THE IMMUTABILITY OF THE SPECIES.*

III.

No alleged factor of evolution is so capable of arresting the attention of a physiologist as correlation of growth. To this law we have before often incidentally alluded. But as we conceive that it furnishes strong confirmation of our views, it behooves us to extend to it a somewhat more lengthy treatment.

The current impression is, that every authenticated instance of variation is so much added to the probabilities of the evolution of the species; and that the refutation of Darwinism is rendered difficult just in proportion to the number of proofs of variability. It is natural, then, that Darwin should accord prominence to those factors which play a part in inducing modification. Conspicuous among these factors is cor-

* In the definition of a species, propounded in the last article, there occurred two mistakes. "Character" should have been characters; and the semicolon immediately following should have been absent.

relation, the nearest approximation to a law of all the colligations of facts involved in Darwinism.

Correlation is a bond, *nexus*, or connection subsisting between different growths. Owing to it, a modification seldom arises in any portion of the organism without involving a corresponding change in another part. It is often not a little difficult to determine which part first varies and induces the modification of the other. Frequently, characters simultaneously vary, and are apparently affected by some distinct cause. Correlation is an important subject for Darwin; for, owing to its operation, varieties seldom differ from each other by a single character alone. He declares that "all the parts of the organism are, to a certain extent, connected or correlated together," and that "of all the laws governing variability, that of correlation is the most important." Parts, however, differ greatly with respect to the strength of their connection. In some parts, the tie is ever manifesting itself; in others, it is seldom traceable. Each character, when developed, tends to stimulate the development of others. But, owing to adversity of conditions, or to being systematically suppressed by man, these correlated growths lose all ability to respond to this stimulus, and, in consequence, fail to develop.

We intended to adduce quite a number of facts from Darwin, in order to enable our readers clearly to understand the precise nature of correlation. But want of space forces us to change our mind. We do this with less reluctance, when we consider that those for whom this article is more especially written have already familiarized themselves with those facts.

All the phenomena of correlation show increase of growth corresponding to increase, and decrease corresponding to decrease. Now, the antithesis to correlation is compensation or balancement of growth. This alleged law, as applied to species under nature, was propounded by Goethe and Geoffroy St. Hilaire. It implies that the development of any one part is attended with the reduction or starvation of some other part. Not a little diversity of opinion exists respecting the validity of this law. Darwin inclines to believe that compensation occasionally occurs, but conceives that its importance has been overestimated.

We, however, are of opinion that there is really no such law. That correlation obtains, there is not the slightest doubt. The instances of correlation are innumerable; and every one of them is a disproof of the doctrine of compensation of growth. For the law of correlation is totally incompatible with the law of economy of growth. The latter, according to the hypothesis, makes decrease correspond to increase, and increase to decrease. The former entails the reverse. Both laws, then, cannot stand. One must, of necessity, fall. One must negative the other. Unquestionably, the stronger law is correlation. This law none can invalidate. It follows thence that there is no such law as that of compensation of growth.

The reader is now naturally desirous to know how we explain away the alleged cases of economy of growth. The explanation is, that they are merely manifestations of correlation. The reduction of the given parts is consequent, not, as alleged, upon the building up of some other parts, but upon the suppression or reduction of correlated parts. Strong confirmation of this view is given by the fact that seeming compensation of growth is more observable under nature than under domestication. As development

under nature is slow and occasional, we would expect to find, upon the theory of Goethe and St. Hilaire, very few instances of apparent balancement of growth. On the contrary, the instances are most numerous; which fact is strictly in accordance with our hypothesis. For where we find the conditions entailing the reduction of many parts, there must we also find the reduction of other parts, induced by correlation. These parts, then, being in close proximity with characters which neither the conditions nor correlation have affected, their suppression is naturally referred to compensation of growth. Under domestication, however, development is carried on rapidly and to a great extent. A very large number of characters is selected and developed. Here, then, we should look for the most striking manifestations of compensation of growth. But it is a fact, of which the significance is at once apparent, that, instead of meeting with the fulfilment of our expectations, the converse thrusts itself most obtrusively upon our attention. Nature here is most prodigal; giving growth for growth, and meeting the development of one feature with the corresponding development of another. The cases illustrating apparent balancement of growth are here exceptional. They bear a very insignificant proportion to those under nature. Hence we conclude that the law of compensation of growth never obtains, that its apparent manifestations are really due to the operation of the law of correlation.

But there are two classes of cases of which correlation is not an interpretation. The first is the instances in which the tie of correlation is in a measure broken by man's selection of one part, and by his systematic suppression of another. Darwin refers to these when he declares it "scarcely possible in most cases to distinguish between the supposed effects of such compensation of growth, and the effects of long-continued selection, which may at the same time lead to the augmentation of one part and the diminution of another."

The following is an example of the second class of cases: The Polish fowl is distinguished by the possession of a crest of feathers on the head. In consequence of its development, there arises a protuberance on the skull. This is due to correlation. But in the cock, the skull is so perforated with small holes that at any point a pin may be sunk to the brain. This is adduced as an instance of compensation of growth. But a rational explanation may readily be assigned. Darwin has shown that the crest of feathers is abnormal in the male, that it normally belongs to the female. The feature has been gained by the male by the somewhat mysterious law of the transmission of secondary sexual characters. The economy of growth may then be considered as abnormal, and may reasonably be attributed to the character not completely harmonizing with its fellows.

The facts of correlation meet with an exhaustive treatment at the hands of Darwin. Herbert Spencer, however, almost totally ignores them. Although they are seemingly most striking exemplifications of evolution, he passes with only an occasional incidental notice. What we conceive to be Mr. Spencer's reason for thus ignoring them, we will venture to give further on. But, while Darwin extends to the facts of correlation a full recognition, he is by no means over-desirous to ascertain their cause. Correlation is another of those laws which it pleases Darwin to consider as ultimate.

