varieties of protectively coloured insects are frequently confined to very limited areas. Some will only be found on a certain species of tree or plant; others only on rocks or a stone wall of some particular colour; others, again, only on small patches of soil or gravel; while a short distance from these there may be other objects differently marked, which may be frequented by insects altogether different in colour, although belonging to the same or to an allied species. Are we to suppose that every tree, plant, rock, every stone wall, and every distinctive patch of soil or gravel, has been the scene of natural selection? There is no other conclusion open to the Darwinist. But when it is considered that natural selection may take hundreds of thousands or even millions of years, to effect a given result, the strain upon our forbearance must be great when we are asked to believe that this process is the only one we have to reckon with. If the phenomena can be accounted for by a shorter or simpler process, why should the longer and more complex one be insisted on? Is it not more reasonable to suppose that animals have sufficient intelligence to fly to, and remain in, the place where experience has shown they are least exposed to observation? Can anyone doubt that animals possess such knowledge? How otherwise are we to explain the action of the butterfly, for instance, in darting at once when disturbed to some object which resembles itself, and then lying perfectly still, when one might in vain attempt to find it, although within a few inches of it?

This view also receives corroboration from the fact that many unprotected animals render themselves inconspicuous by covering themselves with materials which resemble their environment. Thus certain Lepidopterous larvæ form cases for themselves out of the fragments of the substance on which they feed, the cases of the larvæ of the Psychidæ, for instance, being made of leaves or of brown grass stems; those of the Essex emerald moth of fragments of leaves spun together with silk; certain species of sea-urchins and many Mollusca cover themselves with grains of sand, shell, and bits of stone, while, according to Poulton, certain species of crabs fasten species of seaweed to their bodies for the same purpose.

Topical selection will also explain the protective coloration of certain vertebrates, as rabbits, hares, and deer. Thus Mr. H. A. Brydon, who has an extensive acquaintance with the habits of deer in South Africa, writes ("Kloof and Karoo," p. 298) as follows:—

"In some localities where the 'zuur veldt' clothes the upper parts of the mountains, and the 'rooi' grass the lower portions, the vaal and the rooi rhebok may be found on the same mountain-side, but each adhering to its own peculiar pasturage. When the hunters come upon the ground to shoot, the rooi rhebok immediately fly from their lower slopes to the higher ground of their grey brethren, and the two species are seen galloping in close company over the mountain heights. If the hunter rests quietly after his shot and looks about him, he will presently see the two kinds of antelope, as soon as they think they may safely do so, separating, the rooi rhebok quitting the 'vaal' pastures, and betaking themselves again to their own feeding-grounds. To this habit they invariably adhere, and will not delay their departure an instant longer than their safety admits of. If the vaal rhebok in turn are driven out of their own ground, they pursue exactly the same tactics, and will on no account remain for long in their red brethren's territory."

The occurrence of so many trimorphic and polymorphic varieties of the same species have always been a puzzle to Darwinists, as the numerous varieties which the Darwinian theory postulates would all be killed off by natural selection, except the "fit"; but according to the theory which I have advanced, most variations would find their appropriate environments and live. If this theory of topical selection be correct, its application to the phenomena of mimicry is obvious. We have only to suppose that one animal may find safety in associating with another animal to which it has some resemblance, without invoking the aid of either mimicry or natural selection.

I shall not attempt to reply to the other remarks of your critic further than this, that no one who contents himself with reading Dr. Wallace's review will be able to form the slightest idea of the views put forth in my book. That it has taken a lifetime, as Dr. Wallace correctly enough says it has, to build up "the vast edifice" of Darwinism is surely no guarantee of the truth of that system, and certainly no reason why it should be above criticism, as my reviewer seems to think it should be.

Melbourne, 1891. DAVID SYME.

MR. SYME now says: "The references to Darwin in my book are absolutely correct," and—"In every reference to Darwin's views I gave the page and the edition from which the quotation was taken." Assertions, however, are not proofs; but if Mr. Syme will point out where Darwin defines natural selection as "the struggle for existence," and where Darwin "insists that variations are created by natural selection," statements which occur at p. 8 and p. 15 of Mr. Syme's book, I will acknowledge that I have misrepresented him. Otherwise I see nothing that requires modification in my article. But as Mr. Syme claims to have taken "the utmost pains" to quote Darwin's exact words, I will refer to other cases. At p. 12 he says, "The second assumption is that favourably modified individuals should be few in number, 'two or more';" and for this he refers to "Plants and Animals under Domestication," vol. ii. p. 7. The true reference is to vol. i. p. 7, where Darwin says: "Now, if we suppose a species to produce two or more varieties, and these in course of time to produce other varieties, &c." Here we see that Mr. Syme puts "individuals" in the place of "varieties," and thus makes Darwin appear to say the exact reverse of his main contention, which is, that ordinary variability occurring in large numbers of individuals, not single sports, are the effective agents in the modification of species.

