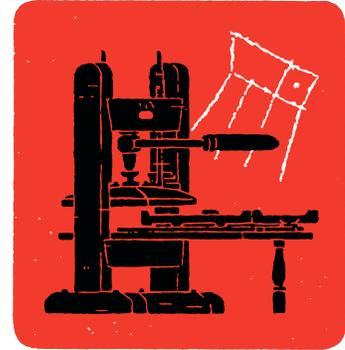
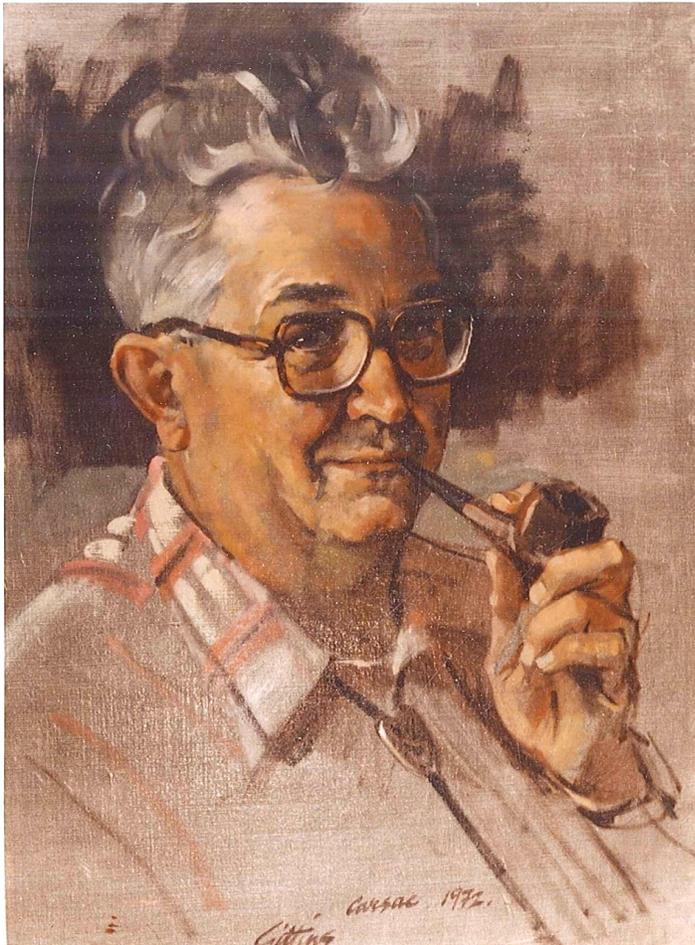


James Sackett



FRANÇOIS BORDES and the Old Stone Age



Portrait of Bordes by Alvin Giddens

François Bordes was one of the pre-eminent Paleolithic (Old Stone Age) archaeologists of the latter half of the Twentieth century. His name is associated with no outstanding discoveries comparable, say, to the revolutionary findings his era saw being made in sub-Saharan Africa. And indeed he mostly labored in what was already the most thoroughly plowed field of Stone Age research, that is, the rockshelters of the Perigord region of Southwestern France. These are not caves but rather large and deep cavities exposed along its limestone valley walls, which served as highly favored places for repeated Paleolithic occupations that left a succession of rich archaeological horizons incorporated into the shelter infill deposits as they gradually built up through erosion by the elements. The Perigord's rockshelter sites traditionally constituted the standard frame of reference for our knowledge of the Middle Paleolithic (that is, the Mousterian, largely

attributed to Neanderthals) and the Upper Paleolithic (that is, the block of stone tool traditions such as the Aurignacian and Magdalenian, which are conventionally attributed to fully modern *Homo sapiens*). The importance of Bordes' work lies then not so much in the archaeological novelty of his findings but rather in the innovative approach he brought to excavation itself and in the equally innovative method he brought to analyzing the stone tool assemblages his digging brought to light. These had a profound effect upon the conduct of Paleolithic archaeology throughout Eurasia, and in some instances well beyond. In addition, perhaps more by chance than design, they played a significant role in the controversies over archeological method and theory that were overheating New World archaeologists at the time.

