

A HUNDRED AND FIFTY YEARS
OF ARCHAEOLOGY

CHAPTER FIVE

THE DEVELOPMENT OF TECHNIQUE AND METHOD BEFORE 1900

1. *The Beginnings of Excavation*

THE half-century from 1850 to 1900 not only saw the birth of archaeology from antiquarianism, history and geology, the discovery of the proofs of man's antiquity and of the ancient civilisations of Egypt, Assyria and Sumeria, and the creation of a complicated "sequence" of man's prehistoric cultures based on subdivisions of the Danish tripartite system: it also saw, slowly and with difficulty, the beginnings of systematic archaeological techniques of excavation, field survey, conservation and protection. It also saw, even more slowly and uncertainly, what Seton Lloyd has aptly called "The Birth of a Conscience",¹ regarding the expropriation of antiquities from other countries.

In the first half of the nineteenth century, and for a long while afterwards, excavation was principally concerned with the quick discovery of what was hidden in a barrow, tell, or pyramid, and with the acquisition of works of art to adorn the museums and private collections of Europe. Layard himself described his object of excavating at Nimrud as "to obtain the largest possible number of well-preserved objects of art at the least possible outlay of time and money", and Loftus frankly admitted in much the same words that in excavating at Warka he was actuated by "a nervous desire to find important large museum pieces at the least possible outlay of time and money".

In these confessions, Layard and Loftus are merely carrying on the old traditions of the west European collector begun in the sixteenth century by the Popes, and by men such as Lord Arundel, who had frankly admitted that his purpose was "to transplant old Greece to England". The English travellers in Greece in the eighteenth and early nineteenth century had no scruples about carrying away to England the antiquities of the Near East and

¹ *Foundations in the Dust*, Chapter XII, *passim*.

TECHNIQUE AND METHOD BEFORE 1900

Aegean peoples, who were regarded as effete and unworthy possessors of such works of art. "Inscriptions we copied as they fell in our way," wrote Robert Wood in his *Ruins of Palmyra*, "and carried off the marbles wherever it was possible, for the avarice and superstition of the inhabitants made the task difficult and sometimes impracticable." The Cambridge don E. D. Clarke discovered the tomb of one Euclid—not the mathematician—in Athens, and wrote: "How interesting . . . such an antiquity must be for the University of Cambridge, where the name of Euclid is so particularly revered." And Clarke's account of the removal of the colossal Cistophoros of Eleusis, now in the Fitzwilliam Museum, is a fascinating revelation of archaeological methods at the time. "I found the goddess in a dunghill," he writes, "buried to her ears. The Eleusinian peasants, at the very mention of moving it, regarded me as one who would bring the moon from her orbit. What would become of their corn, they said, if the old lady with the basket were removed? I went to Athens and made an application to the Pasha, aiding my request by letting an English telescope glide between his fingers. The business was done."¹

In excavation, no less than in collection, methods were such as we to-day find strange. The early eighteenth-century excavations at Herculaneum and Pompeii were conducted over many years with small groups of workmen—four, eight, or at most thirty. Houses were exposed and looted, paintings were sawn off, and the robbed houses left to decay away.² Right at the end of the eighteenth century and at the beginning of the nineteenth, due to the keen and generous interest taken by the Napoleonic kings of Naples, carefully planned excavations of Pompeii took place—the first large planned excavations in history. The plan was due to a scholar of Naples, Michele Arditi, and enough money was forthcoming to employ at times six hundred men at this work.

This premature birth of organised and conscientious excavation had no counterpart in England, where the old methods of the early antiquaries continued for a long while. We have already

¹ Otter, *Life of Clarke*, 505.

² Herculaneum was first excavated in 1711, after which excavation was forbidden, until undertaken by the Government from 1738 to 1766. The excavations were abandoned in 1766 because of insurmountable difficulties occasioned by thick layers of hardened ashes and pumice-stone. The ruins of Pompeii were accidentally discovered in 1748; it was easier to excavate, and it soon took the place of Herculaneum.

referred to the first meeting of the British Archaeological Association at Canterbury in 1844. During this meeting several barrows were excavated in the course of one day's expedition. The account of this excavation, only too typical of mid-nineteenth-century barrow digging, is worth quoting here. The barrows "were excavated to within about a foot of the bottom, before the arrival of the visitors, in order that the deposits might be uncovered in their presence. . . . The archaeologists assembled at Breach Down between nine and ten o'clock . . . and eight barrows were successively opened for their inspection. The only interruption arose from a heavy shower of rain, which was so far from damping the zeal of the visitors, that many, both ladies and gentlemen, raised their umbrellas (if they had any) and stood patiently looking at the operations of the excavators, whilst others sought a temporary covering in a windmill which stood in the middle of the scene. The barrows . . . were less productive than was anticipated. All however contained human remains, and in some were found different articles, which appeared to indicate the character of the person interred in them. . . . From Breach Down the party proceeded to Bourne Park . . . where two barrows were excavated which proved much richer than those at Breach Down." After the excavation of these ten barrows the party "partook of . . . a plentiful repast".¹

Nearly a quarter of a century after this, in 1866, a party of archaeologists of international reputation went off to excavate at Hallstatt. The party included John Evans, Lubbock, Lartet, Morlot and Franks. Evans describes their activities in letters to his wife. "We arranged with the Bergmeister to set some men at work digging and are going up there early to-morrow morning to see the result: it may be that we shall stop there all day," he writes; and then next day: "We found our diggings too pleasant for us to be able to tear ourselves away from them. Lubbock and I breakfasted soon after 6 and about half past 7 were up at the cemetery . . . and found the men had already discovered a bronze bracelet and a broken fibula. I subsequently found in one of our trenches and dug out with my own hands one of the iron socketed celts with a part of the handle remaining in it and having on one part the impression of a fine twilled cloth against which it had lain. . . . I hope to be able to arrange in Vienna for our friend the

¹ Wright, *The Archaeological Museum*, 6-8.

Bergmeister Stapf to carry on some further excavations for us."¹

Exciting though the excavations in Mesopotamia were in the middle of the nineteenth century, and remarkable though the discoveries were, made by pioneers like Botta, Layard and Rassam, they can hardly be acclaimed the originators of scientific excavation. The excavation methods of Botta and Place at Khorsabad consisted of tunnelling along the face of the walls and getting light by occasional vertical shafts to the surface. Even after years of work in Mesopotamia, Rassam was incapable of recognising sun-dried brickwork. And nowhere was there much attempt at conservation of monuments or artifacts. Layard visited Khorsabad some time after Botta had finished his excavations and wrote: "Since M. Botta's departure, the chambers had been partly filled up by the falling in of the trenches; the sculptures were rapidly perishing; and shortly, little will remain of this remarkable monument." But Layard himself did little for the preservation of his own finds. *Nineveh and its Remains* abounds in the phrase reminiscent of the earlier barrow diggers in England, and the early nineteenth-century excavators of Egyptian mummies: "entire when first exposed to view, it crumbled into dust as soon as touched". Thus copper helmets, iron armour, copper vessels, painted frescoes, ivories, all "fell to pieces almost immediately on exposure to the air". Rassam found, sixty feet to the north-west of the famous Shalmaneser II gates at Balawat, a second pair of gates, but: "as soon as they were exposed to the air, they crumbled to pieces", he wrote.

There had also arrived in Egypt by the middle of the century that villain in archaeology, the tomb robber—under the guise of scientific enquirer—in the person of Giovanni Belzoni, surely one of the most eccentric characters in the history of archaeology—that long calendar of bizarre characters. Belzoni was a native of Padua, who made a living in England by performing prodigious feats of strength at circuses, went to Egypt to sell hydraulic machinery for irrigation purposes, and stayed on to rob tombs. He began his search for Pharaohs in 1817 at Thebes. He broke into one tomb, crashing his way through antiquities; "every step I took I crushed a mummy in some part or other," he declares. "When my weight bore on the body of an Egyptian it crushed like a band-box. I sank altogether among the broken mummies

¹ Joan Evans, *Time and Chance*, 123.

with a crash of bones, rags and wooden cases. . . . I could not avoid being covered with bones, legs, arms and heads rolling from above." Small wonder that Belzoni fainted frequently during these outrageous escapades, and that he was so overcome after one tomb exploration that he wandered about for three weeks in a trance. Small wonder, too, that the modern Egyptologist when he reads about the doings of Belzoni becomes, as Magoffin and Davis write, "a human chameleon", turning "green with envy, then red with shame, and then white with rage".¹

The robbing of tombs and mounds and the private sale of antiquities and art treasures was typical of Egypt and Mesopotamia during much of the nineteenth century, and was, of course, a great hindrance to serious archaeology. In 1859 Mariette's workmen found near Thebes the gilded sarcophagus of Queen Aahhotep. Before Mariette could arrange for it to be sent to Cairo, a local potentate, the Muḍir of Kenh, stole it, opened it in his harem, and set off by boat with the jewellery to present to the Khedive as an offering from himself. Mariette pursued the Muḍir in a steamboat, caught up with him and boarded his boat, forcing him by physical violence to give up the jewellery, which he then took safely to Cairo. Rassam left his excavations unsupervised, and tablets from them found their way on to the market both before and after he completed his excavations. Budge, in 1888, was able to recover hundreds of these tablets from Baghdad dealers, though his own methods of getting the tablets out of the country were, to say the least, unusual—Seton Lloyd describes them as a "very blatant piece of sharp practice"² and his outspoken comments on the leakages from Rassam's digs involved him in an action for slander brought by Rassam.