Again, at p. 102, Mr. Syme says, when discussing cross-fertilization and variability: "No doubt self-fertilization is a great factor in producing uniformity of colour. That this uniformity is not due to the plants having been 'subjected to somewhat diversified conditions,' as Darwin intimates, is shown by the fact, &c." But Darwin, as every student knows, said exactly the reverse of this—that the somewhat diversified conditions produced *variability*; and Mr. Syme's great efforts to understand him and to quote him correctly again fail of success.

One more example is to be found at p. 110, where he says: "Darwin has distinctly laid down the principle that if it can be proved, by a single instance, that one organism exists for the benefit of another organism, his whole system would fall to the ground." But the statement made by Darwin was, that if any part of the structure of one species could be proved to have been formed for the *exclusive* good of another species it would annihilate his theory ("Origin," 6th edition, p. 162). Mr. Syme omits the essential word "exclusively," and thus appears to have a strong case against the theory.

As an example of general misrepresentation, I will refer to p. 86, where Mr. Syme states that "the Darwinist" "carefully ignores the facts which point in the opposite direction" (of the necessity for insect fertilization of flowers); and on the next page, after referring to cleistogamic and other self-fertilized flowers, he asks: "Why does the Darwinist omit mention of such structures as these?" But he does not refer us to the Darwinists in question who, while discussing insect fertilization, "carefully ignore" self-fertilization; and as his statement will be taken to include all, or at least the majority of Darwinists, it must be held, by those who are acquainted with the facts, to be a very absurd misrepresentation.

Other examples might be given, but these are sufficient to support my statement that Mr. Syme has both misquoted and misrepresented Darwin.

The exposition of his theory of "topical selection" to explain the phenomena of mimicry, as given above, may be left to the judgment of the readers of NATURE.

ALFRED R. WALLACE.

PROF. PICTET'S LABORATORY AT BERLIN.

IT has often been remarked that purely scientific research frequently bears fruit of practical value. A fresh illustration of this fact is afforded by the work of Prof. Pictet, the eminent man of science of Geneva, who is turning to practical account the apparatus by which, in 1877, he first reduced hydrogen and oxygen to the liquid state. At Berlin, where he now resides, he has established, on the scale of a small factory, what he terms a "laboratoire à basses temperatures." The following account of the work carried on and the results obtained is taken from papers read by the Professor before different scientific Societies of Berlin.

The refrigerating machinery, driven by several powerful steam-engines, is intended to withdraw heat from the objects under observation, and to keep them at any temperature between −20° and −200° C. as long as may be required. Prof. Pictet's experience has led him to the conclusion that among the refrigerating agents known, such as rarefaction of gases, dissolution of salts, evaporation of liquids, the latter is to be preferred. A long course of research has further enabled him to choose the most convenient from amongst the great number of suitable liquids. In order to avoid the great pressure required in handling the highly evaporative substances of lowest boiling-point which serve to produce extreme cold, it is necessary to divide the difference of temperature into several stages. Each stage is fitted with especial apparatus consisting of an air-pump worked by steam, which drains off the vapours of the liquid from the refrigerator, and forces them into a condenser, whence, reduced to the liquid state, they are again offered for evaporation in the refrigerator. Thus the liquid, without any loss beyond leakage, passes through a continuous circuit, and the operations can be carried on for any length of time. The liquid made use of for the first stage is the mixture of sulphurous acid and a small percentage of carbonic acid called "*liquide Pictet*." It is condensed at a pressure of about two atmospheres in a spiral tube merely cooled by running water. Oxide of nitrogen (laughing gas) is the liquid chosen for the second stage. Its vapours are condensed in the same way at a pressure about five or six times as great in a tube maintained at about −80° by the action of the first circuit. As medium for a third stage, in which, however, continuous circulation has not yet been attempted, atmospheric air is employed, which passes into the liquid state at a pressure of no more than about 75 atmospheres, provided the condenser is kept at −135° by the first two circuits. The evaporation of the liquefied air causes the thermometer to fall below −200°.

By this combination quite new conditions for investigating the properties of matter are realized. In various branches of science new and surprising facts have already been brought to light. Many laws and observations will have to be re-examined and altered with regard to changes at an extremely low temperature.

