*leucolophum*), a Madagascar Porphyrio (*Porphyrio madagascariensis*), five Tambourine Pigeons (*Tympanistria bicolor*), three Schlegel's Doves (*Chalcopelia puella*) from West Africa, presented by Mr. J. B. Elliott; a Common Cormorant (*Phalacrocorax carbo*), European, presented by Mr. T. M. Oldham; a Great Eagle Owl (*Bubo maximus*), European, a Virginian Eagle Owl (*Bubo virginianus*) from North America, presented by Mr. Charles Clifton; a Hygian Snake (*Elaps hygiæ*) from Port Elizabeth, presented by Mr. W. K. Sibley; a Tarantula Spider (*Mygale*), from Bahama, presented by Mrs. Blake; a Sand Lizard (*Lacerta agilis*) from Jersey, presented by Mr. F. T. Mason; a Prince Albert's Curassow (*Crax alberti*) from Columbia, a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, deposited; three Oyster-catchers (*Hæmatopus ostralegus*), European, purchased; a Blood-breasted Pigeon (*Phlogœnas cruentata*), bred in the Gardens.

## OUR ASTRONOMICAL COLUMN.

MAGNITUDES OF "NAUTICAL ALMANAC" STARS.—In order to expedite the publication of short articles upon astronomical and meteorological subjects, prepared at the Harvard College Observatory, Prof. Pickering has decided to print each as completed as successive numbers of a series, which, when a sufficient amount of material has been collected, will constitute the eighteenth volume of the Annals of the Observatory. Each number is to be published and distributed soon after it is prepared.

The first of this series of papers is a collection the stars employed in the standard lists of the *Nautical Almanacs* published by the Governments of Great Britain, the United States, France, Germany, and Spain, together with their magnitudes, as derived from four standard authorities: the *Harvard Photometry*, the *Uranometria Argentina*, Wolff's photometric observations, and the *Uranometria Oxoniensis*, the second and third being reduced to the photometric scale employed in the other two catalogues, the Harvard and Oxford scales agreeing closely. At present the magnitudes assigned to these stars in the respective Almanacs do not agree, nor do they represent the most accurate results available. Prof. Pickering therefore offered to the Superintendents of the Almanac Offices to supply a discussion of the best values of the magnitudes at present attainable; and favourable replies having been obtained in the cases of the French, Spanish, and American Almanacs, it is expected that the improved values here given will be used in those works in future.

The list embraces 800 stars, and of these the magnitudes of all but 64 depend at least upon two, and generally upon three, authorities; 132 stars being common to all four of the adopted standard catalogues of brightness. The average values of the residuals from the adopted means for these 132 stars are respectively: Harvard, 0·062; Argentine, 0·093; Wolff, 0·094; Oxford, 0·106. The average probable error of the adopted magnitudes is 0·09, assuming the absence of systematic error. The total number of residuals is 2188, of which only 67 exceed two-tenths of a magnitude, and only 17 three-tenths. There are only two cases of a residual exceeding four-tenths, both in the Oxford *Uranometria*; the one being the low star $\theta$ Ophiuchi, the other the double star $\theta$ Serpentis.

COMET 1887 *e* (BARNARD, MAY 12).—The following ephemeris for Berlin midnight is given by Dr. H. Kreutz (*Astr. Nachr.*, No. 2799). The comet is very favourably placed for observation, but is extremely faint.

| 1887 | R.A. h. m. s. | Decl. ° | log $r$ | log $\Delta$ |
|---|---|---|---|---|
| Aug. 24 | 18 41 48 | 7 40·8 N. | 0·2320 | 9·9641 |
| ,, 28 | 18 49 6 | 7 16·5 | 0·2402 | 9·9868 |
| Sept. 1 | 18 56 22 | 6 51·3 | 0·2484 | 0·0093 |
| ,, 5 | 19 3 34 | 6 25·7 N. | 0·2567 | 0·0315 |

## ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 AUGUST 28—SEPTEMBER 3.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

*At Greenwich on August* 28

Sun rises, 5h. 7m.; souths, 12h. 1m. 8·9s.; sets, 18h. 55m.; decl. on meridian, 9° 45′ N.: Sidereal Time at Sunset, 17h. 22m.

Moon (Full on September 2, 11h.) rises, 16h. 6m.; souths, 20h. 25m.; sets, 0h. 45m.*; decl. on meridian, 19° 44′ S.

| Planet. | Rises. h. m. | Souths. h. m. | Sets. h. m. | Decl. on meridian. ° ′ |
|---|---|---|---|---|
| Mercury | 3 50 | 11 15 | 18 40 | 15 26 N. |
| Venus | 8 25 | 13 47 | 19 9 | 8 6 S. |
| Mars | 1 48 | 9 48 | 17 48 | 20 57 N. |
| Jupiter | 10 28 | 15 34 | 20 40 | 11 10 S. |
| Saturn | 1 54 | 9 48 | 17 42 | 20 6 N. |

* Indicates that the setting is that of the following morning.

*Occultations of Stars by the Moon* (visible at Greenwich).

| August. | Star. | Mag. | Disap. h. m. | Reap. h. m. | Corresponding angles from vertex to right for inverted image. ° ° |
|---|---|---|---|---|---|
| 28 | $\xi'$ Sagittarii | 6 | 18 54 | 19 56 | 36 304 |
| Sept. | | | | | |
| 1 | 45 Capricorni | 6 | 0 15 | 1 29 | 108 330 |
| 1 | 44 Capricorni | 6 | 0 33 | near approach | 216 — |
| 2 | $\chi$ Aquarii | 5½ | 23 7 | near approach | 190 — |

| August. | h. | |
|---|---|---|
| 28 | 17 | Mars in conjunction with and 0° 49′ north of Saturn. |
| 29 | 11 | Venus stationary. |

*Variable Stars.*

| Star. | R.A. h. m. | Decl. ° | | h. m. | |
|---|---|---|---|---|---|
| U Monocerotis | 7 25·6 | 9 33 S. | Aug. 31, | | *m* |
| W Virginis | 13 20·2 | 2 48 S. | ,, 31, | 23 0 | *M* |
| δ Libræ | 14 54·9 | 8 4 S. | ,, 29, | 4 57 | *m* |
| | | | Sept. 2, | 20 40 | *m* |
| U Coronæ | 15 13·6 | 32 4 N. | Aug. 29, | 20 18 | *m* |
| S Libræ | 15 14·9 | 19 59 S. | Sept. 2, | | *M* |
| U Ophiuchi | 17 10·8 | 1 20 N. | Aug. 31, | 4 46 | *m* |
| | | and at intervals of | | 20 8 | |
| X Sagittarii | 17 40·5 | 27 47 S. | Aug. 31, | 22 0 | *m* |
| W Sagittarii | 17 57·8 | 29 35 S. | ,, 28, | 0 0 | *M* |
| R Scuti | 18 41·5 | 5 50 S. | ,, 28, | | *m* |
| R Lyræ | 18 51·9 | 43 48 N. | ,, 31, | | *M* |
| S Vulpeculæ | 19 43·8 | 27 0 S. | Sept. 2, | | *m* |
| χ Cygni | 19 46·2 | 32 38 N. | Aug. 29, | | *m* |
| S Sagittæ | 19 50·9 | 16 20 N. | ,, 31, | 0 0 | *m* |

*M* signifies maximum; *m* minimum.

