Art. VIII.—Cross-Fertilisation of Plants, and Consanguineous Marriage.

1. *The Effects of Cross and Self Fertilisation in the Vegetable Kingdom.* By Charles Darwin. London: John Murray. 1876.
2. *Marriages between First Cousins in England, and their Effects.* By George H. Darwin. Read before the Statistical Society. March 16, 1875.
3. *Note on the Marriage of First Cousins.* By George H. Darwin. Journal of the Statistical Society. September 1875.

THOUGH Mr. Darwin's work, "The Cross and Self Fertilisation of Plants," is written primarily as a part of his wide and fertile Philosophy of Nature, it has necessarily also a particular bearing on the subject whether in-and-in breeding is, or is not, harmful in itself, apart from any intensification of morbid inheritance. Mr. Darwin wishes to show that there is no natural and impassable boundary between the various species; and not only that the various organisms *may* vary through the effects of natural selection, &c., but that they *have* not varied so much but that they can intercross; that many new varieties have arisen through crosses, and that there is even a natural law that all organisms should occasionally intercross. His work is marked by all the exactness and forethought of so practical a biologist. His results are lit up by the brilliant generalisation to which we are accustomed from Mr. Darwin, but which he shares with scarce half-a-dozen other men in all the world. It is the peculiar prerogative of a great thinker, not only clearly to explain the subject on which he happens to be writing, but to light up every little fact upon which he happens to touch; and in this quality of suggestiveness Mr. Darwin is particularly happy. Each one of his works supplies food for months of mental digestion; each one of his works marks an epoch in the history of its particular subject; and this particular work before us marks an epoch in the history of our knowledge concerning the effects of close interbreeding compared with the effects of crosses.

While physiologists have been busy for centuries in investigating the properties and functions of every organ of the human body; while they have busied themselves with experiments on the effects of various articles of diet and of medicine; while they have carefully recorded hundreds and hundreds of observations on the inheritance of peculiarities—such as the possession of six

fingers, or an abnormal number of limbs, or the transposition of the internal organs; or such as the porcupine man, the hairy woman, the Siamese twins, and the two-headed Nightingale—it will scarcely be believed that, until lately, there has hardly been a single serious investigation concerning the results of close interbreeding and consanguineous marriage. Legislation on the latter subject has indeed been common enough, nor is it necessary to say with much the same result as the commercial legislation before the age of Adam Smith. Never, perhaps, has there been a more baneful exercise of authority in matters of opinion in repressing original investigation than in this subject. And the prohibited degrees, as handed down to us, are regarded with a reverence difficult to comprehend, when we know that, so far from taking their authority from the Bible or the statutes of England, they are, in one case, in opposition to these, and in others have no authority at all.

It is, however, beside our subject to enter into any controversy on this question. What we wish to call attention to is the curious ignorance on the authority of our prohibited degrees; the extraordinary fear or apathy with which all inquiry on the subject is regarded, in view of the important interests involved in, and general feeling of alarm as to the consequences of, marriage between first cousins—an alarm which must be widely spread if we may judge by the many letters in medical journals, the inquiries which clergymen and physicians are constantly receiving, and the clandestine marriages which every now and again disturb the harmony of families, embitter the relations between parents and children, and figure amongst the law-news in the daily press.

We should be led too far were we to show that this fear is due to no law, natural or divine, but simply to the spirit of asceticism which flourished in the early Christian Church; that it is inherited from the time when marriage was regarded with pious horror, and only tolerated as the least objectionable means for the production of monks and nuns. Marriage with a niece being first called into question, marriages with cousins were also prohibited. Marriages with more distant relations were in turn forbidden, until a corrupt and powerful Church, finding that people were willing to pay for the privilege of marrying whom they choose, extended the prohibitions from time to time up to the fourteenth degree, and ultimately as far as any relationship could be traced, including also affinity and god-parentage in their wicked and immoral laws. To enforce them was, of course, not intended. They were merely meant to act as a milch-cow to fill the priestly coffers. Their effect, however, upon generations of credulous and unthinking men and women was

to engender the belief that marriages between cousins were immoral, and would be punished by Heavenly wrath; their orchards "would not hit," their sheep would get the rot, and the children would suffer for the sins of their fathers. Few people, we imagine, even nowadays, would think it otherwise than a shocking and a horrid act were a god-father to marry his god-daughter, even though no relationship existed between them.

It was long before any one doubted that consanguineous marriage was harmful to the offspring. For proof, the usual system was to gather a few statistics of cases which happened to have caught the physician's attention because a whole family happened to be dumb or deformed; or, to mention some particular case, where long-continued in-and-in breeding in horses or in dogs had ended badly. What it was mattered little; the fact was undoubted. No one took the trouble to deny it, and few took the trouble to announce it. But at last among practical cattle-breeders it became a practical question. From hearsay and opinion among these, it worked its way into the medical academies, and produced fuller, and apparently most alarming, statistics on the dreadful effects of marriages between first, second, third, or even seventh cousins. The whole question began to assume a more tangible character. Facts could be opposed by facts. But even then the discussion was somewhat one-sided, because on the one side weighed the whole power of the Church and a mighty mass of tradition, while on the other there was little to oppose it; for cases of consanguineous marriage in which there was nothing abnormal failed to strike the attention of any observer, and were not collected.

