Abace, Ltd., in introducing two new crops (cocoa and oil palms). The report also stresses the way in which opportunities are taken for partnership with private industry and association with local interests, and apart from stressing the importance of public relations and encouraging both at home and overseas a correct understanding of the Corporation's purpose and work, the report contains two interesting sections discussing various aspects of management and training schemes which merit separate consideration.

ECHO-SOUNDING EXPERIMENTS IN THE BARENTS SEA

NEARLY all large fishing vessels carry echo-sounders for locating fish. A 'ping' of sound is sent down into the water, and any fish between the ship and the sea bed give echoes which can be detected. In the case of vessels trawling for cod, it is fish within a few fathoms of the sea bed which are of interest, and because of the short separation in time between the echoes from these and the much stronger echo from the sea bed, certain difficulties arise. These have to a large extent been overcome by displaying the echoes on a cathode-ray oscilloscope, arranged to present echoes from near the sea bed on an expanded scale.

A recent booklet contains five papers concerned with the use of such equipment*. They cover the identification and measurement of the echoes, the relationship of the number and size of echoes to the catch of fish, the measurement of the strength of echoes from individual fish, practical and theoretical work on the properties of the equipment, and its use for surveying the distribution of fish on the fishing grounds. Taken together, they form a comprehensive survey of the characteristics and use of the equipment considered primarily from a practical point of view.

One is particularly struck by the high correlation between the count of echo signals, that is, the sum of the strengths of each echo during a trawling run, corrected for depth, and the number of fish caught. The correlation coefficients are in the region of 0·9, in spite of the necessarily rather crude methods of echo measurement.

In the past, much of the information required for systematic design of echo-sounders for fish detection has not been available. Design has been based largely on the properties of previous models known to give reasonably satisfactory results, followed by sea trials and alterations as required. The third paper describes an attempt to obtain some of the necessary basic data. The echo strength given by (dead) cod of different lengths has been measured and compared with the echo given by a standard target, so that the target strength of the cod is known in absolute units. The water-borne noise has been measured as a function of ship-speed and water-depth (though unfortunately only in terms of received voltage without the sensitivity of the receiver being known). The information about the target strength of cod is of universal application, and that about the water-borne noise allows the prediction of the performance of this particular sounder over a wide range of conditions.

The checking of the performance of equipment under working conditions is highly desirable, and helps to give one confidence in both the equipment and one's methods. The first part of the fourth paper describes checks of the beam pattern and of the variation of echo-strength with depth. Such checks have been made on many other acoustic devices, and since in the present case they also agree with theoretical expectations within experimental accuracy, the fifteen pages devoted to this work seem excessive.

In conclusion, these papers describe a great deal of careful work, mostly 'in the field', and form a very valuable contribution to the subject.

M. J. TUCKER

* Ministry of Agriculture, Fisheries and Food. Fishery Investigations, Series II, **22**, No. 9 : Echo Sounding Experiments in the Barents Sea. By I. D. Richardson, D. H. Cushing, F. R. Harden Jones, R. J. H. Beverton, and R. W. Blacker. Pp. vi+57. (London : H.M. Stationery Office, 1959.) 20*s*. net.

ORIGIN OF TEKTITES

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LAST January, Dr. G. Baker[1] criticized in considerable detail my suggestion[2] that comet heads colliding with the Earth heated the surface to the melting point and spread small fragments of glassy objects to great distances and that these objects are tektites. I believe these objects have compositions which are not greatly different from some of the more acid terrestrial rocks, but that no source of sufficiently high temperatures to melt these materials is known to be present on the Earth's surface or somewhat below the surface. My communication was written to suggest a possible source of high temperatures. I really thought everyone who had worked on this problem would be pleased to consider a mechanism by which terrestrial material could be melted and scattered to great distances even though it did not necessarily answer all questions. This was certainly not true. I have thought over Dr. Baker's very thoughtful criticisms and would like to make a brief reply.

Dr. Baker is quite right about tektites not being spaced some 50 million years apart in age, but Prof. Z. Kopal very soon pointed out to me that cometary collisions with the Earth were probably ten or more times as frequent as I had assumed. I have not troubled to correct this incorrect estimate, for it was very uncertain in any event. We probably have only the most approximate ideas in regard to the numbers of non-luminous comet heads moving in the solar system ; and comet heads must certainly collide

with the Earth from time to time and should produce some effects which we could look for, and they should produce an effect of sufficient magnitude possibly to melt bits of glassy materials.

Also, Dr. Baker is right about the orientation of tektites during flight through gas. The stable position is motion through the gas with a maximum surface exposed in the direction of flight.