## THE FACTORS OF ORGANIC EVOLUTION.

WHILE reviewing, a short time ago, Mr. Herbert Spencer's essay on the above subject (NATURE, vol. xxxv. p. 262), I promised to consider the present standing of the question as to whether, or how far, use and disuse admit of being regarded as true causes of change of organic type. Of course there is no question about the effects of use and disuse as regards the individual: the only question is as to whether, or how far, these effects admit of being inherited, so that modifications of structure which are produced by modifications of function in the individual become causes of corresponding, and therefore of adaptive, changes of structure in species. The importance of this question is second to none in the whole range of biology. For not only is it of the highest importance within the range of biology itself—governing, by whatever answer we give it, our estimate of the importance of natural selection, and thus requiring to be dealt with on the very threshold of biological philosophy—but its influence extends to almost every department of thought. For, as Mr. Spencer remarks in his preface, upon the answer which this question may finally receive will depend in chief part the sciences of psychology, ethics, and sociology. If functionally-produced modifications are inheritable, the phenomena of instinct, innate ideas, moral intuitions, and so forth, admit of a scientific explanation at the present moment; otherwise they do not, or, at least, not in so distinct nor in so complete a manner. Therefore, we can hardly feel that Mr. Spencer exaggerates the importance of this question when he says of it, "Considering the width and depth of the effects which our acceptance of one or other of these hypotheses [namely, that functionally-produced

modifications are inherited, or that they are not] must have upon our views of Life, Mind, Morals, and Politics, the question—Which of them is true? demands, beyond all other questions whatever, the attention of scientific men."

That functionally-produced modifications are inherited was the great assumption upon which Lamarck founded his theory of evolution. Erasmus Darwin adopted the assumption, and it was also accepted by Charles Darwin as representing a highly important factor of organic evolution, although subsidiary to that of natural selection. Lastly, Mr. Spencer has always upheld the assumption, and, as we shall subsequently see, has done more than anybody else in the way of its justification. On the other hand, of late years a growing tendency has been displayed by those evolutionists who out-Darwin Darwin, not only to assign to natural selection a monarchical government over the whole realm of organic Nature, but also, and consequently, to deprive use and disuse of those lesser sovereignties which were so freely accorded to them by the "Origin of Species." This tendency has now reached a climax in the publication of an essay, by no less an authority than Prof. Weismann, wherein the Lamarckian principles of use and disuse are denied *in toto*.[1] We may therefore best begin our stock-taking of the whole subject by considering what Prof. Weismann has said; for assuredly the doctrine of use and disuse as themselves useless could nowhere meet with an abler champion.

In the first place, he is committed to this doctrine as a necessary consequence of his own theory of heredity, according to which *any* change *acquired* by the individual cannot be transmitted to progeny. This theory regards the individual organism as nothing more than what may be termed a temporary receptacle of "germ-plasma"—this germ-plasma being handed on from generation to generation, without ever being affected by any changes that may take place in the organisms which contain it. And the only reason why such *appears* to be the case—or why in the course of generations one specific type gradually changes through inherited modifications into another—is because the germ-plasma itself is liable to variation, and when the variations happen to be of a kind which lead to favourable modifications of the store-houses (organisms), these store-houses are preserved by natural selection, and with them the peculiar variations of the germ-plasma, which are thus carried on to the next generation. Hence natural selection is really at work upon variations of the germ-plasma, and hence also no change occurring in an organism during its own life-history can at all affect its progeny—any more, for instance, than the chipping or the twisting of a vessel can modify the chemical constitution of whatever substance the vessel may contain. In short, it is only so-called congenital variations—or variations of germ-plasma—that can be inherited; and, therefore, it is only upon such variations that survival of the fittest is able to act. All variations afterwards superinduced in the organism—whether by way of mutilation, disease, acquisition of faculty, or degeneration of structure—are destined to be immediately extinguished by the death of the organism. Now, from this general theory it necessarily follows that the effects of use and disuse in the individual cannot be transmitted to progeny; for, if they could, the fact would be fatal to the theory. Hence it is, as above observed, that Prof. Weismann is committed by his theory of heredity to a denial of the Lamarckian assumption, which, as we have seen, was accepted by Darwin.

But besides this merely *a priori* ground of deduction from his own theory, Prof. Weismann stands upon the affirmation that there is, as a matter of fact, no real evidence of the effects of use and disuse being inherited. For, he maintains, all the supposed evidence on this head admits of being fully interpreted by quite another principle. When an organ (or any structure) falls into disuse, in the course of generations it atrophies, becomes rudimentary, and finally disappears. This fact is generally taken as proof of the inherited effects of disuse—seeing that it is so strikingly analogous to these effects in the case of individual organisms. But there is an alternative possibility. The *raison d'être* of the organ before it fell into disuse, was its utility: it was originally built up under the nursing influence of natural selection solely on account of its serviceability. When therefore from changed conditions of life, or for any other reason, the organ ceased to be serviceable, the premium which had been previously set upon it by natural selection was withdrawn; individuals which happened to present the organ of a size below the average were no longer eliminated in the struggle for existence, but were allowed to propagate. Thus, by free intercrossing, the average size became less and less in every succeeding generation, until eventually, according to Weismann, it must altogether disappear. In short, as the organ was originally built up by natural selection, when natural selection was withdrawn, is any other explanation required of the fact that the organ progressively dwindled?

[1] "Ueber den Rückschritt in der Natur" (Freiburg, 1886).

Unknown to Prof. Weismann, this principle, under the name "Cessation of Selection," was enunciated by the present writer in a series of articles published in these pages so long ago as 1873–74. Attention is now drawn to this fact merely for the sake of informing biologists that the principle met with the full approval of the late Mr. Darwin, and also to state exactly the shape in which it was thus approved by him. For in one or two particulars the idea as published in NATURE differs from that which has been recently and independently arrived at by Prof. Weismann. As the issues of NATURE in question are out of print, and as the matter cannot be more briefly stated now than it was stated then, I may best begin by reprinting the portion of these articles which sets forth the principle of the cessation of selection, as this was accepted by Mr. Darwin.

"In a former communication (NATURE, vol. ix. p. 361) I promised to advance what seemed to me a probable cause—additional to those already known—of the reduction of useless structures. As before stated, it was suggested to me by the penetrating theory proposed by Mr. Darwin (NATURE, vol. viii. pp. 432 and 505), to which, indeed, it is but a supplement.[1] Epitomising Mr. Darwin's conception as the dwarfing influence of impoverished conditions progressively reducing the average size of a useless structure by means of free intercrossing, the present cause may be defined as the mere cessation of the selective influence from changed condition of life.

