificates," for instance, would be fatuous, and would meet with the ridicule which it deserves.

One must deplore the fact that civilisation has carried us so low in social and moral perception that it is now necessary to teach Eugenics at all. How much more virile and free our people would be, if the eugenic conception were an instinct, born with us at our birth, and guiding us through life. Better the inherent *instinct* than the necessarily incomplete fruit, laboriously begotten, of a prolonged and expensive *education*. There seems little eugenic instinct in civilised peoples at the present time. One marvels when one looks at some mens' wives,

and at some wives' husbands. This sad condition of affairs no doubt has always, more or less, been with us. For always we suppose there have been hasty youths of real worth, dissimulating maidens of no civic value, and fools who believed that a Lie could endure for the remainder of their lives.

But some races seem to have instinctively possessed an eugenic ideal. We do not allude to so-called savage tribes, such as those described by Mr. Torday in this Journal, but to people of our own kith and kin. Dr. Rutherfurd has been kind enough to send us some quotations from an old book, which we think eminently worth publishing as they afford a contrast between what we may call the instinctive manifestation and the educative emergence of eugenic conceptions.

Dr. Rutherfurd says: "About 1725-6 a certain Captain Burt was stationed in the Scottish Highlands in connection with the making of General Wade's roads. A series of quaint descriptive letters of his were (anonymously) published in London in 1754 under the title of 'Letters from A Gentleman in the North of Scotland to His Friend in London, ...' In the fifth letter occurs the following, which it must be remembered was probably written about 1725 or 1726, although not published until 1754."

"'The Men have more Regard to the Comeliness of their posterity, than in those Countries where a large Fortune serves to soften the hardest Features, and even to make the Crooked straight; and, indeed, their Definition of a fine Woman seems chiefly to be directed to that Purpose; for, after speaking of her Face, they say, 'She's a fine, healthy, straight, strong, strapping Lassie.'

"'I fancy now I hear one of our delicate Ladies say, "'Tis just so they would describe a Flanders' Mare." I am not for confounding the Characters of the two Sexes one with another; but I should not care to have my Son a valetudinary being, partaking of his Mother's nice Constitution.

"'I was once commending to a Lady of Fortune in London, the upright, firm, yet easy Manner of the Ladies walking in Edinburgh. And when I had done, she fluttered her Fan, and with a Kind of Disdain, mixed with Jealousy to hear them commended, she said, "Mr. ——, I do not at all wonder at that, they are *used to walk*."'"

The Coroner and a Biological Fact.

A CUTTING from an evening newspaper was sent to us some time ago by a subscriber, in which appeared a report of an inquest held at Islington.

A six-weeks-old baby had died from natural causes "due to heart and lung trouble." The medical man who conducted the post-mortem, and knew the family, said that the father of the child was not a healthy man, and that the mother was physically weak. In his opinion "they ought not to have had children at all." These two people had begotten fifteen children and only three were alive.

The Coroner, Mr. Danford Thomas, said "there had been a discussion in medical circles with reference to the children of unfit parents, and he considered that such cases ought to be gone into." He added: "It is a disgraceful thing to have fifteen children and twelve die. It is high time something was done, I am quite sure."

Now some of these remarks will, we feel certain, commend themselves to everyone who loves humanity well enough to desire to see it healthy and vigorous. We shall all endorse the remark that it is disgraceful for such people to have fifteen children. But personally we fail to see the disgrace in the natural death of twelve of them. There is much to be said for the ethics of those so-called benighted and heathen tribes in Central Africa, described by Mr. E. Torday in another part of this Journal.

With regard "to the discussion in medical circles" alluded to by the Coroner, two facts need emphasizing. First: the rank and file of the medical profession, namely, the general practitioners, who daily see the consequences of human recklessness and folly, are, we believe, dead against the modern sentiment which would deliberately save these defective races. Second: there is a small minority of medical men who for some reason or another, prefer official posts to general practice. These are the men who initiate discussions and start societies for the "Ethical Salvation of Unethical Races." It is with them we find all sorts of panaceas more or less plausibly advocated for the amelioration and prevention of infantile mortality, and for the employment of other methods of cheating a beneficient Nature. And however varied may be the panaceas proposed there is a remarkable uniformity and universality in one proposition which runs throughout them all as a conspicuous and dominant thread. It never fails to show itself. Always it is proposed that there shall be an increase of *salaried* medical officials. In the long run such a policy is short-sighted and must inevitably lower the status and endanger the best