For instance, a remarkable difference was noted in the radiation of heat. Material considered a non-conductor of heat does not appear to affect much the passage of heat into a body cooled down to below −100°. Or, to state the fact according to Prof. Pictet's view: "The slow oscillations of matter, which constitute the lowest degrees of heat, pass more readily through the obstruction of a so-called non-conductor than those corresponding to a higher temperature, just as the less intense undulations of the red light are better able to penetrate clouds of dust or vapour than those of the blue." If the natural rise of temperature in the refrigerator, starting from −135°, is noted in a tracing, and afterwards the same refrigerator carefully packed in a covering of cotton-wool of more than half a yard in thickness, and cooled down afresh, and the rise of temperature again marked, on comparing the tracings hardly any difference will be found in the two curves up to −100°, and only a very slight deviation even up to −50°. On this ground it is clear that the utmost limit of cold that can possibly be attained is not much lower than that reached in the famous experiment of liquefaction of hydrogen. The quantity of warmth which hourly floods a cylinder 1250 mm. high by 210 mm. wide (the size of the refrigerator) at −80°, is no less than 600 calories, and no packing will keep it out. At a lower temperature, the radiation being even greater, the power of the machinery intended to draw off still more heat would have to be enormous. And as −273° is absolute zero, the utmost Prof. Pictet judges to be attainable is about −255°.

As an example of the surprising methods which the refrigerating machine permits the investigator to employ, it may be mentioned that, in order to measure the elasticity of mercury, Prof. Paalzow had the metal cast into the shape of a tuning-fork, and frozen hard enough for the purpose in view. On this occasion it appeared that quicksilver can be shown in a crystallized state, the crystals being of a beautiful fern-like appearance.

Glycerine was likewise made to crystallize; and cognac, after having been frozen, was found to possess that peculiar mellowness commonly only attained by long keeping.

But the most important application of the refrigerating machinery has been the purification of chloroform, undertaken by Prof. Pictet at the instance of Prof. Liebreich, of the Pharmacological Institute, Berlin. Chloroform has hitherto been considered a most unstable and easily defiled substance. The action of sunlight, the slight impurities retained from the different processes of manufacture, perhaps the mere settling down during protracted storage, have invariably resulted in a more or less marked decomposition. By the simple process of crystallization this unstableness is got rid of, and a practically unchangeable liquid is produced. The crystals begin to form at −68°, first covering the bottom of the vessel, and gradually filling it up to within one-fifth of the whole volume. This residue being drained off, the frozen part is allowed to melt under cover, so as to exclude the atmospheric moisture. Chloroform thus refined has, by way of testing its durability, remained exposed on the roof in a light brown bottle from November till June without the slightest sign of decomposition.

Prof. Pictet has already taken steps to introduce his process into manufacture, and proposes to apply the principle to various other chemical and technical objects. Sulphurous ether, for instance, has by a similar process been produced in a hitherto unknown degree of purity. At the same time, the Professor continues eagerly to pursue the various purely scientific inquiries with which he started.

R. DU BOIS-REYMOND.

RESULTS OF EXPERIMENTS AT ROTHAMSTED ON THE QUESTION OF THE FIXATION OF FREE NITROGEN.[1]

FROM the results of the experiments of Boussingault, and also of those made at Rothamsted under conditions of sterilization and inclosure more than thirty years ago, Sir J. B. Lawes and the author had always concluded that at any rate our agricultural plants did not assimilate free nitrogen. They had also abundant evidence that the Papilionaceæ, as well as other plants, derived much nitrogen from the combined nitrogen in the soil and sub-soil. Still, they had long recognized that the source of the whole of the nitrogen of the Papilionaceæ was not explained; that there was, in fact, "*a missing link*"! They were, therefore, prepared to recognize the importance of the results first announced by Prof. Hellriegel in 1886; and they had hoped to commence experiments on the subject in 1887, but they had not been able to do so until 1888. Those first results showed a considerable formation of nodules on the roots, and coincidently great gain of nitrogen, in plants grown in sand (with the plant-ash) when it was microbe-seeded by a turbid watery extract of a rich soil.

In 1889 and since, they had made a more extended series. The plants were grown in pots in a glass-house. There were four pots of each description of plant, one with sterilized sand and the plant-ash, two with the same sand and ash, but microbe-seeded with watery extract, for some plants from a rich garden soil, for lupins from a sandy soil in which lupins were growing luxuriantly, and

[1] Abstract of a paper read before the Agricultural Chemistry Section of the Naturforscher Versammlung at Halle a.S., by Dr. J. H. Gilbert, F.R.S., September 24, 1891.