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Mendel's Principles of Heredity

William Bateson (1861-1926) began his academic career working on variation in animals in the light of evolutionary theory. He was inspired by the rediscovery in 1900 of the 1860s work on plant hybridisation by the Austrian monk Gregor Mendel to pursue further experimental work in what he named 'genetics'. He realised that Mendel's results could help to solve difficult biological questions and controversies which others had glossed over, and to challenge the status quo in evolutionary studies. Annoyed by the 'apathetic' stance of his evolutionist colleagues, and incensed by the Oxford professor Raphael Weldon's scathing critique of Mendel in a journal article, Bateson incorporated an English translation of Mendel's work into this 1902 book along with a thorough defence of Mendel's statistical experiments and the principles of heredity derived from them. His book is an impassioned appeal for scientists to adopt this 'brilliant method' which he felt could revolutionise both scholarship and industry. This volume also contains, in an appendix, the original German texts of Mendel's Versuche über Pflanzenhybriden, as published in Leipzig in 1901.

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Mendel's Principles of Heredity

A Defence, with a Translation of Mendel's Original Papers on Hybridisation

William Bateson
Gregor Mendel

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GREGOR MENDEL

Abbot of Brünn

Born 1822. Died 1884.

From a photograph kindly supplied by the Very Rev. Dr Janeischek, the present Abbot.

MENDEL'S PRINCIPLES OF HEREDITY

A DEFENCE

BY

W. BATESON, M.A., F.R.S.

WITH A TRANSLATION OF MENDEL'S ORIGINAL PAPERS ON HYBRIDISATION.

CAMBRIDGE:
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PREFACE.

IN the Study of Evolution progress had well-nigh stopped. The more vigorous, perhaps also the more prudent, had left this field of science to labour in others where the harvest is less precarious or the yield more immediate. Of those who remained some still struggled to push towards truth through the jungle of phenomena: most were content supinely to rest on the great clearing Darwin made long since.

Such was our state when two years ago it was suddenly discovered that an unknown man, Gregor Johann Mendel, had, alone, and unheeded, broken off from the rest—in the moment that Darwin was at work—and cut a way through.

This is no mere metaphor, it is simple fact. Each of us who now looks at his own patch of work sees Mendel's clue running through it: whither that clue will lead, we dare not yet surmise.

It was a moment of rejoicing, and they who had heard the news hastened to spread them and take the

instant way. In this work I am proud to have borne my little part.

But every gospel must be preached to all alike. It will be heard by the Scribes, by the Pharisees, by Demetrius the Silversmith, and the rest. Not lightly do men let their occupation go; small, then, would be our wonder, did we find the established prophet unconvinced. Yet, is it from misgiving that Mendel had the truth, or merely from indifference, that no naturalist of repute, save Professor Weldon, has risen against him?

In the world of knowledge we are accustomed to look for some strenuous effort to understand a new truth even in those who are indisposed to believe. It was therefore with a regret approaching to indignation that I read Professor Weldon's criticism*. Were such a piece from the hand of a junior it might safely be neglected; but coming from Professor Weldon there was the danger—almost the certainty—that the small band of younger men who are thinking of research in this field would take it they had learnt the gist of Mendel, would imagine his teaching exposed by Professor Weldon, and look elsewhere for lines of work.

In evolutionary studies we have no Areopagus. With us it is not—as happily it is with Chemistry,

* *Biometrika*, I., 1902, Pt. II.

Physics, Physiology, Pathology, and other well-followed sciences—that an open court is always sitting, composed of men themselves workers, keenly interested in every new thing, skilled and well versed in the facts. Where this is the case, doctrine is soon tried and the false trodden down. But in our sparse and apathetic community error mostly grows unheeded, choking truth. That fate must not befall Mendel now.

It seemed imperative that Mendel's own work should be immediately put into the hands of all who will read it, and I therefore sought and obtained the kind permission of the Royal Horticultural Society to reprint and modify the translation they had already caused to be made and published in their Journal. To this I add a translation of Mendel's minor paper of later date. As introduction to the subject, the same Society has authorized me to reprint with alterations a lecture on heredity delivered before them in 1900. For these privileges my warm thanks are due. The introduction thus supplied, composed originally for an audience not strictly scientific, is far too slight for the present purpose. A few pages are added, but I have no time to make it what it should be, and I must wait for another chance of treating the whole subject on a more extended scale. It will perhaps serve to give the beginner the slight

assistance which will prepare him to get the most from Mendel's own memoir.

The next step was at once to defend Mendel from Professor Weldon. That could only be done by following this critic from statement to statement in detail, pointing out exactly where he has gone wrong, what he has misunderstood, what omitted, what introduced in error. With such matters it is easy to deal, and they would be as nothing could we find in his treatment some word of allusion to the future; some hint to the ignorant that this is a very big thing; some suggestion of what it all *may* mean if it *be* true.

