

# Japanese Ancestors and Palaeolithic Archaeology

*Received 23 October 1974*

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WHILE archaeology in North America is sometimes considered a set of techniques that may be utilized by researchers in various disciplines in the humanities and social sciences, archaeology in Japan is generally regarded as a separate discipline, within a rubric of historical studies. Unlike certain parts of Europe, where prehistoric archaeology, especially Pleistocene archaeology, has an identity separate from historical archaeology in the humanistic tradition, such a division does not exist in Japan, as Oi (1970: 3) confidently states: "It should be self-evident today that archaeology, including prehistoric archaeology, is part of history."

I have suggested elsewhere (Ikawa-Smith, in press) that Palaeolithic archaeology since 1962 has been characterized by a series of debates and controversies. In spite of this, Japanese archaeologists, including Palaeolithic archaeologists, appear to share a sense of identity as historians of a special kind, and it seems to me that it is this sense of identity which has actually created some of the controversies, including the debate over radiocarbon chronology and the preoccupation with the homelands of specific Palaeolithic industries.

## RADIOCARBON CHRONOLOGY

The debate over radiocarbon chronology in Japan centers on the principle of acceptability of physicochemical methods of dating in archaeology in general, and is only indirectly related to such problems as were raised by bristlecone-pine calibration. In other words, the debate is not over the issues created by what Neustupný (1970) called "the second revolution" in radiocarbon dating method; the problems that remain unresolved in Japanese archaeology are those created by the first revolution.

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The debate was initiated by Yamanouchi and Sato (1962), who argued that the overwhelming majority of artifacts recovered from Pleistocene deposits in Japan are not "Palaeolithic" but "non-ceramic Neolithic," and that pottery making in Japan, notwithstanding the radiocarbon dates to the contrary, did not begin until about 3000 B.C. I have discussed their reasons for the short chronology in some detail elsewhere (Ikawa 1964; Ikawa-Smith, in press), but the thrust of their thesis is that the chronological sequence of archaeological materials should be based primarily on archaeological data themselves, that is, relative stratigraphy and typological comparison, not on non-archaeological data such as those provided by geology, paleontology, or physicochemical analyses. The archaeologists' reliance on such non-archaeological data, in their view, indicated the lack of confidence in their own methodology. The same sentiment, it appears, may be detected in Tsunoda (1965, 1971), who does incorporate geological findings and radiocarbon dates in site reports (Nippon Kyusekki Kenkyu Iinkai 1968), but who nevertheless insists on defining "Palaeolithic" with reference to the particular way of life (which he has never specified), rather than by association with extinct mammals or by geological contexts in the Pleistocene.

It seems that the continued debate over the acceptability of the radiocarbon method in archaeology in Japan actually has little to do with the reliability of this chronometric method itself, but rather with the concern over the integrity of archaeology as an independent discipline. Thus Oi (1971), who neither minimizes the importance of geological contexts nor rejects chronometric methods altogether (cf. Oi, in press), has challenged Serizawa (1971*a*) to present evidence other than radiocarbon dates for placing the Chojakubo-Mikoshiiba assemblages of north and central Honshu at 12,000 B.P.–13,000 B.P. Sato (1970), on the other hand, carries the typological method to its logical absurdity. He assigns the Iwajuku I assemblage to ca. 4000 B.C. and backed blade assemblages to the fourth millennium B.C., with reference not only to the somewhat outdated chronological sequence for Indonesia as described by van Heekeren (1957), but also to the 4000 B.C. date assigned for "microlithic" arrowpoints at Tepe Jari in Iran, which in turn was derived from the "Ubaid I" in Lower Mesopotamia. An argument such as was presented by Sato, which is hardly acceptable to international scholarship in the 1970s, is understandable only with reference to the structure of Japanese archaeology as an independent historical discipline, whose *raison d'être* among historical disciplines consists of its expertise in reconstructing culture history by means of artifact typology.

It would be grossly unfair as well as inaccurate if I were to convey an impression that defense of the typological method has been the major preoccupation of all Palaeolithic archaeologists in Japan. For, during the two and a half decades since the Iwajuku excavation in 1949, most Palaeolithic archaeologists, adopting the strategy followed by Pleistocene archaeologists elsewhere, have vigorously sought cooperation of natural scientists and have incorporated chronometric data in their overview (e.g., Serizawa 1967, 1970*a*, in press; Sugihara 1967*a*; Yoshizaki 1967).