Now, the supposition that the correlated part has arisen by evolution,

involves the absurd conclusion that a centre of growth normally preëxists without a relative arrangement of parts. And on the evolution hypothesis, we are forced to believe that an evolved part is correlated to another part not yet in existence; that all the parts of the organism anticipate, as it were, the birth of the new feature, and so adjust themselves as to become immediately susceptible to its influence; and that, while the previous coördination of parts is destroyed, owing to the influence of the newborn feature ramifying throughout the whole organization, the organism is capable of immediately effecting a re-coördination. To assume for any organism such powers as these, is virtual hylozoism. The only escape for him who admits the evolution of variations, is to adopt the explanation furnished by the Duke of Argyll—that correlations are the *direct* manifestations of design.

This interpretation of the teleologist precludes all further argument. We, of course, concur in design. But we do not deem ourselves therefore bound to take for granted the validity of every argument adduced in proof thereof. We conceive that design can be proved by incontrovertible evidence, and that it can be shown to manifest itself in conformity to laws not merely empirical.

As for the ultra-evolutionist, if he were to cease regarding correlation as an ultimate fact, and if he were tc employ himself in placing an interpretation upon it, he would perceive that the tie of correlation is strongly suggestive of reversion, and that its phenomena completely negative the hypothesis of evolution.

On the hypothesis of reversion, correlation is perfectly explicable. The supposition of reversion necessarily involves the conclusion that all the features of the species coexisted in each individual, saving, of course, the characters peculiar to the opposite sex. The perfect organism, then, is a balance of all the parts. The parts are correlated to each other with respect to centres, and these centres are correlated to each other with respect to the axis or the aggregate. All the parts are mutually dependent. When a part is reduced, it tends to involve the reduction of its corresponding part. The centre of the parts is then weakened, and this weakening entails the weakening of the other centres, to which this center is correlated. The loss or suppression of even one part, then, manifestly disturbs the physiological balance—destroys the coördination of the parts. Under nature, many parts have been lost or reduced, and these have entailed the loss or reduction of others. When, under domestication, characters develop, owing to selection and favorable conditions, they concur with the different centres of growth to effect a return to the balance, and, in consequence, the correlated parts arise and assume their primordial relations to their correlatives and to the aggregate. When all the parts are developed, by correlation and otherwise, there result an equilibrium and a consequent perfect coördination. Correlation is the inseparable concomitant of coördination. Each implies the other. And this is the reason, we apprehend, why correlation is barely noticed by Mr. Spencer. He feared, we surmise, that a lengthy philosophical treatment of the subject would suggest the conception that correlated growth necessarily implied previously imperfect coördination.

In order to facilitate the reader's conception of our meaning, it may be well to adduce an analogy. Analogies between organic and inorganic nature, the advocates of evolution ever delight in. And as that of the

crystal has found especial favor in their sight, we will venture to use it. As we conceive that there are laws governing the organism, which are *sui generis*, we would request our readers to regard the analogy only as an illustration of our views, and not in the light of an argument.

In crystallization, the initial force involved in the deposition of the first molecule determines the form and shape of the crystal. This molecule is correlated, as it were, to the aggregate to be formed. It controls the whole formative process, with a view to the shape eventually to be attained. Otherwise, how are we to account for the due tempering and modification of the forces implied in the deposition of each of the atoms of the accretion? From the first, there must of necessity be but one normal process. But this correlation between the first molecule and the aggregate is not the correlation which we wish particularly to illustrate. The crystal having been fully formed, a couple of edges are truncated. The crystal is then placed in a solution similar to that in which it was formed. Now, the absence of these edges implies an abnormal distribution of the forces. This is manifest; for correlation, directly with the corresponding edges and indirectly with the aggregate, leads to the reproduction of the lost parts—a fact manifestly implying previously imperfect coördination, and a present equilibrium of all the parts, or due coördination. The parts reproduced assume their previous relations, and effect a return to the balance impaired by their truncation. It is hence clear that correlation implies coördination, and that coördination implies correlation. Correlation, then, is a necessary corollary from the hypothesis of due coördination, or proportionate development. It will be seen that, while it receives a clear, consistent, and rational interpretation upon the theory of reversion, it carries with it implications at variance with the hypothesis of evolution.

As our knowledge of crystallography is that of an amateur, these views respecting crystallization may be open to modification; though we are assured that they are not so in essentials.

The analogy of the crystal most happily illustrates our views of correlation. With equal felicity it illustrates the opposing views of the evolutionist and the reversionist, respecting the main points in the controversy.

Suppose three crystals, similar in shape, to have been formed in a solution. The truncation of six of the edges of each has, in some manner or other, been effected. With these edges thus reduced, the crystals are found by a person anxious to prove the theory of evolution. He places them in solutions similar to those in which they were formed. The development of the lost edges then ensues. But, instead of allowing them all to develop, only a single edge in each crystal is suffered to reproduce itself; and this edge is in each crystal a different one. This is done in order to render the crystals as unlike as possible. Practically, however, this would be not a little difficult to effect. Our friend, imbued with the inquiring spirit of the age, now seeks to ascertain the cause of the growth of the edges. In his observation of the phenomena of crystallization, he has noticed that the growth of an edge is often due to reproduction. But this fact he now finds it convenient to forget. He at last affects to believe himself forced to conclude that the growth of the edges is an ultimate fact; and, at the same time, refers the phenomenon to evolution, an explanation which has the strong recommendation of being a mere re-