Other Anglo-Saxon archaeologists (as the French insist upon labeling their British and North American counterparts) might still be found who could create a richer portrait of François Bordes than I am capable of. Nonetheless, I did happen to see much of the man during the last two decades of his life; I frequently worked in close association with members of his research team; and for six years he served as my joint principal investigator of the excavations I directed at the open-air site of Solvieux. I enjoyed cordial--but by choice never intimate--terms with him, and indeed it was not until after his death that I came to appreciate fully his personal qualities and professional accomplishments. In any case, what follows is only brief sketch of the man and his work. My treatment of Bordes himself may have particular interest, since most such attempts one encounters in the literature are exercises in hagiography whose subject emerges more in a form reminiscent of an heroic equestrian statue than of a complex living man. Gratifyingly, the only public monument to Bordes is, in fact, the tram station accessing his old laboratory at the University of Bordeaux. On the other hand, my treatment of his work must of necessity be too brief to do the job adequately. Interested readers can, however, find more thorough exegeses of the topic in several of my publications which remain readily available.

BORDES' HISTORY

Henri Louis François Bordes was born in 1919 in the Perigord region of Southwestern France, a landscape of lofty cliffs, dense stands of forests, and green valleys richly scattered with charming villages that retain much of their medieval and Renaissance character. However, as evidenced by the appalling slaughter represented by the heart-wrenching lists of names on their World War monuments, they no doubt seem more charming to passing tourists than to local inhabitants whose roots go deep. Bordes grew up as a member of the provincial bourgeoisie, comfortable and well educated but still—like the rough-hewn peasantry that surrounded it—solidly grounded in the traditional culture, prejudices, and manners of the region. His enthusiasms as a boy involved the then famous Stone Age romance *La Guerre de Feu* (largely forgotten until given new life in the recent American film *The Quest for Fire*) and a bicycle, on which his far-ranging explorations of the Perigord soon gave him a expert knowledge of its natural history and archaeology. His talents were recognized early, and at the remarkably young age of fifteen he was issued a permit to excavate a rockshelter site at Le Roc de Gauvaudun. In 1936 he entered

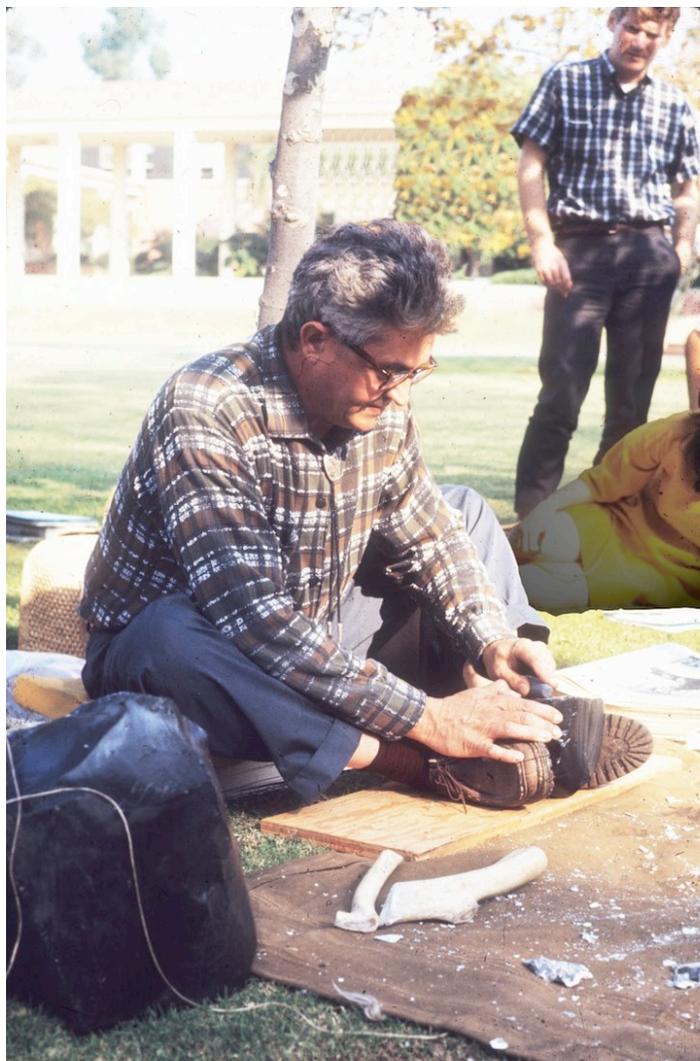
the University of Bordeaux, majoring in geology and biology, and meeting his future wife—Denise de Sonneville-Bordes—who was to become a leading expert on the Upper Paleolithic.