"Suppose a structure to have been raised by natural selection from 0 to average size 100, and then to have become wholly useless. The selective influence would now not only be withdrawn, but reversed; for, through Economy of Growth—understanding by this term both the direct and the indirect influence of natural selection—it would rigidly eliminate the variations 101, 102, 103, &c., and favour the variations 99, 98, 97, &c. For the sake of definition we shall neglect the influence of economy acting below 100, and so isolate the effects due to the mere withdrawal of selection. By the conditions of our assumption, all variations above 100 are eliminated, while below 100 indiscriminate variation is permitted. Thus, the selective premium upon variation 99 being no greater than that upon 98, 98 would have as good a chance of leaving offspring which would inherit and transmit this variation as would 99; similarly, 97 would have as good a chance as 98, and so on. Now there is a much greater chance of variations being perpetuated at or below 99, than at or above 100, for at 100 the hard line of selection (or economy) is fixed, while there is no corresponding line below 100. The consequence of free intercrossing would therefore be to reduce the average from 100 to 99. Simultaneously, however, with this reducing process, other variations would be surviving below 99, in greater numbers than above 99; consequently the average would next become reduced to 98. There would thus be 'two operations going on side by side—the one ever destroying the symmetry of distribution' round the average, 'and the other ever tending to restore it.' It is evident, however, that the more the average is reduced by this process of indiscriminate variation the less chance there remains for its further reduction. When, for instance, it falls to 90, there are numerically (though not actually, because of inheritance) 89 to 9 in favour of diminution; but when it falls to 80 there are only 79 to 19 in such favour. Thus, theoretically, the average would continue to diminish at a slower and slower rate, until it comes to 50, where, the chances in favour of increase and of diminution being equal, it would remain stationary.

"Having thus, for the sake of clearness, considered this principle apart, let us now observe the effect of superadding to it the influence of the economy of growth—a principle with which its action must always be associated. Briefly, as this

[1] As stated in the text, the leading idea in Mr. Darwin's suggestion was that impoverished conditions of life would accentuate the principle of Economy of Nutrition, and so assist in the reduction of useless structures by free intercrossing. Now, in this idea that of the cessation of selection was really implied; but neither in his own article nor in a subsequent letter by Mr. George Darwin on the same subject (NATURE, October 16, 1873), was it exhibited as an independent principle. It was inarticulately wrapped up with the much less significant principle of impoverished conditions. Afterwards, however, Mr. Darwin expressed himself as fully persuaded of the independent character of the more important principle, which he was really the first to perceive, although not clearly to express. Moreover, he then thought it was probably a principle of universal application, not only as regards rudimentary organs, but also as regards degenerated structures in general.

influence would be that of continually favouring the variations on the side of diminution, the effect of its presence would be that of continuously preventing the average from becoming fixed at 50, 40, 30, &c. In other words, the 'hard line of selection' which was originally placed at 100, would now become progressively lowered through 90, 80, 70, &c.; always allowing indiscriminate variation below the barrier, but never above it.[1]

"It will be understood that by 'cessation of selection from changed conditions of life,' I mean a change of any kind which renders the affected organ superfluous. Take, for example, the exact converse of Mr. George Darwin's illustration, by supposing a herd of cattle to migrate from a small tract of poor pasture to a large tract of rich. Segregation would ensue, the law of battle would become less severe, while variation would be promoted in a cumulative manner by the increase of food. The young males with shorter horns would thus have as good a chance of leaving progeny as 'their longer-horned brothers,' and the average length would gradually diminish as in the other case. Of course, as the predisposing cause of impoverished nutrition would now be absent, the reducing process would take place at a slower rate. Moreover, it is to be remarked that this principle differs in an important particular from that enunciated by Mr. Darwin, in that it could never reduce an organ much below the point at which the economy of growth, together with disuse, ceases to act. For, returning to our numerical illustration, suppose this point to be 6, the average would eventually become fixed at 3.

"That the principle thus explained has a real existence we may safely conclude, theoretical considerations apart, from the analogy afforded by our domestic races; for nothing is more certain to breeders than the fact that neglect causes degeneration, even though the strain be kept isolated."

Evidence of the wide-reaching operation of this principle under Nature must be sought for in cases where it is impossible that disuse can have had any part in the reducing process—seeing that we cannot all agree with Prof. Weismann in dismissing the agency of disuse on *a priori* grounds of deduction from his theory of germ-plasma. Now, although it is not at all an easy thing to find cases where the influence of the cessation of selection admits of being demonstrably dissociated from the possible influence of disuse, the following appear to meet the requirements of the proof:—

(1) The whole multitude of instances where recapitulative phases are absent from the developmental history of an embryo may stand for so many proofs of reduction without the agency of disuse. For, inasmuch as none of the structures represented in those phases elsewhere can ever have been of any *use* to the embryo from which they have disappeared, it is sufficiently evident that their obliteration can never have been due to *disuse*. And, forasmuch as such structures persist in the embryos of *allied* species, it appears equally evident that their reduction cannot be ascribed to natural selection acting through the economy of nutrition; for, were this the case, natural selection ought to have effected the reduction in the embryos of all the species.

(2) Even in adult organisms we meet with many structures which, although of obvious *use* in the sense of affording protection, yet cannot be said ever to be *used* in the sense of being actively employed, or of being employed in any way that could possibly lead to their structure being modified by their function. Of such, for example, are the hard coverings of animals and of parts of plants. It is impossible that the thickness of shells, for instance, can ever have been increased by their "use" as protective coverings, seeing that the use is here wholly passive—is not of the active kind which determines a greater flow of nutrition to the part. Hence, we can only attribute the formation of such structures to the unaided influence of selection. But, if so, we can only attribute to the cessation of selection their subsequent degeneration in all cases—such as that of male cirripedes, hinder parts of hermit crabs, &c.—where changed conditions of life have rendered these parts no longer needful in the struggle for existence. Here, indeed, economy of growth may have assisted in the reduction; but, whether or not, disuse can scarcely have done so, and this is the point with which we are at present concerned.

(3) In many species of social Hymenoptera the neuter insects have lost their wings. Now, as these neuter insects never have progeny, it is evident that the reduction of their wings cannot possibly have been due to the inherited effects of disuse. We must, therefore, set it down to the cessation of selection, joined, perhaps, with the economy of growth. This is a particularly cogent line of proof, seeing that in some species the head, jaws, and other parts of the neuters have been *enlarged*, in order the better to fit them for heavy work where strength or fighting is required. Had such an enlargement been met with in the case of an animal which leaves progeny, the fact might well have been attributed to the inherited effects of increased *use*. But, as the matter stands, these neuter insects are available as a demonstrative and a double proof of the possibility both of the development and the degeneration of important structures without the aid either of use or of disuse.

