each other. They moreover make equal angles of 22½° (or a sixteenth of a circle) with the cross centre line of Stonehenge.

This symmetry is very striking, and is so complete

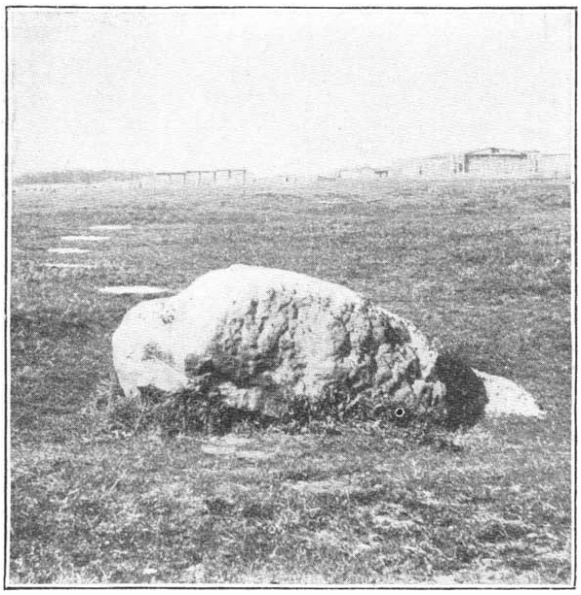

FIG. 2.—Stone No. 91. To south-east.

that it cannot be accounted for as a mere coincidence. It obviously points to the conclusion that the Four Stations, Nos. 91 to 94, were all specially located in relation to one another as parts of one scheme to serve some definite purpose in the general design of Stonehenge.

FIG. 3.—Stone No. 93. To north-west.

On this Flinders Petrie remarks :—

"On examining the stones and mounds 91 to 94 on the earth bank it will be seen that they are exactly opposite, stone to stone, and mound to mound. This strongly shows that they are contemporaneous ; as is also shown by the fact that the diameters joining their centres cross each other at . . . just half a right angle ; and further the diameters are complementary to each other, being symmetrical about the axis of the structure" ("Stonehenge," p. 21).

We cannot doubt that at one time there was a stone in each of the positions now indicated by the two mounds ; and that, whatever the purpose of the arrangement may have been, it had nothing to do with the neighbouring Round Barrows.

The two stones (Nos. 91 and 93), now in place, are shown in the accompanying photographs (Figs. 2 and 3). It will be observed that the ground is level around the base of each stone.

The two mounds (Nos. 92 and 94) are of very slight elevation, and are scarcely noticeable on the ground. Assuming that each of these two sites had at one time been occupied by a stone, we may suppose that the small amount of earth forming the present mound was thrown out of the excavation made when the stone was removed. When, later on, the cremated interment was buried, the incipient mound was perhaps trimmed up and added to.

It may be considered certain that the Four Stations were in no way connected with the "Aubrey Holes," and that they belong to a different period of Stonehenge history.

E. HERBERT STONE.

The Retreat, Devizes.

Improvement of Visibility of Distant Objects.

IN connection with the subject of some recent letters in NATURE on "A Method of Improving Visibility of Distant Objects" by Prof. C. V. Raman (October 20, 1921) and by Mr. A. G. Lowndes and Sir David Wilson-Barker (November 10, 1921), it may be of interest to mention that two years ago I published a complete essay on the same question in the French Bulletin Officiel de la Direction des Recherches Scientifiques et industrielles du Ministère de l'Instruction Publique, No. 4, February, 1920, pp. 229-48, under the title "Sur l'utilité de la lumière polarisée dans les observations faites en mer ou au bord de la mer, et sur une jumelle à polariseurs."

Every advantage of polarised light mentioned by your correspondents, such as improvement of optical contrasts, visibility of colours in distant objects, etc., was considered and discussed in detail in that paper. I must mention that I took the research in hand in 1916 for military purposes, in connection with the French Ministry of Invention and Research and with the French Admiralty. The results and my former reports were communicated to the English Board of Invention and Research (1917). A little later, Prof. W. F. Durand, of Leland Stanford University, then the Scientific Attaché to the American Embassy in Paris, having been kind enough to order the translation of my reports into English, that English version was given likewise to the official agent of the British Ministry of Munitions Optical Department, Mr. F. C. Dannatt, now representative of the British Scientific Apparatus Manufacturers, Ltd., in Paris. At the end of the war the French Navy had a small number of binoculars equipped with spar polarisers of the Glazebrook-Ahrens type cemented with a special oil, as described in my paper. The constructor, M. A. Jobin, member of the French Bureau des Longitudes, supplied a few of those binoculars, at the request of the British Government, through Mr. Dannatt.

I do not wish to take up the space available in NATURE with a translation of my paper, but a short summary of a few of my conclusions may be of interest.

Contrary to Mr. Lowndes's opinion, I objected very

strongly to the tourmaline plates (except for some special purposes dealing with naval artillery) on account of the high colouring they give, either green or pink, which, unfortunately, results in an inaccurate rendering of the natural tints of objects and makes ineffective one of the most striking advantages of polarised light, the wonderful and delightful disclosure of true colours in far-distant objects.