He joined the military at the outbreak of World War II, but was soon demobilized after the fall of France--a devastating event that no doubt fed his aggressive Francophilia. In 1942, when the Germans extended their occupation into to the entire country, he joined the Resistance as a Maquisard. He sometimes worked in obscurity as a coal miner to avoid being sent to Germany as a labor conscript. He referred to this period as the time he truly worked in the “underground”. Then came a dangerous and eventful two years as a fugitive fighting in the Maquis, followed by another brief stint in the military which was brought to an end by a serious grenade wound. At the end of the war he returned to university life, gaining his doctorate at the Sorbonne (1951) with a classic dissertation that substantially revised the loess and gravel geo-archaeological sequence of the Paris Basin. In 1956 he became a Professor at the University of Bordeaux, quickly gathering about him a coterie of students and researchers in what was to become the famous Institut du Quaternaire. And so Bordes found himself reigning over the conduct of Paleolithic archaeology in the Perigord as a whole. And he remained doing so until his untimely death by heart failure twenty-five years later during a visit to the University of Arizona, Tucson. He was buried in the Perigordian village of Carsac, where he had long maintained a house that served both as his retreat and the operations center for his excavations.

BORDES THE MAN

Bordes was one of a kind. He was a sturdy, vigorous, restless, volatile man. He could be gruff, rough, curmudgeonly. His occasional, seemingly uncontrollable outbursts of anger could be as unsettling as they were inappropriate to the occasion. And he practiced a congenital but milder testiness as a kind of art form. To cite but one example: when knapping flint tools, at which he was an expert, it always seemed to upset him when he inevitably arrived at the point of exhausting the flake core, which he would then hurl away in disgust accompanied by a yell of *merde!* Although often bad-humored he was nonetheless very good-natured, quite a different thing, especially when he found himself in the high spirited atmosphere of a student site crew, or when cleaning a stratigraphic section with one or two experienced colleagues, or, again, when relaxing in the company of a French household which shared his own cultural background and outlook. Especially among the last he could be charming, thoughtful, and a delightful conversationalist. His habitual suspicion of other nations never extended to their individual citizens (with the possible exception of the English, who he seems never to have forgiven for Trafalgar and Waterloo), whom he welcomed both as crew members and students. And he was uncommonly generous in sharing his data with any researchers, regardless of nationality, once they had mastered some degree of expertise in the trade.

I used the word “trade” advisedly because a key facet of Bordes’ character was his pride in being a journeyman field worker, or *homme de terrain*. Given his natural contrariness, this often



Bordes knapping flint at UCLA, circa later 1960's.
The author is standing behind him.

tempted him to assume the role of an intellectual philistine. (He once listed for me all of the classic books which he would not want on a desert island, taking care to omit all of the great works most of us at least claim we would want to have.) Yet he was in fact a cultivated man. Even more importantly, he was a well-recognized writer of science fiction, under the pen-name of Francis Carsac, inspired of course from his beloved home in the Perigord, publishing seven full-length novels and a score or so of short stories. Interestingly enough, these were translated into several European languages (they were especially well-received in Russia) but were never destined to appear in English. I am not a fan, let alone a judge, of the SciFi genre, but remember being struck by one of his stories which dealt with a mysterious planet whose inhabitants were totally immobilized by apathy. I cannot help but suspect that the premise especially intrigued, and at some level disturbed, Bordes, who to my knowledge was never incurious, who possessed an almost tactile sensitivity to the world

around him, and who was never bored. It bears adding that his career as a popular writer both contributed to, as well as reflected, the remarkably straightforward, unpretentious, and solid style of his scientific writings, which to my mind have been rivaled by few prehistorians.

Although, on balance, I regard Bordes as a remarkable man, and certainly an eminent scientist, there are two forms his egoistic character took which to my mind marred, or at least tempered, his achievement. One was the factionalism he promoted in French academic life, especially marked by his failure to come to terms with the Paris school headed by André Leroi-Gourhan. True, it is difficult to imagine two such different characters: the boisterous, hard-headed geologist from Bordeaux and the somewhat diffident, highly cultured, but perhaps over imaginative scholar from Paris. Yet, despite their intellectual differences and remarkably

different styles, the two schools played complementary roles and had much to learn from one another. Some sort of reconciliation between the two would have lent a holistic hybrid vigor to French Paleolithic archaeology that it sorely lacked. A more far-sighted man than Bordes would have realized this.