statement of the phenomenon to be explained. He next observes that, in each crystal, a new angle develops in correspondence with the angle first developed. This gives him two characters peculiar to each crystal. Recognizing a new factor in the induced development of the last angle, he propounds the law of correlation, and affirms that it concurs with and subserves evolution. The three crystals, originally alike, are now widely distinct. These varieties of crystals, exclaims our friend with the proud and patronizing smile of conscious superiority, present differences almost equally great with those displayed by species. Given, then, an indefinite number of hours and the requisite conditions, and all the species of crystals can be shown to evolve one from another. You cannot assume a limit to the development of parts, otherwise than gratuitously. There cannot possibly be any such thing as the immutability of the species; for individuals vary, and the species is composed of those individuals. This argument of our friend cannot be invalidated, if we concede that the growth of the edges forming the peculiarities of the varieties is new growth, is evolution, and that it is not reproduction. But it is obvious that it is reproduction, or reversion back to the state which existed previous to the truncation of the edges. It is equally obvious that correlation, or the growth of the last edge in correspondence with that of the former, is merely a return to more perfect coördination. It is also manifest to every physicist, that the absence from each crystal of the four edges which constitute the peculiar characters of the other varieties implies an imperfect coördination of the remaining parts. In other words, their absence involves a departure from a state of chemical integrity. For there can be a normal distribution of the forces of a crystal only when all the angles and parts are present, and proportionately developed. The views of the evolutionist are therefore wholly erroneous. For the principles of physics preclude the possibility of the normal existence of more than one variety. The existence of a plurality of varieties of a species implies disproportionate development of some of the parts. With crystals, however, varieties may normally exist when their differences are merely those of size. But the only way in which the relations of the parts can normally be changed is by a totally new distribution of the forces; which would involve complete dissolution, a modification of the force originally implied in the deposition of the first molecule, and reintegration. Now, just as, in a crystal, the loss of any part involves a departure from a state of chemical integrity, so, in an organism, the reduction, suppression, or disproportionate development of any part involves a departure from a state of physiological integrity. In the perfect type alone are the relations of the different parts perfect. The only way in which these relations could be normally changed, is by complete dissolution and new creation.

Not a little prejudice exists against a perfect type. This prejudice is, in a measure, justifiable, owing to the vague and gratuitous manner in which the perfect type has been assumed. But it cannot reasonably be extended to the perfect type which we here assume. This, of ours, is an individual in which all the characters of the species are fully and proportionately developed. It is no Platonic idea; we assume it to prove it; and it is no more metaphysical than the assumption for a crystal of a specific shape, which, owing to perturbations of the forces of the solu-

tion, it has been incapable of attaining.

In "A Theory of Population," propounded in *The Westminster Review* for April, 1852, Mr. Herbert Spencer defines life as "the coördination of actions." This definition is, equally with his others, exceedingly felicitous in every respect but one. It is not a definition of life, as it purports to be, but merely a definition of the conditions of life. In a note on page 74 of his *Principles of Biology*, wherein he repels the imputation of being a disciple of Comte, he declares that the conditions *constitute* existence. Recognizing the fact that the *onus probandi* rests upon him, he presents phenomena in an aspect which at first gives not a little plausibility to his view. But these phenomena derive all their significance from the circumstance that Mr. Spencer's readers concur in the conception of the evolution of variations. When this conception is demurred to, his arguments lose all their force. The theory of reversion negatives the validity of his premises; and the hypothesis of the conditions constituting existence is then sustained by no proof greater than that of gratuitous assertion.

But, whatever may be the diversity of opinion respecting the truth of Mr. Spencer's definition of life, there is none, at least between him and us, on the subject that "the coördination of actions" is a definition of the conditions of life. On this point both he and we are fully agreed. His belief that the definition is more than that which we concede, is a matter immaterial in connection with the argument immediately to be adduced. We wish now to observe which theory consists more with the definition, the theory of evolution or that of reversion.

The coördination of actions is the attribute which characterizes all organisms. All the parts of each organism must work in concert. "If one of them does too much or too little—that is, if the coördination be imperfect—the life is disturbed; and if one of them ceases to act—that is, if the coördination be destroyed—the life is destroyed." These remarks of Mr. Spencer more particularly refer to the *vegetative system;* but, as he shows, they are, with little modification, applicable to the *animal system.* He says:

> "How completely the several attributes of animal life come within the definition, we shall see on going through them *seriatim.*
>
> "Thus, *strength* results from the coördination of actions; for it is produced by the simultaneous contraction of many muscles, and many fibres of each muscle; and the strength is great in proportion to the number of these acting together; that is, in proportion to the coördination. *Swiftness*, also, depending partly on strength, but requiring, also, the rapid alternation of movements, equally comes under the expression; seeing that, other things equal, the more quickly sequent actions can be made to follow each other, the more completely are they coördinated. So, too, is it with *agility;* the power of a chamois to spring from crag to crag implies accurate coördination in the movements of different muscles, and a due subordination of them to the perceptions."

On page 61 of his *Principles of Biology*, he further assures us "that arrest of coördination is death, and that imperfect coördination is disease."

A superficial view of Mr. Spencer's definition would involve the inference that, upon the evolution hypothesis, only one of two things is possible. Either there is an ever-continuing imperfect coördination, or there is an always perfect coördination. As parts subserve actions, the perfect coördination of the latter must be dependent upon the perfect coördination of the former. Now, evolution implies a constant change. In fact, according to the hypothesis, constant change

is the only normal state. The variation of parts, then, would entail their imperfect coördination, and, consequently, the imperfect coördination of their actions; for the only conceivable way in which the imperfect coördination of actions is possible, is by a change in the parts subserving those actions. As variations, then, are ever occurring, imperfect coördination must always exist.

The following is the alternative view. The evolutionist might assume an ability in each organism to effect, on the occurrence of each variation, a re-coördination. This view manifestly admits only of perfect coördination. But the advocate of evolution may avoid these absurd conclusions by affirming, as he has tacitly done, that, while the organism is capable of coördinating any number of characters, imperfect coördination may ensue by a too sudden change in any part or parts. This is the issue which we desired to produce, the decision of which will, we conceive, legitimately preclude further argument. The question is, Is the organism capable of coördinating any number of characters? or, are all the characters of the species alone susceptible of coördination? The reader will perceive that the latter is a mere recurrence of our proposition that the proportionate development of all the parts is necessary to perfection, and that the absence of any part is deleterious to the organism. If we prove this, we shall have completely disproved the evolution hypothesis.