For some while the rivalry between excavators in the Near East made the search for antiquities nothing short of piracy. The archaeological pirates were none the better even when working on behalf of the British Museum or the Louvre. "Those were the great days of excavating," declared Howard Carter. "Anything to which a fancy was taken, from a scarab to an obelisk, was just appropriated, and if there was a difference with a brother excavator, one laid for him with a gun."³ On one occasion, Belzoni, who had been working on behalf of the British Consul-General in

¹ *The Romance of Archaeology*, 50

² *The Tomb of Tut-ankh-Amen*, i, 68

³ *Foundations in the Dust*, 193.

Egypt as well as on his own account, had a remarkable encounter with the agents of Drouetti, who had been working on behalf of the French Consul. Belzoni had secured the Philae Obelisk and was returning to Luxor when, as he writes, "I saw a group of people running towards us; they were about thirty Arabs, headed by two Europeans, agents of Mr Drouetti. On their approaching, Mr Lebulo was first, and the renegade Rossignano second, both Piedmontese and countrymen of Mr Drouetti. Lebulo began . . . by asking what business I had to take away an obelisk that did not belong to me; and that I had done so many things of this kind to him, that I should not do any more. Meanwhile he seized the bridle of my donkey with one hand, and with the other laid hold of my waistcoat and stopped me from proceeding any farther: he had also a large stick hung to his wrist by a string. . . . At the same moment the renegade Rossignano reached within four yards of me, and with all the rage of a ruffian levelled a double-barrelled gun at my breast, loading me with all the imprecations that a villain could invent; by this time my servant was disarmed and overpowered by numbers. . . . The two gallant knights before me, I mean Lebulo and Rossignano, escorted by the two other Arabian servants of Mr Drouetti, both armed with pistols, and many others armed with sticks, continued their clamorous imprecations against me, and the brave Rossignano, still keeping the gun pointed at my breast, said it was time that I should pay for all I had done to them. The courageous Lebulo said . . . that he was to have one-third of the profit derived from the selling of the obelisk when in Europe, according to a promise from Mr Drouetti, had I not stolen it from the island of Philae. . . . I have no doubt that if I had attempted to dismount, the cowards would have despatched me on the ground, and said they did it in defence of their lives, as I had been the aggressor."¹

This encounter between Belzoni and the agents of Drouetti is very typical of the picturesque methods of archaeological reconnaissance and excavation in the early nineteenth century. It is worth remembering, as Baikie has pointed out,² that these methods represent not the worst but the best of Near Eastern excavations at the time. Much of Belzoni's work has been praised by later workers, and it was certainly due to the unprincipled

¹ Belzoni, *Narrative of the . . . Recent Discoveries in Egypt and Nubia* [1821] 366-367.

² *The Glamour of Near East Excavation*, 42

depredations of Belzoni, Drouetti and their kind that representations of Egyptian art appeared in London, Paris and Turin, just as many of the British Museum's Assyrian treasures owe their existence to the surprising methods of Rassam.

Rouet, who replaced Botta as French consul at Mosul, sent agents hurriedly around the countryside opening mounds at random to stake a claim and prevent Layard from digging in them; and Layard himself was compelled to do much the same thing. At one moment, the French, disbelieving Layard's claim to have had permission to dig at Nineveh, were digging little pits while Layard was excavating elsewhere in the same mound. Rassam engaged himself in what can only be described, in Seton Lloyd's words, as "an undignified scramble for archaeological loot". Using Mosul as his base he examined mounds over an area of two hundred miles, leaving groups of his workmen digging away, often without any supervision, in a frantic attempt to get out of the ground whatever treasures there might be, before the mounds were claimed by agents or archaeologists of another nation. And at times Rassam unashamedly excavated mounds already conceded to other missions. His discovery of Ashur-bani-pal's library was due to a blatant act of piracy. Kuyunjik had, with the permission of Rawlinson, been divided into a northern or French area, and a southern or British area, and in 1853 Place's men and Rassam's men were both excavating there. Rassam was alarmed to see Place's men, while still working within their conceded areas, getting towards what he thought the most promising area at Kuyunjik. At the risk, as he said, "of getting into hot water with M. Place", Rassam excavated at night in the French area and discovered Ashur-bani-pal's palace with its library and lion-hunt gallery. Rassam described this episode as "using strategy": and says that Place accepted his act of piracy with equanimity, even congratulating Rassam on his "good fortune" at the time. But when Place came to write his *Ninive et L'Assyrie* he found himself unable to give Rassam credit for the discovery, claiming it for Loftus and his artist Boutcher.

Rassam was not so successful in his piratical attempts at Telloh. De Sarzec had excavated there in 1877 and 1878 and, while he was out of the country on a visit to France in 1878, Rassam decided to excavate there on his own. He collected a gang of Arab workmen who began fighting among themselves

after three days, and thus thwarted his plans. Rassam was annoyed. "From what I have seen of the place of M. de Sarzec's discoveries," he wrote, "I am certain that if I had continued my researches there one day longer, I should have come upon the nest of black statues which were discovered in the highest mound."

It is very easy at the present day to be critical of the methods and achievements of the mid-nineteenth-century archaeologists. Every generation of archaeologists is likely to be critical of the methods of its predecessors. So far it is almost only Pitt Rivers among nineteenth-century British excavators whose reputation survives entirely unscathed. Layard, Rassam, Belzoni and their contemporaries have all been adversely criticised. Yet they were all excavating according to their lights. Very few archaeologists, either then or now, genuinely feel that they and their contemporaries are not technically equipped to undertake a complicated excavation and that it should be left to a later generation. It was, from our present point of view, perhaps unfortunate that many of the great discoveries were made in Egypt and Mesopotamia when they were. We may regret this, but we cannot convict the discoverers of any greater villainy than that they were not well ahead of their times. And we must remember that without their work the beginnings of Near Eastern archaeology would never have been made, nor an interest in archaeology fostered in mid-nineteenth-century England and France. Nor can we blame them unduly for the removal of antiquities and art treasures from Mesopotamia and Egypt to Europe. Botta, Layard, Belzoni and Drouetti were merely carrying on the traditions started in Greek lands by the agents of Charles I, the Earl of Arundel and the Duke of Buckingham in the seventeenth century, and maintained by Stuart and Revett, Hamilton, Robert Wood, E. D. Clarke and others in the eighteenth century. It should be remembered that only some forty years separate the arrival of the Elgin Marbles in London and the Kurnah disaster, but very gradually, towards the end of the nineteenth century, some standards of archaeological excavations were beginning to be recognised and established, and serious thought entertained on the propriety of removing to the museums of Europe the ancient treasures of the Near East. Traditions of excavation were beginning in western Europe as well as in the Near East. Greenwell wrote to John Evans in 1867 saying:

"That scoundrel Mortimer has been spreading calumnious reports to the effect that I am destroying all the Wold barrows and missing half the interments, in fact, doing the work in a thoroughly bad way. This is with a view to stopping my getting leave. His conduct . . . has been that of a rascal. . . . I may possibly have to get you to give me a testimonial as to my mode of barrow opening."¹ Censure of excavation methods became rife in the Near East, although perhaps it was not untouched with national sympathies and antipathies. Fossey, a Frenchman, described Rassam's campaign of 1879-82 as resembling "plus à un pillage qu'à une fouille scientifique". Breasted, an American, criticised British methods at Kuyunjik as "unscientific". Hilprecht, another American, expressed disapproval of all Mesopotamian excavations before those at Nippur in which he took part. Budge, an Englishman, disparaged the work of Rassam and de Sarzec, and wrote of the technique of the Americans—Hilprecht included—at Nippur: "More travellers than one who have seen the site of the American excavations at Nippur have failed to see there any exhibition of scientific digging."²

This critical awareness of the need for standards in archaeological technique is in itself an indication of the gradual development of new techniques of scientific archaeology. Excavations in England, the continent of Europe, in classical lands, in Egypt and in Mesopotamia all contributed to this development. Credit has been variously apportioned. Many names stand out as those of the great pioneers of the new archaeology of scientific excavation—Mariette, Conze, Newton, Curtius, Schliemann, Petrie and Pitt-Rivers. We shall discuss the contribution of these in the remaining sections of this chapter. But here we should especially remember the work of the Danes in their bogs, and of the Swiss archaeologists in their lake dwellings. The foundations of the stratigraphical principles of excavation were devised in Denmark and Switzerland. And it was the Swiss lake-dwelling excavations that revealed the extent to which apparently perishable material could, under special conditions, survive, and could, under very special conditions of excavation, be preserved. Earlier discoveries of tree coffins had also demonstrated this.

In July 1834 a barrow at Gristhorpe, between Scarborough and

¹ Quoted in Joan Evans, *Time and Chance*, 123.

² *The Rise and Progress of Assyriology*, 144.