The second fault I see is no doubt related to the first. This was his apparent belief that he had hit upon the right approach to Paleolithic archaeology and that, consequently, the job of his successors would largely be to work out those of its permutations he himself had neither time nor inclination to pursue. Seemingly, these were likely to amount to little more than intellectual embellishment of what he himself had already achieved. As he once put it to me: “Look Jim, I’ve built a town, laid out the streets and sewers and electric lines, and constructed the houses; if you want to put a fountain in the town square, feel free to do so”. Of course, this kind of hubris is not rare in eminent archaeologists habituated to dominating their field, but it is sooner or later defeated by science’s restless need to more or less constantly be taking new, often unexpected turns. In the face of this one has the choice either of encouraging one’s followers to wrestle with new advances or of moving to a new venue where one’s expertise, customary practices, and reputation still carry great value. Possibly this accounts for Bordes’ ultimately devoting his last three years of research not to the Perigord but instead to early man sites in the Murchison Basin of western Australia.

THE TRADITIONAL APPROACH

We turn now to Bordes’ professional contribution. It is a daunting task to review this for readers who may not know, nor particularly care about, such questions as to whether a bladelet qualifies for the Dufour type unless it is longitudinally twisted and carries *retouche alterne*, or whether the Perigordian II stage is in fact a “phantom” industry produced by the contamination by frost-heaving of Perigordian I levels with the Aurignacian I levels which overlie them, or, again, whether the so-called Levalloisian culture of the Paris Basin is in reality no more than a Mousterian techno-complex whose distinctive tool shapes are the mechanical by-product of having been made on a special kind of flake. Probably no corpus of archaeological literature could seem more arcane and inaccessible to the non-specialist than the 170 articles on such topics which make up the bulk of Bordes’ oeuvre. However, he was not simply one of the ablest and knowledgeable practitioners of his trade, but in addition the creator of an innovative approach whose impact, as we have noted, influenced Old Stone research well beyond the Perigord itself.

The nature of this contribution is most easily defined in terms of the background against which it arose, which we shall call the “traditional”—in contrast to the “Bordesian—era which followed. Given the taphonomic complexity of their sites and the recalcitrant, one might say defiantly alien, nature of Paleolithic stone tools, traditional researchers understandably adopted a form of what I call straight archaeology. In other words, they pursued a craft-like involvement with their

archaeological record, cultivating a narrowly empirical preoccupation with the typology of its artifacts and the make-up of the deposits from which they derive. Dismissing paleoethnological interpretation as premature speculation at best, they concentrated their efforts upon industrial systematics--that is, the task of defining typological variation among their stone tool assemblages—and, in turn, taking advantage of whatever light could be shed by stratigraphy, constructing taxonomic skeletons of how the lithic industries they segregated organized themselves over time and space. Their aim, in short, was (and, in truth, largely still remains) not to interpret but rather to map the lithic industrial variation that structures the Paleolithic archaeological record.

Of course, all disciplines involved in digging up and making sense of the past are based in one form or another of straight archaeology. But what set traditional Paleolithic research apart was the assumption—quite understandable in the light of its empirical grounding in stone tools and stratigraphy, along with its strong historic connection with geology—that the Stone Age archaeological record should be attacked in a manner that emulated the approach that earlier had been used by paleontologists in unraveling the fossil record. They might use words like “culture” and “tribe” (tribus) but these signify no more than that stone tools were made by people and that these people were no doubt primitive. Yet, given the quite special nature of their archaeological record, it is not surprising that efficacy (perhaps science’s most powerful tool when confronting empirical ambiguity) prompted them to regard stone implements at least metaphorically as if they were indeed fossils. The most singular expression of this idiom of research was their attempt to ground systematics upon fossiles directeurs, that is diagnostic artifact types like Mousterian hand-axes, Gravette points, and Solutrean laurel leaves, whose restricted distributions as “index” or “zone” type-fossils in the archaeological record were believed to delineate the major “cultural” traditions (perhaps more accurately termed as “industrial blocks”) of Paleolithic times.

Now, the notion of the fossile directeur tended to foster two more implicit assumptions. The first was that the Paleolithic record paralleled the paleontological record in such a manner that we can expect to find a one-to-one correlation between its archaeological levels and the obvious natural stratigraphic units of the site deposits in which they are found. The second was that any given industrial complex, like any given paleontological complex, should be more or less invariant in the manner in which it expresses itself: in other words, that a specific stone tool tradition should give rise to but one characteristic type of industry in any specific block of time and space in the archaeological record.

