type connected with the Devonian movements, and gives no indication of entering the Carboniferous. Microscopic examination of contact-altered rock suggests that the cleavage is later than the recrystallization. Thus the granite is older than the chief movements (Devonian). It is associated with an anticline demonstrably pre-Bala, running oblique to the Devonian folding, and is therefore probably itself pre-Bala and of the same age as the surrounding intrusions which belong to the Borrowdale suite.

The following lecture was delivered:—

"Diagrams illustrating the Significance of Rock Analyses." By John William Evans, D.Sc., LL.B., F.G.S.

The diagrams are of two kinds: (1) Individual rock diagrams intended to indicate at a glance the significance of the analyses of a rock or complex mineral silicate; (2) Linear rock diagrams. The different types of linear or variation diagrams, in which the chemical constituents of different rocks are represented by vertical distances, were reviewed and a new type proposed in which each rock is represented by two diagrams: (*a*) alumina diagram, (*b*) silica diagram.

In describing these diagrams, the lecturer discussed various problems connected with the genesis and composition of igneous rocks.

2. *July* 5, 1918.—J. F. N. Green, B.A., F.G.S., President, in the Chair.

The following lecture was delivered:—

"A Visit to Christmas Island and the Cocos-Keeling Islands." By C. W. Andrews, D.Sc., F.R.S., F.G.S.

The structure and physical geography of Christmas Island (Indian Ocean), a raised coral island, was described and compared with those of the Cocos-Keeling Islands, a typical atoll. Some account of the fauna and flora of Christmas Island was given, with special reference to the means of colonization of oceanic islands.

The lecture was illustrated by lantern slides.

CORRESPONDENCE.

THE PRE-THANETIAN EROSION OF THE CHALK.

Sir,—I should like to express to Mr. C. N. Bromehead my grateful thanks for his criticism of my paper on the Pre-Thanetian Erosion of the Chalk of the London Basin. I am afraid I must plead guilty to the charge of not having made use of all the evidence available, in ignorance as I was of the appearance of the Geological Survey Memoir on *The Geology of Windsor and Chertsey*. This memoir has, I suppose, been published since the outbreak of war and I have been continuously on active service, first flying in France and later serving afloat, since 1915. In any geological work with which I endeavour to beguile the tedium of life afloat I am greatly handicapped by being unable to make references to authorities, or even to consult earlier work of my own. The paper under present discussion was simply the outcome of an attempt on my part to apply cartographic methods to the data in my possession bearing upon the

question at issue. The results seemed to me interesting and worthy of print. Yet while being the first to admit the scantiness of my data, I cannot bring myself to agree with a policy which refrains from any kind of cartographical expression until ample data have accumulated. Comparative treatment and correlation of items of information in ways such as I have adopted, as well as being the strictly scientific method of procedure, is that which puts each item to its maximum of utility. I am convinced that field-geologists would often save themselves much haphazard wandering if they made greater use of cartographical methods beforehand. It is true that very limited data produce very generalized results, but the method works out its own salvation in the long run, and the results then achieved are unattainable in any other way. It is very faint praise to say that "when the amount of evidence available is larger, the method may be of some value". Had I been in the more fortunate position of Mr. Bromehead, I should long ago have given cartographical expression to my data. The result would have been a map full of imperfections, and doubtless in places at variance with field observations. Yet the steady incorporation of each fresh item of information would bring that map nearer and nearer to perfection and more and more in agreement with field observations.

Much as I should like to, I am unable, at the moment of writing, to discuss in any detail with Mr. Bromehead the other points which he raises. He remarks that "it seems natural to ascertain as far as possible the zone of the Chalk immediately underlying the Tertiary at the boundary of the latter, and to check the zones whose presence beneath the Tertiary is deduced from borings by these facts". I take it, then, that if Mr. Bromehead observed the *Marsupites* zone (say) immediately underlying the Tertiary at the boundary of the latter, he would naturally expect to find the same zone beneath the Tertiary cover. This seems to me a wilful ignoring of the teachings of tectonics, and since we already know that a strong unconformity exists between the Tertiaries and the Chalk, a most unsound view to take.

With regard to the "series of gentle folds whose axes run about E. 15° S.", referred to by Mr. Bromehead, I spent considerable time in 1913 studying these as well as evidences of disturbances of quite different relationship. A fact which made a profound impression upon me was that whereas in the Isle of Wight the Upper Chalk is 1,200 feet thick, in Dorset 1,000 feet, in Berks 800 feet, and in Norfolk 1,000 feet, yet in the London Basin, beneath the Tertiary cover, it is often less (and in places considerably less) than 300 feet. In view of this and many other facts, I concluded that while the evidence of a system of approximately east and west folds, including those referred to by Mr. Bromehead, was indisputable, yet this folding did not take the place of premier importance in determining the character of the denudation undergone by successive members of the Mesozoics, including the Chalk. The interesting items of field observation cited by Mr. Bromehead support his tentative suggestion of a set of approximately east and west folds in the Beaconsfield–Winkfield area, and it should be borne in mind that these

undulations must affect the level of the base of the Chalk, and in constructing my map showing levels in the base of the Chalk I was without adequate data concerning these anomalous levels in this particular area. With the incorporation of a sufficiency of data in the maps the final result would agree with field observations—and while the data accumulate a faulty map is better than no map.

H. A. BAKER.

AFLOAT,
H.M.S. "CARYSFORT".
July 25, 1918.

NOTES AND QUERIES FROM NEW ZEALAND.

SIR,—I am forwarding two photographs in which perchance some of your readers may be interested. One represents the common rhombohedral multiple twin of calcite, seen on a weathered surface of the mineral, and the other an unknown fossil. [We omit the description of the photographs, which is appended to the figures given below.—ED. GEOL. MAG.]

I have discarded all thought of inorganic origin for the "fossil" on account of the great regularity and the successive layers shown, but can offer no convincing suggestion as to the actual nature, and shall be grateful for any information thereon.

JOHN A. BARTRUM (Lecturer in Geology).

UNIVERSITY COLLEGE,
AUCKLAND, NEW ZEALAND.
May 6, 1918.

Having referred Mr. Bartrum's photographs to our colleagues Dr. G. F. H. Smith and Dr. F. A. Bather, of the British Museum (Natural History), Cromwell Road, we have been favoured with the following remarks thereon.—ED. GEOL. MAG.

FIG. 1: CALCITE CLEAVAGE.—This shows clearly the crossing lamellæ seen on a weathered surface of a specimen of twinned calcite from Port Waikato, New Zealand. The lamellæ are usually rendered conspicuous owing to readier solution along the composition planes, but occasionally it appears as if there has been differential solution of the opposed sets of twin-lamellæ.—G. F. H. S.