Filey, was excavated by Mr Beswick, its owner, and a Mr Alexander of Halifax. They found an oak-tree coffin containing the perfect skeleton of "an Ancient Briton" wrapped in the skin of a sheep or goat. The bones were stained an ebony colour—a fact satisfactorily explained by Dean Buckland in a letter to the *Literary Gazette* as due to the tannin and gallic acid of the oak. In the coffin, with the skeleton, were also found "the flint head of a small javelin . . . two rude arrowheads of flint . . . the head of a spear or javelin formed of brass or some other composition of copper . . . a pin, and a beautifully formed ornament of either horn or the bone of some of the larger cetaceous tribe of fishes . . . an instrument of wood resembling in form the knife used by the Egyptian embalmers . . . fragments of a ring of horn . . . a wickerwork basket" and "a quantity of vegetable substance" described as the leaves and berries of the mistletoe—"they were however very tender and soon crumbled to dust".¹

Gristhorpe and the other tree-coffin finds taught an important lesson to archaeologists, namely that it might be possible to recover a very detailed picture of early man, and that archaeology would not be a discipline like geology, dealing only with the fossils of man, but would deal with material survivals of a far less tangible form. It would be a long time before archaeological technique permitted the study of material objects like Queen Shub-ad's harp and the spear from Palestine which had left only their "ghosts" behind, but the tree coffins made a point which was hammered home by the finds from the Swiss lake dwellings—that archaeology had to develop a technique of excavation and preservation all its own, and that careful excavation of specially preserved sites would yield the most detailed knowledge of the way of life of early man. It is indeed a long way from Stukeley's "garden of the Druids" to the list of plants and animals recovered from the Swiss lake dwellings, but it is the measure of the change that was taking place in the early nineteenth century in the awareness and practice of archaeological technique.

2. Mariette

Mariette's own methods of excavation and research have frequently been decried. He himself excavated over thirty

¹ This find is described in a pamphlet published in 1834 by W. Williamson, and by W. J. Thoms in his introduction to his translation of Worsaae's *Primeval Antiquities of Denmark* (1849), from which these quotations are taken.

important sites in as many years. He was mainly concerned with obtaining splendid results, and was always after precious and imposing finds and historic monuments, rather than the common things of everyday life. Petrie describes how Mariette made excavations near the Sphinx and had blasted away with dynamite the fallen ruins of a temple. "Nothing was done", wrote Petrie sadly in 1883, two years after Mariette's death, "with any uniform plan; work is begun and left unfinished; no regard is paid to future requirements of exploration and no civilised or labour-saving appliances are used. It is sickening to see the rate at which everything is being destroyed, and the little regard paid to preservation." Mariette certainly never published his results adequately; some of his excavations were not published at all. He was content with gathering together a vast mass of material without recording the details of its provenance or assessing its historical significance.

Yet, with all these limitations, he was a pioneer of new methods in archaeology. Even if his own methods were, in the judgement of subsequent archaeologists, still primitive, at least he saw that no one practised even more primitive methods. As Director of the Egyptian Service of Antiquities he forbade any excavations in Egypt except those which he himself conducted. Thus, for a while, by the high-handed institution of a personal monopoly in excavation, he stopped the undignified and regrettable scramble for antiquities such as was taking place at the time in Mesopotamia, and, for a while, he rid Egypt of tomb robbers and art collection touts masquerading as archaeologists. Mariette regularised and systematised the various works undertaken in Egypt, providing for the first time a proper control of excavation and research.

This in itself was a great service to the development of a new outlook in archaeology. But Mariette did more than this; he devoted himself strenuously to prevent the exportation of Egyptian antiquities to Europe. He wanted the remains of Ancient Egypt properly housed in modern Egypt, and, to this end, bent all his efforts to prohibit the removal of antiquities from Egypt and to the creation of a National Museum of Egyptian Antiquities. He felt that, once there was such a museum in Egypt, it would no longer be possible for the agents of European missions to justify their removal of treasures from Egypt on the

grounds that the Egyptians were not fit to look after their own antiquities.

In pursuance of his aims he had first to convince Said Pasha, the Khedive, that Egyptian antiquities mattered at all. The creation of the Egyptian Service of Antiquities and Mariette's appointment as Director were largely due to the machinations of de Lesseps and Napoleon III, rather than to the interest of the Khedive who, as Maspero wrote, reluctantly "came to the conclusion that he would be more acceptable to the Emperor if he made some show of taking pity on the Pharaohs". Mariette had a difficult task turning this diplomatic demonstration of pity into real interest. He could never secure a regular and permanent grant from the Egyptian Government but had to apply for *ad hoc* grants direct from the Khedive. These applications were refused or granted according to the whim of the moment; several of Mariette's digs had to be stopped owing to the cessation of funds by Said Pasha in a moment of bad temper. Mariette collected all he could from every excavation in the hope that the size of his collection would eventually shock the Khedive into providing a museum. We must not therefore censure too sharply his pre-occupation with large and dramatic finds.

At first his scheme bore no fruit. To house his treasures he was first allocated "a deserted mosque which was falling into ruin, some filthy sheds, and a dwelling-house alive with vermin in which he lived himself".¹ He turned this unpromising site into the first Egyptian Museum, and continued collecting, and importuning the Khedive for a real museum. It was Queen Aahhotep's jewellery that eventually moved Said Pasha. We have already described how the Mudir of Keneh stole this jewellery and how Mariette boarded the Mudir's boat and forced him to return it. The Khedive was amused by this incident, kept a gold chain for one of his wives, a scarab for himself, and ordained that the rest of the jewellery should go into a specially built museum. The gold and jewellery had produced at once the effect which the large historic monuments had failed to do. In 1859 a special museum was built at Boulak.

Even so, Mariette had to maintain the greatest watchfulness over the new museum, which he filled with fresh treasures after each year's excavations. The Khedive would often have liked

¹ Baikie, *A Century of Excavation in the Land of the Pharaohs*, 26.

to give away exhibits from the Boulak Museum to his friends, and on occasion suggested that the whole collection should be pawned as security for a loan. The Empress Eugénie was so delighted by the Egyptian jewellery exhibited in the Exposition at Paris in 1867 that she informed the Khedive Ismail that she would be graciously pleased to receive the whole collection as a present. It was a great moment in the history of archaeology when the Khedive, surprised by this request, yet anxious to please France, and short of money, still made his consent conditional on Mariette's agreement. "There is someone at Boulak more powerful than I," he said to the Empress's agent, "and you must address yourself to him." Mariette firmly refused, and the collection of Egyptian antiquities returned safely from Paris to Boulak. What strength of character and fixity of purpose is revealed in that refusal of Mariette, made to the highest in his native land, in the interests of what he thought right for archaeology and Egypt. It cost him the support of France and, for a short while, the favour of the Khedive. But after 1870 he was again in favour and from then until his death in 1881 his schemes prospered. Ismail even had grand plans for the extension of the museum buildings at Boulak, but these never came to anything, and on one occasion during this period the proposal for raising a loan on the collections was again mooted, and again defeated by Mariette. When he died he could look back on three magnificent achievements—the creation of the first National Museum in the Near East, the creation of the first National Service of Antiquities, and the birth of a conscience about the expropriation of antiquities.¹

3. *The Development of Classical Archaeology*

Up to the sixties and seventies of the nineteenth century, classical excavations, apart from Pompeii, had been confined to the recovery of single objects or single structures. There had been little attempt at complete excavation, and no idea of recovering a succession of occupations. From 1870 onwards a new archaeological method was developed in classical excavations which became the pattern of all subsequent excavational technique every where. This development was largely due to the Austrian

¹ In 1889 the museum was moved from Boulak to a disused palace at Gizeh. In 1902 the collections were moved again to the present museum in Cairo, in the Qasr-el-Nil.

and German excavators in the Aegean, especially Conze, Curtius and Dörpfeld. Its aims have been summarised by Michaelis, himself a friend and fellow-pupil of Conze, as follows: "To ascertain the original form both of the general plan and of its separate parts, to follow the successive alterations that have come in the course of time, to assign to each detail its place in the development, and thus to make the excavation a reconstruction of the lost whole, is the distinguishing mark of the new method."¹

The names of two archaeologists must be mentioned who, working in classical lands before the Germans, yet showed the beginnings of scientific technique. These were Fiorelli and Newton. Giuseppe Fiorelli took over the excavation of Pompeii in 1860. Hitherto complete excavation of houses had rarely if ever been done; the upper storey had been dug and collapsed into the trenches, burying the lower levels. Now, Fiorelli uncovered whole insulae and dug them carefully stratum by stratum, preserving any features of interest *in situ*. He founded a "Scuola di Pompei", where foreigners as well as Italians could learn archaeological technique, and himself made a special study of the materials and technique of building at Pompeii. Michaelis calls Fiorelli "a thoroughly scientific man": he was certainly one of the pioneers of stratigraphical analysis.

This is what Gaston Bossier wrote in summing up the new methods initiated by Fiorelli in 1863 at Pompeii: "He declared and repeated in his reports that the centre of interest in the Pompeian excavations was Pompeii itself; that the discovery of works of art was a matter of secondary importance; that efforts were directed, above all, to reviving a Roman city that would depict for us the life of bygone ages; that it was necessary to see the city in its entirety and in its minutest details in order that the lesson it taught might be complete, that knowledge was sought not only of the houses of the wealthy but also of the dwellings of the poor, with their common household utensils and crude wall decoration. With that end in view, everything became important, and nothing could legitimately be overlooked." Here was a manifesto which might still be used as a statement of the essential aims of excavations in a settlement.