Dr. Baker accepts the conclusion that a swarm of objects of very considerable mass is required if such a swarm is to remain as an entity in the field of the Sun, and he tries to account for a large loss of this mass by abrasion during flight through the air, erosion after arrival at the Earth, and destruction by birds and men. If we postulate very many small masses, say, of millimetre sizes, then it is difficult to answer the first objection. But 100 gm. per cm.^{2} over an area of 5 million km.^{2} is 5×10^{12} tons, and hence we must assume a very large fraction of very fine material in order to account for its destruction in this way. The total energy of this mass of material, using his area of 5×10^7 km.^{2} (2×10^7 square miles) and a thickness of 100 gm./cm.^{2} and a velocity of 20 km. per sec., is 10^{31} ergs. This is equivalent to about 10^{10} ordinary Second World War atom bombs, or 2,000 per km.^{2}. The energy per cm.^{2} is 2×10^{14} ergs. The heat capacity of the atmosphere is about 10^{10} ergs per cm.^{2} and thus very high temperatures would be produced. Possibly such energetic processes occurred over southern Australia some 5,000 years ago, but also some evidence of it might remain. Possibly my scepticism based on inadequate knowledge is not unjustified.

The etching experiments with 4 per cent hydrogen fluoride do not seem to be very appropriate as a test of chemical erosion, since this reagent is used to dissolve rocks of all kinds and is known to be a very effective chemical for such purposes, since it converts silicon dioxide to gaseous silicon fluoride. Carbon dioxide and water is the usual erosion acid in Nature and it is much milder in its effects. But tektites have persisted on the Earth since the Eocene apparently, and during this long period should have been exposed to all the types of erosion that occur in Nature and they still have survived. Also tektites are abundant in the Philippines and other tropical humid regions and are fairly well preserved. The erosion of a layer of tektites of 100 gm./cm.^{2} from the southern Australian continent in 5,000 years does not seem probable to me, especially in view of the fact that, as Dr. Baker emphasizes, some of these objects are beautifully preserved. In fact, most of those that I have seen are so preserved. Could this vast amount of material of a very resistant kind have disappeared so completely in a short period of time ? It seems improbable to me that both this great and complete destruction of so much material and the perfect preservation of specific samples under essentially similar conditions could have occurred.

Again, birds and men cannot dispose of such quantities of material, or even a small fraction of it, in so short a time, and it would seem likely that tektites would have become distributed over the whole of Australia instead of being limited to the southern part if such agents moved any important amounts of these materials.

But my mention of the swarm of objects and its elimination was very brief. If we are seriously to assume the existence of such a massive swarm, we must consider whether it would not have accumulated due to its own gravitational forces into a more compact object and hence have arrived at the Earth as such. In this case it approximates to the cometary model, or to a large meteorite model, and then we are back to scattering materials over vast areas. However, with these models it is unnecessary to assume a layer of 100 gm./cm.$^{-2}$, since this estimate depended on the extended cloud model and its stability in the Sun's field ; also the energy dissipated, while very great, would be more limited in area and might well initiate volcanic or flooding processes, for example, which would obscure the location of the collision site.

But we are learning more about these objects. It is now found[3] that they do not contain aluminium-26, which was previously reported. There is no evidence that they have been exposed to cosmic rays. If they came from space in a swarm, the time in space was very brief. They may have come as a large compact object or they may have come from the Earth. Also, the potassium-40/argon-40 ages have been determined[4], and it is found that these ages for all tektites of south-east Asia and Australia are the same within the limits of error, namely, 610,000 $\pm$ $\sim$ 50,000 years. The ages from geological studies that Dr. Baker mentions might be in agreement with this age for all these objects with the exception of the age of the australites, which he estimates as 5,000 years or "definitely Holocene". The potassium-40/argon-40 ages are calculated assuming that complete degassing took place at some time in the past. The consistency of the ages supports this, though one could wish that the variations, either real or due to observational errors, were less. If the age of 5,000 years given by Dr. Baker and that of 610,000 years given by the potassium-40/argon-40 ratio for the australites are correct, a degassing occurred 610,000 years ago, but delivery to Australia occurred much later. One wonders how and where that heating process occurred and where the objects were kept in the refrigerator free from cosmic rays since that time ? I really am most reluctant to assume that the carefully considered age of the australites is wrong, but if they arrived 610,000 years ago it would greatly simplify our attempts to arrive at a satisfactory model. If the differences in geological age estimates and the potassium-40/argon-40 ages for all these groups of tektites are real, then similar questions in regard to the heating and cooling histories can be raised in regard to all of them.

If the agreement in potassium-40/argon-40 ages is significant—and I believe it is—a very great and extensive process must have produced these objects and we must all think again about the entire problem.

Did they come from the Moon ? Could a collision of an object with the Moon splash them off in such a closely related pattern that they would all fall in these close patterns[5] ? But would all such collisions produce such closely controlled conditions, or would they miss the Earth occasionally or, in fact, usually, and thus on a second, third or the nth pass fall all over the Earth ? My answer is that they most probably would be very widely, and in fact uniformly, scattered over the Earth[6].