There is a fact adduced by Darwin which places the validity of our theory beyond all doubt, and which is, at the same time, grossly at variance with the conception of evolution. The fact to which we allude is, that good results from crossing. Observing this result, Darwin propounds a general law of nature, that all organic beings are benefited by an occasional cross. This law he employs as a somewhat important factor of evolution, and essays to harmonize it with his theory. In this attempt he succeeds. But mere congruity with a law is no proof of the validity of a theory, where that law is only an empirical one. Of this every person conversant with science is aware. It is equally well known, however, that when a theory is shown to accord with a law; to furnish an explanation of it; and to resolve it into a higher law, thus changing it from an empirical into a derivative law; proof conclusive and incontrovertible has been adduced. If the reader has not already mentally anticipated our argument, it remains for us to prove that the theory of reversion fulfils these requirements.

Our theory manifestly implies that the more proportionate the development, the greater is the approach to perfection. It also implies that the more characters of the species there are in each variety, the nearer is the approximation to perfect coördination. It is apparent at a glance, then, that crossing furnishes a crucial test of the truth of our views. For most varieties are distinguished from each other by the possession of positive features. The presence of the peculiar character of one variety, of course, implies its absence in the others. Each variety possesses a character or characters which the others lack, and lacks what the others peculiarly possess. When, then, two such varieties cross, good must of necessity accrue to their offspring. For, in the formation of the latter, each variety supplies a deficiency of the other. Could a reason be more obvious? or could proof of a view be more conclusive? So conclusive is it, we conceive, that were

any other result consequent on crossing, such a circumstance would be at variance with our theory.

Of the fact that good results from crossing, not a doubt can reasonably be entertained. Darwin, so far from questioning the fact, is its most strenuous advocate. But upon his conception, it is crossing *per se* which produces the favorable effects. In other words, this is another of Darwin's ultimate laws. Being purely empirical, the general law of nature which he assumes, fails utterly to explain the cause of the variations in the quantity of the effects. The crossing of pigeons, for instance, is attended by the greatest gain in constitutional vigor, while comparatively little good results from the crossing of the varieties of the horse, sheep, or cow. On our doctrine, the explanation is clear. The many widely distinct varieties of the pigeon necessarily imply great disproportionate development of each. They are, then, extremely susceptible of improvement. The races of the horse, sheep, and cow, on the other hand, approximate, as we have seen, to proportionate development. There is, therefore, much less room for improvement. Strikingly in harmony with this interpretation is the fact that, with pigeons, the more highly bred the crossed varieties are, the greater is the gain from a cross. Equally congruous is the fact that the more highly bred the breeds of the horse, cow, and sheep are, the less is the gain. The reason is, careful and select breeding produces increased divergence of character with pigeons; but with horses, sheep, and cattle it induces increased convergence. The former become widely distinct, while the latter converge in character. All the characters are developed in each variety of the latter; but in the former different characters are developed in different varieties. While, then, coördination in the horse, sheep, and cow advances toward perfection, coördination in the pigeon is rendered more imperfect by careful breeding. Each variety of the pigeon possesses a character which, when joined with those of another variety, will entail a great advance toward due coördination. This concurrence is effected by crossing, and the result is, as one would be led to expect upon our doctrine, great beneficial effects. With the horse, sheep, and cow the effects of a cross between varieties are less marked, owing to less imperfect previous coördination.

In noting the advantage accruing to crossed offspring, we have particularly referred to gain in constitutional vigor. We have occasion now to speak of gain in fertility. Seeing that hybrids—the product of a cross between species—are invariably sterile, it is clear that, if the conception that varieties are incipient species is a valid one, we are bound to expect that the more marked, distinct, and widely divergent varieties are, the greater will be their sterility. The mere circumstance that such an effect is not observable, goes far to invalidate the conception. What, then, must the inference be when an effect diametrically opposite to that necessitated by the conception is shown to result—when increased fertility is seen to follow crossing, and when this increased fertility is observed to be directly proportionate to divergence of character? Such results would, we apprehend, negative completely the hypothesis of evolution, and would conclusively confirm our view, that the beneficial effects are owing to the disproportionate development which a multiplicity of widely distinct varieties necessarily implies. These results we have, and they are indisputable. For the fact that crossing induces increased fertility, and

that this increased fertility is directly proportionate to divergence of character, is so well known that it is scarcely necessary to adduce proofs from Darwin in support of it. But that the least shadow of a doubt may not remain, we will quote a few of Darwin's remarks on the subject.

Constant reference to crossing may be found in any portion of his late work. But a somewhat lengthy chapter is devoted exclusively to this subject and to close interbreeding. In the conclusion of this chapter (p. 142, vol. ii.) he says:

"In the early part of this chapter it was shown that the crossing of distinct forms, whether closely or distantly allied, gives increased size and constitutional vigor, and, except in the case of crossed species, increased fertility to the offspring. The evidence rests on the universal testimony of breeders. . . . . Although animals of pure blood will obviously be deteriorated by crossing, as far as their characteristic qualities are concerned, there seems to be no exception to the rule that advantages of the kind just mentioned are thus gained even when there has not been any previous close interbreeding. The rule applies to all animals, *even to cattle and sheep*, which can long resist breeding in-and-in between the nearest blood relations. It applies to individuals of the same sub-variety, but of distinct families, to varieties or races, to sub-species, as well as to quite distinct species.

"In this latter case, however, while size, vigor, precocity, and hardiness are, with rare exceptions, gained, fertility, in a greater or less degree, is lost; but the gain cannot be exclusively attributed to the principle of compensation; for there is no close parallelism between the increased size and vigor of the offspring and their sterility. Moreover, it has been clearly proved that mongrels which are perfectly fertile gain these same advantages, as well as sterile hybrids."

On page 174, he reiterates these statements, which place the subject of increased fertility beyond all doubt.