Both to expose each error and to supply effectively what is wanting, within the limits of a brief article, written with the running pen, is difficult. For simplicity I have kept almost clear of reference to facts not directly connected with the text, and have foregone recital of the now long list of cases, both of plants and animals, where the Mendelian principles have already been perceived. These subjects are dealt with in a joint Report to the Evolution Committee of the Royal Society, made by Miss E. R. Saunders and myself, now in the Press. To Miss Saunders who has been associated with me in this work for several years I wish to express my great indebtedness. Much

of the present article has indeed been written in consultation with her. The reader who seeks fuller statement of facts and conceptions is referred to the writings of other naturalists who have studied the phenomena at first hand (of which a bibliography is appended) and to our own Report.

I take this opportunity of acknowledging the unique facilities generously granted me, as representative of the Evolution Committee, by Messrs Sutton and Sons of Reading, to watch some of the many experiments they have in progress, to inspect their admirable records, and to utilise these facts for the advancement of the science of heredity. My studies at Reading have been for the most part confined to plants other than those immediately the subject of this discussion, but some time ago I availed myself of a kind permission to examine their stock of peas, thus obtaining information which, with other facts since supplied, has greatly assisted me in treating this subject.

I venture to express the conviction, that if the facts now before us are carefully studied, it will become evident that the experimental study of heredity, pursued on the lines Mendel has made possible, is second to no branch of science in the certainty and magnitude of the results it offers. This study has

one advantage which no other line of scientific inquiry possesses, in that the special training necessary for such work is easily learnt in the practice of it, and can be learnt in no other way. All that is needed is the faithful resolve to scamp nothing.

If a tenth part of the labour and cost now devoted by leisured persons, in this country alone, to the collection and maintenance of species of animals and plants which have been collected a hundred times before, were applied to statistical experiments in heredity, the result in a few years would make a revolution not only in the industrial art of the breeder but in our views of heredity, species and variation. We have at last a brilliant method, and a solid basis from which to attack these problems, offering an opportunity to the pioneer such as occurs but seldom even in the history of modern science.

We have been told of late, more than once, that Biology must become an *exact* science. The same is my own fervent hope. But exactness is not always attainable by numerical precision: there have been students of Nature, untrained in statistical nicety, whose instinct for truth yet saved them from perverse inference, from slovenly argument, and from misuse of authorities, reiterated and grotesque.

The study of variation and heredity, in our ignorance of the causation of those phenomena, *must* be

built of statistical data, as Mendel knew long ago; but, as he also perceived, the ground must be prepared by specific experiment. The phenomena of heredity and variation are specific, and give loose and deceptive answers to any but specific questions. That is where our *exact* science will begin. Otherwise we may one day see those huge foundations of "biometry" in ruins.

But Professor Weldon, by coincidence a vehement preacher of precision, in his haste to annul this first positive achievement of the precise method, dispenses for the moment even with those unpretending forms of precision which conventional naturalists have usefully practised. His essay is a strange symptom of our present state. The facts of variation and heredity are known to so few that anything passes for evidence; and if only a statement, or especially a conclusion, be negative, neither surprise nor suspicion are aroused. An author dealing in this fashion with subjects commonly studied, of which the literature is familiar and frequently verified, would meet with scant respect. The reader who has the patience to examine Professor Weldon's array of objections will find that almost all are dispelled by no more elaborate process than a reference to the original records.

With sorrow I find such an article sent out to the world by a Journal bearing, in any association,

the revered name of Francis Galton, or under the high sponsorship of Karl Pearson. I yield to no one in admiration of the genius of these men. Never can we sufficiently regret that those great intellects were not trained in the profession of the naturalist.

Mr Galton suggested that the new scientific firm should have a mathematician and a biologist as partners, and—soundest advice—a logician retained as consultant*. Biologist surely must one partner be, but it will never do to have him sleeping. In many well-regulated occupations there are persons known as "knockers-up," whose thankless task it is to rouse others from their slumber, and tell them work-time is come round again. That part I am venturing to play this morning, and if I have knocked a trifle loud, it is because there is need.

March, 1902.

* *Biometrika*, I. Pt. I. p. 5.

CONTENTS.

ERRATA.

p. 22, par. 3, line 2, for "falls" read "fall."
p. 63, line 12, for "*AabbC*" read "*AaBbc*."
p. 66, in heading, for "OF HYBRIDS" read "OF THE HYBRIDS."

Note to p. 125. None of the yellow seeds produced by *Laxton's Alpha* germinated, though almost all the green seeds sown gave healthy plants. The same was found in the case of *Express*, another variety which bore some yellow seeds. In the case of *Blue Peter*, on the contrary, the yellow seeds have grown as well as the green ones. Few however were *wholly* yellow. Of nine yellow seeds produced by crossing green varieties together (p. 131), six did not germinate, and three which did gave weak and very backward plants. Taken together, this evidence makes it scarcely doubtful that the yellow colour in these cases was pathological, and almost certainly due to exposure after ripening.