Although the choice and careful making of the Bénard-Jobin spar prisms cemented by a special poppy-oil prepared by M. Duffieux (then my assistant) to equip the Jules Huet prism-binoculars of the French Navy gave unqualified satisfaction, I must confess—and I did so in the introduction of my paper—that I feel very much inclined to consider a very thin plate of *herapathite* (sulphatoperiodide of quinine) of a few square millimetres, having its crystallographic directions quite uniform in the whole area covering the ocular-ring, as the best of all polarising equipments, I cannot say it is the most practical one, because beautiful transparent and uniform herapathite plates are not easily obtained. However, I sincerely hope that my conclusion will please W. B. Herapath's fellow-countrymen, particularly as quinine salts are more easily obtained than Iceland spar.

Nevertheless, I mentioned at the end of p. 230 of my paper the old use of tourmaline spectacles to discover the fishes in deep water, and so on. *Nihil sub sole novum.* The optical constructor to whose talents I referred in my paper, without mentioning his name, as having, when he was a young man in the 'eighties, played a hoax on his fellow-anglers along the River Marne with tourmaline spectacles forty years ago, is now living in Grenoble. His name is M. Ivan Werlein, formerly well known and appreciated for his skilfulness by French physicists and crystallographers when he was working in Paris.

HENRI BÉNARD.

University of Bordeaux, February 8.

Statistical Studies of Evolution.

SINCE Dr. Willis and Mr. Udny Yule in their reply to my letter in NATURE (March 2) have asked me to explain the case of the New Zealand flora, I feel that I should attempt to do so.

The time taken for almost all animals and probably many plants to spread to the boundaries of a continuous area of habitable environment is short compared with geological time: witness the progress of *Elodea* in this country since its introduction only some sixty years ago. Surely, therefore, the majority of species at any particular time have already reached the boundaries of that area of habitable environment to which they are isolated (*e.g.* the Marsupials of the isolated Australian region).

Now the Indo-Malayan flora of New Zealand has arrived recently, geologically speaking, and has not yet reached a state of equilibrium; it is still spreading, unlike the majority of species. As Dr. Willis and Mr. Udny Yule showed clearly in their original article, the distribution of a fauna or flora that is still spreading will conform to the "Size and Area" curve. I believe that not only a spreading fauna or flora but also one which has reached the boundaries of its habitable environment will conform to the "Size and Area" curve.

The oldest endemic families of New Zealand must have reached this state of equilibrium and, on my theory, should conform to the "Size and Area" curve. Perhaps Dr. Willis could tell me if they do so in this or in a parallel case.

C. A. F. PANTIN.

Christ's College, Cambridge, March 13.

MR. PANTIN has not replied to our query as to why neither the northern nor the southern group of plants in New Zealand shows any increase of local species when it reaches the region where the other group shows its maximum of such forms. Why is one group represented by its most widely ranging endemics at the place where the other shows chiefly its endemics of least range?

If the Indo-Malayan invasion is so young in New Zealand, why do its members, though mostly trees, show a rather greater average range than those of the herbaceous southern invasion of plants of northern-hemisphere type? Though it is a long time since Britain was cut off from the Continent, why have 227 of its 1548 species not yet reached a distribution of more than 5 vice-counties out of 112, and why have only another 229 reached one exceeding 100?

All observation goes to show that dispersal of introductions is rarely rapid, unless, as in Ceylon or New Zealand, St. Helena or North America, man has completely altered the conditions, and destroyed or interfered with the societies that already existed. A few cases like *Elodea*, chiefly water plants, are known, and it is probable that the plant entered a society that was very incomplete. No other introduction has spread rapidly in England for centuries, though when the Romans came here, and cut down the forest, thus altering the conditions, many introductions were rapidly dispersed about the country.

To suppose that species have mostly reached their possible limit of dispersal is to return to a position like that taken up by the advocates of special creation, invoking incomprehensibility. Why should *Coleus barbatus* be found through tropical Asia and Africa, including the summit of Ritigala mountain in Ceylon, while *C. elongatus*, differing only in the form of the calyx and inflorescence, and a few minor points, is confined to that summit? Why should a species of the New Zealand flora that reaches the outlying islands range much further *in New Zealand* than a species that does not? Why should one that reaches the Chathams range much further than one that reaches the Aucklands or the Kermadecs? Nothing but Age and Area can even suggest an explanation of such facts.

No theory based upon natural selection will enable one to make predictions about distribution, whereas Age and Area has already been used successfully in this way nearly a hundred times, and has increased our knowledge of the subject. If we suppose that dispersal is already completed there is little left to investigate, and to explain the distribution of species about the world (as opposed to purely local dispersal) becomes a task that has been abandoned as hopeless by leading authorities upon distribution. The fact that Age and Area can be used for successful prediction shows that it is probably correct, and it offers an explanation incomparably simpler than does the natural selection theory, and explains with ease facts utterly incomprehensible to the latter, such as that the Auckland Is. contain 45 per cent. of Monocotyledons in their flora, the Chathams 31 per cent., and the Kermadecs only 21 per cent. How can natural selection explain the remarkable maps in *Ann. Bot.* 32, 1918, pp. 343 *seq.*, and the curves on pp. 357, 360? Mr. Pantin's theory seems to us to lend itself neither to explanation nor to prediction. We feel compelled again to emphasise that his supposition as to random combinations of environmental limitations does not appear to us to bear any relation to facts. Nor, if it did accord with facts, can we agree that his conclusions would follow.

J. C. WILLIS.
G. UDNY YULE.