By little and little the nature of the proofs required began to be recognised. It was shown that the collection of cases was worse than useless; that we must have the evidence of a careful and wide statistical inquiry, or observations on small isolated communities, or observations on the lower animals. The two first would decide the question whether marriages between cousins were practically harmful; the last would decide the question whether the harm done, if any, was due to an intensification of a hereditary taint merely, or whether it was due to an organic necessity that there should be an occasional cross.

This last question is that which Mr. Darwin in his valuable work has attempted to answer, and we fear that the answer is to some extent unsatisfactory. Not on account of what Mr. Darwin has done, however, in the particular branch of inquiry which he has undertaken, but on account of the darkness in which all other branches are left enshrouded. The light Mr. Darwin throws is like that of a bull's-eye, making the surrounding darkness greater by the contrast; and hence there is a probability that this work may induce others without any pre-

vious research to apply his results to other parts of the natural kingdom. Mr. Darwin has indeed made experiments on the comparative growth of many small flowering plants crossed and self-fertilised. He has given a full relation of his manner of procedure, with a judicial account of his results, and has come to the conclusion that there is no necessary intensification of hereditary taints; that in-and-in breeding is harmless so long as the sexual elements are sufficiently differentiated; or, in other words, that plants whose habitat was constantly changed, could be allowed to fertilise each other for ever; while, if this was not attended to, crossed plants, with some exceptions, grew a little taller, or were heavier, or produced more seed.

We must remember, however, that Mr. Darwin's observations were confined to that part of the organic world in which we should, from theoretical reasons, consider that the prevention of crosses would tell most hardly. These highly organised structures have, as it were, outgrown the position they originally occupied in nature. They require their little luxuries, such as change of soil and climate, or, what comes to the same thing, crosses, as absolutely as we find we cannot do without houses and well-cooked food. They require much, and are rooted to one place. They are, compared to the lowly Algæ and Fungi, delicate and ill-prepared to withstand the evils of adverse circumstances. They are, like civilised men, unable to live without higher appliances; but *with* those appliances immeasurably superior to their less specialised brethren. In their peculiar line of development, the highest plants are almost as superior in organisation as are the highest animals compared to the lowest. But in one sole point there is an immense difference between animals and plants. When the seed has once struck root, there can be no further change for the higher plant; while the higher animals can and do subject themselves to many and various changes of circumstances. The lower animals and plants, again, have a certain amount of mobility, a shorter life, and being less highly organised, have less need of change. Emigrants to Canada, for instance, are farmers, smiths, carpenters, clothiers, all in one; while in the more highly organised society of England, not only are the different trades followed by different persons, but in factories a man passes his life in performing one simple operation over and over again. We should say, therefore, that change is necessary for the latter individual, while for the former it is not; and so with organic structures the same rule doubtlessly holds good, that the more specialised organisms need change, while the simpler do not. Speaking generally, the higher plants have a longer life, and a more highly organised structure, and therefore require a greater amount of change

than do others. Moreover, their only chance of escaping competition, exhaustion of soil, or that obscure tiring of certain cells called "want of change," is only experienced once, at the beginning of the life of each individual, when the seed is dispersed. If the seed fall close to the parent plant, the young individual would be subject to the same circumstances its parent had experienced, and would be subject also to great competition. Even were it not theoretically obvious, the importance of dispersion of the seed is shown by the great variety and wonderful adaptation of means for their dissemination. It is not improbable, indeed, that it is solely for this reason that the seed and its analogies are formed; for if seeds were formed merely to multiply the plant, this object might as well be attained by means of buds, which drop off and grow around the parent, or by the spreading of roots and growth from them of new shoots; while, if the object of the seed were to secure the preservation of the plant under adverse circumstances, such as frosts or floods, which would kill the parent but not harm the seed, portions of the stem, such as the "eye" of the potato, or certain buds, might easily be endowed with the requisite quality of resistance. Yet so anxious is Nature to scatter the various organisms far and wide, that we find not seed alone supplied with the means of conveyance from place to place, but every stage of the organism, from the first germs of its appearance on the parent, to the time when it finally strikes root, both in plants and in the (comparatively to other animals) non-locomotive lower animals, is also furnished with the means of transport as perfect as is consistent with other necessary conditions of life. In the lower plants, for instance, the *zoospores*, or sexual elements, are frequently dispersed *before* they unite to form a fertile cell or seed; and the seeds of the higher plants have wonderful contrivances by which they may be wafted through the air on feathery plumules, as in the dandelion, or on wings, as in the ash; or they may be carried by birds or quadrupeds, attached to the wool, &c., &c. While in the lower animals the eggs may be dispersed by the parent, as in winged insects, or by water, as the floating eggs of the dog-fish; they may be laid in an animal, or the larval form may itself have great power of locomotion.