Now, it is clear that Darwin's being necessitated particularly to note that the rule that advantage results from crossing obtains even in the cases of cattle and sheep, implies that comparatively little good accrues to the offspring from the crossing of the breeds of either of those animals. This shows, as the varieties of the sheep and cow are convergent in character, that the less divergent the varieties the less is the good attendant on crossing. The converse, that the more divergent the varieties the greater the good, is plainly seen in the case of the pigeon, of which the varieties are manifestly and confessedly the most divergent. The following assertions are unequivocal proof of our view:

"All the domestic races pair readily together, and, what is equally important, their mongrel offspring are perfectly fertile. To ascertain this fact, I made many experiments, which are given in the note below; and recently Mr. Tegetmeier has made similar experiments with the same result. The accurate Neumeister asserts that when dovecots are crossed with pigeons of any other breed the mongrels are extremely fertile and hardy. MM. Boitard and Corbie affirm, after their great experience, *that with crossed pigeons, the more distinct the breeds, the more productive are their mongrel offspring.*" (Page 236, vol i., American edition.)

Mere mention of crossing in connection with our theory would, we conceive, suffice. But if any doubts have been entertained of the conclusiveness of the proofs furnished by the law, or of the competency of the theory of reversion to account for the good resulting from crossing, they are now surely dissipated by the evidence adduced from Darwin. The law of crossing which we propound is no ultimate law. It fulfils every requirement of a derivative law. The good which flows from crossing varies in degree in different animals, as is well known. This is quite explicable upon our theory; and the amount of good accruing to the offspring from the union of two given varieties, is even susceptible of prevision. Crossing *per se* does not produce the increased good; it is attributable to the lack of full

and proportionate development. Of course, for increased good to result, each of the crossed animals must contribute to the formation of the offspring a part or parts which the other lacks. We have, then, given what Darwin's law, being purely empirical, is utterly incompetent to do—a rational and consistent interpretation of the variations in the quantity of the effects. Logic requires no greater proofs of a theory than those which we have here adduced.

Darwin has informed us, in his late invaluable work, that crossing induces the appearance of new characters. Great stress is laid upon this fact by several writers, and some of them, among whom Pallas is conspicuous, have even gone so far as to ascribe variability exclusively to crossing. The theory of reversion furnishes a rational explanation of the appearance of these characters. We do not allude merely to the fact that their reversion is more probable than their evolution; for Darwin inclines to this opinion rather than to the contrary one. On page 264, vol. ii., after demurring to the conception that variability is solely induced by crossing, he says:

"Nevertheless, it is probable that the crossing of two forms, when one or both have long been domesticated or cultivated, adds to the variability of the offspring, independently of the commingling of the characters derived from the two parent forms; and this implies that new characters actually arise. But we must not forget the facts advanced in the thirteenth chapter, which clearly prove that the act of crossing often leads to the reappearance or reversion of long-lost characters; and in most cases, it would be impossible to distinguish between the reappearance of ancient characters and the first appearance of new characters. Practically, whether new or old, they would be new to the breed in which they reappeared."

But there is another factor subserving evolution, to which we particularly allude. This is correlation, which we have seen reason to conclude exists, not only between different growths, but also between different centres of growth. Now, when a cross ensues, the offspring generally acquires from each parent a character or characters which the other lacks. The union of these characters strengthens the centres to which they are joined, and also all the centres of which the related parts are developed. By correlation, the centre to which these centres are most closely allied becomes more firmly established. The more firm establishment of this centre, then, induces the development of its formerly connected parts. These parts are the characters consequent upon crossing.

If, as we maintain, the proofs furnished by crossing are conclusive, then the phenomena of close interbreeding must be proofs amounting to demonstration. For the law of close interbreeding, which is the converse of that of crossing, also holds good; is, if possible, more in accordance with the theory of reversion; is also susceptible of resolution into the law of proportionate development; and, being a derivative law upon our theory, fully accounts for all the variations in the quantity of the effects. The different data, moreover, esteemed so mutually inconsistent, of those who concur in and of those who demur to Darwin's law of close interbreeding, can be shown, by the light furnished by the hypothesis of proportionate development, to be perfectly congruous. If we can prove, then, that our law of close interbreeding, founded upon the facts furnished by Darwin, is capable of all this, we shall have fulfilled our promise to place our theory beyond the reach of cavil.

As has been more than once asserted, our views necessitate the conclusion that a multiplicity of divergent varieties implies the loss in each of

what constitute the peculiar characteristics of the others. The circumstance that some few varieties are distinguished by the possession of negative features, but slightly modifies this conclusion. Now, it is clear to the comprehension of every one who is likely to have followed us this far, that, as the loss of any part or character is deleterious, the pairing of the members of a variety would tend to aggravate the evil consequent on the absence of the peculiar characters of the other varieties.

Quite in harmony with this view is the following assertion, one of a vast number of a similar kind made by Darwin: "The consequences of close interbreeding, carried on for too long a time, are, as is generally believed, loss of size, constitutional vigor, and fertility, sometimes accompanied by a tendency to malformation." (Page 115, vol. ii.)

Now, according to our theory, the evil effects of close interbreeding must be proportionate to the divergence of character; or, rather, to the disproportionate development which divergence involves. Darwin admits that different species of animals are differently affected by the same degree of interbreeding. Among species of which the varieties are divergent, the pigeon and fowl are preëminently conspicuous. Here, then, we must look for the greatest evil effects from the interbreeding of the members of the varieties. The facts fail not to realize our anticipations. No writers have expressed so strong a conviction of the impossibility of long-continued interbreeding as Sir J. Sebright and Andrew Knight, who have paid the most attention to the breeding of the fowl and pigeon. Darwin gives us, as the result of his wide experience and extensive research, the following opinion:

"Evidence of the evil effects of close interbreeding can most readily be acquired in the case of animals, such as fowls, pigeons, etc., which propagate quickly, and, from being kept in the same place, are exposed to the same conditions. Now, I have inquired of very many breeders of these birds, and I have hitherto not met with a single man who was not thoroughly convinced that an occasional cross with another strain of the same sub-variety was absolutely necessary. Most breeders of highly improved or fancy birds value their own strain, and are most unwilling, at the risk, in their opinion, of deterioration, to make a cross. The purchase of a first-rate bird of another strain is expensive, and exchanges are troublesome; yet all breeders, as far as I can hear, excepting those who keep large stocks at different places for the sake of crossing, are driven after a time to take this step." (P. 117, vol. ii.)

