Antiquity

VOL. XLVI No. 184 DECEMBER 1972

Editorial

On Thursday and Friday, 9 and 10 November of this year, the Zoological Society of London, in association with the Anatomical Society of Great Britain and Ireland, held a symposium on 'Grafton Elliot Smith and the concepts of human evolution'. It was organized by Professor Lord Zuckerman, OM, FRS, and three of the four main themes were the first Australopithecine fossils, comparative anatomy and the evolution of the forebrain, and primate systematics and the Tarsius problem. The fourth theme was Elliot Smith: Egypt and Diffusionism, and the speakers the Editor (who was Chairman), and Professors Forde, Fortes, Leach, Piggott and Renfrew.

It is well known to all readers of this journal that Elliot Smith pioneered an extreme view of diffusionism which attempted to prove that almost all sociocultural traits of interest to anthropologists were invented in Egypt, and nowhere else in the world: and spread from Egypt to the rest of the world. He developed, practised and preached the Egyptocentric hyperdiffusionist model of the past. Its adumbration by Elliot Smith in the early years of this century, and its persistent and often violent advocacy by him until his death in 1937, certainly earned him a place in the history of archaeology and anthropology; no one who concerns himself, as we all should, with the rise and fall of the models of the past, can afford to neglect the English hyperdiffusionists, just as they cannot neglect the German hyperdiffusionists, mainly members of the Roman Catholic clergy, who developed the Vienna-based Kulturkreis approach.

In his admirable *The rise of anthropological theory: a history of theories of culture* (New York, 1968)—a book, incidentally, to be read by all archaeologists and kept reverently on the shelf alongside Lowie's *History of ethnological theory*—Professor Marvin Harris of Columbia writes of the English and German hyperdiffusionists: 'Both of these movements were palpably bankrupt by mid-century: they require our attention today only as evidence of the international extent of the tide that was running against nomothetic principles.' This is too sweeping a statement: we still find both scholarly and unscholarly people who hanker after a monogenetic origin for civilization and its spread from one centre to the whole of the world; the success of the *Ra* expeditions, and the brilliant demonstration by Thor Heyerdahl that a reed boat could be sailed from Africa to America, have stimulated a fresh interest in the Elliot Smith/Perry theories.

Elliot Smith's views on the role of the Archaic Civilization of Egypt was first clearly set out in 1911 in his *The ancient Egyptians*. The sub-title of the book was then 'and their influence upon the civilization of Europe'. When the second revised edition came out in 1923, it was called *The ancient Egyptians and the origin of civilization*. The Haddon Library of the Faculty of Archaeology and Anthropology in Cambridge has A. C. Haddon's copy of the second edition. It is annotated in Haddon's writing but the annotations are not his: they are copied by him from the notes made by Flinders Petrie in his copy of this strange book. Petrie's annotations are fascinating: 'No such thing!', 'Nonsense!' 'What a romance!', 'No evidence', 'No, No', 'No evidence whatsoever', 'Here the author is cowrie-shell mad: models of steatopygous women are not copies of cowrie-shells', and so on. Above the preface Petrie had written

'The asserted facts are largely untrue and the vague statements unsupported', and on a sheet of paper inserted in the book he set out his views on some of the points argued by Elliot Smith; we reproduce this list of notes here, reminding readers that the writing is Haddon's, not Petrie's. Here, and elsewhere in his annotation of the book, Petrie is setting out what was, even in the early twenties, the main argument against the Egyptian origin of all things: namely, that everything was not earlier in Egypt, and that Sumerian civilization had a claim to be earlier than Egypt.

1. Agriculture. The food grains are native to Asia, & Egyptian names are Babylonian.
2. Irrigation, not before 1st Dynasty; & probably older in Babylonian home of corn.
3. Working of metals probably originated where metals are found, & not in Egypt.
4. Linen originated where flax is native in the North; it is exotic in Egypt.
5. Carpentry & masonry, no evidence of origin.
6. Ship-building equally practised by other peoples on different designs, see Arab knife-handle.
7. Calendar origin no evidence, certainly independant in Babylonia, see Zodiac.
8. Writing brought by dynastic race into Egypt.