(4) In his essay on "Degeneration," Prof. Lankester names three distinct sets of conditions as those under which the process has taken place, and all these are conditions under which the cessation of selection must have taken place. First, "Any new set of conditions occurring to an animal which render its food and safety very easily attained, seem to lead as a rule to degeneration. . . . The habit of parasitism clearly acts upon animal organisation in this way. Let the parasitic life once be secured, and away go legs, jaws, eyes, and ears." In other words, so soon as these organs, which were originally built up by natural selection for the purpose of securing "food and safety," are rendered superfluous by food and safety being otherwise secured, all selective premium on their efficiency is withdrawn, and so they are allowed to degenerate by indiscriminate variation. Second, "Let us suppose a race of animals fitted and accustomed to catch their food, and having a variety of organs to help them in the chase—suppose such animals suddenly to acquire the power of feeding on the carbonic acid dissolved in the water around them just as green plants do. This would tend to degeneration; they would cease to hunt their food, and would bask in the sunlight, taking food in by the whole surface, as plants do by their leaves. . . . These vegetating animals . . . . show how a degeneration of animal forms may be caused by *vegetative nutrition*." Now, to "cease to hunt their food" is here equivalent to their ceasing to be under the influence of natural selection with respect to their food-hunting organs, just as in the previous case. Third, "Another possible cause of degeneration appears to be the indirect one of minute size. . . . The needs of a very minute creature are limited as compared with those of a large one, and thus we may find heart and blood-vessels, gills and kidneys, besides legs and muscles, lost by the diminutive degenerate descendants of a larger race." But, if "the *needs* of a very minute creature are limited as compared with those of a large one," this is the same as to say that in the "diminutive descendants of a larger race" natural selection will no longer operate for the maintenance of structures which have become needless. In fact, in this passage Prof. Lankester comes very near an express statement of the principle of the cessation of selection.

The sundry instances given in the above paragraphs may, I hope, be held sufficient firmly to establish this principle, and to show that it is one of universal application, wherever an organ or a structure has ceased to be of service to the species presenting it.[1] Now, quite apart from the reference in which we have

[1] It is desirable to remark that this numerical mode of representing the principle is adopted only for the purposes of exposition. The exact point at which equilibrium would be reached *in actual fact* we have no means of ascertaining, since such would depend in any given case upon the original force of inheritance, or the persistence with which heredity would assert itself when left entirely to itself—and of this we have no means of judging. Therefore, I adopt the numerical mode of representing the progressive decline of a structure under the cessation of selection merely to show that at *whatever* point we may suppose equilibrium to be reached—or a state of balance between heredity and indiscriminate variation to be attained—this point must become progressively lowered by the superadded influence of the economy of growth. It may, however, be remarked that the initial stages of reduction would probably take place more rapidly than subsequent stages, seeing that the maximum efficiency of a structure is maintained, not only by heredity, but also by the continued influence of selection. Therefore, when the influence of selection is withdrawn, indiscriminate variation would rapidly degrade the structure through the initial stages of its reduction.

[1] Or, if these instances are not held sufficient for this purpose, I may refer to Prof. Weismann's essay, where further instances are given, and also supplement them with the following passage from my old articles in NATURE:—

"If it be supposed that disuse is the chief cause of atrophy in wild species, then it has not produced so much effect in tame species as we should antecedently expect. . . . For, supposing the cessation of selection to be here the *only* cause at work, what degree of atrophy should we expect to find? Before I turned to the valuable measurements given in the 'Variation of Plants and Animals under Domestication,' I concluded (cf. NATURE, vol. ix. p. 441) that from 20 to 25 per cent. is the maximum of reduction we should expect this unassisted principle to accomplish, in the case of natural as distinguished from artificially-bred organs. Now on calculating the average afforded by each of Mr. Darwin's tables, and then reducing the averages to parts of 100, I find that the highest average decrease is 16 per cent., and the lowest 5; the average of the averages being rather less than 12. Only four

hitherto been considering this principle—or with reference to use and disuse—we have here a consideration of great importance in regard to the subject of Prof. Lankester's essay above quoted. Apparently without having either heard or thought of the principle of cessation, Dr. Dohrn was led to attribute an important part in the drama of evolution to the effects of cessation, as these are witnessed in the phenomena of degeneration.[1] About the *facts* of degeneration there can be no doubt, and to this naturalist belongs the credit of having first perceived the wide range of their importance. But, on account of having missed the principle of cessation, both Dr. Dohrn and his English expositor, Prof. Lankester,[2] fell into an omission of *interpretation*. For they both attributed the facts of degeneration to a *reversal* of natural selection; they represented that degeneration could only take place under a change in the conditions of life such that organs or structures previously useful become, not merely useless, but deleterious. Degeneration was thus regarded as always the result of what may be termed active hostility on the part of natural selection; not as the result of a merely passive disregard. Hence the sphere within which the phenomena of degeneration might be expected—or admitted of being satisfactorily explained—was needlessly limited. For instance, Prof. Lankester writes: "It is clearly enough possible for a set of forces such as we sum up in the term 'natural selection' to so act on the structure of an organism as to . . . . . diminish the complexity of its structure." But in order "to diminish the complexity" of any useless structure, it is not necessary that natural selection should "act on the structure": the complexity, like the size, of the structure would necessarily diminish under the mere withdrawal of selection. And hence the phenomena of degeneration do not require, either that the organism presenting them should ever have found its useless organs actively deleterious, or that there should ever have been any "Functions-wechsels" in the case.[3]

The case of degenerated *complexity* proves that the cessation of selection may effect degradation without assistance from the economy of nutrition. I am therefore more disposed to think that the *size* of any useless structure may be reduced to a greater extent by the mere cessation of selection (apart from economy), than I thought when writing the articles above quoted. Here, however, we must remember that the hold which heredity has upon complexity is much less than that which it has upon size. This is evident, not only from obvious considerations of an *a priori* kind, but also from such cases as those of the blind crabs of Kentucky. Here the disused eyes have been lost, while the foot-stalks which originally supported them have been retained. Now, we can well understand why the eyes should have been the first to disappear under the cessation of selection, seeing that they were structures so highly organised that the continuous influence of selection must have been required to preserve them in a state of efficiency before the animals began to inhabit the dark caves; and, therefore, that when the animals did begin to inhabit these caves, such refined and complex structures would rapidly degenerate through the mere withdrawal of selection. But if we were to attribute any large share in this process of rapid degeneration to the economy of nutrition, we should be unable to explain the persistence of the foot-stalks. Therefore, the cessation of selection, when acting alone, is thus proved capable of reducing a complex structure more quickly than it can reduce a larger but less complex structure, in the same species and under the same conditions.

It is true that in a passage above quoted, and which was published two years before Dr. Dohrn's essay, I myself attributed the phenomena of degeneration to a "reversal of natural selection."[1] But I alluded to such reversal only in so far as it arose from the economy of nutrition (*i.e.* I did not suppose that degeneration can only occur when useless parts become actively deleterious, and therefore require the active agency of selection to remove them); and the effect of reading the subsequently published literature on the subject of degeneration has been to make me attribute more importance to the cessation of selection, and less importance to the economy of nutrition. Nevertheless, I still believe that these principles are inadequate to explain the final and total obliteration of organs which by their combined action they have rendered rudimentary.