And again, on page 125, he says: "With pigeons, breeders are unanimous, as previously stated, that it is absolutely indispensable, notwithstanding the trouble and expense thus caused, occasionally, to cross their much-prized birds with individuals of another strain, but belonging, of course, to the same variety." He then dwells at some length upon the great delicacy of constitution entailed by the close interbreeding of nearly-related pigeons, and mentions a circumstance for which the reason is at once obvious upon our theory. He says, "It deserves notice that, when large size is one of the desired characters, as with pouters, the evil effects of close interbreeding are much sooner perceived than when small birds, such as short-faced tumblers, are desired."

"In the case of the *fowl*," says Darwin, "a whole array of authorities could be given against too close interbreeding." (P. 124, vol. ii.) Following this assertion is mention of the great sterility of bantams, induced by close interbreeding. He assures us that he has seen silver bantams almost as barren as hybrids. The Sebright bantam is destitute of hackles and sickle tail-feathers. This in-

volves disproportionate development; and that the evil is attributable to this, Darwin virtually admits when he says, on page 101, that the loss of fertility is to be ascribed "either to long-continued, close interbreeding, or to an innate tendency to sterility correlated with the absence of hackles and sickle tail-feathers."

Of all the phenomena attendant upon close interbreeding, we know of none which so strikingly confirms our view as the following curious case. It is a most delicate exemplification of our doctrine. "Mr. Hewitt says that with these bantams the sterility of the male stands, with rare exceptions, in the closest relation with their loss of certain secondary male characters;" he adds, "I have noticed, as a general rule, that even the slightest deviation from feminine character in the tail of the male Sebright —say the elongation *by only half an inch* of the two principal tail-feathers —brings with it improved probability of increased fertility." (Pp. 124.) The full significance of this singular fact the reader will at once appreciate. For the cause of the phenomenon is obvious. The increased probability of fertility, consequent on the growth of the secondary sexual characters, is owing to the induced return to proportionate development.

Darwin says, "There is reason to believe, and this was the opinion of that most experienced observer, Sir J. Sebright, that the evil effects of close interbreeding may be checked by the related individuals being separated during a few generations and exposed to different conditions of life." (Pp. 115.) Now, different conditions are, as we have seen, favorable to the development of different parts. Exposure, then, to conditions other than those to which their brothers are subjected, would lead to the growth or strengthening of certain parts in the separated animals. Interbreeding between members of the two lots of animals would, in consequence, be equivalent to crossing. The check to the evil effects is to be attributed to a slight dissimilarity of structure.

These quotations from Darwin place beyond doubt the fact that the greatest evil effects flow from the close interbreeding of fowls and pigeons. It now remains for us to show that, in animals which are comparatively proportionately developed, the evil effects are very small. It must be observed that it does not rest with us to show a total absence of evil. For no animals are, in all respects, proportionately developed. Our very ability to discriminate between different breeds necessarily implies the disproportionate development of all but one of them; that is, when their differences are not merely those of size. With cows, want of proportion is often caused by blind conformity in certain breeds to certain standards. Thus, when a breed acquires a reputation, all its points are faithfully preserved, as if the preservation intact of the existing condition of all the features was a *sine qua non* of the animal's good quality; and this occurs even when some of the features are shockingly out of proportion, or greatly reduced. If one breed were fully and proportionately developed, the others could be distinguished from it only by negative features.

Of the close interbreeding of the cow Darwin says:

"With *cattle* there can be no doubt that extremely close interbreeding may be long carried on, advantageously with respect to external characters and with no manifestly apparent evil as far as constitution is concerned. The same remark is applicable to sheep. Whether these animals have been rendered less susceptible than others to this evil, in order to permit them to live in herds —a habit which leads the old and vigorous

males to expel all intruders, and in consequence often to pair with their own daughters—I will not pretend to decide. The case of Bakewell's longhorns, which were closely interbred for a long period, has often been quoted; yet Youatt says the breed 'had acquired a delicacy of constitution inconsistent with common management,' and 'the propagation of the species was not always certain.' But the shorthorns offer the most striking case of close interbreeding; for instance, the famous bull Favorite (who was himself the offspring of a half-brother and sister from Foljambe) was matched with his own daughter, granddaughter, and great-granddaughter; so that the produce of this last union, or the great-great-granddaughter, had fifteen sixteenths, or 93·75 per cent, of the blood of Favorite in her veins. This cow was matched with the bull Wellington, having 62.5 per cent of Favorite blood in his veins, and produced Clarissa; Clarissa was matched with the bull Lancaster, having 68.75 of the same blood, and she yielded valuable offspring. Nevertheless, Collings, who reared these animals, and was a strong advocate for close interbreeding, once crossed his stock with a Galloway, and the cows from this cross realized the highest prices. Bates's herd was esteemed the most celebrated in the world. For thirteen years he bred most closely in-and-in; but during the next seventeen years, though he had the most exalted notion of the value of his own stock, he thrice infused fresh blood into his herd; it is said that he did this, not to improve the form of his animals, but on account of their lessened fertility. Mr. Bates's own view, as given by a celebrated breeder, was, that 'to breed in-and-in from a bad stock was ruin and devastation; yet that the practice may be safely allowed within certain limits when the parents so related are descended from first-rate animals.' We thus see that there has been extremely close interbreeding with shorthorns; but Nathusius, after the most careful study of their pedigrees, says that he can find no instance of a breeder who has strictly followed this practice during his whole life. From this study and his own experience, he concludes that close interbreeding is necessary to ennoble the stock; but that in effecting this the greatest care is necessary on account of the tendency to infertility and weakness. It may be added that another high authority asserts that many more calves are born cripples from shorthorns than from any other and less closely interbred races of cattle." (Pp. 117, 118, vol. ii.)