Social and other customs were peculiar to Egypt, and were not copied elsewhere.

The successful crossing of the Atlantic in *Ra II* in 1970 by Thor Heyerdahl in a reed boat built in the shadow of the Pyramids has demonstrated clearly that ancient Egyptians could have crossed the Atlantic in a reed boat. It does not demonstrate that they did so, or, if they did so, that they had any cultural influence on the development of pre-Columbian America. But to some people who confuse the demonstration of a possibility at the present day with the certainty of an historical fact, the *Ra* expeditions appear to have done something to rehabilitate the Elliot Smith theories. The possible trans-Atlantic journeys of the ancient Egyptians must be seen in the context of their historically known achievements in navigation and travel. J. V. Luce has summarized these recently in these words: 'There is no evidence that the ancient Egyptians ever traded further west than Crete ... Crete for the Egyptians lay at the western limit of the world. There is no evidence that the ancient Egyptians ever looked, much less went, any further west. Speculations about the Old Kingdom "explorers" and "colonists" diffusing Egyptian culture far and wide have no basis in fact, and are most implausible, given that the Egyptians never even explored their own river to its upper reaches.' (*The quest for America*, ed. Geoffrey Ashe, London, 1971, pp. 71 and 73.)

A few months before the Elliot Smith symposium there appeared a book called *The Piltdown Men* by Ronald Millar (*London: Gollancz, 1972, 264 pp., 8 pls., 1 fig. £3.20*). It is written for the general public, begins with a survey of the history of archaeology and human palaeontology, recounts the story of the discovery of Piltdown Man and its unmasking as a forgery in 1952, and then proceeds to determine who was the forger, deciding, surprisingly, that it was Elliot Smith. The book is hastily put together and there are many errors. Boucher de Perthes' collections were not housed in a 'hotel', and to describe him as 'either an innocent victim of enthusiasm or a downright charlatan' is to display gross ignorance of the life and thought of this remarkable man. W. J. Sollas was not a leading anatomist from Cambridge: he was Professor of Geology at Oxford. The marks in Bacon's Hole in the Gower Peninsula, claimed as Upper Palaeolithic art by Breuil and Sollas in 1912, have long ago been disproved. Keith's reference, in his lecture to the British Association meeting in Dublin in 1912, to English fossils that supported his view of the antiquity of modern man is not 'inexplicable unless he had heard a whisper of the developments at Piltdown'. There was a Galley Hill man, at that time argued by Keith to be an ancient example of modern man.

The names of many people have been canvassed as the Piltdown forger: Charles Dawson, Smith-Woodward, Teilhard de Chardin, Lewis Abbott, de Vere Cole and his circle of expert hoaxers, an unnamed person on the staff of the British Museum (Natural History); and it is always possible that more than one person was involved in this strange and fascinating affair. Now Millar adds a new suspect: 'Although the realization that Sir Grafton Elliot Smith might be the hoaxer dawned on me about half-way through the preliminary research for this book', writes Millar, 'try as I may I have not been able to come up with concrete evidence of the Australian's participation. In fact it is hard to visualize anything that would come into this category other than a straightforward confession. I do hope, however, that I have shown that Dawson does not fit the bill, and that Smith does.' Millar, by his own admission, unable to find the facts to support his theory, fails equally in finding any reason why Elliot Smith should have committed this forgery. First he says, 'if a sufficiently primitive man were to be discovered in England, this would lend support to Smith's almost obsessive views on migration'. Secondly, 'at the time of the planting of the fossils, Smith was in what might be considered a backwater appointment in Cairo. It is therefore possible that he coveted the job at South Kensington.' But Millar rejects these two arguments, and rightly so, for Elliot Smith's obsession with migration relates to Egypt in early Dynastic times, and at the time of the planting of the fossils Smith had just moved to the Chair of Anatomy at Manchester. Millar has to fall back on his conviction that 'Smith would have loved a chuckle at the expense of what he thought, possibly correctly, was stick-in-the-mud palaeontology and anatomy. Somehow the whole affair reeks of Smith.'

