

randomly oriented material. Thus preferred orientation is sought for, not avoided.

Mr. Astbury described the treatment that has been found to work in practice. He emphasized, however, that other considerations are important; traces of carbon or sulphur can more than cancel out the gains made by the processing.

Several methods are in use for examining the texture; X-ray methods are not necessarily the most useful, since the large grain-size introduces difficulties in obtaining ordinary polycrystalline photographs, and in some cases other methods such as measurement of magnetic or strain anisotropy, or optical reflexion from etch pits, are more informative. X-rays still provide a basis for interpretation of the other results.

Each session of the conference was followed by discussion in which many of the points made by the speakers were enlarged upon. These discussions brought out very well the importance that metallurgists attach to X-ray methods and the interest the X-ray crystallographers have in the metallic state.

H. LIPSON

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GEOLOGY OF THE SALT RANGE OF THE PUNJAB

IN the issue of *Nature* of March 23, 1946, p. 359, I gave a résumé of a "Symposium on the Age of the Saline Series of the Punjab", held at Poona in 1944 under the auspices of the National Academy of Sciences of India (published 1945¹). At the time I wrote, a second symposium had already been held on the same subject at Udaipur in 1945 (published 1947²). Page references in the sequel refer to this second symposium.

The point at issue, it will be remembered, is whether the Punjab Saline Series, which commonly occurs beneath fossiliferous Cambrian at the foot of the Salt Range escarpment, is of Cambrian or Tertiary date. The interval between the appearance of the reports of the two symposia has been well employed by the main protagonists, E. R. Gee and B. Sahni. Gee has led two additional excursions to exposures which he considers leave no doubt of the 'natural' superposition on the Saline Series of, in some places, Cambrian Purple Sandstone, and, in others, unconformable Carboniferous Talchir Boulder Bed. His companions have been, at one locality or another, J. B. Auden, J. Coates, B. S. Downward, W. D. Gill, B. S. Lamba, G. M. Lees, E. S. Pinfold, B. Sahni and M. R. Sahni (pp. iv, 95; Gee, Coates, Lamba, Pinfold and B. Sahni have published in the second symposium, though Coates only in the general discussion, p. 240).

Sahni, as well as accompanying Gee on the two excursions, has had much to do with mobilizing further palaeontological evidence, now published by himself, M. S. Mani, J. Hsü, R. N. Lakhanpal, B. S. Trivedi and R. V. Sitholey (see list of contributors). Furthermore, Sahni has taken advantage of a visit to Britain to give a number of demonstrations (p. xxxix). I shall always treasure the memory of how perfectly Sahni and Pinfold, out-and-out opponents, co-operated on one of these occasions at the British Museum (Natural History) to make certain that those who attended should understand all sides of the question.

Eleven other authors, not mentioned above, have sent in contributions to the second symposium, though for the most part they had not undertaken recent special investigations; and two more, similarly situated, are recorded among those who joined in the general discussion. In what follows I make no detailed attempt to attribute particular ideas to particular authors; but for brevity treat Sahni as representing the Tertiary correlation, and Gee the Cambrian. I also assume that readers can turn back to my previous digest. I confess that I have not reached a firm conclusion; but I do incline to the view that Sahni is correct. I say this in the full knowledge that most of my brother field-geologists will disagree with me.

Let it be granted that so many specimens of the Saline Series have been sampled that we know there is throughout its components (salt, saline marl, dolomite, oil-shale) a quasi-universal distribution of organic debris in the form of almost microscopic dust.

This proposition, advanced by Sahni, does not seem to have been challenged by anyone. It has been argued that the dust may have been introduced into the host-rocks long after the formation of the latter, and may have no more significance for dating these rocks than water or oil commonly found in sandstones. I confess that we have here a conception, so wonderful, regarding subterranean travel of dust, that I shall feel more than compensated for my incredulity if such migration be eventually proved. My incredulity is for the present strengthened by learning that Gee seems as unwilling as myself to invoke migration to account for the contents of certain oil-shales that he accepts as an integral part of the Saline Series (p. 96). "It is agreed," he says, "that the kerogen and free oil is indigenous in the shales, and that it has been derived from either vegetable or animal remains." But he adds: "No evidence of animal remains within the oil-shales has been met with, though many geologists have made an intensive research. What is the evidence of the plant remains that have so far been recognized in these oil shale strata; can they not possibly represent a Cambrian flora?" Sahni's reply is: "Mr. Gee's statement that 'no evidence of animal remains within the oil shales has been met with' evidently refers only to macrofossils. The microfossils described and figured by Messrs. Trivedi, Mani and Lakhanpal and by myself are on the table, open to inspection, along with the rocks from which they were prepared" (p. 246). Another, though less potent, argument against the subterranean dust-diffusion hypothesis is that, in three localities tested in detail, "while angiosperm and insect remains always occur in the Saline Series rocks (that is, below the suspected thrust-plane) there is no trace of these groups in any of the samples taken from the overlying Palaeozoic beds, respectively the Purple Sandstone, the Lower Gondwana shales within

the Productus Limestone, and the Talchir shales in the Talchir Boulder Bed" (p. 245; also pp. xviii, xl, 92). Of course, Gee, Sahni and everyone else recognize that the saliferous members of the Saline Series are peculiarly liable to subterranean contamination from without. The only difference of opinion is as to the possibility or impossibility of quasi-universal contamination by introduced dust.

Let it be granted that the dust found everywhere in the Saline Series includes so many examples of recognizable debris of conifers, angiosperms and insects as to be on the whole clearly post-Cambrian.