This last phenomenon is doubtless due to correlation between the legs and the small development of the horns.

Now, these remarks of Mr. Darwin unequivocally show that extremely long-continued close interbreeding is possible with cattle. They also acquaint us with the fact that, although this may long be carried on, evil at length begins to manifest itself. This is easily explained. A small want of proportion in the animals interbred entails evil, but evil too small in amount to be capable of manifesting itself at once. But continued exacerbations, consequent on frequent pairing with related individuals possessing an evil identical in kind, so augments the evil as eventually to involve its display.

If further proof of the possibility of the long-continued interbreeding of cattle is needed, it may be found on page 44 of *The Westminster Review* for July, 1863. This review is the stronghold of Darwinism. The writer of the article to which we refer says, that "Dr. Child gives the pedigree of the celebrated bull Comet and of some other animals, bred with a degree of closeness such as no one who has not studied the subject would believe possible. In one of these cases, the same animal appears as the sire in *four* successive generations." So striking is the pedigree of Comet, that the writer cannot refrain from inserting it.

The sheep is another animal in which there is an approximation to proportionate development. Let us see, then, if our doctrine equally obtains in this case. Before going further, we may request the reader to call to mind Darwin's assurance that his remark, "that extremely close interbreeding may be long carried on with cattle," is equally applicable to sheep.

On page 119, vol. ii., he remarks that,

"With *sheep* there has often been long-continued close interbreeding within the limits of the same flock; but whether the nearest relations have been matched so frequently as in the case of shorthorn cattle, I do not know. The Messrs. Brown, during fifty years, have never infused fresh blood into their excellent flock of Leicesters. Since 1810, Mr. Barford has acted on the same principle with the Foscote flock. He asserts that half a century of experience has convinced him that when two nearly-related individuals are quite sound in constitution, in-and-in breeding does not induce degeneracy; but he adds that he 'does not pride himself on breeding from the nearest affinities.' In France, the Naz flock has been bred for sixty years without the introduction of a single strange ram."

In connection with this subject *The Westminster Review* says that,

"M. Beaudouin, in a memoir to be found in the *Comptes Rendus* of August 5th, 1862, gives some very interesting particulars of a flock of merino sheep bred in-and-in, for a period of two and twenty years, without a single cross, and with perfectly successful results, there being no sign of decreased fertility, and the breed having in other respects improved."

Of all animals, the horse is manifestly the most proportionately developed. In him all the parts maintain, to a great extent, the due proportions. Our doctrine, then, leads us to expect that, in this case, little evil results from close interbreeding. We would be greatly surprised that the horse was not the most striking instance of the possibility of long-continued in-and-in breeding, were we not conscious of the fact that a great portion of the evil eventually resulting from close interbreeding is attributable to augmentation of the diseases to which the horse is singularly susceptible. The following is the only evidence we shall adduce in the case of the horse; but it "is clear and decisive":

"Mr. J. H. Walsh, well known, under the *nom de plume* of Stonehenge, as an authority upon sporting matters, says distinctly, in his recent work, that nearly all our thorough-bred horses are bred in-and-in." (*Vide West. Rev.* for July, 1863, p. 44.)

"Writers upon sporting matters are pretty generally agreed that no horse either bears fatigue so well or recovers from its effects so soon as the thorough-bred, and it is a subject upon which such writers are the best of all authorities. Thus, 'Nimrod' concludes a comparison between the thorough-bred and the half-bred hunter in the following words: 'As for his powers of endurance under equal sufferings, they doubtless would exceed those of the 'cock-tail,' and being by his nature what is termed a better doer in the stable, he is sooner at his work again than the others. *Indeed, there is scarcely a limit to the work of full-bred hunters* of good form and constitution and temper; and yet these, as we have seen, are almost all close bred." (*Ibid.* p. 45.)

The mention of "good form" is a fact of significance; for the current conception of symmetry is, in the case of the horse, a safer criterion of proportionate development than in the case of any other animal.

In all the discussions on close interbreeding, no case meets with such frequent mention as that of the pig. Those who endeavor to gainsay the conclusion that evil is attendant on in-and-in breeding, signally fail to invalidate the fact that pigs die out altogether after being bred in-and-in for several generations. Those persons are the exceptions, however, who consider the fact as questionable. On page 121, vol. ii., Darwin says, "With *pigs* there is more unanimity among breeders on the evil effects of close interbreeding than, perhaps, with any other large animal." He then gives quite a number of facts, which we will not quote, as they are indisputable.

Close interbreeding being attended, in pigs, by evil effects is, at first sight, at variance with our doctrine. For, not only does utility guide the selection of pigs, but they are, as Darwin has informed us, the most striking instance of convergence of character.

We have seen the greatest evil effects of in-and-in breeding in those species in which selection is guided by fancy, and of which the varieties were the most divergent in character. A superficial consideration, then, would lead one to expect that, where the converse obtained—where utility was the motive in selection, and where the varieties were convergent in character—interbreeding would entail little or no evil effects. But the incongruity between the facts and the doctrine is only apparent, not real. There is presence of evil effects, because, in this case, the motive of utility and convergence of character also involve disproportionate development. Disproportionate development is the only never-failing criterion. In our last article we showed that, while divergence of character is solely caused by disproportionate development, convergence of character may be induced by either proportionate or disproportionate development. We further showed that the pig's convergence of character is caused by disproportionate development, and that the pig has many characters either wholly or partially suppressed. Its coat of bristles is greatly diminished, and its tusks are wholly reduced. Owing to a misguided policy, its legs are of the smallest possible size, and, by correlation, the front of the head is remarkably short and concave. Being, then, thus disproportionately developed, the pig, of all large animals, must be, upon our doctrine, the most susceptible of evil from close interbreeding. Allow the legs to be of proportionate size, and a marked decrease in the evil entailed by interbreeding will be observable. So impressed are we with the idea of the truth of our doctrine, that we will stake its validity upon the result, confident that, in doing so, we venture nothing.