Stuff and nonsense! Millar, hoping to end his book with a sensational denouement in which he revealed which of the Piltdown men was the forger, is forced to an admittedly new, but quite untenable solution. This is not the way to solve the Piltdown problem—a rapid journalistic jump. Does anyone know the answer? We will return to this matter in the next issue of ANTIQUITY, having by then digested what Dr Oakley said to the Geological Society on 18 October, and with the benefit of very considerable discussion of the whole matter with Dr Oakley, and also with Dr Louis Leakey, who has already said in lectures and interviews in America that he firmly believes that Dawson and the world were hoaxed by Father Teilhard de Chardin. As this suggestion, which has also been advanced by others in private, will need the careful consideration of our readers in March, may we urge them to read, or re-read, Pierre Teilhard de Chardin, *Lettres d'Hastings et de Paris 1908–1914* in the *Editions Montaigne* of *Aubier, Impasse de Quai de Conti, Paris VI*. These fascinating letters, which cover the whole Piltdown period, are annotated by Auguste Demoment and Henri de Lubac, and there is a preface by the latter. Letter 128, to his father and mother, dated 6 August 1913 from Canterbury, says: 'Je vais m'arrêter 48 heures à Lewes (près Newhaven) chez mon ami Dawson, pour chercher dans les graviers où l'on trouva l'an dernier l'homme du Sussex.' The editors add this footnote: 'Jusqu'à présent, Teilhard n'a eu aucune part aux découvertes de Dawson, et il lui fait confiance.' In our view Elliot Smith also had no part in the discoveries of Dawson. On 21 November 1912 he wrote from Manchester to Professor Anthony: 'I do not know whether you have heard that a very early (pre-Heidelberg, said to be Pliocene) skull had been found in England and I want to be able to compare the brain-cast with your La Quina cast next week.' This quotation comes from (ed. Warren R. Dawson), *Sir Grafton Elliot Smith: A biographical record by his colleagues* (London, 1938), and in the chapter written by Warren Dawson himself, he says, 'Elliot Smith paid several visits to Piltdown and stayed there for some days in July 1916 and July 1917.' By then Dawson was dead.

But if we criticize Millar for his most unconvincing conclusions, and castigate him for his errors, let us say that his book is very readable and, in its general survey, brings up

many interesting points. For instance, what is really the origin of the celebrated borer story? Millar says that it was at a meeting of the Royal Society when Sir Ray Lankester had addressed the Society on eoliths. Worthington Smith had got up declaring that nothing would induce him to believe in the pebbles: he said, 'We have here choppers that do not chop and borers that do not bore.' To which remark Millar says Lankester replied, 'You, sir, are a bore who does bore.' Quite another version of this story is given by R. R. Marett in his *A Jerseyman at Oxford* (1941). He refers to the report in the *Oxford Magazine* (5 November 1906) of a meeting in which Sollas and Lankester were confronted: 'Sollas, small and sarcastic, had exclaimed: "I ask any impartial person, 'To what possible human use could that lump of road-metal be put?' " And Lankester, grim and gigantic, having picked the thing up in his huge fist, had replied, "Well, I could kill you with it . . ." Sollas had referred to "these scrapers that will not scrape, and borers that will not bore". Lankester shouted out: "You at any rate are one of those *borers* that can bore." ' The mythology of archaeology grows, and we wonder whether anyone knows how this tale really began?