But besides this imperative and universal need of *dispersion*, there is another and somewhat antagonistic need of *defence* or parental care. Dispersion is in itself to some degree a defence, because where an immense quantity of seeds are widely dispersed, it is more probable that some will escape adverse circumstances, and light upon favourable circumstances, than were they not widely dispersed. But parental care of the offspring

will undoubtedly prevent the waste production of many which would otherwise be destroyed, and it is therefore a saving of labour. Indeed it seems to be a law, that the higher an animal is in the scale of creation, the longer does it remain a burden to its parents; that is, the higher animals have only arrived at their higher stage of development through circumstances which have enabled them to remain a long time under parental care. The consequence of this antagonism to dispersion is shown in the fact that they are not spread abroad until they have reached a comparatively advanced age. In organisms a little lower in the scale, distribution takes place in the larval form. Lower still, the fertilised seed is distributed; and at the foot of the scale, in those organisms whose life is short, whose distribution must be very wide, or who must run the risk of utter extermination from temporary or local adverse circumstances—in these the distribution takes places even before fertilisation, as in those low organisms which multiply by means of zoospores. But we have already seen that to conquer adverse circumstances by the production of immense quantities of seed is as wasteful as to conquer an enemy by hurling immense masses of troops against him. An unskilful officer does so, but a more skilled throws up temporary defences, or advances under cover. So in plants there is a constant effort to combine judicious defence with attack, judicious dissemination with as much parental care as possible. Thus, to take the most salient instance, in the fern species a seed is not produced immediately from the parent, but an immense quantity of spores or immature seeds, which cost little in the production, because they are as it were unfinished, are scattered far and wide by the wind. By this step, therefore, the fern has produced a greater quantity than would otherwise be possible, and of lighter quality, so they can be widely dispersed; but they are immature, and here is the danger. Many of course fall in unsuitable places, and are lost; many again find themselves unable to struggle with their neighbours, and are also lost; but a greater proportion succeed, and germinate into a sort of plant called a *prothallium*, which only exists to produce the sexes. It is a very simple and unspecialised structure, as the spore from which it grew; and hence it may produce the sexes from almost any part, and throw rootlets out from almost any part: its office is to supply the parental care. It produces and nourishes the sexes, which, when mature, sometimes fertilise each other, and sometimes secure a little further dispersion by conjugating with neighbours. A cell or seed is thus formed, which contains sufficient protoplasm to germinate into a new form, and the process is complete. In short, every plant strives to disseminate its spores, &c., as widely as possible, and, at the

same time, to ensure their growth by supporting them as long as possible.

In this sketch it will be noticed that no importance is assigned to the question of crosses, and indeed we look upon it as something of entirely secondary importance. We do not think that crosses are important enough, or so necessary as to produce the various contrivances for crossing; but we do think that distribution is. For the former we see no obvious necessity; we do not see that it is constant; we do not see that it is fundamental. The importance of the latter is obvious; it is constant and present from the beginning. In the lowest types, those types which were the parents of and formed the habits of the higher, the union of spores is by no means a necessity, and though they do frequently conjugate, they can, nevertheless, develop of themselves. The earliest form of conjugation is but the addition of plastic matter to a growing cell, and is really the same thing as the addition of nutriment to a growing bud. It is extremely difficult otherwise to explain the almost absolute identity of all kinds of spores; as, for instance, in *Ulothrix zonata*, which, though it produces two kinds of spores, small and large, which represent to some extent the male and female elements, yet, as Dr. Arnold Dodel points out,* these need not conjugate. They *may* do so, but the one will grow without conjugation like a seed. In short, this almost absolute identity, morphologically, of all spores, their power of independent growth, leads us to believe that any difference in size or form between them is not due to a difference of nature between them, but due rather to the division of sex which took place to secure greater perfection and economy of labour. If the sexes became divided for this reason (and why should they not, seeing that all other functions of organised bodies have become subdivided between different organs?), then two sexual elements are produced instead of one, and they must meet to give this sexual perfection effect. Since, however, dissemination is a necessity, one kind is generally motile; and since, also, parental care is necessary, one is generally for a time fixed to the parent until it be still further developed. The former, being motile and soonest cast off from the parent, will have a tendency to be smaller than the other, which is fixed, and in the constant receipt of nourishment. We see, therefore, that the larger or female cell may be identical in constitution with the smaller or male cell, although it may differ in appearance, size, and action. We find, in fact, in nature this constant struggle between dissemination and parental care illustrated in

* *Nature*, vol. xv. pp. 512, 513.

every variety of way. In some plants distribution is so urgent that the conjugation of the two sexes is quite a secondary consideration, notwithstanding that each is probably somewhat deficient in the necessary material; in others, one cell is motile and the other under parental care; in others, again, both may conjugate before dissemination.