That the cause assigned for the lessened fertility and delicacy of constitution of pigs is a true one, is placed beyond all doubt by the fact that, with those members of the species of which but little care is taken, there is comparatively very little evil entailed by close interbreeding. The reason lies in the circumstance that, in these animals, the legs are far more proportionately developed than in well-bred pigs; and that there is absent the shortness and concavity of the front of the head. The more well-bred the animals, the greater are the injurious effects of in-and-in breeding. This fact needs not proof; it is too well known. Care in breeding pigs almost invariably induces the small development of the legs and of the front of the head. A case somewhat analogous is presented by the fowl and pigeon. With them, the more careful the selection, the greater are the evil effects of interbreeding. With cattle, sheep, and horses, however, good breeding is a condition *sine qua non* of their exemption from the evil generally consequent on close interbreeding. Why care should be attended by different results in different species, is at first not clear. But this is the explanation. In fowls and pigeons, care in the formation of varieties induces greater disproportionate development by augmenting the divergence of character. In cattle, sheep, and horses, on the contrary, care, by inducing greater convergence, causes increased proportionate development. This convergence, be it remembered, is attributable to a cause other than that which creates the convergence of character of the breeds of well-bred pigs.

We incline to believe that the extremely small amount of evil attendant on reduced size never manifests itself by close interbreeding. That some evil, though inappreciably small, does result from reduced size, may

reasonably be inferred from the fact that, where animals disproportionately developed are crossed, increase in size follows, and that, where those animals are closely interbred, decrease in size results.

We are assured that there are cases in which crossing, instead of resulting in good, induces evil effects. Darwin says he has not met with any well-established case, with animals, in which this occurs. Now, our theory contemplates such evil effects under the following circumstances. The varieties crossed must each be distinguished from other varieties by a negative feature. In addition to this, they must lack features in common. The evil resulting would then be attributable to the same cause which induces the evil consequent on close interbreeding.

It is now clear that these phenomena of crossing and close interbreeding tell a tale the direct converse and refutation of that which Darwin would have us believe. They are manifestly, grossly, absolutely, and irreconcilably at variance with the doctrine of evolution. They show conclusively that no divergence of character is normally possible; that all the characters of the species are alone susceptible of perfect coördination; that the exclusive possession of any positive character by any variety is to the detriment of the other varieties; that the possession of any negative feature is deleterious to the organism; and that there can normally exist but one variety—the perfect type, that variety in which all the positive features are fully and proportionately developed. These conclusions cannot be gainsaid; for they irresistibly force themselves upon one by observation of the phenomena of crossing and close interbreeding, furnished by Darwin.

We have now propounded a counter-theory and a refutation of Darwinism. In doing so, we have introduced no new factors. We have used only those with which Darwin has furnished us. There are, however, three factors recognized by Darwin which we have eliminated. These are an innate tendency in organisms to vary, evolution, and the law of compensation of growth. Of these, the first is confessedly unscientific; the second, irrespective of the well-founded doubt as to whether it obtains or not, must share in the same discredit which is accorded to the first; and the third is viewed with distrust even by Darwin himself. The factors, however, which we have retained must be conceded to be immeasurably more amenable to the canons of scientific research, upon the theory of reversion, than when they are adduced to subserve the hypothesis of evolution. In our treatment of them they have fulfilled the highest requirements of logic. Take, for example, the four principal laws involved in the controversy—variation, correlation, crossing, and close interbreeding. These we found ultimate or empirical laws, and left them derivative laws. The law of variation we resolved into the law of reversion; and the laws of correlation, crossing, and close interbreeding we resolved into the law of proportionate development. Now, it is not possible for a theory to be capable of all this, and yet to be false. If the laws upon which we based our theory were merely empirical, a doubt of its validity might reasonably be entertained. But, as the case stands, it cannot.

But—may exclaim a tyro who affects a love for science, and whose conception of biology is limited to protoplasm and cells—assuming that the hypothesis of reversion is vastly more conformable to the phenomena of variation than the hypothesis of evolution, yet your theory fails to supply

the greatest requirement of biologic science. It fails to satisfy our yearnings after a knowledge of the development of the species. Darwin starts with cells, the lowest congregations of organic matter. Because he does this his theory is, at least philosophically, the more scientific.

But, even in this respect, our theory is more philosophical than that of Darwin. Darwin assumes three or four cells, and intrusts spontaneity or chance with the development of the species. We assume, not "a myriad supernatural impulses" going to the formation of each species, not the creation of each species in its maturity, but one cell alone for each species, (or, perhaps, one cell for each sex of each species.) For evidence of the fact that the assumption of a multiplicity of cells is more philosophical than the assumption of only three or four, we appeal to an article in the *North American Review* for October, 1868, entitled "Philosophical Biology," of which the writer is a professed Darwinian, and to G. H. Lewes's articles in the *Fortnightly Review*. Given, then, these cells, we intrust the development of the species, not to spontaneity or chance, but to the operation of laws similar to those obtaining in the crystal. The forces implied in the creation, formation, or existence of each cell determine, as in the case of the crystal, the whole form and structure of the species. The process of development is that predetermined, from which no departure is normally possible. Time, however, is an unimportant element. This kind of evolution of the species we concede. That which we deny is the evolution of the species one from another.

In conclusion, we cannot refrain from stating that our views are quite consistent with a high admiration of the great ingenuity and vast research displayed by Mr. Darwin. His desire to be frank and candid none can gainsay. For the ability of Mr. Spencer, who is somewhat less candid, but immeasurably more so than the petty retailers of his conceptions, we have the deepest respect. His exquisitely constructed mind we ever delight to study. Both Mr. Darwin and Mr. Spencer have rendered great services to the cause of science. And we must in candor admit that the British "infidels" generally present their theories in a form which admits of their eventual confirmation, or their eventual refutation. As we are confident that their refutation will follow whenever they are really at variance with religion, we anticipate with pleasure many a warm but amicable controversy within the next half-century.