interests of the medical profession. But with that we have nothing to do. It lies outside the scope of this Journal. We are, however, concerned in pointing out that an army of medical officials, of sanitary inspectors, carrying around with them hundredweights of new palates in gold, of teeth in enamel, of pig's thyroid, of baby incubators, and the other paraphernalia, such as spectacles and free food, nurses and ambulances, that pertain to the craft of saving the unfit, could not possibly make those twelve lost children, derived from such a stock, worthy or happy citizens. In anguish they were born, but in peace they have died, for they died early. Is it kindness to save them, that they may suffer the prolonged misery arising from civic unfitness and the pain which springs from physical imperfectness ? Does it reduce the ultimate or sum total of human misery, to help this weakly-viable race prolong its existence until it can reproduce its like ? We who ask that these things shall be left alone are the true lovers of humanity. We are, too, the true artists of life, for we give an ideal transcending that false sentiment which seeks to save life at all cost, even though by so doing it degrades it. We seek for facile strength, surpassing beauty, and spontaneous happiness. That is a true sentiment, because it is a virile conception based on truth and the noblest love of our race.

Of course, we do not urge that these twelve children should have been left to die in pain or to endure anguish. No good can be served by such conduct. So long as the parents can afford to employ medical aid under the conditions of *voluntary* service and mutual contract, we do not see that anyone has a right to interfere. But the moment this race comes to the State or to the Local Authority for aid, then the Nation has the right to say : " You are obviously an unfit race, a burden to your community, and a source of misery to yourselves. We will relieve your pain and distress, but at the same time understand that your civic liberties are much curtailed and your freedom to propagate your race is altogether and irrevocably prohibited." Such a policy has no latent dangers to better citizens. It can be practically worked without any increase of officialdom, which just now is swarming over the land and devouring its harvest like locusts. It gives all men a chance of maintaining themselves free if they so desire it. It is unaccompanied with those social dangers of momentum which inevitably follow every attempt to exercise unnecessary compulsion or duress upon, or official inspection and control of the people. Sooner or later such compulsion and control gathers velocity and mass and ultimately affects the liberties, rights, and legitimate privileges of the Classes. We are afraid the Classes who initiated free compulsory education did not recognise that fact. And yet it is an obvious biological truism, that as soon as we create a

parasitic official class,* it will, true to the instincts and attributes of parasites, swarm by multiplication over the body of its host, until at last host and parasite are both destroyed.

As soon as a hooligan or other criminal is arrested for some felony or misdemeanour, or an unemployable seeks the shelter of the workhouse, the Nation has a right to say: "In the light of modern knowledge—which endorses some of the conceptions of Plato—we know that the committal of crime or perennial laziness is in many instances, perhaps in most, a manifestation of congenital civic defectiveness. We, therefore, want to know your history. We shall construct your pedigree, and if we find that your race is unworthy, you and all its afflicted members shall lose your ordinary civic liberties, and for you and your race there shall be no further opportunity of propagation." That is the only sensible line to adopt towards *congenital* criminals and loafers. The demagogic policy of providing concerts and lectures, and palatial dwellings in lordly grounds, and the singing of hymns and psalms, may be effective play to the gallery, but so far as statesmanship is concerned it is a mere casting of pearls before an inappreciative audience. It certainly is not Biology applied to Life.

The Local Government Board for Scotland and "The Mendel Journal."

In Part 2 of *The Mendel Journal*, in the course of a reply to Dr. Cobbett's criticisms of a previous article in Part 1, entitled "A Plea for the Operation of a More Virile Sentiment in Human Affairs," Mr. Geo. P. Mudge described certain lamentable results of the boarding-out system of pauper children as practised by the Glasgow Parish Council. As a result of that article and of the comments it called forth in the Press the Local Government Board for Scotland sent a Commissioner to enquire into the allegations contained in Mr. Mudge's article. The Commissioner issued a report last September, and a copy was forwarded to us for publication in this Journal. Its length, however, together with Mr. Mudge's reply, which is long and introduces new matter, precludes us from publishing either in this Journal. We understand that it is Mr. Mudge's intention to publish his reply together with a copy of the Report in book form very shortly. A brief reply was made in the chief Scotch newspapers immediately after the publication of the Report.

* Of course we do not class all officials as parasites. Some, like those of the Army, the Navy, and the Bench, are absolutely essential in *all* states of society, whether barbaric or civilised. And we need to obtain the best men we can for the work.

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