Given our current heightened knowledge of the complexity of the Paleolithic record, it is easy to see how the paleontological model greatly restricted the grasp of traditional prehistorians. Assuming to find a lock-step correlation between cultural and natural stratigraphy, they deemed it sufficient to excavate a site only in terms of its more obvious stratigraphic units--that is, the often thick zones of relatively homogeneous sedimentological composition which may indeed represent the major episodes of its depositional history but which at the same time can in reality

incorporate several distinct archaeological horizons that often vary significantly in the details of their typological make-up. As a result, excavation techniques themselves inadvertently mixed these horizons and consequently blurred the archaeological record. At the same time, the differences which distinguish one archaeological industry from another were framed largely in terms of the presence or absence of the key fossiles tool forms. Indeed, so little attention was paid to the supposedly “banal” areas of typology that a large proportion of the stone tool artifacts excavators initially brought to light never found their way back to the laboratory at all. One of the sadder but highly informative tasks any historian of Paleolithic archaeology should undertake is to occasionally leave off reading the old site reports and instead excavate the spoil heaps of their excavators in order to discover the amount and kind of lithic material so many of them discarded.

As a result of all this, the artifact assemblages recovered for any given time period within any given region tended to exhibit a homogeneous and quite stereotyped aspect. And when in turn those from different time periods were compared they inevitably appeared to exhibit fairly distinctive qualitative breaks reminiscent of the manner in which index fossils delineate temporal phasing in geological history. Thus it might be said that prehistorians literally created an empirical archaeological record that did in fact parallel the paleontological record in consisting of a more or less straightforward succession of industrially invariant stages which could be simultaneously identified and defined by a series of index fossile forms. It should be obvious that all this promoted a kind of methodological circularity whereby prehistorians were capable of observing as excavators only what they had already assumed to be true as taxonomists. To be sure, I exaggerate, over-generalize, and in the case of a handful researchers probably fail to do justice to their efforts. Furthermore, the above paragraphs qualify as no more than informed supposition on my part, since traditional Paleolithic archaeologists were too insular intellectually and at the same time too bonded by shared if unstated assumptions ever to write explicitly about issues of method and theory. In any event, I believe it fair to state that the traditional logic of inquiry promoted excavation techniques and classificatory procedures that dramatically reduced the ability of researchers to perceive novelty or to appreciate the more subtle kinds of variability and alternate patterning that might find reside in Paleolithic archaeological deposits.

THE BORDESIAN APPROACH

However tedious, the above recital of what traditional Paleolithic archaeologists did should serve to greatly simplify my abbreviated treatment of what Bordes himself did. Although his achievement was massive, it is fair to say that he did not so much reinvent the field as reformulate it: a matter of clearly perceiving the shortcomings of his predecessors and developing new methods to compensate for them. To be sure, as in any science, many of Bordes’ contemporaries were on the same track, conducted research along similar methodological lines, and in certain respects probably surpassed him. Nonetheless, the bulk of his own contributions,



Bordes excavating at rockshelter of Pech de l'Azé with student crew.

promoted no doubt by his stature in the field and the impact of his personality, so branded European Paleolithic archaeology in the period 1959-1980 that it is altogether fitting that the era bears his name. My treatment of the substance and bite of the Bordesian approach will no doubt seem colorless to those readers unfamiliar with rockshelter sites like Laugerie-Haute, Combe Grenal, and Pech de l'Azé, with which his name is directly attached, let alone with those, such as the Harvard excavations at the Abri Pataud, which he generously placed in the hands of foreigners. A serious reader might wish to consult the most exhaustive, if now outdated, treatment available of the world according to Bordes, *Rockshelters of the Perigord* (1980) authored by Henri Laville, Jean-Philippe Rigaud, and myself. Here, however, we must be content with the barebones of the story.

