and the animals have not strength to move the instrument, so that "both ploughs and ploughmen succumb, and the antediluvian implement of the ryot is found to be the only feasible one."

SOME idea of the damage done to vegetation by locusts in tropical countries may be gathered from the following account of a raid made by them in an East Indian cotton plantation. The means adopted to repel them was by recourse to the discordant sounds of native music—horns, tom-toms, and pipes—aided by the waving of flags and branches of trees. These measures, undoubtedly, saved the produce; for, judging by the performance of the very small number that succeeded in gaining admission to one of the finest fields unobserved, had a full complement effected a lodgment, one hour would have sufficed to strip every tree of its leaves, though the foliage was abundant, and the plants in one field from five to six feet high. The immunity which the native Indian cotton enjoyed from the attacks was considerable, considering the avidity with which they devoured the exotic descriptions, and, true to their early traditions, the Egyptian was evidently an especial favourite. Some of the swarms that passed over the country at that time were exceedingly numerous. The arrival and settlement of one mighty mass was a remarkable sight. What was first observed was a sort of haze on the verge of the horizon, in a long line, as if a steamer had passed, and its smoke was rising into vapour; this was some hours before the insects arrived. The cloud gradually thickened, and rose higher as they approached. When they got fairly overhead the air became darkened as if night was setting in, it being yet mid-day, and the peculiar sound which accompanied their flight resembled that of the rustling of the leaves of the peepul tree when agitated by light winds; but it is not until they have settled down that any idea can be formed of the immensity of their numbers, and the early dawn, before sunrise has warmed them into life and motion, is the time to witness this most extraordinary sight. In the instance now referred to the appearance the face of the country wore would be best described by supposing that a tolerably heavy fall of snow had taken place, only that the colour of it was a light brown, and this extended for miles, as far, indeed, as the eye could reach. Trees were favourite perching-ground for the night, and the manner in which they contrived to crowd upon them, piles over piles, concealing every vestige of leaf and branch, gave the trees a singular appearance. At one spot a stout and wide-spreading branch of a banyan tree had snapped at its stem from the incumbent weight of the insects.

RICH mines of gold and silver are being daily discovered in the State of Tulima, in Columbia, according to late advices.

A RECENT number of the American *Journal of Chemistry* contained the following story of the first introduction of the stereoscope to the *savants* of France. The Abbé Moigno took the instrument to Arago, and tried to interest him in it; but Arago unluckily had a defect of vision which made him see double, so that on looking into the stereoscope he saw only a medley of four pictures. The Abbé then went to Savart, but he was quite as incapable of appreciating the thing, for he had but one eye. Becquerel was next visited, but he was nearly blind, and consequently cared little for the new optical toy. The Abbé, not discouraged, called next upon Puillet, of the Conservatoire des Arts et Métiers. He was a good deal interested in the description of the apparatus, but unfortunately he squinted, and therefore could see nothing in it but a blurred mixture of images. Lastly, Biot was tried, but Biot was an earnest advocate of the corpuscular theory of light, and until he could be assured that the new contrivance did not contradict that theory, he *would* not see anything in it. Under the circumstances, the wonder is that the stereoscope ever got fairly into France.

MIMICRY AND HYBRIDISATION *

SOME time since I had occasion to study with care, for the purposes of a work on which I am engaged, the phenomena of mimetic analogy made known by Mr. Bates, which have lately formed the subject of discussion at the British Association, and in the pages of NATURE, in which I observe with pleasure that one of our body, Mr. A. W. Bennett, has borne an honourable part. Neither he nor any of the gentlemen who have written on the subject, have, however, so far as has come under my notice, brought the point to its real issue. They have accepted battle on the field on which Mr. Bates has placed it, and although they may have achieved a victory over him, they have not succeeded in rescuing the subject from its obscurity. He may be wrong without their being right. I am not surprised at their having been led to accept his premises; when I first approached the subject I did the same; but the longer I live and the more extended my experience becomes, the more surely do I find that when a theory looks shaky and unsound, the place to look for the flaw is not in the upper story, but in the basement. It is in the foundation that the crack will almost invariably be found. I am sure it is so here.