But we are most grateful to Millar for drawing our attention to the splendid report in *Man* for 1906 (VI, 31–2) of the first meeting of the *Congrès préhistorique de France* which was held in Périgueux between 26 September and 1 October 1905. We quote from this report:

> Into the five *séances* of the Congress were crowded some sixty communications, and as many of these exceeded the average limit of time, it was necessary for some of the lecturers to speak with a bewildering rapidity. Nevertheless, order was well maintained . . . . We may note the entry upon the stage of the anthropomorphic flint and tertiary man, who were allowed to make their exit without either definite applause or condemnation . . . . One perfect autumn day will live in the memory of those who were privileged during hours of unbroken sunshine to walk and drive through what M. Cartailhac fitly called an enchanted landscape, with its green valleys threaded by white roads and overhung by precipitous walls of limestone. The party halted at well-known sites, like La Madeleine and Laugerie Haute, and everyone fell upon the *débris* with any instrument which was ready to his hand: lance-heads were extracted with walking sticks and scrapers with umbrellas. Descents were made into three very important caves, La Mouthe, Font de Gaume, and Combarelles, the last tortuous, interminable, mysterious as the cavern of Trophonius. Its low roof bristled with stalactites, impartially distributing among the incautious wounds and contusions of unpleasant frequency, which in one case gave rise to piteous lamentations and a hasty retreat to the upper air . . . . There was a reception at the town hall by the Mayor of Périgueux, while on two occasions weary bands of excursionists were entertained by the owners of neighbouring châteaux. The luncheons and dinners at various hostelries were never dull: even certain scrambling breakfasts partaken of at local inns when the sun had hardly risen were enlivened by the infectious gaiety of the presiding spirits.

Millar says, 'although the report was anonymous, the touch is unmistakably that of Mark Twain. Certainly Samuel Langhorne Clemens was in Europe at the time of the Congress on a lecture tour.' We ask, does anyone know whether Mark Twain did write this account for *Man*?

Mention of *Man* reminds us to draw attention to the plans for the reorganization and revitalization of the Royal Anthropological Institute of Great Britain set out in a recent policy statement. The members of the Institute are lineal successors of the founding members of the Ethnological Society of London who, in February 1843, formed a breakaway group of the Aborigines' Protection Society which had been founded in 1837 in the aftermath of the early-19th-century Quaker campaign against the African slave trade. The new society was to be 'a centre and depository for the collection and systematization of all observations made on human races', and its members were distinguished from those of the parent body by their bias towards scholarly rather than humanitarian ideals. Almost from the start the membership was divided over racialist issues, and between 1863 and 1870 there were two

organizations: the Ethnological Society and the Anthropological Society. The Anthropological Institute of Great Britain and Ireland was the result of an amicable merger between these two rival bodies. This happened in 1871 and permission to add the word Royal was granted in 1907.

Now the Institute has issued a centenary appeal for £500,000. The first object of the appeal is to acquire new premises. In June 1971 the Institute was forced to abandon its former premises in 21 Bedford Square when, overnight, the annual rent went from approximately £1,600 per annum to £16,000 per annum. The resources of the Institute are at present dispersed. The Library, considered by many to be one of the foremost anthropological libraries in the world—it consists of some 58,000 volumes including 20,000 volumes of periodicals—is housed temporarily in the basement of the Department of Ethnography of the British Museum in Burlington Gardens. The headquarters offices are in very cramped premises in Craven Street near Charing Cross Station. When suitable premises have been acquired it is anticipated that a number of other academic associations such as the Association of Social Anthropologists, the Prehistoric Society, the Society for Human Biology and the British Sociological Association, will wish to share the new facilities and work in conjunction with the new Institute. It is proposed that a full-time salaried director of the Institute should be appointed. It is intended that the new Institute should develop as a centre for the human sciences; membership is to be opened to non-professionals and publications are to be aimed at a wider public who may have peripheral and non-academic interest in anthropology.