To a great extent, what might be called the Bordesian approach simply involved a strategy designed to greatly enhance the overall quantity and quality of the data that were realized from the archaeological record. Excavation techniques now took on the character of stratigraphic

dissection whereby artifact assemblages are segregated not according to a site's major depositional blocks but instead with reference to the specific "occupational" horizons and the minimal sedimentological units discernible within them. (In some instances this has led to a nearly ten-fold increase over the divisions traditional excavators earlier recognized at the same sites.) All lithic material, including unused tool blanks and industrial debris is saved along with the standardized tools shaped (as a rule) by retouch. They are richly documented with respect to their provenance and, equally important, accompanied by representative samples of faunal, palynological, and sedimentological data carefully segregated with reference to their respective archaeological horizons. This latter information is employed to develop a chronostratigraphic approach to space-time systematics, wherein the design of regional space-time schemes entails a holistic level-by-level correlation of the site stratigraphies involved, founded as much upon their paleoenvironmental contents as upon the artifactual contents of their occupational horizons. While it might fairly be argued that such advances in the technology of archaeological research are simply refinements of earlier sampling and analytic procedures, their combined effect has nevertheless been to cause a leap in the degree of resolution with which prehistorians are able to observe and control the archaeological record. Moreover, although they constitute common practice today, this was not the case a half-century ago.

No less important, this effort is complemented by a new approach to systematics that has had an equally profound effect. In brief, the traditional concept of the qualitative *fossile directeur* has been set aside in favor of the notion that it is the relative frequencies of several tool types viewed in the ensemble, rather than the presence or absence of a few of them viewed individually, that is essential to refined systematics. Again, the basic idea is not new. But it was Bordes who first saw clearly that translating the notion of what one regards as industrially diagnostic into quantitative terms was not simply a matter of counting, but in addition that it required the introduction of two new elements into the methodology of systematics. One is that artifact typology and the ordering of archaeological assemblages must constitute distinct procedures: in other words, that the definition of an assemblage's formal content must be operationally distinguished from its genetic affiliations to other assemblages in space-time systematics. As we have seen, this distinction was never clearly made in the traditional approach, since it was the inherently circular role of *fossile directeurs* to define simultaneously an assemblage's content and assign it within some larger ordering scheme. The second is that artifact classification must be extended to the entire range of formal variation occupied by recognizable tools rather than simply to those specific areas which potentially possess the greatest diagnostic value in assemblage ordering. This "banalization" of artifact classification in the form of standardized type-lists makes it possible for every artifact recognized as a purposefully fashioned stone tool can be assigned to a specific type category and subsequently be counted. Obviously, without this global typological inventorying, which was by no means consistently recognized in traditional systematics, quantitative statements about relative tool frequencies lose most of their meaning.

Armed with its comprehensive type-lists and some relatively simple techniques of statistical description, Bordesian systematics revealed that the archaeological record is a vastly more complicated affair than the traditional approach envisaged. Perhaps most importantly, we no longer conceive of it as comprising simple linear successions of stereotyped industries, but instead as a complex of highly polymorphic industrial complexes which can assume a variety of alternate expressions at one and the same time in any given region. Bordes coined the term *évolution buissonnante* (literally “bushy”, but perhaps better translated as “ramifying”, evolution) to account for this polymorphism.

INDUSTRIAL VARIABILITY AND THE MOUSTERIAN QUESTION

And here arises an unresolved issue regarding the nature of Bordes’ thought. The term *évolution buissonnante* obviously has a paleontological ring to it and in his mind may well have had more than simply metaphorical value. We must ask then whether Bordes’ innovations, regardless of how important, were still largely a matter of methodology rather than of theoretical perspective. In other words, did he in fact continue to embrace the traditional “paleontological” mindset? We can only give an equivocal answer, reminding ourselves in passing that the fact that ideas which cohabit in one and the same mind are not necessarily logically interdependent, and that some indeed may logically preclude others.

For one thing, Bordes enthusiastically promoted many projects which were framed by “anthropological” aims, such as the search for habitation structures by the eminent amateur prehistorian Jean Gausson, one of the pioneers of open-air Paleolithic research in a region hitherto dominated by rockshelter excavation. Gausson’s results ultimately led to my own work at the vast open-air station of Solvieux, at which Bordes himself served as co-collaborator. And one of our principal aims, or at least hopes, was to establish recurring clusters of horizontally segregated “tool kits” whose distribution might serve at least as a kind of structural grammar—if hardly an explanation—underlying the patterns of activities conducted on Paleolithic living floors. The idea was not a new one, to be sure, as attested by much of the work begun earlier by prehistorians in sub-Saharan Africa and



Bordes (note cowboy hat and bola tie), Dr. Jean Gausson, and author at the site of Solvieux, the largest excavation in an open-air deposit in the Perigord. Gausson, a pioneer in open-air research, was the last of the great French amateur prehistorians.

by the splendid open air sites being attacked at the same time as our own by Leroi-Gourhan's group in the Paris Basin.