Mr. Bates found in the Valley of the Amazons a number of species of a Northern tribe of butterflies, wearing the colour and form of a Brazilian tribe, and so like in their varieties and strains that they obviously represent some different phenomenon from the ordinary one of mere difference in species. To account for this he devises a theory on the Natural Selection plan. The Brazilian tribe has a bad smell, and birds and insects of prey consequently do not feed upon them, and the Northern tribe, in the course of their variation in the dark, accidentally produce one something like the Brazilian one, which produces others in the same direction by Natural Selection, until the mimics are brought to perfection. Every inch of the ground he goes over here is mined and unsound—the bad smell has not been observed in North America where similar mimicry occurs—birds and insects of prey hunt by sight and not by smell, and the various communications on the subject in NATURE point out a variety of other insuperable objections. But my object is not so much to show that a friend and entomological brother has been seduced by a "bad smell" to go on a wrong scent, like a good dog after a red herring, but to find out the true explanation of the phenomenon.

The explanation seems to me to be simply Hybridisation; but before committing myself to it, as there were one or two points on which I was not sure how far the phenomena corresponded with those of hybridisation in plants, I applied to my friend Mr. Isaac Anderson Henry for information upon them, and he has sent me a paper (for the Scientific Committee of the Horticultural Society), as well as some other information, which enables me now to say that there is not a phase or a fact in the mimicry in question, for which I cannot produce the exact counterpart in the hybridisation of plants.

In the first place the mimicked and the mimickers are always found together, and even the mimickers of varieties are only found beside the varieties that they mimic. Now, it is plain that if the resemblances be due to hybridisation, it is inevitable that the two must always be found together, at least in the first instance. It may be that after the hybrids are established and advanced into the position of actual species, the species (*i.e.* the parent and offspring) might diverge from their primary locality, the one to the right and the other to the left, and so cease to be found together; but this must be an after act, and consequently an exception. The natural condition is to find both together, and so they are always found together. But this would not be the natural condition if the mimicry were produced by Natural Selection. The same enemies are found over thousand of miles, and the same kind of enemies over tens of thousands; and there is no advantage to be gained by mimicking one variety of Danais more than another. The same advantageous results would be obtained by mimicking in the east the form that prevails in the west, or in the north the form that prevails in the south, but the imitation of each variety is limited to the district which it inhabits, however narrow and restricted it may be. Natural Selection, therefore, fails entirely to account for the localisation of the mimickers of varieties.

In the next place the mimicked occur always in overwhelmingly greater numbers than the mimickers. Mr. Bates says:—"The Ithomiæ (Danaids) are all excessively numerous in

* This paper was originally presented to the Scientific Committee of the Horticultural Society (Dec. 7, 1870), but has not yet been published elsewhere.

individuals, swarms of each kind being found in the localities they inhabit. The Leptalidæ (mimics) are exceedingly rare; they cannot be more than 1 in 1,000 with regard to the Ithomiæ." This is quite what we should expect if the resemblance is due to hybridisation. Hybridisation is not the normal mode of producing either species or individuals. It is not the plan laid down by Nature. Being exceptional, it is, of course, comparatively rare. But there is no reason for rarity if it be the result of Natural Selection. That operation is going on equally upon all, and under that hypothesis mimicry is just as powerful an influence in modifying and producing forms as any other; and there is no reason whatever why it should have less conspicuous results; indeed, it should have more, if we judge by the long-continued persistence of influence which must have been in operation to produce such exact resemblances, and which, indeed, seems very much thrown away when confined to the 1 in a 1,000 mentioned by Mr. Bates.

Although mimicry occurs between various tribes or genera, it has been observed most frequently in connection with the most common species of the country. This is what would naturally be the case with hybridisation, supposing all to start fair and to be equally liable to hybridisation. But this is an assumption which we are scarcely warranted in making, and I therefore do not press this inference further than as of some conditional value.

After the second generation of hybrids in plants, it was first shown by M. Naudin, and is now well known to all hybridisers, that those which do not revert to type break out into an overflow of irregular variation, which supplies many of his most remarkable sports to the horticulturist, and many of his most puzzling difficulties to the systematic botanist. On the assumption that the mimicry in question is the result of hybridisation, we should therefore expect to find a marked degree of variation among the mimicking species. And so we do. Mr. Bates figures no less than fifteen varieties of *Leptalis Ithomiæ*, one of his mimics, which itself mimics seven different species (all very close to each other, however, and perhaps scarcely deserving the name of independent instances.) Mr. Trimen figures six varieties of *Papilio Merope*, which supplies four of his instances of mimicry, and Mr. Wallace's imitating Papilios were in like manner remarkable for their variations. It seems a fair inference that when the mimicking species are not variable they have been established before the second generation of hybrids, and where they are variable they have been established subsequent to the second generation, and have experienced the usual shock to stability occasioned by such repeated loosening of the fetters of specific identity.