Charles Thomas Newton was an official of the British Museum who arranged to be sent by the Foreign Office for seven years of

¹ *A Century of Archaeological Discoveries*, p. 158.

diplomatic service in the Levant, and to combine his consular duties with collecting material for the British Museum. In 1852 he was vice-consul at Mytilene, later acting as consul at Rhodes and Rome, returning in 1861 to the British Museum to take charge of the department of Greek and Roman Antiquities there. Newton had been excited by reliefs sent to the British Museum in 1846 by Sir Stratford Canning, who had taken them from the Turkish fortress of Budrum. Newton suspected these fragments were from the Mausoleum of Halicarnassos. He succeeded in identifying the site of this wonder of the world, and in assembling in the British Museum all that was left of it, including fragments that had been taken to Genoa, Constantinople and Rhodes. In 1858-59 he went to Cnidos and discovered the plan of the Greek city there; this was the first time that an old city plan had been carefully and accurately recovered. Newton made extensive use of photography in his work.

Alexander Conze dug in Samothrace in 1873. He had two architects working with him and a photographer, as well as a man-of-war placed at his disposal by the Austrian Government. His second Samothrace expedition was in 1875. A complete record was published of these excavations; it is the first "modern" excavation report in existence. All the plans were beautifully drawn in detail by the architects, and the reports were illustrated by photographs. This was the first time that photography had been used to illustrate archaeological reports. While Newton had used photography on his excavations, he had had lithographs made from the photographs to illustrate his reports.

The German Archaeological Institute became a Prussian Government institution in 1871, and an Imperial institution in 1874. A branch was set up in Athens and in 1875 excavations were begun at Olympia under the energetic and skilful direction of Ernst Curtius, assisted by the architect Friedrich Adler. This work occupied the six winters of 1875-80. The German Empire spent £30,000 on the work, and the expenses of the last winter were borne personally by the Emperor William. As the Greek Government prohibited the export of antiquities the German Government renounced all claims except in the case of duplicates being found. Everything that was discovered at Olympia was preserved, and a small museum built on the spot to house the finds. The stratigraphy of the sites was carefully and completely

studied. The architectural work at Olympia was undertaken first by Richard Bohn and later by Dörpfeld, who became the moving spirit in the new methods of preservation and excavation.

The great work of Conze at Samothrace and Curtius at Olympia initiated a period of thirty years of great classical excavations, and classical lands became a school for the method and technique of excavation. The French at Delos and Delphi, the Greeks themselves in Athens, and the American Archaeological Institute under Bacon and Clarke at Assos, on the south coast of the Troad, and at Neandreaia, north of Assos, carried on the traditions which the Germans had established. These traditions were transmitted by Dörpfeld to a perhaps unwilling Schliemann.

In assessing the work of the Germans and Austrians in Greece at this time we must especially mention the work of Adolf Furtwängler, who, as Albright has recently insisted, developed to the highest degree the chronological value of pottery, more especially painted pottery.¹ It was also Furtwängler who with Löschcke issued, in their *Mykenische Vasen* (1886), a complete publication of all Mycenaean vases and potsherds found in all the islands of the Aegean as far east as Cyprus. This was the beginning of that essential of modern archaeology—the corpus of finds.

4. Schliemann

Schliemann's contribution to the development of archaeological technique and method has been vigorously disputed. To some he was the first modern archaeologist and much of modern methods derives from his excavations. To others he was a plundering, blundering robber whose reputation was made by the astonishing value and interest of his finds. Casson, for example, claims Schliemann as the founder of scientific method in archaeology, declaring that he laid "the solid foundations of a proper archaeological method which could be followed in any land", and that his methods "constituted an innovation of the first order of importance in the study of the antiquity of man by archaeological methods".² Michaelis, on the other hand, said Schliemann was "a complete stranger to every scientific method of treatment of his subject and had no idea that a method and a well-defined technique existed".³ Karo, less enthusiastic than Casson

¹ *From the Stone Age to Christianity*, 1946, 20.

² *The Discovery of Man*, 221.

³ *A Century of Archaeological Discoveries*, 217.

but more charitable than Michaelis, gives this verdict on Schliemann's work: "No one with archaeological experience will refuse to acknowledge a great debt to Schliemann and his young wife for their achievements; without any training in technique and method, and without any of the resources that are available to-day, they brought thousands of objects to light."

On one point there can be no dispute. Schliemann's excavation at Hissarlik was the first excavation of a tell: "the first large-scale dissection of a dryland settlement unguided by the remains of great monuments such as simplified the task in Babylon and Nineveh", as Myres has put it. In this Schliemann was the precursor of a long line of excavators in the Near East. He cut right through the mound, distinguishing seven occupation levels: it was a great moment in the application of stratigraphical principles to archaeology. The principles of stratigraphy in their general application to archaeology were already appreciated and the work of the Danish archaeologists in their peat bogs, of Keller and others in the Swiss lakes, and of Gastaldi and Stroebel in the *etremare* mounds of Piedmont and Lombardy had been ample demonstration of geological superposition as a key to relative chronology in prehistoric archaeology. Schliemann demonstrated the applicability of these principles to the excavation of a great mound. But it would be only fair to say that the stratigraphy of Hissarlik only gradually forced its way into his understanding. He puzzled his way slowly and laboriously to an appreciation of what he was uncovering; at one moment he thought the whole mound covered remains of Priam's Troy, and the widespread occurrence of stone tools was confusing: "I cannot understand", he wrote, "how it is that I am unearthing stone implements throughout the length of my excavations."

We should distinguish two periods in Schliemann's digging: the earlier period from 1870 to 1882, and the second from 1882 until his death, when he was assisted by Dörpfeld. In his first season at Troy, from 1871-73, Schliemann was alone with his young and gifted Greek wife. At Mycenae, from 1874-76, he had Greek colleagues, but they suspected him, and were most uncooperative. In 1879, in his second season at Troy, he was assisted by, and learnt much from, Virchow and Burnouf. From 1882 onwards, for his third and last season at Troy, he was assisted by Dörpfeld, who, as we have said, was a practical architect and

had worked under Curtius at Olympiä. Dörpfeld brought to the Schliemann digs the new system and efficiency of the German classical archaeologists. Perhaps "assisted" is not the right word to use in describing the relations of Dörpfeld to Schliemann at Troy. Myres calls Schliemann in his last season "a constitutional sovereign among expert ministers", and Ludwig calls him "a captive king". Whatever were the human relations, Dörpfeld was able to expose the stratigraphy more clearly than previously; he revolutionised Schliemann's technique, changing it, as Myres says, "from digging to dissection".

But it would be wrong to suppose that all that was good in Schliemann's methods came from his half-reluctant adoption of the methods of Curtius and the Germans in Greece. Long before Dörpfeld joined him he was working on certain principles basic to good excavation. He preserved everything he found; although he was driven on by the desire to recover the treasures that were buried in the earth, he preserved all his finds, realising the importance of ordinary things in providing a true picture of the past. He recorded carefully the level at which all finds were made, and he had every important find drawn or photographed as soon as possible. And he was prompt in publishing his results as fully as possible. In fact, Schliemann was applying to his excavations the business methods which he had learnt in his long career as a merchant, and we may consider the great success of the excavations he made at Troy as the result of the happy combination of his own business methods and acumen with the methods and training brought by Dörpfeld.

5. Pitt-Rivers

General Pitt-Rivers made two great contributions to the method of prehistoric archaeology. The first related to the analysis of artifacts, the second to the technique of excavation. Pitt-Rivers was professionally concerned in the Army with investigations into the use and improvement of the rifle between 1851 and 1857 at Woolwich, Enfield, Malta and Hythe. He was virtually the originator of the Hythe School of Musketry. Pitt-Rivers had great talents for organisation and for experimental research. In studying the development of firearms he found himself arranging collections of types in developing or evolutionary sequences. He was also interested in the evolutionary ideas

which at that time were informing biological knowledge. From his own detailed study of British firearms, and the Darwinian concept of evolution, he formulated the idea that all material objects developed in an evolutionary way and could be arranged in typological sequences. In this he was developing the idea of the early Danish and Swedish antiquaries who had discussed typological sequences, and was working parallel to John Evans who was busy classifying and arranging prehistoric flint and bronze implements in categories and in sequences. But he was not consciously indebted to the northern antiquaries or to Evans; his work on the material culture of primitive peoples and of prehistoric man was a direct offshoot of his work on firearms and his interest in biology. It was essentially from the study of firearms that "he was led to believe that the same principles must probably govern the development of the other arts, appliances and ideas of mankind".¹

To prove this thesis and to illustrate its truth he began collecting everything he could lay his hands on—first weapons and then boats, looms, dress, musical instruments, magical and religious symbols. Soon his house was filled from cellar to attic with these ethnographical and prehistorical collections, and in 1851 his collections were lent to the museum at Bethnal Green. Later the collection was moved to South Kensington, and finally to Oxford, where a special annexe of the University Museum was designed for it. The Pitt-Rivers collections were specially interesting for two reasons: in the first place they were arranged typologically and not geographically. Hitherto ethnographical and prehistoric collections had been set out by sites or countries. Pitt-Rivers disregarded find spots and arranged the material on a taxonomic and typologic basis. He thus performed an enormous service to comparative archaeology, and also demonstrated the values of the comparative ethnographic method in archaeology. This method had been hinted at by Nilsson; Pitt-Rivers showed how it could best be used, and how a comparison of prehistoric artefacts with contemporary ethnographical material made one the better able to appreciate the function and importance of prehistoric artefacts. Secondly, Pitt-Rivers began what may justly be called a sociological approach to artefacts, whether contemporary or prehistoric. He stressed that his collection

¹ Balfour, in (ed. J. L. Myres) *The Evolution of Culture*, v.

was "not for the purpose of surprising anyone, either by the beauty or value of the objects exhibited, but solely with a view to instruction. For this purpose ordinary and typical specimens rather than rare objects have been selected and arranged in sequence". He was stressing the point made later by Petrie in Egypt—the importance of the ordinary artifact, and the necessity of collecting complete collections of the material culture of contemporary or prehistoric societies. Pitt-Rivers and Petrie were the leaders of the revolution in archaeology which led it away from the contemplation of *art* objects to the contemplation of *all* objects. Archaeology as a study of art treasures was a nineteenth-century legacy from the late eighteenth-century study of classical antiquities. It survived to the Egyptian tomb robbers and in the persons of Botta and Layard, and it survived into the arrangement of the British Museum as late as the seventies of the nineteenth century. Pitt-Rivers and Petrie were mainly responsible for the transformation of the archaeological outlook from one of curiosity to one which was frankly sociological.