Yet, on the other hand, and much more familiar to the North American audience, was Bordes' redefinition of the Mousterian as a polymorphic complex of four distinct tool complexes, or assemblage-types, which supposedly interstratified in the rockshelters of the Perigord and which he consequently assumed to be somehow quasi-contemporary. Here his thinking does seem to have paleontological overtones, since he argued that these assemblage-types had independent genetic connections to earlier, pre-Mousterian (that is, lower Paleolithic) industrial traditions. Since Bordes sometimes referred to these connections as being "cultural", it seemed to follow—at least when filtered through the mindset of anthropologically trained American archaeologists—that he must believe that the Mousterian assemblage-types themselves actually represented four distinct ethnic groups, or "tribes", which somehow shared the Perigord more or less simultaneously. And, their argument continued, would not a more likely explanation be that the assemblages in fact represented four different sets of spatially segregated activities conducted by one and the same ethnic group rather than more or less the same set activities practiced by four different ethnic groups? Finally, it did not require, at least among the "New" archaeologists of the time, much of a reductionist intellectual jump to see in Bordes' alleged position a parallel to the outmoded normative approach they attributed to traditional Americanist archaeology, in contradistinction to the processual approach they themselves advocated.

The most noticeable reaction to this particular instance of Bordes-style polymorphic variability (at least in the sense that bellowing smoke is the most noticeable attribute of a steam engine) was of course the so-called Mousterian "debate" between Bordes and Lewis Binford. To be sure, the basic issue it raises is of the utmost importance, particularly in prehistoric archaeology: how are we to distinguish in the archaeological record between what might be called activity and ethnicity, task and group, in other words, between what was going on and who was doing it? We need not attempt to untangle the course of the debate here, both because any Anglophone archaeological student has at least a rough idea of what it was about and because my own role as a participant was far from dispassionate or unbiased. Suffice it to say that, to my mind, the direction it took was in about equal measure salutary and damaging to the progress of archaeological thought in this country and to the manner in which New World archaeologists regarded their Old World colleagues.

In any event, our concern here is restricted to François Bordes, whose role in the business is marked by ambiguity and irony in about equal measure. For one thing, it was already starting to become clear to him, as well as many other knowledgeable researchers at the time, that his four assemblage types did not adequately organize Mousterian industrial variability. For another, fault lines were beginning to appear in his chronostratigraphic scheme for the Perigord which suggested that the assemblages were not in fact contemporaneous in the manner Bordes first



A horizontal exposure of a stone pavage found in the uppermost Paleolithic occupation floor at Solvieux.

thought. Hence the premise on which the debate was founded became questionable fairly early in the game. Then again, Bordes himself actually debated very little, at least in print, and then largely in English. For the issue was very much an Anglo-Saxon affair which was met with a mixture of bemusement, if not indifference, in France itself. Finally, to be frank, it would be interesting to know whether the “debate” itself was not something of a canard in any event. The only source for many of the popular notions Americans hold about it derive from the imaginative *Selbfestschriften* which Binford himself wrote as companion pieces to his own articles. This is not to say, I hasten to add, that Bordes would not have enjoyed a confrontation with Binford along personal lines. Although I never saw the two of them together, I suspect that Bordes was genuinely attracted to Binford, a man whose intelligence, brand of humor, combativeness, and need for self-assertion matched his own, and who—not being a Paleolithic archaeologist himself—could enthusiastically confront Bordes in a manner he might not tolerate from a fellow expert in the trade.

There is little I can add, since I can recall Bordes speaking to me only twice of the matter, and even then simply in the form of casual remarks tossed off during the course of fieldwork. The first time, he said that he in fact initially suspected that the Mousterian assemblage types were



A horizontal exposure of a stone pavage found in the uppermost Paleolithic occupation floor at Solvieux.

indeed activity-specific, most likely representing varying expressions that one and the same culture might take in its seasonal rounds; and that he only later abandon this view because he could find no corresponding differences in their associations with faunal assemblages, hearths, site organization, and so forth. I took this to mean that he viewed the question of Mousterian variability—as he did most Paleolithic issues—as a largely empirical matter which could only be resolved on strictly empirical terms. Here of course he was speaking in the guise of a straight archaeologist for whom paleoethnological interpretation could wait. The second time the subject came up his words took a more cynical turn: in short, that the debate's real value lay in the fact that it was an easily grasped and easily popularized matter that served to promote his reputation among Anglo-Saxon archaeologists and students who were otherwise too ignorant of Paleolithic archaeology to know the difference between a burin and a hand-axe.