And these remarks lead me to indicate the points wherein my hypothesis of the cessation of selection differs from that which has recently been published by Prof. Weismann. Briefly, he does not mention the assistance which this principle derives from that of the economy of nutrition, and he believes that it is in itself sufficient to explain the final and total obliteration of useless parts. Having already given my reasons for holding different views with regard to both these points, it will now suffice merely to re-state the principles which I suggested in the NATURE articles as having been most probably concerned in this final and total obliteration of useless parts. These principles are two in number, and are both quite independent of those which we have hitherto been considering. The first of them is inheritance at earlier periods of life, which progressively pushes back the development of a useless rudiment to a more and more embryonic stage of growth; and the second is the eventual failure of the principle of inheritance itself. For, "whether or not we believe in Pangenesis, we cannot but deem it in the highest degree improbable that the influence of heredity is of unlimited duration."[2] This view of the matter renders it abundantly intelligible why it is that, when once the cessation of selection—co-operating with the economy of nutrition—has with comparative rapidity reduced any useless organ to a rudiment, the latter should then persist for so enormous a length of time that in the result, as Mr. Darwin observes, "rudimentary organs are so extremely common that scarcely one species can be named which is wholly free from a blemish of this nature."

We have seen that in the cessation of selection we must recognise one of the principal causes of atrophy in species; in whatever measure we hold the presence of selection explanatory of evolution, in a corresponding measure must we hold the withdrawal of selection accountable for degeneration. But from this it does not necessarily follow that no other causes either of evolution or of degeneration are to be found. Those naturalists who adopt the light and easy method of out-Darwining Darwin, or close their eyes to every other "factor" save that of natural selection, may indeed rest satisfied with these two complementary principles as in themselves adequate to explain all

individual cases fall below 25 per cent., and of these two should be omitted (cf. 'Variation,' p. 272). Thus, out of eighty-three examples, only two fall below the lowest average expected (*i.e.* on the supposition that disuse has not had anything to do with the reduction). Moreover, we should scarcely expect disuse alone to affect in so similar a degree such widely different tissues as are brain and muscle. The deformity of the sternum in fowls also points to the cessation of selection rather than to disuse. Further, the fact that several of our domestic animals have not varied at all is inexplicable upon the one supposition, while it affords no difficulty to the other. We have seen that disuse can only act by causing variations; and so we can see no reason why, if it acts upon a duck, it should not also act upon a goose. But the cessation of selection depends upon variations being supplied to it; and so, if from any reason a specific type does not vary, this principle cannot act. Why one type should vary, and another not, is a distinct question, the difficulty of which is embodied by the one supposition, and excluded by the other. For, to say that disuse has not acted upon type A, because of its inflexible constitution, while it has acted on a closely allied type B, because of its flexible constitution, is merely to insinuate that disuse, having proved itself inadequate to cause reduction in the one case, may not have been the efficient cause of reduction in the other. But the counter-supposition altogether excludes the idea of a causal connection, and so rests upon the more ultimate fact of differential variability, as not requiring to be explained. Lastly, it is remarkable that those animals which have not suffered reduction in any part of their bodies are likewise the animals which have not varied in any other way, and conversely; for as there can be no causal connection between these two peculiarities, the fact of the intimate association between them tends to show that special reduction depends upon general variability, rather than that special variability depends upon special reducing causes."

[1] "Der Ursprung der Wirbelthiere und der Princip des Functionswechsels" (Leipzig, 1875).

[2] "Degeneration: a Chapter in Darwinism" (London, 1880).

[3] The same considerations apply to the size of an organism as a whole. If for any reason it ceases to be an advantage to be kept up to the ancestral standard of size, the cessation of selection as regards size would result in a gradual diminution of size, even though the ancestral standard of size were not actually deleterious. Yet, in the last of the passages above quoted from Prof. Lankester—and the passage in his essay where he most nearly approaches the principle of selection as withdrawn—the context shows that he only has in view the principle of selection as reversed. For he says:—"It cannot be doubted that natural selection has frequently acted on a race of animals so as to reduce the size of the individuals. The smallness of size has been favourable to their survival in the struggle for existence." Of course "it cannot be doubted" that this has been so in many cases; but as little can it be doubted that it has not been so in all. In any given case of diminution, it is not necessary to suppose that "the smallness of size has been *favourable* in the struggle for existence": it is enough if the previous largeness of size has *ceased* to be so, or that smallness of size is no longer *deleterious*. Moreover, the same considerations apply to instincts. For example, it can scarcely ever have been a *fatal disadvantage* to the slave-making ants that they should be able to eat their own food; therefore the loss of their original instincts, which now renders them dependent on their slaves for being fed, can only have been brought about by the *cessation* of selection—not by its *reversal*.

[1] Prof. Weismann christens the principle which I have called Cessation of Selection, *Kehrseite der Naturzüchtung*; but, for reasons above given, I do not think that this is so good a name as that which he elsewhere uses incidentally, and which, indeed, is an unconscious translation of my own term—namely, *Nachlass der Naturzüchtung*.

[2] NATURE, *loc. cit.*, where see for a fuller discussion of the causes leading to eventual and total suppression.

the facts both of progress and of regress. But, unless we are satisfied to walk upon the high *priori* road to the exclusion of every other, we must not too readily assume that the presence and the absence of selection have been the only factors at work. In particular, we have now to consider whether use and disuse have co-operated with the presence and the absence of selection in bringing about the existing state of matters in organic Nature as a whole.

Now, the only way in which this inquiry can be conducted is by the method of difference. We must search through organic Nature in order to ascertain whether there are any cases either of evolution or of degeneration where it is manifestly impossible that either the presence or the absence of selection can have had anything to do with the process. If we can find any such cases, we shall not merely save Darwin from his friends by justifying his acceptance of the Lamarckian assumption: we shall prove that presumably in *all* cases where the presence or the absence of selection has been concerned in either building up or breaking down organic structures, these principles have been largely *assisted* in their operations by the inherited effects of use and disuse. For if it can be proved that these effects are inherited in cases where it is impossible that the principle of selection—or its cessation—can have obtained, it would be irrational to deny that they are also inherited in other cases where these principles do obtain.

Seeing that so accomplished a naturalist and so philosophic a thinker as Prof. Weismann has declared that there is no one case to be found such as those of which we are in search, we must be prepared to expect some difficulty in meeting with examples of the uncompounded influence of use and disuse—even supposing use and disuse to be the true causes of specific modification that they were taken to be by Darwin. In order to show the kind of difficulty that here besets inquiry, I will quote a passage from Mr. Spencer's recently-published essay upon the subject.