Pitt-Rivers explained his point of view in several lectures and papers, notably his lectures on *The Principles of Classification* to the Anthropological Institute in 1874, on *The Evolution of Culture* to the Royal Institution in 1875, his three lectures on *Primitive Warfare* to the United Services Institution in 1867, 1868, 1869, and his lecture on *Early Modes of Navigation* to the Anthropological Institute in 1874.¹ He urged everywhere the study of *all* material objects of human culture, and their arrangement in typological sequences. His general principle for the arrangement of all these sequences was that the "seemingly more primitive and generalised forms were the nearest natural forms". He was not, however, a slave to this principle; he recognised the existence of periods of degeneration. He was startled by the achievements of Upper Palaeolithic Art, compared with the degeneration in art that succeeded it, and by Mariette's discovery that the most faithful and true sculptures in Egypt were the work of the IIIrd dynasty. He appreciated the work of Sir John Evans on British coins, and also Schliemann's Trojan finds illustrating the devolution of the human-face motive. He stressed, moreover, that formal

¹ These lectures, together with an extract from Balfour's address to the British Association in 1914 on Pitt-Rivers, were reissued under the editorship of J. L. Myres as *The Evolution of Culture and other Essays* (Oxford, 1906).

identities between objects were not enough, and taught a lesson begun by Nilsson, that before identity could be proclaimed, functional as well as formal identity must be proved. Pitt-Rivers believed implicitly in progress. He arranged his artifacts in this belief, and was sure that they demonstrated the truth of progress. But he remained uncertain of the possibility of forecasting the course of progress. "Progress is like a game of dominoes," he declared in the closing words of his 1874 lecture on *The Principles of Classification*. "Like fits on to like. In neither case can we tell beforehand what will be the ultimate figure produced by the cohesions: all we know is that the fundamental rule of the game is sequence."

As Colonel Lane-Fox, Pitt-Rivers carried out many excavations in various parts of England and Wales, and in some of these he worked with Canon Greenwell. After 1880, when he inherited the Rivers estates, comprising over 29,000 acres of land, including much of Cranborne Chase, he began a remarkable campaign of excavation which comprised camps, villages, cemeteries, barrows and ditches. Among the most famous of these excavations between 1880 and 1900 were the now classic settlements of Woodcuts and Rotherley, Woodyates, Wor Barrow, Bokerly Dyke and Wansdyke.¹ Unlimited by considerations of finance, time, or labour, Pitt-Rivers was able to make these excavations a model of scientific excavations. He indulged to the full his talent for organisation and research, and his experience gained during a long career of soldiering and administration. His excavations were well organised and thoroughly carried out. He demanded and obtained the highest standard of accuracy and care from his assistants and workmen. All his excavations were on a grand scale, and in most of the sites he studied he examined them comprehensively and completely down to the bedrock. For the technique of the mid-nineteenth century, which consisted of digging a hole in a barrow to find the primary interment and any associated grave goods, Pitt-Rivers substituted the *total* excavation of sites. He stressed the importance of stratigraphical

¹ Pitt-Rivers was one of the first archaeologists to concentrate on dwelling places rather than barrows. Casson has cited the excavation of an Iron Age village at Standlake, Oxon, by Stone in the eighteen-fifties as the first example of a non-barrow dig in England. Of course settlement sites were more difficult to excavate than barrows, less rewarding, but more fruitful in common things.

observation, and of the necessity of recording the position of everything found. He caused accurate plans and sections and detailed drawings and descriptions to be made of all his excavations, and constructed models of all the main sites.

Pitt-Rivers carried into prehistoric archaeology the insistence on the importance of common things which had marked his collections of ethnographical and prehistoric objects. "Common things are of more importance than particular things, because they are more prevalent," he wrote in 1898. "I have always remembered a remark of Professor Huxley's in one of his addresses: 'The word *importance*', he says, 'ought to be struck out of scientific dictionaries; that which is important is that which is persistent.' Common things vary in form, as the idea of them passes from place to place, and the date of them and of the places in which they are found may sometimes be determined by gradual variations in form. There is no knowing what may hereafter be found to be most interesting. Things are apt to be of the greatest value in tracing the distribution of forms. This will be admitted when it is recognised that distribution is a necessary prelude to generalisation."¹

Pitt-Rivers believed in, and practised, the prompt and complete publication of all his excavations. In four privately printed sumptuous volumes produced between 1887 and 1898, under the title of *Excavations in Cranborne Chase*, he set and achieved the highest standard of archaeological publication.² From his publications, and the models in the Farnham Museum, it is possible at the present day to re-create every stage of his excavations. In every way his work was of the highest standard; in fifteen years he transformed excavation from the pleasant hobby of barrow digging to an arduous scientific pursuit. He was at the time far in advance of his contemporaries; indeed Collingwood has suggested that in many ways he was in advance of modern archaeological technique, and many a modern excavator must envy his resources and the way in which he was able to carry out his excavations with the precision, thoroughness and discipline of a military operation.

Pitt-Rivers has been claimed as the father of scientific

¹ *Excavations in Cranborne Chase*, iv (1898), 27.

² Volume i (1887), ii (1888), iii (1892), iv (1898), and an Index by St George Gray (1905).

excavation. He himself said in 1875 that "science is organised common sense", but his excavations were not only organised common sense—they had, too, an element of genius, and it is this that makes him stand out in nineteenth-century English archaeology. We must not consider Pitt-Rivers's techniques of excavation in isolation—as something completely new that developed suddenly between 1880 and 1900. His is the development of the techniques at which Cunnington and Colt Hoare were aiming at the beginning of the century, and Canon Greenwell is the link between the early nineteenth-century antiquaries and Pitt-Rivers.¹ To this tradition Pitt-Rivers brought his own flair for efficiency and administration and his own particular genius.

Pitt-Rivers was regarded at the time by many as an eccentric and his standards of excavation and publication impracticable and even undesirable. He was, in many ways, eccentric, with his Gypsy School, and his Larmer Tree grounds, where llamas and yaks roamed wild, and public concerts were given free on Sundays. But all this was a part of a deliberate policy to interest the ordinary person in prehistory and ethnography. He wanted to give prehistory a place in the general framework of everyday education. He built a museum at Farnham, which contained the finds from his Cranborne Chase digs as well as the models of the excavations, a second collection of ethnographical objects which he began assembling as soon as his first collection was set up at Oxford, and the nucleus of a folk museum. He sensed the gradual shift of power to the educated masses and insisted they must be educated aright. "What they lack is history," he said. "They must learn the links between the past and present."

6. Petrie

Petrie, although his name will always primarily be associated with excavations in Egypt and Palestine, began as a British archaeologist, visiting and surveying prehistoric monuments in southern Britain. A work on *Stonehenge* published in 1880 is among his earliest publications. When he transferred his attention to Egypt in 1881 and the following years he took with him a thorough training as a surveyor, as well as the strongest beliefs in

¹ Randall (*History in the Open Air*, 11) rightly emphasises the part played by Cunnington and Greenwell in the building up of the tradition of English field archaeology.

archaeological excavation as a painstaking and laborious research not to be undertaken lightly or hurriedly. In his autobiography he claims that when he was eight he was horrified at the description of how a Roman villa had been unearthed in the Isle of Wight: "I was horrified", he wrote, "at hearing of the rough shovelling out of the contents and protested that the earth ought to be pared away inch by inch to see all that was in it, and how it lay."¹ However true it be that, even as a child, he was, as he claimed, "in archaeology by nature", it is certainly true that he was one of the main founders of archaeological method as we now understand and practise it.

When he first went to Egypt, Petrie was full of criticism for Mariette, whom he describes as having "most rascally blasted to pieces all the fallen parts of the granite temple by a large gang of soldiers", as only visiting his excavations once in a few weeks, and being completely hoodwinked by his *reises*, or overseers, who salted his sites with *antikas* bought in Cairo to keep up his interest in the sites!²

Petrie at first had little assistance from anyone. His techniques were self-devised. He complained that he got no training or criticism from the Egypt Exploration Fund and wrote in 1885 that his work was "in fact a case of breaking new ground in archaeology". His method was based on the following principles: First, care for the monuments being excavated, and respect for future visitors and excavators; Second, meticulous care in excavation and the collecting and description of everything found; Third, the accurate planning of all monuments and excavations; Fourth, the full publication of all excavations as soon as possible. These were principles that Petrie was formulating consciously as the basis of his method in 1889. They were an enormous advance on anything that had existed before in Egypt. It was only after these principles had been carried out, argued Petrie, that the archaeologist could begin to write history; then he would see the importance "of everything found" and in setting out his story would use "all materials of inscriptions, objects, positions and probabilities".