BORDES IN AMERICA

Finally, a word is in order regarding Bordes' quite special relationship to America, which he first saw in 1959, revisited numerous times, and where he ultimately met his untimely death. His feelings about the USA, true to his contradictory character, were highly mixed. For he was

intensively chauvinistic, as we have seen, and in fact viscerally anti-American when it came to matters of foreign policy. Some of his remarks on the topic were callously insensitive, especially to those of us who had lost family, friends, and neighbors on French soil in two world wars. Yet his love of our land, as opposed to our nation, was itself altogether genuine. He was particularly attracted, as are many Europeans, by the vast and raw beauty of the Southwest, an attraction no doubt enriched by an almost juvenile nostalgia for the lore of the old Far West created by American cowboy novels and movies. And there was something in the openness of the American character he particularly enjoyed, perhaps, fairly or not, in contrast to the supposed reserve of our Anglophone counterparts across the ocean.

My impression is that Americans were more likely than his own countryman to find him in a relaxed, congenial, and receptive mood. In part this was due to the fact that he was as welcome in New York as Los Angeles, in Chicago as in San Francisco. And intellectual life in America probably seemed less factionalized and partisan than it is in France (a fact, as we have seen, for which he himself must bear some responsibility). Then too was the great esteem he enjoyed among American replicators of stone tools, stemming from his early association with Donald Crabtree. Knappers all belong to the same fraternity and practice a craft and mindset that overrides ethnic, linguistic, and even archaeological boundaries. As a result, Bordes was able to forge close and empathetic bonds with skilled colleagues who may never have known nor cared how the stratigraphy of Pech de l'Azé correlates with that of Combe-Grenal or why some researchers argue that conventionally recognized Early Magdalenian industries constitute an historically distinct techno-complex, the Badegoulian. I imagine he welcomed the intellectual vacation this afforded.

Bordes was fond of American students, and they reciprocated warmly. They found it difficult to resist someone who loved to show off, spoke so colorfully and amusingly in a strong French accent, all the while sporting a cowboy hat and a Far-West bola tie. But, at a more fundamental level, they felt the force of his scholarly dedication and eagerness to share his knowledge; they appreciated the fact that he took them seriously, even if they did not always have the preparation needed to follow the details of his argument. I believe this is why he took so much care in writing that lucid exposition of Mousterian archeology, *A Tale of Two Caves* (1972), which to my knowledge sadly never appeared in French..

Bordes' relations with his fellow prehistorians in America are not so easily summarized. While he was highly respected by nearly all—he was (and remains so thirty years after his death)--the center of controversy with respect to theoretical matters. I doubt he took it all too seriously. To be sure, he admired the accomplishments and vigor of North American archaeologists, and for obvious reasons followed developments in Paleo-Indian research closely. But he never bought into the proposition that archaeology is anthropology or it is nothing. And he thought the philosophical posturing of the New Archaeology of his era pretentiously naive. At the same time, he seemingly felt that that the problem was exacerbated by the fact that most American

archaeologists at the time era were to be found in academic departments, intellectual settings whose nature it is to promote theoretical controversy for its own sake (especially among those of its members who otherwise would have nothing of substance to say). I suspect he held, probably rightly, that American archaeology would be better served if the country possessed a semi-independent, empirically oriented scientific establishment comparable to the excellent Centre National de la Recherche Scientifique, which supplied the bulk of the full-time archaeological researchers in France.

Again, of course, I simplify. No one denies that a scientific engine cannot be driven without good theory, and Bordes knew this as well as any American. But to the end he remained a militantly down-to-earth homme-de-terrain. It was ignorance and intellectual pretension, not ideas, that he opposed. And if he sometimes struck Americans as being diffident and over simplistic in dealing with theoretical questions, we must keep in mind the dualistic nature of his character. For archaeological theory must have seemed rather dull in comparison to the rich store of novelty and imagination which he found in sharing the same mind with his alter ego, François Carsac. Perhaps Americans would have had a greater and more nuanced appreciation of François Bordes had they also been given the opportunity to know Francis Carsac. But Carsac, unfortunately, never spoke a word of English.

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