In those plants in which the conjugation of two cells has become necessary from the deficiency of plastic material in one alone; the motility of the male spore is an advantage not so great as the motility of the product of the union of the two, but it is nevertheless an advantage, though a lesser one. For suppose a family of plants, which we shall call, for the sake of the illustration, red tulips, among which was a yellow individual which produced more pollen than was needful for fertilising its own female organs; and suppose this superfluous pollen year after year conveyed to the neighbouring tulips, these would after a time produce a series of half-breeds with more and more yellow in their colouring, and with a tendency also to a greater production of pollen, until at last all the tulips growing in that spot would become yellow; they would have become almost entirely the descendants of an individual. By the excess production of one sex only, the tulips therefore would have gained an increase in numbers, which would be equivalent to increased distribution. Indeed, we may imagine some such course of excess of production, being the possible precursor of sex. Suppose a state in which there is no sex, but the plants produce only one kind of spore, which will grow without any conjugation. Their greater production would cause a greater tendency to conjugation; for those which happened to meet and conjugate would possess a greater joint-stock of nutriment, and would, therefore, be less likely to succumb under adverse circumstances. Again, since the two most important objects in reproduction are dissemination and parental care, the tendency will be for certain of the spores to be nourished longer by the parent, and for certain other spores to be produced in greater quantity. As we have seen, the spores which are longest nourished must possess the greatest vitality or size, and will be female spores, while the others, or male spores, have a tendency to conjugation, and, as they are not fixed, increase their tendency to motility and production in greater numbers than is needed for the fertilisation of the female cells. Many of them will, therefore, go to form half-breeds of a like tendency.

A cursory glance at the systems of reproduction in the vegetable world will show a decided disposition, as plants rise in the scale of nature, to reproduce themselves less by immature reproductive cells that is, that the more highly developed and

specialised plants trust less to dissemination by imperfect spores, and more to parental care of the embryo. In the flowering plants there is no reproduction by spores, nor even a go-between such as we have seen in ferns; and before the fertile product of the union of male and female cells is turned loose on the world, it is furnished with a little capital in the shape of a store of nutriment in the seed. Yet they have really undergone the same process as the lower plants. The two generations which go to produce a young form are expressed in the formation of the seed. But when a flowering plant propagates itself otherwise than by seeding, it is almost always by tillering or branching, and the offspring is thus not separated from the parent until it has formed, with parental aid, a step or root for itself. The only form of immature dissemination which they retain is the dispersion of the pollen, or motile male spores, which, however, are incapable of growing by themselves, but retain for the parent, in some measure, the benefit that the plant which succeeds in fertilising most possesses in comparison with others: it fertilises its own flower, and has enough pollen over to fertilise others, and thus produce half-breeds of its own family.* To such an extent is this crossing carried, that many plants depend upon it entirely, and devote their entire energy to the production of one sex.

Now it is urged in favour of the absolute necessity of crosses, that every organism, from the lowest to the highest, sometimes crosses; that there are, besides, many contrivances which prevent self-fertilisation, and favour crosses; that the division of the sexes between two individuals can only be that crosses may occur; that in some plants, pollen from their own flowers is positively poisonous to themselves, while pollen from any other flower of their kind is perfectly fertile; and, finally, Mr. Darwin has shown, by careful and long-continued experiments, that crossed plants are more vigorous than inbred plants.

If what we have urged above, however, has any germ of truth in it, it would be an astonishing thing were any organism never to cross; though the cross in the lower organisms would not probably be a benefit to the one crossed, but to the one which crosses. A plant can never get the full advantage from the

* An instance is recorded by Mr. M. S. Evans, who writes from Natal, that a plant of the sub-order *Coffeæ* bears flowers so arranged that, to get at the honey, ants must cover themselves with pollen. But the anthers ripen and cover the stigma with pollen before the bud opens. (See *Nature*, vol. xiii. p. 427.) It is, therefore, usually self-fertilised; but, should it miss self-fertilisation, it will be crossed. Moreover, the plant which succeeded in crossing most would also have produced the greatest number of offspring.

division of sex, never devote its whole power to the production of one sex at one time, without becoming at least functionally unisexual. If it is a hermaphrodite, either the anthers will be ready before the stigma, or *vice versâ*, and crosses will be necessary, either by means of the wind, or by water, or by insects; but in the last case, the flower must also produce coloured petals, scent, or honey, to attract them. Or it may be that many hermaphrodite flowers first produced honey, &c., to assist in dispersing their pollen and fertilise themselves. When once an hermaphrodite, from accident or division of labour, has become unisexual, it will always cross; and should it afterwards resume the sex it has dropped, and become again hermaphrodite, there is no reason why the customary method of fertilisation should be altered, provided it was efficient. The necessary insects are there, and need not be again formed.* It is by this long-continued custom of crossing that we may probably explain the self-impotency of some plants, and why plants accustomed to cross are benefited by a cross. On the other hand, a plant, when once, by change of circumstances, it has become habituated to self-fertilisation, also seems to suit itself to circumstances, and a cross does not benefit it in the least.

It might be objected to this view, that it is absurd to suppose that all the elaborate structures which secure cross-fertilisation are created in vain; that were crosses useless, plants could as easily adapt themselves to self-fertilisation as they now do to crosses. But we must remember that a cross need not be useless, even though it is of minor importance. We must remember that distribution is the one factor which is ever present, while crosses are not; and that contrivances for crossing, even in the most wonderfully formed flowers, are but the lineal descendants of contrivances for dissemination. If the beneficial effects of crosses were a fundamental, and not a secondary and partial truth, how shall we account for those plants which habitually fertilise themselves? Or how should we explain the superiority of some inbred kinds to the same kind crossed? Why should the sexes be divided in those closed flowers called "cleistogene" which must fertilise themselves? Or why are the sexual organs nearly always enclosed in the same flower, instead of being divided among separate flowers? The only apparent explanation appears to us to be, that the sexes have gradually become divided as other functions have become specialised; and that every care was taken to combine sure fertilisation with as much dissemina-