Mr. Bates's list of mimics and mimicked species shows, too, that when a species is mimicked by one species or genus it is often mimicked by more, a fact which, applied to the idea of hybridisation, simply means that that species had a readiness to take to itself wives of more than one of the nations round about. Out of twenty-eight Danaoid species cited by him, which had been mimicked or had families from strange husbands, fourteen had families from one each, three from two each, and six from three each. It is only what we find in plants, that some are more open to hybridisation than others; or perhaps, analogous to our moral experience, that where scope is allowed to our own passions, license soon degenerates into libertinism.

Another feature, familiar to all hybridisers, occurs in these mimicries. Notwithstanding the statement of Wichura to the contrary, it is now perfectly well known that in attempting to obtain a cross between two species we often fail when we work with the male of one species and the female of the other, while we succeed when we reverse the process and take the male of the latter and the female of the former. In plants the cases where this capability of crossing in only one direction occurs are beyond number. Mr. Isaac Anderson Henry cites many of them in his late Presidential Address to the Botanical Society of Edinburgh, and in the paper which I have now the pleasure to lay before the Committee. The very same thing has occurred with the mimicries recorded by Mr. Bates. They are all on one side of the house. According to my view (indeed if hybridisation is once allowed to have been the motive power, it must be according to every one's view), the parents were the Danaids on the one side, and the cabbage whites (Pieridæ) on the other, for all the mimicked are Danaids with their special characters, viz., only four apparent legs, while all the mimickers, like the whites, have their special characters, six legs apparent. If they had been hybridised from both sides, we should have had some Danaids with the form and colour of the whites, as well as whites with the form and colour of Danaids; but we have not. The case which so often occurs in plants has obviously occurred here. The cross was taken only from one side. Which is it? I apprehend, from other examples, that it should be on the side of highest organisation—that is, that the male parent has been of the lower organisation, and the female parent (the actual bringer forth) of the higher. Now, which is the side of highest organisation in the Danaids and Pieridæ? Is it that of greatest strength? If it were so, it would then be the Danaids, for they are larger, finer, and more powerful than the more northern whites. But organisation is a higher test than mere strength. This, too, seems to be on the side of the Brazilian tribe. Mr. Bates so considers it, and his reason is that, the essential quality of butterflies being flight, the type which has most attention paid to its wings and least to its legs, must be highest of its order. Others think differently, and say that a type which has had two of its limbs (its anterior legs) almost atrophied, cannot be so perfect an animal as one which has them all in perfection. But I agree with Mr. Bates on this point (at all events in his conclusion). The greater number of legs cannot be any indication of higher organisation, or a centipede might dispute supremacy with ourselves, and push us from our stools. Multiplicity of sub-division or repetition of parts is acknowledged by all physiologists to be an indicatior on inferiority of organisation. The fewer limbs, that is the simpler the apparatus that a creature can do its work with, the higher the perfection of the machine. Therefore, doubtless, Brazilian Danaids are the higher type, and if (as I believe to be the case), in crosses of difficult accomplishment, the female is the higher parent, then the cross from which these mimics resulted was one by the males of the whites upon the females of the Danaids.

In what I have above said as to one-sided crossing, I have assumed that in plant-hybridisation the fact would be admitted; but as it is in contradiction to the statement of so eminent an authority as Wichura, I shall remove all doubt from the subject by quoting Mr. Anderson Henry. He says:—"I regret to differ from so great an authority as Wichura (who has maintained that 'the products which arise from reciprocal crossing in plants, unlike those which are formed among animals, are perfectly alike'), and must venture to demur to the doctrine in more decided terms than Mr. Berkeley does. I have had so many instances of hybrids taking sometimes to one side and sometimes to another, *but most frequently to that of the mother*, that to those who, like me, have tried their hand with many genera, it would be a matter of supererogation to give instances. I have had them by the score."