The Royal Anthropological Institute receives no financial support from the Government and is maintained solely from private funds. The Institute has never before made a similar appeal since it came into existence in 1871—incidentally the year of publication of Charles Darwin's *The descent of man and selection in relation to sex*. This centenary appeal was launched in July by the Institute's Royal Patron, HRH Prince William of Gloucester, whose sudden death in an air accident on Monday, 28 August, is a matter of the deepest regret to all. Contributions to the appeal should be sent to *The Administrative Secretary, Royal Anthropological Institute, 36 Craven Street, London, WC2N 5NG.*

☞ There is at present a lot of uncertainty as to how archaeologists are using and quoting radiocarbon dates, what half-life they are using and whether they are quoting calibrated dates, and what sources for calibration they are using. It is true that as yet we have no calibration curve generally available to and used by everyone, but we hope that such will be available in the not too distant future and we will publish it in these pages. Meanwhile as a guide to those writing for ANTIQUITY we set out these five instructions:

1. Dates should be quoted based on the old Libby half-life of 5568 years.
2. They should always give the figure for the standard error as plus/minus.
3. They should always quote the laboratory number.
4. They should be followed by lower case letters, viz., bp, bc, or ad (our style: no full points).
5. Any interpretation of these dates in radiocarbon years which the writer wishes to make (e.g. in terms of other half-life figures, or by calibrating tree-ring corrections to the conventional C14 dates) should be done in writing with an explanation, or set out diagrammatically with double-column dates as was done in A. C. Renfrew's note on Malta in *Antiquity*, 1972, 143. Dates in calendar years should be followed by upper case letters, viz., BP, BC or AD (again, our style: no full points).

☞ It is good news that the British Museum has decided to lend the Rosetta Stone to the Louvre. It is only the fortune of war that brought the stone to London. When Napoleon invaded Egypt in 1798 he took with him a number of savants interested in ancient Egypt, who collected Egyptian antiquities, including the block of basalt inscribed in hieroglyphics, demotic and Greek found by accident by French

soldiers at Rosetta. On 19 July 1799 the members of the French Institute were most excited when a letter from Egypt was read to them announcing 'the discovery at Rosetta of some inscriptions that may offer much interest'. The British army which invaded Egypt in 1801 demanded the stone and the other collections of the French savants as prizes of battle. General Hutchinson claimed everything under Article XVI of the treaty of capitulation. The French General Menou disputed every point and insisted that the Rosetta Stone was his private property and was not covered by the treaty.

The British force included several people interested in Egyptology, among them that delicious character Edward Daniel Clarke (1769–1822), Fellow and Bursar of Jesus College, Cambridge, University Librarian and first Professor of Mineralogy in the University, author of *The Gas Blowpipe,* who, when an undergraduate, sent up his kitten attached to a balloon, and when a young man travelled with Malthus in northern Europe and secured the Kistiphoros of Eleusis for his old University. Clarke entered Alexandria with the advance guard of the British army because, as J. D. Wortham says in his admirable *The genesis of British Egyptology 1549–1906* (Norman, 1971, 50), 'he intended to make certain that the French did not send any Egyptian antiquities back to France'. Elgin had sent W. R. Hamilton to Alexandria with orders to seize the French collection of Egyptian remains. To quote Wortham again, 'Clarke and Hamilton succeeded in stealing from the French a great many antiquities—including a great green sarcophagus hidden by the French in a hospital ship—that the French had stolen from the Egyptians.'

Clarke immediately realized the importance of the Rosetta Stone, and that the trilingual inscription could enable the hieroglyphic alphabet to be deciphered. He got to know a young colonel, T. H. Turner, who shared his enthusiasm for Egyptology. When General Hutchinson insisted on the Rosetta Stone being given up, General Menou replied, 'You want it, Monsieur le général? You can have it, since you are the stronger of us two . . . . You may pick it up whenever you please.' Colonel Turner collected a detachment of soldiers and seized the stone from Menou's house. He accompanied it all the way to England and gave it into the safe keeping of the Society of Antiquaries of London until it finally came to rest in the British Museum. It was the arrival of the Rosetta Stone and the many other acquisitions of Egyptian remains through conquest of war that forced a reluctant parliament to approve additions to the British Museum. It is a pleasant thought that at last it will be making a journey to the home originally planned for it.