* The only change that is likely to take place at first would be some such case as that of *Caryanthes macrura*, given by Professor Müller, which is so formed that *self*-fertilisation by bees is almost a necessity.—*Nature*, vol. xv. p. 358.

tion as possible. Of course there are also other incidental advantages. Plants which have the power of crossing form, as it were, one vast spreading organism; and hence should one set of sexual organs become aborted or useless, the other set will nevertheless be fertilised; while had the flower been unable to cross, the perfect female organs would have been produced in vain. Mr. Darwin also points out that did not crossing occasionally take place, plants might run the danger of becoming too much accustomed to one set of circumstances. They might become perfectly adapted to those circumstances, but were these to change, the plants might be unable to exist. Plants, however, which are subjected to an occasional cross become cosmopolite, and able to withstand the same changes to which their ancestors have been accustomed.

Where we venture to differ from Mr. Darwin is not so much in the deductions which he has drawn from the experiments he has conducted, but in their universal application. It is an application from the particular to the general; and though he appeals to the universality of crosses in nature, that appeal is not absolutely convincing, unless crosses originated merely for the purpose of securing an occasional change—the very thing which requires proof. Because highly organised flowering plants are the better for an occasional cross, it is hardly safe to conclude that all organisms are the better for a cross. Because crosses are extremely usual in nature, it is hardly safe to conclude that crosses are a primary necessity. Observations on highly organised plants may explain their own constitution, but will not explain the constitution of the lower organisms, still less that of the whole organic world.

Since Mr. Darwin looks upon the necessity of crossing as the key to the whole machinery of sex, he does not hold the common belief that in the earliest stage there is no sex, and reproduction takes place through the subdivision of the parent or by budding; that afterwards only a part of the plant is subdivided into spores; that afterwards, in a higher stage, these spores get differentiated and quasi-sexual; that finally different individuals produce the different sexes. He does not, in short, believe that the sexual organs have originated, as the other organs have originated, from a specialisation of function, so as to perform better, and at a less physiological cost, what a less specialised organism performs worse at a greater cost. But he considers the earliest stage to have been unisexual—

"As is still the case to a large extent, . . . if we admit the view, which seems highly probable, that the conjugation of the Algæ and of some of the simplest animals is the first step towards sexual reproduc-

tion; and if we further bear in mind that a greater and greater degree of differentiation between the cells which conjugate can be traced, thus leading apparently to the development of two sexual forms" (p. 409).

Again—

"The object gained by the two sexes becoming united in the same hermaphrodite form probably is to allow of occasional or frequent self-fertilisation, so as to ensure the propagation of the species, more especially in the case of organisms affixed for life to the same spot. There does not seem to be any great difficulty in understanding how an organism, formed by the conjugation of two individuals which represented the two incipient sexes, might have given rise by budding first to a monœcious and then to a hermaphrodite form" (pp. 462, 463).

But what was the primary unisexual form? If unisex is the separation of the two sexes between two distinct individuals, how can we look upon the lowest Thallophytes as the primary form? How can we look upon any organism as unisexual which produces spores that may unite with spores produced by the same plant, or by another plant, or simply grow of themselves without uniting with any other? We could hardly call it hermaphrodite, and then only in the sense that we might call formation of free cells hermaphrodite, by the collection of the protoplasm, or contents of the mother-cell, around certain centres, and thus forming so many new cells. Mr. Darwin might instance some of the *Characeæ* as a type which produces the different sexes on different individuals, and yet is almost at the bottom of the vegetable world, but would he consider them to be the primary form of sex? Moreover, the morphological position of the sexes in those which only produce one sex on each individual would rather point to the suppression of the second sex in each case than to the plant having subsequently developed another sex in those forms which produce both sexes on each individual. We fear that we have misunderstood Mr. Darwin's meaning, but it appears to us that he would recognise no connection, no chain of development, between the different methods of reproduction, between budding, adventitious shoots, subdivision of the parent, subdivision of a part of the parent to form spores, conjugation of similar elements, conjugation of dissimilar elements, hermaphroditism, and unisex. In the absence of instances, we fail to see how the sexes become *united* in an hermaphrodite; we should rather have put it, became *differentiated.* From a differentiated to an undifferentiated state, from a state in which the two sexes are apportioned between two individuals to a state in which both sexes are given to one individual, seems to us not progression, but retrogression; and

what warrant is there from the other functions of organisms that the most differentiated state which we know to exist was the primary state of the whole organic world? Indeed, it seems rather that, looking merely at the consequences of the faculty of crossing, any theory would be wanting which ignores the importance of distribution, and the gain of division of labour.