"When discussing the question more than twenty years ago ('Principles of Biology,' § 166), I instanced the decreased size of the jaws in the civilised races of mankind as a change not accounted for by the natural selection of favourable variations; since no one of the decrements by which, in thousands of years, this reduction has been effected would have given to an individual in which it occurred such advantage as would cause his survival, either through diminished cost of local nutrition or diminished weight to be carried. . . . Reconsideration of the facts does not show me the invalidity of the conclusion drawn, that this decrease in the size of the jaw can have had no other cause than continued inheritance of those diminutions consequent on diminutions of function, implied by the use of selected and well-prepared food. Here, however, my chief purpose is to add an instance showing, even more clearly, the connection between change of function and change of structure. This instance, allied in nature to the other, is presented by those varieties—or, rather, sub-varieties—of dogs, which, having been household pets, and habitually fed on soft food, have not been called upon to use their jaws in tearing and crunching, and have been but rarely allowed to use them in catching prey and in fighting."

There follows an account of a somewhat laborious examination of dogs' skulls in the Museum of Natural History, the result of which was to show that "we have two, if not three, kinds of dog, which, similarly leading protected and pampered lives, show that in the course of generations the parts concerned in clenching the jaws have dwindled;" after which the passage proceeds as follows:—

"To what cause must this decrease be ascribed? Certainly not to artificial selection; for most of the modifications named make no appreciable external signs: the width across the zygomata could alone be perceived. Neither can natural selection have had anything to do with it; for even were there any struggle for existence among such dogs, it cannot be contended that any advantage in the struggle could be gained by an individual in which a decrease took place. Economy of nutrition, too, is excluded. Abundantly fed as such dogs are, the constitutional tendency is to find places where excess of absorbed nutriment may be conveniently deposited, rather than to find places where the cutting down of the supplies is practicable. Nor, again, can there be alleged a possible correlation between these diminutions and that shortening of the jaws which has probably resulted from selection; for in the bull-dog, which has also relatively short jaws, the structures concerned in closing them are unusually large. Thus, there remains as the only conceivable cause, the diminution of size which results from diminished use."

Evidently Mr. Spencer has never heard or thought of the cessation of selection, either as explained thirteen years ago by myself, or as republished within the last few months by Prof. Weismann. For it is evident that, far from his having excluded all conceivable causes of the diminution save that of diminished use, it would be difficult to find a case more favourable to the influence of the cessation of selection. The dogs in question have been "habitually fed on soft food, have not been called on to use their jaws in tearing and crunching, and have been but rarely allowed to use them in catching prey and in fighting." In other words, for at least a hundred generations these dogs have been "leading protected and pampered lives," wholly shielded from the struggle for existence and survival of the fittest. Never having had to use their jaws either in "tearing, crunching, catching prey, or fighting," they, more than any other dogs—even of domesticated breeds—have not been "called on" to use their jaws for any life-serving purpose. Clearly, therefore, if the cessation of selection ever acts at all as a reducing cause in species, here is a case where it is positively bound to act. And, of course, the same remark applies to the analogous case of the diminished size of the jaws in civilised man.

Be it observed, I am not disputing that disuse may in both these cases have co-operated with the cessation of selection in bringing about the observed result. Indeed, I am rather disposed to allow that the large amount of reduction described in the case of the dogs as having taken place in so comparatively short a time, is strongly suggestive of disuse having co-operated with the cessation of selection. But at present I am merely pointing out that Mr. Spencer's investigations have here failed to exhibit the crucial proof of disuse as a reducing cause which he assigns to them: it is not true that in these cases disuse "remains as the only conceivable cause."

Far more successful, however, is his second line of argument. Indeed, to me it has always appeared, since I first encountered it fifteen years ago in the "Principles of Biology," as little short of demonstrative proof of the Lamarckian assumption. Therefore, if, as a result of reading the passage above quoted, one feels disposed to regret that before publishing it Mr. Spencer did not have his attention called to Prof. Weismann's essay on the cessation of selection, still more must one regret that before publishing that essay Prof. Weismann should have failed to remember the "Principles of Biology." For, had he done so, it seems impossible that he could ever have committed himself to the statement that there is no evidence of functionally-produced modifications being inherited, and thus he might have been led to pause before announcing—at least in its present shape—his theory of germ-plasma.

The argument whereby in my opinion Mr. Spencer succeeds in virtually proving the truth of the Lamarckian assumption is expanded in his recently-published essay, from which, therefore, I will quote.

"If, then, in cases where we can test it, we find no concomitant variation in co-operative parts that are near together—if we do not find it in parts which, though belonging to different tissues, are so closely united as teeth and jaws—if we do not find it even when the co-operative parts are not only closely united, but are formed out of the same tissue, like the crab's eye and its peduncle; what shall we say of co-operative parts which, besides being composed of different tissues, are remote from one another? Not only are we forbidden to assume that they vary together, but we are warranted in asserting that they can have no tendency to vary together. And what are the implications in cases where increase of a structure can be of no service unless there is concomitant increase in many distant structures, which have to join it in performing the action for which it is useful?

"As far back as 1864 ('Principles of Biology,' § 166) I named in illustration an animal carrying heavy horns—the extinct Irish elk; and indicated the many changes in bones, muscles, blood-vessels, nerves, composing the fore-part of the body, which would be required to make an increment of size in such horns advantageous. Here let me take another instance—that of the giraffe: an instance which I take partly because, in the sixth [last] edition of the 'Origin of Species,' issued in 1872, Mr. Darwin has referred to this animal when effectually disposing of certain arguments urged against his hypothesis. He there says:—

"'In order that an animal should acquire some structure specially and largely developed, it is almost indispensable that several other parts should be modified and co-adapted. Although every part of the body varies slightly, it does not follow that the necessary parts should always vary in the right direction and to the right degree' (p. 179).

"And in the summary of the chapter, he remarks concerning the adjustments in the same quadruped, that 'the prolonged use of all the parts, together with inheritance, will have aided in an important manner in this co-adaptation' (p. 199): a remark probably having reference to the increased massiveness of the lower part of the neck; the increased size and strength of the thorax required to bear the additional burden; and the increased strength of the fore-legs required to carry the greater weight of both. But now I think that further consideration suggests the belief that the entailed modifications are much more numerous and remote than at first appears; and that the greater part of these are such as cannot be ascribed in any degree to the selection of favourable variations, but must be ascribed exclusively to the inherited effects of changed functions."

The passage then proceeds to trace these modifications of structure in detail; showing that the changes in the fore-quarters entail corresponding changes in the hind-quarters, which when running "perform actions differing in one or another way and degree from all the actions performed by the homologous bones and muscles in a mammal of ordinary proportions, and from those of the ancestral mammal which gave birth to the giraffe." Thus it is shown that bones, muscles, blood-vessels, nerves, and indeed nearly all the constituent structures of the body, have everywhere been more or less modified as to relative size and function, in order to adapt the giraffe as a whole to the unusual development of its neck: this unusual development has entailed changes, and changes, and counter-changes, which have eventually spread throughout the whole organisation of the animal.