Furtwängler had appreciated the value of painted and decorated pottery as an archaeological chronometer. It was Petrie who first showed how unpainted pottery could be used in the same way,

¹ *Seventy Years in Archaeology*, 8.

² See also p. 162 *supra*.

if studied in detail. He had begun to appreciate this fact in the eighties when working at Naucratis, but it was his excavation of a Palestinian tell, Tell el-Hesi, in 1890, that drove home the lesson to him. Here he had to deal with sixty feet of occupation debris. The debris was clearly divided into occupation levels each characterised by pottery types, although, as Petrie also realised, the pottery types and the levels did not coincide exactly. Petrie was able to synchronise some of the Tell el-Hesi levels with Egyptian dynasties and so build up an absolute chronology of the whole sixty feet of occupation. Here was a model of the study of a stratified occupation site—a model far in advance of the work at Troy.

In addition to the definition of general method, Petrie made three specific contributions to the development of prehistoric archaeology. The first, the extension of comparative archaeology by cross-dating Egyptian and Greek remains, we have already discussed. The second was the scientific study of the materials used by prehistoric and protohistoric man. The archaeologist, he declared, must study "all details of material, colour, fabric and mechanical questions of tools". He laid down the basis of artifact analysis in archaeology, a method which has yielded such interesting results in the twentieth century. In the third place, Petrie invented the idea of sequence-dating. His work at Naqada, Ballas, Diospolis Parva and elsewhere forced him to study material that dated before the Ist dynasty. How was this to be given an absolute chronology? It would have been possible to give guesses in years assisted by dead reckoning from the thickness of deposits. In Europe prehistoric material had been arranged in typological sequences, and had been classified according to its place in the subdivisions of the three-age system. Petrie had little respect for the system of prehistory built up by subdivisions of the three-age system with its illusory appearance of exact chronology. "Such a piecemeal plan is well enough for a beginning," he wrote in 1904, "but it is not capable of exact definition: it is cumbersome, and it does not express the relation of one period to another."¹ He therefore devised a system of sequence dates which he applied to the typological sequence of prehistoric pottery worked out at Diospolis Parva. He started his sequence with S.D. 30, supposing very rightly that he had not as yet discovered the earliest prehistoric

¹ *Methods and Aims in Archaeology*, 127.

material in Egypt, and carried on to dynastic times at S.D. 80. For each of the fifty numbers in his series he used the contents of twenty graves.

This system of sequence-dating was first set out in *Diospolis Parva* and in *Methods and Aims in Archaeology*. Petrie regarded it as one of his main contributions to prehistory and wrote of it: "This system enables us to deal with material which is entirely undated otherwise; and the larger the quantity of it the more accurate are the results. There is no reason now why prehistoric ages, from which there are groups of remains, should not be dealt with as surely and clearly as the historic ages with recorded dates."¹ This was too optimistic a hope. Actually Petrie's system has not been adopted widely outside Egypt, and for a very good reason. Although it appeared to be something very new, it was merely a restatement of a typological method of dating in a new guise. But it was objective in the sense that, although it, too, was not in terms of absolute chronology, it did set out a relative chronological sequence in terms of numbers and not in terms of ambiguous phrases like "Early Bronze Age" or "Mycenaean". It was a system that answered no questions of absolute chronology, but it begged no questions, which is after all the first requirement of an objective chronology.

It is difficult to overestimate the contribution made to archaeological method in the last quarter of the nineteenth century by Schliemann, Pitt-Rivers and Petrie. It would be no exaggeration to say that, with the experience of the Danes and the Swiss behind them, they forged the essential technique of archaeology. The remarkable thing is that these giants were not really contemporaries, both Schliemann and Pitt-Rivers were nineteenth-century figures, but Petrie was only forty-seven years old when Pitt-Rivers died, and lived to complete over seventy years of archaeological work, and to be the mentor and inspiration of many archaeologists who are still young to-day. It is, then, interesting to recollect now that Pitt-Rivers visited Petrie's excavations in Egypt, and so did Schliemann. Schliemann came in 1888 with Virchow, and Petrie described him as "dogmatic, but always ready for facts".

¹ *Methods and Aims in Archaeology*, 129.

7. *Prehistoric Archaeology in 1900*

The changes in prehistoric archaeology between 1870 and 1900 had been very many. They had been due to the discoveries described in the previous chapter, and to the changes in technique which have been discussed in the present chapter. The result was that by the end of the century prehistoric archaeology was in a more assured position. But recognition was still slow, and the appreciation of the existence of ancient civilisations was gradual. Petrie has recorded a meeting of the Board of Studies in History of the University of London in 1901 when the teaching of the history of art was discussed: "The scope of ideas on the Board may be seen by the proposal that the history should begin at A.D. 1500. I remarked that we had done nothing but copy since then: dead silence. Then someone proposed 1400."¹ In the early eighties there were rumours and discussions about the establishment of a Professorship of Archaeology in Oxford; Sir Arthur Evans was being advised to stand until he learnt that it was to be confined to classical archaeology. In a letter to Freeman in 1883 he said "to confine a Professorship of Archaeology to classical times seems to me as reasonable as to create a chair of 'Insular Geography' or 'Mesozoic Geology'".²

But although recognition in universities was slow, the subject was by now thoroughly established, and a broad picture of man's prehistory was available to be repeated in textbooks and encyclopaedia articles. This picture was well summarised by Arthur Evans in his inaugural lecture at the Ashmolean in 1884. Prehistoric archaeology, he declared, in words often to be borrowed, "has drawn aside the curtain and revealed the dawn. It has dispelled, like the unsubstantial phantoms of a dream, those preconceived notions as to the origin of human arts and institutions at which Epicurus and Lucretius already laughed, before the days of biblical chronology. It has taught us that, at a time when Britain formed still a part of the continent of Europe with an arctic climate and another fauna . . . Man was already in existence here, fashioning his flint weapons to aid him in his struggle against the sabre-toothed tiger, or the woolly-haired Rhinoceros. It has tracked him onwards to his cavern homes, and dragged into the light his bone harpoons, and the flint scrapers wherewith he

¹ *Seventy Years in Archaeology*, 179.

² Quoted in Joan Evans, *Time and Chance*.

cleaned the shaggy hides that served for his apparel; it has unearthed in the grottoes of the Dordogne the earliest known relics of other than the purest utilitarian art; it has followed him through the later periods of the Age of Stone in Europe, whetting and polishing his tomahawks, or delicately flaking out his arrowheads and lanceheads. It has dived to the lake bottoms, and reconstructed his pile dwellings; it has fished out the very clothes he wore, the spindle whorls that spun their threads, the cereals that he had learnt to cultivate—nay the very cakes he ate, and the caraway and poppy seeds wherewith he flavoured them. It has shown us the beginnings of metallurgy characterised in this quarter of the globe by the use of implements of Bronze; and by the discovery of great prehistoric cemeteries like that of Hallstatt, in Upper Austria, it has revealed to us that at the close of this first Age of Metal, ancient lines of commerce were already bringing the Mediterranean shores into direct connexion with the Baltic lands of fur and amber."¹

This passage has been quoted *in extenso* because it represents so well the picture of prehistoric Europe current in the late nineteenth century. Underlying it is no clear statement as to whether the cultural changes described in Europe were the result of spontaneous development in Europe or of "influence" from outside Europe. The absence of this clear statement is in its way characteristic, because in the late nineteenth century many prehistorians were content to describe the archaeological record in their own area without following out the implications of their interpretations. This is not to say that prehistory was not already being interpreted in terms either of independent invention or diffusion; it was, and indeed had been since the days of Worsaae and Nilsson. But the disputes in prehistory were not yet consciously in terms of independent invention or diffusion. It would take the development of ethnological theory, and particularly the hyperdiffusionist excesses of Elliot Smith and his followers, to do this. Meanwhile, the issues were disputed on specific grounds, and often enough, as in the typical passage quoted from Evans's inaugural lecture at the Ashmolean, were not regarded as of vital importance. The late nineteenth-century archaeologists were so excited by the discoveries they were making about man's early past that they spent little time in explaining their interrelations.

¹ Quoted in Joan Evans, *Time and Chance*, 270-271.

The discoveries in the Aegean brought into being a local controversy regarding the origin of the prehistoric cultures in terms of independent invention and discovery. To some the Mycenaean civilisation was the work of Aryan invaders from the north: Schuchhardt, Furtwängler and Leaf identified these with the Achaeans and thought they developed their splendid civilisation on the Greek mainland. Leaf's *Companion to the Iliad* (1892) is typical of this viewpoint. To others the Mycenaean civilisation was the work of invaders from outside Greece—the Phoenicians according to Meyer and Busolt, the Carians according to Köhler.