We may admit at once, however, that Mr. Darwin has conclusively shown that crosses in highly organised plants are necessary to plants accustomed to crosses. To have established this is to silence for ever a considerable amount of theoretical doubt as to the desirability of an occasional cross between individuals, and sometimes between distinct species. His experiments besides, however, tend strongly to confirm the truth of the above speculations as to the secondary value of crosses. Mr. Darwin found that certain individuals among the in-and-in bred plants developed a tendency favourable to in-and-in breeding, perfect in one case, less perfect in others, but in most not permanent, because theirs was a struggle. For Mr. Darwin planted a cross-bred seed and an inbred seed on opposite sides of the same pot, and let them struggle against each other. That the crossed plants were generally victorious is not to be wondered at, though it was probably unexpected by most people that the result would be so marked; and all the more since it was the general opinion that in crosses between distinct varities or species the gain in size and luxuriance was due chiefly to the usually impotent state of the reproductive organs.

"Seeing," says Mr. Darwin himself in a former work ("The Variation," &c., ed. 1875, ii. p. 156), "that almost all organic beings when exposed to unnatural conditions tend to become more or less sterile, it seems much the most probable view that with cultivated plants sterility is the exciting cause, and double flowers, rich seedless fruit, and in some cases largely developed organs of vegetation, &c., are the indirect results, these results having been in most cases largely increased through continual selection by man."

But in these cases the crossed flowers grown in competition with the self-fertilised were, as a rule, the most fertile. It seems, therefore, that in the last case, the gain in crosses must be due to the fact that though these flowers are actually hermaphrodite, they are functionally unisexual; hence we should expect to see in such plants the most vigorous offspring derived from the kind of union which is most natural and customary, and to which they have been adapted by the unbroken practice of generations. It is not at all unlikely that some of these plants, with care, might be bred so as to arrive at a state like that of the pea, the bee-ophrys, and other plants which by

some accident, from a state in which crosses were habitual, arrived at a state in which self-fertilisation became habitual; or that qualities like those which appeared in the sixth generation of *Ipomœa*, which, after long in-and-in breeding, was taller and more fertile than the crossed kinds, and was not benefited by a cross itself, might be developed. At the same time we should not expect such qualities to appear without careful cultivation; for self-fertilisation is useless to all flowering plants—indeed, to all organisms where fertilisation is otherwise assured; while, on the other hand, a unisexual state is the highest degree of economy at which any organism can arrive.

These are our reasons for the belief that Mr. Darwin's book, in careless hands, may lead to careless generalisations; and though what we have advanced is mere hypothesis, we think it notwithstanding may be worth some consideration. Mr. Darwin never advances an opinion without having a vast array of facts in its support; and it is probable that he can substantiate his views even in the lower plants. But he has not done so yet. His object having been to relate the result of his experiments on flowering, honey-bearing, scented, and coloured flowers, we could hardly expect a treatise on flowerless plants. The whole system of reproduction, however, is so finely graduated from the lowest to the highest, that we cannot think his observations on flowering plants alone are applicable to all. We want experiments on plants which do not habitually cross. We want experiments on the lower animals also. Till then, we may not venture to say that the sexes were divided for the benefit of crossing; still less can we say that it is a necessity for mankind occasionally to intercross.

Even were it beyond doubt a law of nature that all organisms should occasionally intercross, we should yet have no proof of the banefulness of consanguineous marriage. To breed from the same flower is probably closer, and at least as close, as the hermaphrodite reproduction in each segment of the tapeworm. To breed from the nearest relatives among animals would not be as close as between different plants growing in the same locality, because the animals are constantly changing their conditions, and the plants are not. How much less, then, is the danger of marriage between near kin among human beings, seeing that no human union could be formed as close as is easily done in quicker breeding and maturing animals? Mr. Darwin says (p. 461)—

"From the facts given in this volume we may infer that with mankind the marriages of nearly related persons, some of whose parents and ancestors had lived under very different conditions, would be much less injurious than that of persons who had always lived in the

same place and followed the same habits of life. Nor can I see reason to doubt that the widely different habits of life of men and women in civilised nations, especially amongst the upper classes, would tend to counterbalance any evil from marriages between healthy and somewhat closely related persons."

There are, however, three classes of direct proof that consanguineous marriages are harmless to the offspring—

1. Observations on individual cases.
2. Statistical observations on large numbers.
3. Observations on isolated communities.

Of the first class, we need say nothing beyond the warning that a case of consanguineous marriage which seems to show a harmful result on the offspring is no proof whatever that consanguineous marriages are harmful, because the harm may be caused by ordinary inheritance, just as in families where the parents are not related. On the other hand, if consanguineous marriage is a cause of harm in and by itself, and we find very many cases where the results do not bear out this view, we cannot believe that it is either a very dangerous or constant cause.

It is a remarkable fact, that though immense labour and pains have been bestowed by staticians and physicians on observations concerning these marriages, they seem to have been unaware that, for such observations to be of any use, it is of the utmost importance first to find out the proportion that one kind of marriage bears to the other. To say that 1·4 per cent. of the deaf-mutes are born from marriages between near kin, conveys no meaning unless we know whether the proportion of consanguineous marriages to non-consanguineous marriages also stands at 1·4 per cent., or whether it is greater, or whether it is less. Guesses and estimates can only mislead; and while the point is still doubtful, all observations are unripe for deduction.