However, it has always been a puzzle to me that terrestrial surface materials containing appreciable water should melt into objects having so little water. Why did they not melt into pumice ? But I have only tried to suggest a source of heat for melting this material and scattering it over great distances. It seems more reasonable to try to fashion some hypothetical process to produce them from known terres-

trial materials, which they resemble in many ways, than to assume that they come from other places that we know so little about. Must we fashion another planet to produce their chemical composition, and then if we cannot find ways to melt these materials on Earth, why do we think they could be melted elsewhere ? If geologists will find a volcanic process which will produce these objects from appropriate terrestrial materials, such a solution would be the most satisfactory of any that I could imagine. (Mr. Paul Lowman mentioned this possibility to me in conversation.) But a volcanic process distributing material in limited regions over south-east Asia and Australia within a circle of 8,000 km. diameter would be most remarkable.

On the other hand, cometary collisions should occur occasionally, they should be very energetic, and their volatile constituents should supply a mechanism for propelling materials out of the atmosphere and to great distances, and the agreement in the potassium-40/argon-40 ages would be understandable. It could have been a single collision near the centre of this area, instead of multiple but nearly simultaneous ones as suggested previously.

It is nearly three years since my suggestion on the cometary origin of tektites appeared in *Nature*. I wish to thank Dr. Baker for raising the problem for public discussion and am only sorry that he did not do so earlier. Progress in resolving differences of this kind can be made most expeditiously by a public discussion of this kind in which all interested parties, including the one whose work is criticized, may know what doubts are being entertained.

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ISOTOPIC COMPOSITION AND TEMPERATURE OF FORMATION OF ANTARCTIC SNOWS

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In an earlier communication[1], variations in the isotopic composition of the oxygen in falls of snow collected during 1958 at the Belgian Antarctic Base (King Baudouin Base, 70° 26′ S., 24° 19′ E.) were shown.

We intend, here, to present the variations in the isotopic composition of the hydrogen contained in the same samples.

The analysis of the aerological radio soundings carried out at the King Baudouin Base in the course of the International Geophysical Year[2] has enabled us to estimate the temperature range in the cloud corresponding to each snow-fall. This is, to the best of our knowledge, the first attempt carried out with the view of establishing a direct correlation between the isotopic composition of a snow-fall and its temperature of formation. The interesting results obtained in this field up to the present[4,5] originate from average samples representing accumulations over severa months, or even years.

Variations of the Deuterium : Hydrogen Ratio

The ratio deuterium : hydrogen has been measured at the U.S. Geological Survey, Washington. The mass-spectrometric methods used have already been described[6].

Fig. 1 shows the variations of the deuterium : hydrogen ratio in the course of 1958. Each point represents an individual sample. The composition of the hydrogen is expressed as percentage variation of the deuterium : hydrogen ratio with respect to a conventional standard (standard mean ocean water). The variations of the deuterium : hydrogen ratio present the same characteristics as those of the oxygen-18 : oxygen-16 ratio previously discussed[1]; but their amplitude is 10 times larger[3]. Thus, summer snows are, in the average, 10 per cent richer in deuterium than winter snows. The maximum relative variation in the deuterium content is 20 per cent.

Fig. 2 shows the variations of ∂ (oxygen-18 : oxygen-16 ‰) with respect to Δ (deuterium : hydrogen per cent). As a first approximation, all the points are grouped about a straight line of equation : $\partial = 0{\cdot}8\,\Delta + 1{\cdot}8$. It appears that some points corresponding to August and September, that is, the end of winter, form a separate group.

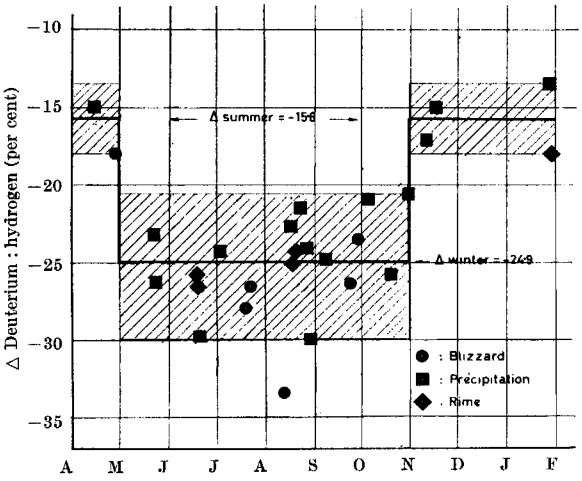

Fig. 1. Variations of deuterium : hydrogen ratio in snow samples during the course of the year (1958)