Reinach, Sergi, Evans and Montelius saw the disputes about the origin of the Mycenaeans as part of a larger dispute about the origin of European prehistoric culture in general. Salomon Reinach attacked the theory of *ex oriente lux* in his *Le Mirage Oriental* (1893) and his paper on "Les Déesses nues" published in the *Revue Archéologique* for 1895, declaring that European prehistorians had for long overrated the "eastern" elements, whether they were labelled Phoenician, Semitic, Aryan, or Carian, and that the Mycenaean civilisation, like other European prehistoric civilisations, was native. Montelius was a strong exponent of *ex oriente lux* and in his *Orient und Europa* (1899) argued forcibly for the derivation of all European culture from the ancient East. "At a time when the peoples of Europe were, so to speak, without any civilisation whatsoever," he wrote, "the Orient and particularly the Euphrates region and the Nile valley were already in enjoyment of a flourishing culture. The civilisation which gradually dawned on our continent was for long only a pale reflection of Oriental culture."

Arthur Evans and Myres saw that the theories of Reinach and Montelius were not mutually exclusive. In 1895 Sergi published his *La Stirpe Mediterranea*: it was translated into German two years later and published in England in 1901. Sergi argued for a Mediterranean race with its centre in North Africa, which spread all over the Mediterranean region. Arthur Evans's *Eastern Question in Anthropology* (1895) accepted this, and distinguished a great Anatolo-Danubian province within which Aegean civilisation was a local manifestation, owing something both to the ancient East and to Europe. In his *Prehistoric Man in the Eastern Mediterranean* (1895) Sir John Myres developed

the same basic thesis. In his own words, written much later: "To recognise adequately the eastern background of European origins is no *Mirage Oriental*. The 'independent European element' is not extinguished by its own capacity for assimilation."¹

In the thirty years from 1840-70 archaeologists had been naturally imbued with the prevailing doctrines of evolution and progress. They saw in the prehistoric sequence both a mirror and a proof of evolutionary progress. Some, like Pengelly, as we have noted, had doubts about the universality of progress. These doubts were shared by other archaeologists in the last quarter of the nineteenth century, themselves affected by current doubts of progress.

"Evolution ever climbing after some ideal good
And Reversion ever dragging Evolution in the mud,"

wrote Tennyson in *Locksley Hall Sixty Years After* (1886). The late nineteenth-century archaeologist began to wonder whether, in presenting his picture of technological progress, he was making objective observations confirming the general Victorian belief in progress or whether he was merely projecting into prehistory his own belief in evolutionary progress. The question which the late nineteenth-century prehistorian began to face was this: Did archaeology really prove the cultural progress of man, or was it merely being used to demonstrate that cultural progress?

The archaeological reason which impelled prehistorians and others to reconsider their views of history as a single progressive development was the discovery of civilisations which had perished. Work in Egypt and Mesopotamia was revealing the extent of the ancient civilisations there and giving suggestions of a civilisation—that of the Sumerians—even earlier than the Babylonians of history; Schliemann had revealed behind classical Greece the barbaric splendours of Mycenaean civilisation and of Troy II. Petrie had revealed the predynastic civilisation behind dynastic Egypt. Perhaps the most startling archaeological discoveries were those of Upper Palaeolithic Cave Art. At first this art was disregarded, but as soon as its authenticity and age were accepted

¹ *The Cretan Labyrinth*, 284. "Ex Oriente Lux" became the motto of a group of German archaeologists at the end of the nineteenth century. It was the title of a series edited by Hugo Winckler, published at Leipzig, and is the text of Fick's *Vorgriechische Ortsnamen* (1905).

it raised a great problem with regard to human history. Here was a remarkably naturalistic and competent art which flourished in south France and north Spain at least ten and probably twenty thousand years ago—and which came to an end. The Upper Palaeolithic artists had no successors and their artistic impetus died out. This was surely a tremendous example of retrogression or degeneration even more dramatic than the decay of the Egyptian, Mesopotamian and Aegean civilisations, or the decay of British civilisation in post-Roman times which Pengelly had noted. Even Pitt Rivers, for so long a staunch supporter of evolutionary progress, began to have doubts whether progressive evolution was the fact that emerged out of the archaeologists' study of man's earliest past. We have already quoted him as saying "that the fundamental rule of the game is sequence". Sequence was very different from evolution. Evolution meant progress—gradual but persistent improvement, a change always in the complexity of technical equipment and the mastery of man over the environment. Sequence meant developments that might retrogress as well as progress. This was the essential difference between late and early Victorian thought in so far as it related to early man. We see the same change in Victorian anthropologists. At first they were dominated by evolution, but later they began to admit doubts as to its universality as an explanation of culture. In his *Primitive Culture* (1871) Sir E. B. Tylor described his aim as "to sketch a theoretical course of civilisation among mankind" which he calls "a progression-theory of civilisation", but he also admitted that "culture gained by progression may be lost by degradation".

The last quarter of the nineteenth century saw the prehistoric archaeologists entertaining not only doubts as to the story told by prehistory, but also as to whether archaeology was the only source for information of man's earliest past. The claims of the physical anthropologist, the philologist and linguistic palaeontologist, and the student of survivals had been made earlier in the century, but were being given a most serious hearing in the last few decades. Classifications of races had been formulated since the pioneer work of Linnaeus and Blumenbach. The second half of the nineteenth century saw new classifications by de Quatrefages, Broca, Virchow, Sergi, Ripley and Deniker. Ripley's *The Races of Europe: A Sociological Study*, and Deniker's *The Races*

of Man: an Outline of Anthropology and Ethnography were both published in 1900, and are good examples of the way in which classifications and deductions valid in physical anthropology were being used as sources in prehistoric archaeology. We have already mentioned how Evans and Myres used Sergi's Mediterranean race in formulating their theories of prehistoric origins in the Mediterranean. Dr John Beddoe's pioneer studies in British anthropology had involved equations between prehistory and physical anthropology. In his excavations in Wiltshire Dr John Thurnam had been impressed by the seeming co-ordination between long barrows and long skulls, and round barrows with round skulls. He published these conclusions in papers in *Archaeologia* and the *Memoirs of the Anthropological Society* between 1860 and 1870, concerning what he termed "the two principal forms of ancient British and Gaulish skulls". His ideas were set out more fully in the great work *Crania Britannica* produced jointly by Davis and Thurnam in 1865.

The comparative study of languages as a key to the early human past goes back to the now famous Presidential Address delivered by Sir William Jones to the Asiatic Society of Bengal in 1788, in which he pointed out similarities between Sanskrit, Greek, Latin, Celtic and German languages and suggested they all were to be derived from a common mother tongue. Bopp confirmed this suggestion in his *Vergleichende Grammatik* (1833-35) using the term Indo-Germanic for the whole group of languages, a term first suggested by Klaproth in 1823. Max Müller suggested the term Aryan as an alternative and this superseded Indo-Germanic as the label for this group of languages. In 1847 Baron Bunsen read a paper to the British Association in which he attempted a classification of mankind as a whole on the basis of language. "At that date", writes Haddon, "it was taken for granted that the study of comparative philology would be in future the only safe foundation for the study of anthropology."¹ It was also put forward as a safe foundation for the study of prehistory, and a whole edifice of supposed fact about prehistoric man was created on linguistic deduction. The prehistory of Europe was seen in terms of the movements of Finno-Ugrians and Aryans. A typical summary of this linguistic prehistory is to be found in Isaac Taylor's *The Origin of the Aryans: An Account*

¹ *History of Anthropology*, 96.

of the *Prehistoric Ethnology and Civilisation of Europe* (1890) and Huxley's essay on *The Aryan Question and Prehistoric Man*, first published in the same year.

Equations were made not only between race and prehistory, and between language and prehistory, but also between race and language. The Aryans became Nordics not only in the minds of "blond-beast" theorists like Boulainvilliers and Gobineau but to serious scholars. Many deplored this confusion. Max Müller wrote: "To me an ethnologist who speaks of Aryan race, Aryan blood, Aryan eyes and hair is as great a sinner as a linguist who speaks of a dolichocephalic dictionary or a brachycephalic grammar."¹ There were plenty of sinners among anthropologists and archaeologists at the end of the nineteenth century. Max Müller might write firmly that "there ought to be no compromise between ethnological and phonological science"²; but it was very characteristic of the writings of the turn of the century to attempt a compromise between the data derived from language, physical anthropology and archaeology, and to deny the primacy of archaeology as a source for prehistory.

In the works of the great Welsh scholar, Sir John Rhys, we have an excellent example of the attempt to unravel the early history and prehistory of a country by means other than archaeological ones. Rhys's *Celtic Britain* was first published in 1882; *The Welsh People*, which he wrote with D. Brynmor Jones, in 1900. Both works had a most considerable influence in and outside Wales; they entirely neglected archaeological sources, and set out an interpretation of the prehistory of Wales entirely in terms of three "peoples" distinguished on linguistic and literary grounds, the Iberians, the Goidels and the Brythons.³

The idea of studying survivals in modern culture from ancient civilisations and from man's barbaric prehistoric past was first put forward by Tylor in his *Primitive Culture* in 1871. In studying survivals we must distinguish two kinds—the fossils and the functioning survivals. The study of "folklore" as the study of survivals was initiated by W. J. Thoms, who invented the

¹ *Biographies of Words and the Homes of the Aryans* (1888), 120.

² *Report of the British Association* (1891), 787.