Now, it appears to me that we have in this a most cogent argument in favour of the inherited effects of use and disuse. For, seeing how immense must be the sum of the organic changes required to produce this mutual co-adaptation of many structures, the chances against their all happening to occur together by way of fortuitous variation must be, as Mr. Spencer observes, infinity to one. Yet unless they all did occur together in the same organism—and this repeatedly—the co-adaptations in question cannot have been due to natural selection.

With more or less success Mr. Spencer develops several other lines of argument; but as they cannot well be reproduced without occupying more space than can here be allowed, I will conclude by adding to his material yet another consideration which appears to me to be entitled to great weight. When we search through the animal kingdom, we meet with certain instincts which cannot reasonably be supposed to subserve any such life-preserving function as that which has led to the survival, through natural selection, of instincts in general. Now the existence of instincts which are thus not of vital importance to the species presenting them can only be explained by the hereditary effects of function. For instance, it is difficult to suppose that the instinct, which is still inherited by our domesticated dogs, of turning round and round to trample down a comfortable bed before lying down, can ever have been of so life-preserving a character as to have been developed by survival of the fittest. Or, if this instance be held doubtful, what shall we say to the courting instincts in general, and to the play-instincts of the bower-bird in particular, which are surely quite without meaning from any utilitarian point of view? And these instincts naturally lead on to the æsthetic faculties of mankind, few of which can be possibly ascribed to natural selection, as Mr. Spencer very conclusively shows.

And here it becomes needful again to say a few words on Prof. Weismann's essay, by way of criticism. For he, too, has there considered the case of instincts, but this in a manner which can scarcely be termed fortunate. For example, he particularly instances the case of hereditary fear of enemies as one which supports his argument against the inheritance of functionally-produced modifications. Now, this happens to be one of the instincts which I have elsewhere specially chosen as yielding particularly good proof of the hereditary transmission of individual experience, apart from natural selection. And the proof consists merely in showing, from abundant testimony, that "the original tameness of animals in islands unfrequented by man gradually passes into an hereditary instinct of wildness as the special experiences of man's proclivities accumulate; and that such instinctive adaptation to newly developing conditions may take place without much aid from selection is shown by the short time, or the small number of generations, which is sufficient to allow for the change."[1] But although I think that Prof. Weismann's *selection of this instinct is a particularly unfortunate one for the* purpose of showing that its *acquisition* can *only* be due to natural selection, I quite agree with him in holding that its degeneration in our domesticated animals is due to the withdrawal of natural selection—at least in considerable part.

Again, he argues that if acquired mental proclivities are ever inherited we should expect the human infant, without any individual instruction, to converse. For, he argues, ever since man became human he has been a talking animal, and therefore, if there were any truth in the view that knowledge acquired by individuals tends to be transmitted to their progeny, here is a case where the fact ought to admit of abundant proof: yet every child requires to be taught its mother-tongue by its own individual experience.

Now, without waiting to show the manifest unfairness of this example—seeing how enormously complex a system of cerebral relations the speaking of even the simplest language implies—it is enough for our present purposes to observe that language has been itself the product of an immensely prolonged and highly elaborate evolution. Although it is true that man has always been a talking animal, it is very far from true that he *has always talked the same language*. As a matter of fact, he has talked in thousands of different languages, and if the genetic history of any one of them could now be traced back to its original birth, the probability is that it would be found to have passed through some hundreds of phases, no one of which would have been fully intelligible to the generations which spoke the others. Consequently, even if we were to adopt the impossible supposition that any length of time could be sufficient to enable heredity to elaborate so huge an amount of instinctive acquisition as would be required to render the knowledge of any language intuitive, there would still remain this answer to Prof. Weismann—namely, that if a child *could* talk by instinct, it would require to astonish its parents by addressing them in at least a hundred unknown tongues, before arriving at the one which alone they could understand.

So much, then, by way of answer to Prof. Weismann's supposed difficulty. But the matter does not end even here; for if he had searched the whole range of human faculties he could scarcely have found a worse example to quote in support of his argument, seeing that it admits of being turned against that argument with the most overwhelming effect. This argument is that the fact of speech not being instinctive is proof that acquired knowledge is not transmitted. Now, we have just seen it to be manifestly impossible that so elaborate, as well as so recent, a body of acquired knowledge should be transmitted—even though it were true that many instincts had been evolved in this way. Nevertheless, it might still be reasonably objected—as, indeed, Weismann says—that the simpler features which serve to characterise all spoken languages alike, and which, therefore, have always constituted the common elements of language as such—it might reasonably be urged that these simpler elements which are thus common to all languages might well be expected by this time to have become instinctive, if there is any truth at all in the Lamarckian doctrine of the inherited effects of continuous function. *But this is exactly what we find.* The only elements that are common to all languages are the simplest elements of *articulation*; and it is now established beyond doubt that the human infant is endowed with the instinct of making articulate sounds. Long before the powers of understanding are sufficiently advanced to admit of the child making any rational use of language, he begins to babble meaningless syllabic utterances. And although these utterances are extremely simple when contrasted with the enormous complexity which they are soon destined to attain in intelligible speech, yet, regarded in themselves, or as merely hereditary endowments, the evolution of mechanism which they represent is by no means contemptible. For they necessitate highly peculiar as well as highly co-ordinated movements of the larynx, tongue, lips, and respiratory muscles, not to speak of the special innervation which all this requires, or the yet more special cerebral conformation which it betokens. In short, the illustration of spoken language, far from making against the doctrine of Lamarck, is one of the best illustrations that can be adduced in its favour; for surely it is in itself a most significant *fact that the young of the only talking animal should alone present* the instinct of making articulate sounds—just as it also alone presents the instinct of alternately placing one leg before the other, in a manner suited to walking in an erect position.

Upon the whole, then, I conclude that the effects of use and *disuse are certainly inherited; that the reducing influences of the* latter are largely assisted by the cessation of selection; that the cessation of selection is itself assisted by the economy of growth

[1] "Mental Evolution in Animals," p. 197, where see for evidence.

which constantly depresses the average size of any useless structure; and that in a comparatively few cases, where changed conditions of life have rendered a previously useful organ actively injurious, the influence of selection may not only be withdrawn, but reversed. And if in justification of these views I were required to adduce any single tests as crucial, I should point on the one hand to the neuter ants, and on the other hand to the bower-birds. For the neuter ants prove to demonstration the fact of developing such important structures as enlarged and strengthened jaws through the agency of selection, and of totally losing such important structures as wings through the cessation of selection—in both cases under circumstances which effectually preclude the possibility of any inherited effects of use and disuse. On the other hand, the bower-birds no less conclusively prove the fact of developing highly elaborate and most remarkable instincts, which are entirely without reference to any life-preserving function, and therefore can be ascribed only to the inherited effects of functionally-acquired peculiarities.

