

verse criticism which the hypothesis has met with, especially in France, has, I think, arisen from the misapprehension thus caused. The expression, "Survival of the Fittest," seemed to me to have the advantage of suggesting no thought beyond the bare fact to be expressed; and this was in great part, though not wholly, the reason for using it.

Prof. Cope's indirect statement, that I have said nothing to explain "the origin" of the fittest, is equally erroneous with his direct statement which I have just corrected. In the "Principles of Biology," sec. 147, I have contended that no "interpretation of biologic evolution which rests simply on the basis of biologic induction, is an ultimate interpretation. The biologic induction must be itself interpreted. Only when the process of evolution of organisms is affiliated on the process of evolution in general, can it be truly said to be explained. . . . We have to reconcile the facts with the universal laws of re-distribution of matter and motion." After two chapters treating of the "External Factors" and "Internal Factors," which are dealt with as so many acting and reacting forces, there come two chapters on "Direct Equilibration" and "Indirect Equilibration"—titles which of themselves imply an endeavour to interpret the facts in terms of Matter, Motion, and Force. It is in the second of these chapters that the phrase "Survival of the Fittest" is first used; and it is there used as the most convenient physiological equivalent for the purely physical statement which precedes it.

Respecting the adequacy of the explanation, I, of course, say nothing. But when Prof. Cope implies that no explanation is given, he makes still more manifest that which is already made manifest by his mis-quotation—either that he is speaking at second hand, or that he has read with extreme inattention. HERBERT SPENCER

Athenæum Club, Jan. 29

#### THE CHANCE OF SURVIVAL OF NEW VARIETIES

AN argument first urged by the writer of an article on the "Origin of Species" in the *North British Review* for June 1867, regarding the probability of the preservation of a new modification or variety among the descendants of a plant or animal, has of late attracted much attention. It has been discussed at length by Mr. Mivart, one of the ablest critics of the Darwinian theory, and Mr. Darwin himself has, with characteristic candour, ascribed great, and as I believe undue, importance to the inferences drawn from it.

To some extent I agree with the remarks of Mr. Davis, published in your journal of the 28th December last, but I venture to think that the soundness of the argument in question has not been thoroughly tested, and that it will not bear close examination. The calculus of probabilities is a very subtle instrument, and, even in what appear to be its simpler applications, a very fallacious one, if every step in the process is not carefully considered.

The reviewer started with a seemingly simple statement of the case—"A million creatures are born; 10,000 survive to produce offspring. One of the million has twice as good a chance of surviving; but the chances are

\* By way of correcting a further misapprehension of Prof. Cope, I may here point out that this conception, in its less developed form, goes back to a much earlier date than the "Principles of Biology" to which he refers. In the *Westminster Review* for April 1852 (pp. 498-500), I have contended that "this inevitable redundancy of numbers—this constant increase of people beyond the means of subsistence—necessitates the continual carrying-off of those in whom the power of self-preservation is the least;" that all being subject to the "increasing difficulty of getting a living which excess of fertility entails," there is an average advance under the pressure, since "only those who do advance under it eventually survive;" and these "must be the select of their generation." There is, however, in the essay from which I here quote, no recognition of what Mr. Darwin calls "spontaneous variation," nor of that *divergence of type* which this natural selective process is shown by him to produce.

50 to 1 against the gifted individual being one of the ten thousand (at first erroneously printed 'hundred') survivors." The fallacy here lies in the assumption that under the conditions which, according to the Darwinian theory, enable natural selection to become an efficient modifying agent, the chance of survival of a favourable modification can be correctly represented by the ratio of 2 to 1.

To avoid complication let us confine the argument to non-dioecious plants or self-fertilising lower animals. The preservation of a new variety or modification of structure depends upon two separate elements related respectively to growth and reproduction. The individual must reach maturity, and must reproduce offspring, and for each of these processes it must be able to overcome the obstacles offered by the action of other organic beings, and by external physical conditions. As a general rule we may assume that the same modification does not affect both growth and reproduction, and as the main stress of the struggle for existence turns on the dangers that affect the early period of growth, and the difficulties attendant on the production of healthy offspring, we shall sufficiently illustrate the subject in hand by considering these separately.