But the mixed product also corresponds with another fact observed in hybridisation. Mr. Henry informs me that in some of his crossings of plants he has only succeeded in altering the flowers, the foliage continuing persistently the same as that of one of the parents. He has not succeeded in distributing the union through all parts. That is exactly parallel to what we see in these mimicries. The number of legs and the nervation of the wings (in other words the more structural portions of the animal) remain special as in one parent, while the colour and form of the wings, &c., is taken from the other. In the butterflies it is the more structural parts (legs, nervures of wings, &c.) of the male parent which are observed in the offspring, while the form and general appearance only of the female parent is adopted. In plants it may be a question whether we should consider the flower or the foliage as the more structural parts—for my part I should take the flower as the more important, and therefore equivalent to the structure of the legs and wings; and the foliage and habit of the plant in Mr. Anderson Henry's case as equivalent to the colour and form of the wings and general appearance of the insect. Another phase of the mimicry, which I have no doubt will be found to have also its parallel in the hybridisation of plants, although I am not able to cite any instances exactly in point, is that in species which have dissimilar sexes, it sometimes extends to both sexes, the males being like the males and the females like the females, but in other instances is confined to the females. I believe that the reason why I have no case in point to cite in plants is that it can only be had in diœcious plants, and the hybridisation of diœcious plants has hitherto been scarcely at all attended to. Mr. Henry has some coming forward, but they have not yet flowered.

The last point to be noticed is one of some importance, as being the only one furnishing a shadow of objection to the explanation of the mimicries in question by hybridisation. It is that the nearest natural allies of both the mimickers and mimicked are *not* always to be found in the same district. This deserves the more attention, since it appeared so strong to Mr. Bates as to lead him to relinquish the idea of hybridisation as an explanation after it had crossed his mind. "The explanation," says he, "that the whole are the result of hybridisation from a few originally distinct species cannot at all apply in this case, because the distinct forms, whose intercrossing would be required to produce the hybrids, are confined to districts situated many hundred miles apart."

Before I proceed to show how simple the explanation of the absence of one of the parents is, I must beg to note in passing the admission that there are distinct forms whose intercrossing would produce the hybrids. That granted, I would remind the reader of what Mr. Bates has obviously overlooked, that we are dealing with a phenomenon probably of a very ancient date, and that one side of the parental stock may have disappeared in the course of time. I have elsewhere suggested, in regard to hybridisation as a possible originator of species, that it must be a necessary accession to such an event that the hybrids should have opportunity of isolation, such as might be obtained by thinly peopled districts where they might settle, spread, and establish themselves. Now, certainly, the Valley of the Amazons, the Malayan Archipelago, and many parts of the South of Africa (lands whence these mimetic analogies come) have at different periods all been at one time unoccupied land; for all of them have been raised from the bottom of the sea, and been peopled by the influx of the inhabitants of neighbouring lands. No one knows better than Mr. Bates that at one time Brazil was unconnected with New Granada or the Andes. The Danaids were then inhabitants of it, but not inhabitants of the countries about it; while the Pieridæ, or cabbage whites, were what I have elsewhere denominated a microtypal tribe from more temperate climes, and were present in the Andes and the mountain countries, as Columbia, connected with them. In the natural course of things, therefore, when the Valley of the Amazons was changed from the bottom of a sea to dry land, the Danaids would spread into it from Brazil, and the Pieridæ from the north and west, and meeting in an open, as yet, unpeopled country, hybridisation might take place under one of the few circumstances where I have thought it possible that it could retain its place and establish its products as species. The objection that frightened off Mr. Bates is, in reality, no objection at all to the hypothesis of the mimicry being due to hybridisation, that we are not always, or even that we should not at all be able to identify the probable parents of the mimickers as inhabitants of the same country as their supposed descendants. One of the parents we know to be present (the so-called mimicked), but there are excellent reasons why the other parent should not be present. It is of a northern type, suited for our temperate regions, but not adapted to the tropics except at a higher elevation and a cooler temperature than the damp, hot valley of the Amazons. Although, therefore, it might descend into that region, it is not only a natural but almost a necessary inference that it would not find it congenial or habitable, and although it might live long enough in it to found a dynasty of mimickers, it would soon die off from unsuitable conditions, while its hybrid offspring bred from the tropical Danaids might, from the black blood so imparted to them, find it sufficiently well suited for them.