The reason for the journey to Paris is to celebrate the 150th anniversary of Champollion's *Lettre à M. Dacier rélative à l'alphabet des hieroglyphes phonetiques* which contained the key to the decipherment of hieroglyphs, and triumphantly succeeded where De Sacy, Akerblad, and Thomas Young had started. Is there any real reason why it should not go further, and return to Egypt whence it came as spoils of war? The whole learned world has copies of it; it has served its purpose. The key to ancient Egypt should really be back home, with those who so generously lent us Tutankhamun's treasures for so long.

The news of the death of Louis Leakey in a London hospital on 1 October came as we were sending these pages to the printer. It is hard to think that the indomitable Leakey, whose adversities of the last few years would long ago have confined a lesser man to a wheelchair and inactivity, is with us no more. He had driven himself, relentlessly, to the end, travelling frequently from East Africa to England and America, always lecturing and writing and still engaged in original research. To quote the admirable obituary in *The Times* (2 October 1972, 17), he will be 'remembered as a prehistoric archaeologist, palaeontologist and physical anthropologist. In all these fields he was one of the foremost authorities of his day: but such was his versatility that he was also an expert on the Kikuyu tribe, on handwriting and on animal life. His keen powers of observation and of criticism, together with his intense interest in anything out of the ordinary,

made him an ideal museum curator and a successful writer and lecturer.'

The Editor first met him when, 40 years ago, he came up to St John's College, Cambridge, as a freshman and Leakey was his supervisor in archaeology. He was an inspiring, exciting, but erratic teacher. We didn't write weekly essays: we just listened to LSBL talk and watched him chipping flints. There was no furniture in his College rooms: we sat on packing cases, tore our clothes and fingers on rusty nails, listened, argued, sorted flints, and drove out to gravel pits. He told us then, and has often repeated this since, that his first inkling of how flints could be chipped came from Llewellyn Jewett's account of Edward Simpson's demonstration in the Geological Society: out of 19th-century forgery came good.

His last visit to Cambridge was in August of this year and, with all the vigour he put in to teaching 40 years ago, he expounded his passionately held convictions that the Piltdown forger was Teilhard de Chardin; and we continued this argument by correspondence and telephone until a few days before his death. We have already mentioned this and will return to the subject in our next issue. He had already prepared a draft of a few chapters for a book on this subject; and had, we hope and believe, done the greater part of the second volume of his autobiography. The first volume, *White African*, was published in 1937. It is very well worth re-reading now, and contains, *inter alia*, the sober account of how he managed to present Kikuyu as a language in his scholarship examinations at St John's, and later in Part I of the Medieval and Modern Languages Tripos. It is an amazing story, and well earns him the Oxford sobriquet of 'The Senior Wangler'. As the flag on the gate-tower of St John's was lowered to half-mast in respect to Leakey, an old Fellow reminded us that he was well remembered in Cambridge as the first person to wear shorts on a tennis court. There may be many people by now who have been brought up short by Louis Leakey—and rightly so. His great contribution to the history of early man will be easier to assess in 20 to 30 years from now. But now, or then, it is outstanding, and more outstanding than one man, even with the co-operation of wife and sons, could be expected to achieve in a lifetime.

In a letter to the Editor, dated 2 October 1972, Dr Kenneth Oakley writes:

> They will say 'there were giants in those days'. Louis was one of them, and the world of prehistory will seem a different place with no LSBL. We may have disagreed with him intensely sometimes, even been maddened by him. But let us remember how many developments and discoveries have stemmed from his boundless enthusiasm. Look at how powerfully he reinforced Darwin's concept that Africa was probably the home of man, his pioneering of fieldwork in the Olduvai Gorge, his initiation of the Pan-African Congress on Prehistory, his opening up of the rich fields of Miocene hominoids around Victoria Nyanza, his promoting the work of Jane Goodall, etc.