To Mr. George Darwin belongs the honour of having by a method, or rather series of methods, as ingenious as they were laborious, ascertained with some degree of accuracy the proportion that marriages between first cousins bear to others. Wishing to ascertain whether consanguineous marriage was really as harmful as it was generally considered to be, he was at once confronted with the fact that there was no basis to start from. But, unlike some of his predecessors, he was not satisfied with a rough estimate; and being gifted with a clear sight and fertility of resource, he accomplished what neither physicians, nor staticians, nor even Governments, have hitherto been able to do. To describe or criticise these methods would take up too much of our space. Let it suffice that his results, although, of course, to some extent conjectural, are beyond comparison safer than

the wild guesses of former writers on the subject. The proportions he gets are that marriages between first cousins, among all classes, are 1·5 per cent. in London; 2 per cent. in urban districts; and 2·25 per cent. in rural districts. While, if we take the different classes, marriages between first cousins are in the proportion of 3·5 per cent. of all marriages in the middle and upper classes and landed gentry, and 4·5 among the aristocracy. Having got these data, he applies them to statistics obtained from various English and Welsh lunatic and idiot asylums; and the results, from somewhat imperfect returns, show that on a total of 4308 patients who could answer, 149 or 142 were the children of first cousins, or 3·45 or 3·29 per cent. If only the most trustworthy returns are taken, on a total of 2301, 92 or 93 were born from first cousins, or as nearly as possible 4 per cent.

It is probable, however, that the returns are even more favourable than this; for it is not enough to state merely the number of patients born from first cousins; we want also to know the number of families represented. It is probable that a far greater proportion of non-consanguineous marriages are affected than consanguineous, because where the parents are relatives there may be some tendency to an intensification of disease, and, consequently, each *affected* family among the consanguineous marriages may produce more deaf-mutes than the others, while a greater proportion of consanguineous marriages may be free from deaf-mutism than the non-consanguineous. And we find this supposition is confirmed by the Irish Census Reports. Taking the average of the last three census returns, we find that every deaf-mute of non-consanguineous origin represents one family; while one and a half deaf-mutes of consanguineous origin go to every family represented; and the proportion would be greater were we only to take first cousins. Now let us take an imaginary case. Say that 10,000 marriages produce 100 deaf-mutes. Of these 10,000 marriages, say 4 per cent. or 400 are between first cousins; and of the 100 deaf-mutes, say that 4 or 4 per cent. are born from marriages between first cousins. Now, since 1·5 deaf-mutes from first-cousin marriages go to a family, these four deaf-mutes represent 2·7 families ($1{\cdot}5 \times 2{\cdot}7 = 4$), while the 96 remaining deaf-mutes represent 96 families. Hence we have (10,000 − 400, or) 9600 non-consanguineous marriages, of which 96 or 1 per cent. turn out harmful to the offspring; while we have only 2·7 out of 400 marriages between first cousins turning out harmful to the offspring, or 0·6 per cent. only.

Applying this to Mr. G. H. Darwin's returns, we have a total of 2301 deaf-mutes, of which 93 were born from marriages between first cousins, and represent ($\frac{9}{1{\cdot}3} =$) 62 families; while

the remaining (2301 − 93 =) 2208 deaf-mutes represent 2208 families. On a total then of (2208 + 62 =) 2270 families represented, only 62 or 2·7 per cent. proved harmful to the offspring. That is, there is less probability of a marriage between first cousins producing a deaf-mute than a marriage between persons who are not related by nearly half per cent., even though we take the proportion of first-cousin marriages to others as low as 3 per cent.

We must, of course, be careful not to deduce too much from these figures, which are too small to settle the question at all satisfactorily. At the same time, they are valuable as an indication; for though we have other statistics on the same subject elsewhere, we cannot apply them, since we do not know the proportion of all consanguineous marriages to other marriages. Whatever Mr. G. H. Darwin's inquiries may be worth, as far as they go (and they were extended to deaf-mutism, sterility, low vitality, and superior mental and physical power), they show that at least there is no danger from marriages between first cousins. Thus, Mr. G. H. Darwin obtained information concerning 366 families who had furnished deaf-mutes to asylums, of which eight were unions between first cousins, or barely 2·2 per cent. Again, by counting the children of cousins in Burke's "Landed Gentry and Peerage," he found that not only were marriages between first cousins more fertile, but the children of cousins, even if they contracted a non-consanguineous marriage, were also more fertile than the average. If we put the average number of children per non-consanguineous marriage at 2·2, that where one parent is the offspring of cousins will be 2·3, and where the marriage is between cousins the average will be 2·4. Mr. G. H. Darwin thinks that this slight preponderance in favour of consanguineous marriage may be due to accident, since much of his data is founded on estimate; but we think he is wrong here, and undervalues the accuracy of his results. According to Oesterlen, 20 per cent. of all marriages in Great Britain were barren in the year 1851; Simpson found 11·7 marriages in Great Britain were barren; Dr. West found the average about the same; and Dr. Duncan puts it at 15 per cent. Taking a low estimate from the last three, we have 12·8 per cent. as the average of sterility. As for the prolificness of marriages in Scotland, where the average stands very high, 4·64 children were born per marriage in 1861; in England the average is 3·89; in France only 3.1.