There is yet another phenomenon connected with Mimicry, which possibly may also be connected with hybridisation, viz., the occurrence of what Mr. Wallace has called dimorphism in insects among the mimicking or mimicked species. We must not, however, confound this dimorphism with Darwin's dimorphism in plants. The two are totally different things, and, as it seems to me, have no relation or analogy to each other. In plants the dimorphism is always confined to the reproductive organs, in insects it has apparently nothing to do with them. Moreover, it seems to me that all the instances of so-called dimorphism in insects that have yet been recorded, are nothing but examples of variation, perhaps complicated by hybridisation. M. Reinhard, of Bautzen, has shown that this is the case with regard to Mr. Walsh's conclusions respecting the dimorphism of certain gall-flies, for he had found that the galls of various species appear to be so transitional between other forms, that they can only be known with certainty when the perfect insect appears. It appears to me to be also the case in all those instances where the dimorphism is confined to particular districts, as in the *Papilio Turnus* of North America, where all the females are yellow in the New England States and in New York, while in Illinois, and farther south, they are all black, and in the intermediate region, both black and yellow females occur in varying proportions. And the case is not open to any doubt, because in the intermediate district, both yellow and black insects have been bred from the same batch of eggs. Now, if the case had been that *both males and females* equally varied, and that in the south all were black and in the north all yellow, with intermediate gradations in the districts between, we scarcely suppose that any one would have thought of calling it a case of dimorphism. If they did, then all climatal variations (and their name is legion) would come under the same category. It is only dimorphism, because the change is limited to the female. But is this a good ground? Physiologists are unanimous in holding that neither the male nor the female is the species, but both; and if that be the case, in what does a variation in the female and not in the male differ from a variation in both but in degree? Most of Mr. Wallace's instances of dimorphism are of this character—the male being the same in a number of islands in each of which the female differs. All these I regard as mere instances of climatal variation, in which the variation shows itself only in that part of the species called the female. An occasional case of variation from some other cause, as from hybridism, may possibly come to complicate this phenomenon; but it appears to me to be sufficiently explained by variation, and the circumstance above mentioned is significant that where mimicry occurs in species having dissimilar sexes, it too is often confined to the female.

A. MURRAY

SCIENTIFIC SERIALS

Silliman's Journal, September 1870.—The opening article of this number is by Prof. E. Loomis, and is entitled "Comparison of the mean daily range of the Magnetic Declination, with the number of Auroras observed each year, and the extent of the black spots on the surface of the Sun." The author first discusses the observations of sun-spots, and points out some corrections that should be made in the numbers obtained by astronomers in the last century; he points out that the period is one of ten years, and is influenced by the heliocentric conjunctions of Jupiter and Saturn, but affected by the conjunctions of the Earth and Venus. By a series of tables and curves the coincidences of periods of the maximum number of sun-spots with the maxima of magnetic disturbance and auroral display are elucidated, from which it appears that the present year is a period of maximum.—In a letter to the editors, Mr. J. W. French proposes *a new period in chronology called the Precession Period*, of 25,782 years, being the time for the precession of the equinoxes. The author prefers this period, since it is founded solely on astronomical facts.—The third article is by F. W. Clarke, "On the atomic volumes of solid compounds," in which are discussed the relations of the volumes of analogous and similarly constituted bodies.—The next article, "Considerations on the apparent inequalities of long periods in the mean motion of the Moon," is by Simon Newcomb, and, after a long discussion on the observations on this subject, and the theories proposed to explain them, the author attributes the phenomenon to an irregularity in the rotation of the crust of the earth, caused by the motion of its fluid contents.—The following is a very interesting article by Dr. A. M. Mayer on "Researches in Electromagnetism." The author has devised a very accurate method of determining the relative values of electro-magnets to replace the one usually employed, which consists of measuring the deflection of a magnetic needle which is produced by the action of the electro-magnet. The author found that this process was liable to error in consequence of the difficulty of keeping the current absolutely constant, resulting in a continual motion of the needle. These difficulties were obviated in the following manner: A line eight feet long and divided into fractions of inches was drawn on a table, the latter being so placed that the line was at right angles to the magnetic meridian; a compass, with a needle nearly six inches long, was placed on this line, and a helix was fixed at each extremity of the line. These helices were traversed by the same current, a tangent galvanometer being placed in the circuit. In this way the needle was influenced by two magnets acting in opposite directions and excited by the same current, and if any deflection of the needle was observed, it must have been due to a difference of power of the magnets. If this occurred the needle might be brought to 0° by moving it from the stronger magnet. A series of experiments was made