We draw attention to two remarkable exhibitions mounted on the continent in the last summer. The first was the exhibition of air photographs in the Musée de Picardie at Amiens: it showed the work of people outside France like St Joseph and Scollar, but its main purpose and relevance was to show the work of Agache and others working in France—to show how important this work was, and is, to our recovery of ancient Gaul and our knowledge of the ancient sites now vanishing under modern cultivation. Our predecessor as Editor of this journal never spared his criticisms of French archaeology, and was, it must be always remembered, the first person to rumble the ridiculous hoax of Glozel which some French people still seem to think unrumbled: he always criticized the French for their curious inability to appreciate what air photography could do for them. The Amiens exhibition has removed that criticism: French air photography is in the process of doing to the past of Gaul what Crawford, Allen and St Joseph have done to the past of Britannia.

The second exhibition was in Brussels and Ronald Jessup writes about it:

A remarkably fine exhibition '*Vingt-cinq années de fouilles archéologiques en Belgique*' has recently been shown in Brussels to mark the

tenth anniversary of *Pro Civitate*, the cultural centre founded by the Crédit Communal de Belgique under a distinguished patronage. It was intended to show to the interested public something of what had been accomplished by the Service national des Fouilles, by well-organized museums, by eminent University specialists and by well-informed amateurs. No less than 80 major sites were represented by objects lent by 33 institutions and private individuals illustrating the Stone Ages, the Bronze and Iron Ages, the Roman occupation, the Merovingian period and the Middle Ages. In his noteworthy introduction to the accompanying catalogue Professor Mertens has stressed what this kind of archaeology really is: a lesson in the better understanding of the country in which we live, 'ce pays qui est à nous'. The catalogue has an authoritative introduction to each period, a short note on each site, sufficient descriptions of the objects and a bibliography. Most of the translation into French is by M. Marcel Amand. A comparison with the *Vingt-cinq années* account written in 1928 by E. Rahir, the then Director of the Service des Fouilles, makes interesting reading and we can congratulate our friends on the enlightened progress of their work in the field, in the laboratory and on paper. The pity is that this exceptionally well-arranged exhibition was open for five weeks only.

☙ It is good to know that a Society for Afghan Studies has been formed in London under the auspices of the British Academy, with Sir Harold Bailey, FBA, as President, Mr Peter Fraser, MC, FBA, as Chairman, and Dr D. W. MacDowall as Secretary. The main purpose of the Society is to establish and maintain in Kabul a British Institute of Afghan Studies as a research centre for British scholars working in the fields of archaeology, history, languages, geography and related subjects. The intention is to provide living accommodation, a library, study facilities, and a base for field archaeology which will be undertaken in co-operation with members of the Afghan Archaeological Service. It is hoped that the establishment of the Society and Institute will facilitate co-operation between British and Afghan scholars working in these fields, and with other foreign archaeological and cultural missions working in Afghanistan. It should strengthen the ties that have been established during recent years by reciprocal visits of Afghan and British scholars and by UNESCO projects for the study of civilizations of Central Asia in the Kushan and Timurid periods.

As we move from galley into page we hear that Dr David Whitehouse, on the eve of another season at Sīrāf, has been appointed the first Director of the Institute at Kabul, and we give him our congratulations and good wishes.

☙ It is hard to pick up a newspaper nowadays without finding one's path bestrewn with obstacles: half-finished or transposed sentences, paragraphs even, omissions, repetitions and all manner of gobbledygook can sometimes turn a column into a curious kind of literary assault course from which the reader emerges breathless and bewildered. We apologize to our readers who may have felt, after sorting out column 1, p. 179, of our September issue, that we were setting them similar traps. A kind correspondent wondered if the fleuron, capstone *arsé-tarsé*, heralding paragraph three was some code indicating that we were now moving into the lunatic tailpiece of the editorial, or was it, she wrote, 'just part of the general printer's omelette on that page?' No tales out of school, but there is certainly egg on somebody's face. Could it be the White Goddess herself: did she, Mother-Father that she is, call her journeymen to Chapel, the while she stole into their comps' room and made pie?

Dr Henrik Tauber points out that in his table (June 1972, p. 107), the C14 age bc for Vroue, V (line 9) should read 2300–2200.