³ In Sir John Edward Lloyd's *History of Wales* (1910) we see the first recognition of archaeological sources. Lloyd tries to marry the archaeological and non-archaeological sources, and equate the three ages of the archaeologists with the peoples of Rhys and Brynmor Jones.

word in 1846, and whose articles in the *Athenaeum* and *Notes and Queries* have already been referred to. The great exponent of survivals in folklore as a source of information about the past was Sir G. Laurence Gomme, whose *Folklore as an Historical Science* was published in 1908. Otis T. Mason in his *The Origins of Invention* (1895) and *Woman's Share in Primitive Culture* (1895) studied survivals in material culture. Field systems, villages, tribal organisations and primitive law as they exist at present, or existed in the historic past, were also studied as survivals from prehistoric times and as sources of light on man's prehistoric past. Sir Henry Maine's *Ancient Law* (1861) and his *Village Communities in the East and West* (1872), Fustel de Coulanges's *La Cité antique* (1872) and *The Origin of Property in Land* (1891), de Laveleye's *Primitive Property* (1874), Gomme's *The Village Community* (1890), Seebohm's *The English Village Community* (1883), *Tribal Custom in Anglo-Saxon Law* (1902) and *The Tribal System in Wales* (1904) are good examples of this approach, also evident in the writings of Maitland, Vinogradoff and Arbois de Jubainville.¹ The subtitle of Maine's *Ancient Law* is significant: it is "its connection with the early history of society and its relation to modern ideas".

This is what so many were hoping to get at the end of the nineteenth century—the early history of society. Archaeology provided the technological development of man, but historians of man as a whole wanted more than this; they wanted the story of the development of man's society, and his mental and moral ideas, as well as the tale of the development of his tools. This is why they turned so avidly to the conclusions of physical anthropology, linguistics and survivals, and why they sought an answer in the study of modern primitive peoples. The early Danish archaeologists, such as Worsaae and Nilsson, had stressed the value of comparative ethnography to the archaeologists, and this cannot be disputed—the archaeologist cannot understand the uses of artefacts except by reference to modern primitive usages. It is of course possible to point to similarities in form and function between prehistoric and modern artifacts. Intoxicated by these appropriate material parallels, and thwarted by their inability to speak of prehistoric man's social and spiritual culture from the

¹ For an admirable critique of these studies see Peake, *The Study of Prehistoric Times*, 10–11.

archaeological record, some archaeologists and anthropologists fell into what may be termed the comparative ethnographic fallacy, namely the belief that identity between the material culture of prehistoric people and that of modern primitives implies an identity of social and spiritual culture. The acceptance of this fallacious equation is implicit in the writings of Lubbock. Some went even further, and accepted as facts the evolutionary sequences of social and spiritual culture which ethnologists had produced, and treated them as objective facts with which the facts of archaeology must be equated.

The nineteenth century was full of classifications of man's past based on the supposed sequence of economies. Coleridge had declared that "the progress from savagery to civilisation is evidently first from the hunting to the pastoral stage",¹ and, as we have seen, Sven Nilsson distinguished four stages in the human past, the savage state, when man was a hunter, fisher and collector, secondly the herdsman or nomad stage, thirdly the agricultural stage, and fourthly civilisation, which he defined in terms of coined money, writing and a well-organised state of society, with labour divided among different professions. These four stages of the evolution of society were first published in Sweden in 1838, and became widely known in England after 1867, when Lubbock translated them and edited them in *The Primitive Inhabitants of Scandinavia*.

Sir Edward Tylor recognised the Danish system of the three ages, and agreed that the Stone Age was the beginning of man's prehistoric development, but proposed to distinguish in the human past three different stages of Savagery, Barbarism and Civilisation. In his *Anthropology: An Introduction to the Study of Man and Civilisation*, first published in 1881, Tylor defines barbarism as beginning with agriculture, and civilisation with writing. These terms were more exactly defined by Lewis H. Morgan, the American anthropologist, in his *Ancient Society: or, Researches in the Lines of Human Progress from Savagery through Barbarism to Civilisation* (1877). Morgan thought the system of the three ages "extremely useful for certain purposes, and will remain so for the classification of objects of ancient art", but he thought that methods of subsistence provided better lines of division: "the great epochs of human progress have been identified more or less directly", he

¹ *Lit. Rem.*, ii, 327 (1836).

wrote, "with the enlargement of the sources of subsistence". He proposed to distinguish seven ethnic periods, as he called them, as follows:

1. Lower Savagery, from the emergence of man to the discovery of fire.
2. Middle Savagery, from the discovery of fire to the discovery of the bow and arrow.
3. Upper Savagery, from the discovery of the bow and arrow to the discovery of pottery.
4. Lower Barbarism. This stage began with the discovery of pottery which, to Morgan, was the line between Savagery and Barbarism, and ended with the domestication of animals.¹
5. Middle Barbarism, from the domestication of animals to the smelting of iron ore.¹
6. Upper Barbarism, from the discovery of iron to the invention of a phonetic alphabet.
7. Civilisation, from writing and the alphabet onwards. Morgan distinguished the Ancient Civilisations of Egypt, south-west Asia, and classical lands, and the Modern Civilisation of which he himself was a product.

Morgan envisaged these seven ethnic periods as providing a progressive sequence of man's cultural evolution, and he thought that this sequence developed naturally in different regions. All showed, he believed, "the unity of the origin of mankind, the similarity of human wants in the same stage of advancement, and the uniformity of the operations of the human mind in similar conditions of society". "Mankind", he declared, "commenced their career at the bottom of the scale and worked their way up from savagery to civilisation through the slow accumulations of experimental knowledge." While Morgan believed in the natural nature of progress, he realised the seven ethnic periods were hemotaxial and not contemporary all over the world. "The *condition*", he wrote, "is the material fact, the *time* being immaterial."

We are not concerned here with a critique of Morgan but with

¹ These distinctions relate to the Old World. Morgan had slightly different criteria for his periods in the Americas.

realising the nature of his classification. It was based primarily not on archaeological evidence but on the comparative study of modern primitive peoples, the arrangement of these existing economies and societies into an evolutionary sequence, and the projection of this hypothetical sequence into the prehistoric past. Morgan's scheme was not securely anchored to the analysis of artifacts and ancient remains. Nevertheless it fitted in tolerably well with the archaeological record, and achieved popularity because it was used by Engels in his *Origin of the Family* (1884), where he declared: "Morgan is the first man who with expert knowledge has attempted to introduce a definite order into human prehistory." This claim of Engels cannot for a moment be sustained, but it is interesting because Morgan's method had, by the use of his projection of hypothetical periods, appeared to surmount the apparent limitations of archaeology—that it could speak only of man's material culture in the early human past.

This seems one of the main reasons why in the latter part of the nineteenth century scholars were turning to sources like physical anthropology, linguistic palaeontology, the study of survivals and comparative ethnology as prime sources for the early human past. They were disappointed that archaeology did not provide them at once with the early history of all aspects of society. A second reason was that prehistoric archaeology was still young and had not yet established its paramountcy among other sources for information about the prehistoric past.

But if they were sensing its limitations, and searching the possibilities of other sources, the late Victorians had few doubts as to the nature of archaeology. It was science, one of the newest of the new sciences which were, they thought, the peculiar triumph of Victorian knowledge. Boyd Dawkins declared triumphantly in 1874 that "Archaeology, by the use of strictly inductive methods, has grown from a mere antiquarian speculation into a science",¹ and Joseph Anderson defined archaeology at the turn of the century as "the science which deduces a knowledge of past times from the study of existing remains".² Charles Boutell in his *Manual of British Archaeology* had declared

¹ *Cave Hunting*, viii.

² The first words of Petrie's *Methods and Aims* were: "Archaeology is the latest born of the sciences."

archaeology to be history, and Tylor spoke of prehistory and history taking their proper place in the general scheme of knowledge. But to Joseph Anderson the story that the archaeological record revealed "is not history. History deals with events and incidents as manifestations of human motive and action; archaeology deals with types and systems as expressions of human culture and civilisation",¹ and the views of Anderson and Boyd Dawkins were most representative of late Victorian thought.

The late nineteenth century did not effect a synthesis of the facts of prehistoric archaeology with history itself. The late Victorians still talked of archaeology *and* history. They had created archaeology—out of history, out of antiquarian studies, and out of geology, but it was this last element that remained the strongest. The historical component was largely forgotten, and we should remember that in any case these were the days when history itself was being claimed as a science. There yawned at the end of the nineteenth century a gulf between the archaeologists who looked to geology and natural science, and thought themselves scientists, and those who looked to history and art, and thought themselves students of the humanities. It is this gulf which, as Taylor has recently insisted,² caused prehistoric archaeology, and the archaeology of the "classical" and other protohistoric civilisations, to develop separately.³

There was much excuse for this state of affairs. The methods of archaeology—excavation, museum analysis, field work—seemed to take it away from the humanities. Then so many of the great nineteenth-century archaeologists were in the first instance geologists and natural scientists. It is, then, quite understandable that, although in its origins archaeology had owed most to historians and antiquarians, throughout the nineteenth century it became more and more allied with science. It was left to the early twentieth century to right this balance, to effect a synthesis of prehistory and written history, to create what Elliot Smith called "human history"—the full story of man, the cultured animal.

¹ *Chambers's Encyclopædia* (1922), s.v. Archaeology.

² W. W. Taylor, *A Study of Archaeology*.

³ It is the gulf which still yawns in the prehistoric scholarship of many countries—France, for example.