The chance of a modified individual growing to maturity depends upon its power of resistance to, or escape from, the various hostile agencies that surround the young animal or plant, whose combined influence is (by hypothesis) such that but one out of every hundred reaches maturity. Let us assume, for the sake of illustration, that the most important dangers to which the creature is exposed arise from physical conditions—such as excessive drought or damp—and from other organisms, as when it is the favourite food of some common animal. Now let the supposed modification affect the former relation. Let the modified organism be better fitted to resist drought; the result will be an enormous probability in favour of its escape from a danger that may destroy nine-tenths of the unmodified creatures around him, and a similar argument will apply to such a modification as would make the individual modified distasteful, or less than usually attractive, as an article of food. In point of fact, the dangers arising from external physical conditions are usually far less constant in their action than those arising from organic foes, and it is quite conceivable that even in the extreme case of a modification originating in one single individual of a species, if it were such as to give a decided advantage in that direction, the balance of probability would be in favour of survival, and in case of reappearance among numerous individuals in the next generation, have a preponderating chance of ultimate preservation.

The application of figures to measure the advantage given by a modification relating to the capacity of a species for reproduction involves no less difficulty, and may lead to the most various estimates of the probability of survival. A variation in a plant which should double the number of seeds produced without lessening their vitality, would give an advantage of 2 to 1 in the chance of producing offspring, but this, as the reviewer has shown, would not much increase the probability of the ultimate prevalence of that variety. But if the numbers of a plant were chiefly kept down by such a cause as the fruit being a favourite article of food, a modification of its flavour that would lead to some other fruit being preferred would almost certainly lead to the perpetuation of the variety with modified fruit, and not only to the rapid destruction of the unmodified form, but also to a reduction in the prevalence of some other plant.

For it must be recollected that the struggle for existence is not limited to the offspring of a single species. The rivals of each organism are all around, and the chance of survival of a new variety may be enormously increased if it be not only better able to resist hostile

agencies that the unmodified form of the same species, but better than other rival organisms that may be its competitors in the struggle for existence.

I make these remarks without any desire to press the conclusion to an extreme length. I am not one of those more Darwinian than Mr. Darwin himself, who believe that the theory of Natural Selection explains everything, and has left no mysteries unsolved. I feel no doubt but that very many modifications arise that do not perpetuate themselves by the survival of a sufficient number of similarly modified individuals, even in cases where the variation may be slightly favourable; but I cannot admit the validity of an argument that goes to the very root of the principle of Natural Selection, and leads, by the appearance of exact reasoning, to a result that every naturalist feels to be absurd.

In truth, it is impossible to assign any limit to the amount of probability in favour of the preservation of a new variety. In the absence of disturbing causes affecting the equilibrium which the conditions hitherto existing in a given region tend to establish between the numbers of each species, it may be safe to assume that the probability of any new variety establishing itself is but small. But let that equilibrium be disturbed—let some hitherto unknown plants spread widely, as so many European weeds have done in Australia. This must lead to a corresponding diminution in the number of individuals of the previous vegetable inhabitants of the country, and a corresponding reduction among the animals that fed upon them. Let one of these animals be modified so as to be able to derive nourishment from the intrusive species. Is it not evident that the chance of its survival, and that of its similarly modified descendants, would be so great as to approach to certainty, unless the modification happened to bring with it other counterbalancing disadvantages?

