## Geological Climates

PROF. DUNCAN is under the impression that the claim of *Araucaria Cunninghami* to have flourished at Bournemouth during the Eocene, rests on "a bit of a leafy part of a tree," and that this bit is "squashed." The foliage is however abundant there, occurring almost wherever vegetable remains are found, from the east of Bournemouth Pier to half a mile beyond Boscombe. In one place, where a bluff is literally full of it, the disarticulated branchlets are perfect, and not in the least degree compressed. Again, the determination was not made by Prof. Haughton, but rests upon my statement that this foliage and that of *A. Cunninghami* cannot be distinguished one from the other. That it is Araucarian foliage I am perfectly satisfied; but whether the existing Australian species is identical and unmodified, must remain doubtful until other

organs besides foliage are found, it being by no means absolutely certain that because the foliage is identical the species are so. The discussion raised by Prof. Haughton, and continued by Prof. Duncan and Mr. Wallace, seems therefore hardly worth prolonging, since it is based upon an assumption that is only probably correct. But even if the identity were proved, a single species is not satisfactory evidence of former temperature.

I am indebted to Mr. Winslow Jones for the only information that I have yet obtained about the growth of either species in England. He recollects a small tree of *A. excelsa*, growing near the water's edge in a garden on the upper portion of Falmouth Harbour, which he believes died three years ago. He has seen flourishing trees at Naples, Cintra, Malta, and Algiers, but even Northern Italy seems beyond the range of successful cultivation. Of the two *A. Cunninghami* seems the more tender, though possibly its less symmetric growth may have excluded it from many gardens. In Madeira it grows generally best close to the sea and in sheltered places.

Lindley was mistaken in regarding the two species as one. All the needle-leaved (Eutacta) section of Araucaria are certainly closely allied, for the species, however distinct in other respects, possess two kinds of foliage, that of the young plants being identical in all: yet otherwise the species are clearly and distinctly marked off from each other.

With further regard to the identification of the Bournemouth foliage with Araucaria, I find that Massalongo[1] gives an excellent photograph of the same foliage from Chiavon, in North Italy, and of an immature cone consisting of 250 scales. Although existing Sequoias have cones with from 16 to 20 scales, Schimper says: "Il est sans aucun doute un *Sequoia* et peut-être identique au *S. Sternbergii*. Les cônes ont la plus grande ressemblance avec ceux du *S. gigantea*" (Pal. Végétale," vol. iii. p. 573). I am beginning to lose all faith in the so-called science of palæo-botany as worked out by our Teutonic brethren. Not only is the above quotation an absurdity, for which Heer is responsible, but I fail to see any good evidence to support the change made by Heer from *Araucaria*[2] *Sternbergii* to *Sequoia Sternbergii*. The foliage is more Araucaria-like than Sequoia-like, and has been found associated with an Araucaria cone, but never with any Sequoia cones. It has nothing to do with the Icelandic foliage, neither with the Upper Miocene foliage from Turin, nor that from Bilin nor Oeningen. The true *Araucaria Sternbergii* characterises a well-marked horizon, that of the Newer Eocene or Oligocene in Central Europe, and has been found at Barton in Hampshire; it differs from the Middle Eocene form (*A. venetus*, Mass.) of England and Italy in the needle-like leaves hugging more closely to the branchlet, as the latter differs in its turn from the Araucaria of the Grès du Soissonnais, which has needles very widely opened out. This progressive change may have taken place *pari passu* with the changing climate. At Sheppey, where foliage is plentiful, I have met with a beautifully-preserved axis of an Araucaria cone with the basal scales attached, exactly as we find them in the existing species.

Now with regard to Mr. Wallace's letter, I pointed out in NATURE, vol. xix. p. 126, that the Tertiary fossil plants, even of the Eocene, require at most an increase in temperature of 20°, and that the land connection between Europe, Greenland, and America, which there is reason to suppose existed then, would, by shutting out Arctic currents, have produced more than the required increment. If this theory appeared for the first time in my article, however clumsily I may have worded it, and if it has been of use to Mr. Wallace, it is only fair that the fact should be acknowledged, while if it has escaped his notice he will perhaps pardon my now drawing his attention to it. At the same time the publication of the Tertiary flora of North-East Siberia, which I had not then seen, and of Saghalien, has modified the views I put forward in a manner which I trust I may shortly find time to explain.

J. STARKIE GARDNER