If this paper has been at all successful in its objects, it must have brought into prominence one point which I am particularly anxious to make clear—namely, that it is a precarious thing to differ, in any point of biological doctrine, from the matured judgment of Charles Darwin. The more deeply his work is studied, the more profoundly is the conviction impressed, that even though he did not always give it, he always had a reason for the faith that was in him. Therefore, before his followers venture to question a doctrine which was sanctioned by him, common prudence should dictate a careful pondering of the matter. Some of the readers of NATURE may have been led to suppose that as to this I am myself living in a glass house. For my recent suggestion of an additional "factor of organic evolution" has had the effect of bringing many stones about my head with regard to this very point. But these have mostly been thrown by men who have not taken the trouble to acquaint themselves with the exact nature of Mr. Darwin's final judgment upon the points in question. As a matter of fact, there is only one point upon which I have deviated at all from the latest editions of Mr. Darwin's works—namely, as to the degree in which free intercrossing is inimical to natural selection—and, curiously enough, this is just the point which my critics for the most part disregard. I am blamed for my arrogance in disputing the universally adaptive character of specific distinctions, in affirming the generality of some degree of sterility between species, and so forth; but all these criticisms only serve to exemplify the truth of what I am now saying—namely, that before anyone ventures to write about Darwinism he should take the trouble to ascertain exactly what it was that Darwin thought.

GEORGE J. ROMANES.

## *THE AUGUST METEORS OF* 1887.

THE circumstances attending the recurrence of this celebrated meteoric display were by no means favourable in the present year. On August 10 and 11 the moon rose before 11 p.m., so that during later hours the smaller and more numerous class of meteors, many of which would have been visible on a dark sky-ground, were obliterated. Apart from this, the night of the 11th was much overcast, and comparatively few observations could be secured. But, making every allowance for hindrances of this character, the recent shower has proved itself decidedly inferior to many of the conspicuous returns recorded in previous years.

But if this notable stream has been deficient in numerical strength, it has exhibited some features which, though previously observed, have never been capable of being so definitely and satisfactorily traced in their development as during the present year. I refer to the displacement of the apparent radiant point amongst the stars, and to the visible duration of the shower, both of which form important elements in determining the physical nature of the system and in theoretical investigations as to the perturbations which our earth may have exercised upon it during the frequent *rencontres* with its materials in past ages.

The very clear weather recently experienced enabled the progress of the display to be watched on fourteen nights between July 19 and August 14, and the radiant point on each one was determined separately, as by combining the results of several nights the changes in its position would have been rendered more difficult of detection. I first pointed out this change in the radiant in NATURE, vol. xvi. p. 362, and subsequently further details were published in the *Monthly Notices* for December 1884, pp. 97–98. In NATURE, vol. xxxiv. p. 373, will also be found the observations of this peculiarity made here in 1886, but they were not so complete as during the present year, when the radiant centres were successively derived as under.

*Great Perseid Radiant Point* 1887.

| Night. | Radiant. α | Radiant. δ | Meteors. | Night. | Radiant. α | Radiant. δ | Meteors. |
|---|---|---|---|---|---|---|---|
| | ° | ° | | | ° | ° | |
| July 19 | 19 | + 51 | 4 | August 1 | 35 | + 56 | 4 |
| 22 | 25 | + 52 | 5 | 6 | 42 | + 55 | 5 |
| 23 | 25 | + 52 | 4 | 7 | 43 | + 56 | 5 |
| 27 | 29 | + 54 | 5 | 8 | 43 | + 56 | 6 |
| 28 | 30 | + 55 | 10 | 10 | 42½ | + 57½ | 22 |
| 29 | 31 | + 51½ | 10 | 11 | 45 | + 57½ | 16 |
| 31 | 35 | + 54 | 11 | 14 | 53 | + 57 | 8 |

It will be noticed that these figures do not show a perfectly regular progression of the radiant in the direction of east-north-east. This is, however, entirely owing to observational errors which cannot be wholly eliminated from such determinations. Thus the radiant given above for August 6 is no doubt slightly east, and the one for August 10 slightly west, of the true positions. But these trivial discordances in individual positions do not affect the general result, which shows in the clearest manner possible that there is a rapid advance of the radiant from night to night. From all my observations since 1867, which include several thousands of Perseids, I believe this shower extends over a duration of at least forty days, from July 13 to August 22. The earliest visible meteors of the stream emanate from a point between Cassiopeia and Andromeda, while the latest ones diverge from the space separating Auriga and Camelopardus.

From its first oncoming to the epoch of culmination on the night of August 10 it does not gradually intensify but reaches a somewhat sudden maximum. I have sometimes found these meteors rather scarce on August 6, 7, and 8, and not much exceeding their observed frequency at the end of July. But on August 9 there is a marked increase, and on the following night it is apparent the shower attains its most brilliant effect. As to the displacement of the radiant this seems to be accelerated during the declining stages of the display. In July I find the degrees of right ascension of the shower nearly correspond with the days of the month, the diurnal advance being equivalent to about 1° of R.A., whereas on nights succeeding the maximum the change amounts to 2° of R.A. or even more. This difference in place is so striking that any observer may determine it for himself by watching the region of Perseus at the right epoch and charting, with the utmost accuracy, the directions of such meteors as presumably originate from the Perseid stream. These meteors generally leave streaks which furnish a ready means of fixing the paths with a degree of precision that could not be otherwise attained.

In NATURE for August 4, p. 318, I described my observations up to July 29 last. On July 31 I recorded 42 meteors in a watch of 3¼ hours, but the moonlight interfered considerably with the work, as it also did on following nights. The Perseids formed one-fourth of the visible meteors on July 31. I saw 25 meteors on August 1 in 3¼ hours, but the Perseid display was only just recognizable. At 12h. 18m. I observed a splendid fireball passing somewhat slowly from 338° + 43° to 164° + 70°. It left a bright streak or thick train in the latter part of its course, and it was evidently a member of the July Aquariads. At first it was scarcely brighter than a third magnitude star, but when near Polaris it became very brilliant, and afterwards lit up the northern sky with a flash much stronger than the moonlight. I saw 7 other Aquariads on the same night.

On August 6 observations were continued, and 28 meteors were seen in 4¼ hours. Besides the usual shower of Perseids I was much interested in finding a companion radiant at 31° + 49°, which was very sharply defined. I observed a shower on August 11–13, 1880, from 30° + 46° which may be the same; and there is a great probability that this system is connected with Comet I. 1870, which passed near the earth's orbit and would give a radiant near that of the meteor shower and at the same epoch.

On August 7, 23 meteors were seen in 2¾ hours. Only 5 Perseids were recorded. On August 8, 14 meteors were seen in 2½ hours during moonlight, and of these one appearing at 10h. 34m. was as bright as Jupiter. Its course was from 6° + 67½° to 302° + 60½°, and it left a bright streak. At 11h. 28m. a fireball was seen moving rather swiftly from 349° + 15° to 9° + 14¼°, so that its path was one of 20° just above γ Pegasi. At its end