Now Dr. Bemiss collected 833 cases of consanguineous marriage, of which only 53, or 6·4 per cent., proved barren; while the remaining marriages produced 3942 children, or an average of 4·7 per marriage, barren and fertile. Of 299 cases collected

from various authors in a recent work on this subject,* we find 17 marriages were barren, or 5·7 per cent. We were inclined to attribute this superiority in fertility of consanguineous marriages to the probability that cousins know more about each other's health before they marry, and also marry earlier, generally, than do persons who are not related; for we know from Dr. Duncan's researches that early marriages are the most prolific. But this would not explain the greater fertility of the offspring of cousins who marry strangers, if we may venture a deduction from 93 marriages only; and Mr. G. H. Darwin suggests that since it is more likely that consanguineous marriages will occur where the family group is large than where it is small, this superior fertility may be inherited.

The general result, then, of such statistics as we possess, in the absence of a census, points to the harmlessness of marriages between near kin. We could bring forward many more figures on this subject, all tending to the same point. But in a short paper of this kind they could not be properly discussed; nor is it necessary, as we conceive that the figures already given are quite sufficient in the present doubtful state of our knowledge on the true proportion between consanguineous and non-consanguineous marriages.

The third proof, or the effects of continued intermarriage in a small community, is next door to direct experiment, and only differs in being less exact. To experiment on human beings, it would be necessary to shut up a community, under favourable circumstances, and see that they contracted only consanguineous and healthy marriages. Luckily, there is a remarkable tendency in all animals to separate off into small communities, and this tendency is exemplified in the human animal by all savage tribes, which refuse to intermarry with their neighbours, or have established castes, and in European countries by many small communities. The fishing populations dotted around the western European coasts regard the peasantry with the greatest contempt, and, of course, refuse to intermarry with them. Inland, the hostility of neighbouring villages has not long been extinct; and in many parts of Europe there are still spots thus artificially or naturally isolated, the inhabitants of which constantly intermarry among themselves. Such instances are particularly valuable in an inquiry of this nature, as the inhabitants not only do not cross, but never subject themselves to any change. We might give many, but confine ourselves to an account of the community at Batz, near Le Croisic, given by Dr. Voisin, who carefully inquired into the history of every marriage.

* See the *Westminster Review* for October 1875.

This commune of Batz is situated on a peninsula, bounded on one side by precipitous sea-washed rocks, and shut off from the mainland by a salt marsh. The inhabitants number 3300, and have but a very limited intercourse with the rest of the department (Loire Inférieure). Their character is simple but intelligent; they are reserved to strangers, and drunkenness and crime are unknown. Though they have been in the habit of closely intermarrying among themselves generation after generation, not a single individual suffered from any disease of the mind, from deaf-mutism, albinoism, blindness, or malformation. At the time of Dr. Voisin's visit, everybody was related of course, but 46 marriages were between near relatives, of which 5 were between first cousins, 31 between second cousins, and 10 between cousins of the fourth degree. The 5 marriages between first cousins produced 23 children, or an average of 4·6 per marriage; while the average for all France is, according to M. Husson, only 3 per marriage. All these children were healthy, but 2 died from acute diseases. The 31 marriages between second cousins produced 120 children, or 3·87 per marriage, none of whom were affected by any congenital malformation or infirmity, but 24 of them died of acute diseases. The 10 remaining marriages produced 29 children, all healthy, but 3 of them died of acute diseases. On the whole 46, only 2 marriages proved barren, or 4·3 per cent.; while the average of barrenness, as we have seen, stands far higher.

We see, therefore, that an ignorant community of people, who are obliged daily to toil in the unhealthy occupation of collecting salt from exposed and foggy salt marshes, may remain healthy notwithstanding constant consanguineous marriages, continued generation after generation. It is, indeed, an extraordinary and unfair test. For, were it even proved that all nations which married exclusively among themselves were dying out, that would be no argument against consanguineous marriage. We might as justly argue, that because the natives of a country where the importation of corn, even in famine years, was strictly prohibited, were in danger of starvation, therefore that country was infertile. The truth is, that any restriction on individual freedom is hurtful in itself, and should be imposed only on the plainest and clearest evidence that freedom causes a greater hurt than its curtailment would produce.

In another way, there can be no doubt that a community isolating itself, whether consanguineous marriage is the rule, as among the Basques, or consanguineous marriage is prohibited, as in China, will fall behind less exclusive communities in the grand struggle for existence. It requires no demonstration that the greater the amount of inter-communication of thought, the

greater will be the progress. But consanguineous marriage need not hinder the exchange of ideas. Such marriages may be constant, as among the Jews, and the community may yet hold unrestricted intercourse with all the world. Or again, there may be impassable barriers between one nation and the rest of the world, and yet marriages between near relations be forbidden. It is the interference with perfect liberty which is the harmful element, whether it acts by forcing or prohibiting marriages of consanguinity.

We regret that the question was not settled once and for ever by the census of 1871, and hope that no misguided opposition may prevent its solution in 1881. Meanwhile, however, we have very various and cogent evidence that such marriages have been unjustly accused. We venture to think that Mr. Darwin's work has not settled the question absolutely as regards the vegetable world; but should other investigations confirm his deductions, it has still to be proved that marriages between near kin are harmful in their results.