This second proposition I am prepared to take as a working hypothesis from Sahni and his co-workers. I admit that it involves two separate debatable sub-propositions:

(1) Sahni and his co-workers are certain that the dust particles, which they recognize as remains of conifers, angiosperms and insects, are unmistakable. They publish numerous illustrations, some of them photographs. If botanists and zoologists are withholding criticism out of regard for the authors, they are doing a disservice to science. Gee has asked whether the chitinous dust might not have come from trilobites, rather than insects, and has received an apparently satisfactory answer (pp. 233, 235).

(2) Sahni and co-workers are certain that the particular dust particles, which they identify as bits of conifers, angiosperms and insects, have the same wide distribution as the organic dust as a whole. It might otherwise be suggested that such identifiable material is probably in small proportion and reasonably assignable to contamination. This idea seems to be implicit in Gee's question as to whether the organic remains in the oil-shales might not present a special problem (p. 96), and prove free from post-Cambrian types; but the answer given by Sahni and co-workers is clear, for they maintain that remains of conifers, angiosperms and insects do occur in the oil-shales (pp. ix, xvii, xxii, 53, 187, 204, 205, 207, 220).

Let it be granted that Gee has demonstrated, on three occasions to first-class audiences in the field, exposures which seem to prove that the Saline Series is 'naturally' overlain by unquestionably Palaeozoic formations.

Five of Gee's companions have openly adopted his interpretation. Only Sahni himself has said in print that he has seen and disbelieved. The evidence derives in part from lack of manifest disturbance at the contact, and in part from contained fragments in the base of the Talchir Boulder Bed, where this latter happens to be the lowest member of the overlying succession.

Above are stated the three assumptions which for the time being guide my conclusions. I think Sahni is right in interpreting the organic dust in the Saline Series as wind-borne, and contemporaneous. Accordingly I think that the apparent sub-Cambrian stratigraphical position of the Saline Series is misleading. I realize how seldom a major thrust can escape detection in a non-metamorphic, well-exposed succession; but I do know one case in Scotland, that of the Kishorn Thrust, a mile south of Broadford, where Torridon Sandstone is brought on to Durness Limestone in such a manner that any one of us might be deceived if he did not know the answer in advance. Let us hope that further research will soon put the Salt Range problem beyond dispute, one way or the other.

Since this review was written, I have read an extremely important letter by A. K. Ghosh and A.

Bose (*Nature*, December 6, 1947, p. 796), in which the discovery is announced of plant dust of apparent post-Palaeozoic type in the Salt Pseudomorph Beds. These beds intervene between the Talchir Boulder Bed, above, and the fossiliferous Cambrian, below, instead of underlying the latter as does the Saline Series so far discussed. The elaboration of this find may well lead to decisive results. At first sight it seems rather to weaken Sahni's position.

In conclusion, it is right to express admiration for the organisation displayed by the National Academy of Sciences of India. Perhaps one suggestion may be proffered. Readers would greatly appreciate guidance from page headings.

E. B. BAILEY

¹ Symposium on the Age of the Saline Series of the Punjab, *Proc. Nat. Acad. Sci. India*, 14, Pt. 6, i-xxxii, 205-334, 9 plates, 29 text-figs. (1945). Price, including postage, Rs. 4/8 (India); Rs. 5 (foreign).

² Second Symposium, *Proc. Nat. Acad. Sci. India*, 16, Pts. 2-4, i-1, 29-257, 45 plates, 44 text-figs. (1947). Price, including postage, Rs. 12/8 (India); Rs. 15 (foreign).

SIR CHARLES SHERRINGTON AND DIPHTHERIA ANTITOXIN

AT a recent informal gathering of some men of science an important matter of scientific interest was brought up by Dr. A. N. Drury, director of the Lister Institute, recording an incident in which Sir Charles Sherrington had played a part more than half a century ago.

The incident concerned the very first occasion on which diphtheria antitoxin was made in England. Dr. Drury exhibited one of the hooves, suitably mounted, of "Tommy", the first horse to be injected in England with diphtheria toxin with the object of obtaining diphtheria antitoxin for clinical use; and he explained how the Brown Institution came to be associated with the British Institute of Preventive Medicine—as the Lister Institute was then called—which had accommodation for small animals only in the kitchen of the private residence in which it was housed. Dr. Drury described how "Tommy" was stabled at the Brown Institution, and how that, here, Dr. Armand Ruffer, then secretary of the British (now Lister) Institute, and Sir Charles Sherrington, who was at that time professor-superintendent of the Brown Institution, made those first injections and withdrew those samples of blood which yielded the first British-made diphtheria antitoxin. Listeners were also interested to learn that "Tommy's" performance as an antitoxin-producer would rank high even at the present day. Many other attendant circumstances of the incident and the time (August 1894 and the later months of that year) were reported by Dr. Drury, among which may be mentioned the lively interest of Sir Joseph Lister (as he then was); the sending of diphtheria antitoxin from Paris to be tried on patients in England pending the material from the horse "Tommy" and, no doubt, other horses immunized by Dr. Ruffer and Sir Charles Sherrington, becoming available; Pasteur's interest and concern that those attempting its preparation in England should benefit by the experience of those colleagues in France who had succeeded in preparing this entirely new type of medicament; and the clinical experience of English medical men who were privileged to use these early preparations. A letter from Dr. Roux of the Pasteur Institute, and others from practitioners who used the antitoxin, were read and exhibited.