JOHN BALL

#### THE USE AND ABUSE OF COMPLIMENTARY NAMES

THOSE whose fortune it is to work in some particular branch of science which has not been by any means exhausted, and to encounter daily some new form from an unexplored region which seems to warrant recognition as a new species, are often in difficulty to obtain a suitable name, one which shall distinguish the new species from its congeners, or give indication of one of its most prominent characteristics. It would seem that some (I fear many) are not so fully impressed as they should be with the importance of giving appropriate specific names to new species. "Trivial" names is in many cases an accurate designation. When a new name has to be given, it seems to me that the first effort should be directed towards applying a name which has at least some connection with the object to which it is applied, and if possible indicate one of the features by which its specific distinction is established. In very large genera this will often be difficult, but seldom impossible, if sufficient reflection be permitted. This presupposes, of course, clear notions of what are the distinctive features of the new species, and something more than a mere superficial knowledge of its congeners. The custom of giving complimentary names has considerably increased of late years, and seems almost to have culminated in absurdity. It is never a thankful office to impute blame, or point out the failings of others, and I should never have ventured to draw attention to this subject did I not conceive that the application of complimentary specific names has become an abuse which needs to be protested against. I am willing to concede that the occasional dedication of a new species to some acknowledged authority, one who has published a monograph of the genus, or who has identified himself more or

less with the subject, may be a graceful compliment; but even this should hardly supersede a name indicative of some special feature in the new species. My own feelings are in favour of wholly restricting such compliments to generic names. But wherefore should a mere collector, one who has stumbled over a new species by mere accident, by collecting everything that came in his way of a particular kind, unable perhaps even to recognise generic distinctions, be flattered by having his name attached to the new form by some one who has had all the scientific labour in examining, describing, and naming it for him? Has science no higher aim than that of scattering compliments? It must cause many a smile to pass across the countenances of the unscientific if they open a new cryptogamic flora, a monograph, or even glance through a volume of some scientific journal, to see on one page how Mr. Brown ventures to name something new in honour of his friend Mr. Robinson, and a few pages further on Mr. Robinson returns the compliment in favour of Mr. Brown; or in another case how in five or six genera, extending over as many pages, the same "indefatigable collector" is honoured by having his name as many times repeated, as if new species were only so many pegs on which compliments are to be suspended. My own experience is very much restricted to cryptogamic botany, and my remarks may be much less pertinent to other branches of natural science. Zoologists may not be addicted to such forms of flattery. Continental mycologists are certainly very great sinners in this respect. My object in drawing the attention of readers of NATURE to this subject is to protest against this "abuse of complimentary names," and to ascertain if some definite restriction cannot be placed upon this tendency to encumber our lists with an array of names which convey only one meaning, and which I would designate "flattery names." I hardly think it necessary to cite particular instances, as a question of this kind should be decided upon its merits, and without the introduction of personalities. The sceptical should make the experiment with some recent volume containing descriptions of new species. In one contingency, I think that it is not only admissible but advisable to use a complimentary name. If an author describes a species under a name which has already been adopted in the same genus, it would be very inconvenient to have the one specific name applied by two authors to different things. In such a case it is the custom for any one who may be working up and publishing a synopsis of the genus to suppress the most recent of the two specific names, and apply to it the name of the author who unconsciously fell into the error. Provided always that he recognises the species having priority of name as a valid member of the genus, there cannot be much abuse of this recognised practice, against which I have nothing to urge. It would be simple folly to make laws which there is no power but "common sense" to enforce; and no decision which I may determine upon will be binding upon any one save myself; yet I cannot but regret that any who have laboured year after year in love for their own special branch of science, often following it for its own sake alone, through many sacrifices, should be tempted to employ the knowledge they have so acquired as a means whereby to compliment their friends or flatter their inferiors, forgetful of the practical sarcasms that they are hurling at their own pursuits.

M. C. C.

#### THE ECLIPSE OBSERVATIONS AT BEKUL

THE illustrations which accompany this, for the loan of which we are indebted to the courtesy of the Editor of the *Illustrated London News*, are from photographs of the Eclipse party stationed at Bekul, taken by Mr. McC. Webster, the Collector of South Canara. The first represents the fort in which Mr. Lockyer and Captain