In recent years a large number of radiocarbon, fission-track, and obsidian-hydration dates have become available (Machida 1971, Suzuki 1973*a*, 1973*b*), and natural scientists themselves have presented chronological frameworks most useful for the study of the Late Pleistocene archaeological remains in the Kanto Plain (Machida and Suzuki 1971; Machida, Suzuki, and Miyazaki 1971). It is

noteworthy that such young scholars as T. Kobayashi, S. Oda, and C. T. Keally have made no reference to available chronometric data, even in their detailed examinations of stratigraphic sequences in the Kanto Plain (Kobayashi et al. 1971; Oda and Keally 1973*b*), except in a negative sense (Kobayashi and Oda 1971: 267; Oda 1969: 240). In a more recent study of edge-ground stone tools, however, Oda and Keally (1973*a*) base their typological groupings of these artifacts on chronometric as well as stratigraphic information. Perhaps the objections to the use of non-archaeological methods of dating will decrease in intensity among the younger generation of archaeologists during the seventies.

#### SEARCH FOR ANCESTRAL HOMELANDS

I have suggested above that archaeologists in Japan are united in their sense of identity as historians, whose unquestioned research goal has been reconstruction of culture history, including the lifeways of the past. It should be noted that there is little discontinuity in personnel, from Palaeolithic research to the archaeological investigation of later phases of prehistory and history. When the first Palaeolithic site was identified at Iwajuku, Gumma Prefecture, in 1948, the excavation was undertaken by the archaeologists who had worked previously with Jomon and Yayoi remains. Although a group of practitioners who specialize in Palaeolithic research is emerging, investigations of Palaeolithic sites are being carried out by archaeologists who have worked, and who continue to work, on Jomon, Yayoi, Kofun, and even Heian Period remains.

These are archaeologists who are accustomed to dealing with remains which could be referred to specific episodes in the history of the Japanese nation, or to possible precursors to the Japanese culture as we know it today. The Palaeolithic remains were therefore approached as a means of increasing the historical time-depth of Japanese culture and the Japanese people, and the inquiry into the cultural affiliation of Palaeolithic assemblages has become equated with the search for the original homelands of the Japanese people. Thus Sato (1970: 57), after suggesting the Southeast Asian origin of the backed blades in Japan, suggests the possibility that there may be certain racial and linguistic characteristics in present-day Japan which corroborate his conclusion. (Although Sato's reasoning may appear naive to anthropologically-trained archaeologists raised in the Boasian doctrine of the separation of race, language, and culture, it should be recalled that most Japanese archaeologists are historians, not anthropologists.)

In this context, the "Early Palaeolithic problem," one of the most controversial issues in Palaeolithic archaeology since the last decade, may be rephrased as a question, "How far back can we trace our ancestors?" The "Early Palaeolithic" in Japan has been defined as those assemblages which predate the base of the Tachikawa Loam formation in the southern Kanto Plain, ca. 30,000 B.P. (Serizawa 1968*a*, 1969, 1970*b*); or, stated another way, those assemblages which appear to predate the fully accepted sequence beginning with the Iwajuku I assemblage of the northern Kanto Plain. Serizawa (1971*b*, in press) feels that the materials recovered from the horizons below the Iwajuku I horizon at the Iwajuku site (called the Iwajuku "Zero" assemblages) and from the lowest strata at the Hoshino site, also in the northern Kanto Plain, may date back in excess of 130,000 B.P. Tsunoda (Nippon

Kyusekki Kenkyu Iinkai 1968) and Ono (1970, 1971) would assign some of the assemblages from Kyushu and western Honshu even as far back as the Mindel-Riss Interglacial. Many archaeologists and geologists are reluctant to accept these specimens as products of human activity (Arai 1971; Oi, in press; Sugihara 1967b), and continued research is needed to verify the "Early Palaeolithic" assemblages now known from over 40 sites (Ikawa-Smith 1974).

Through choice of descriptive terms, such as chopper, chopper-chopping tool, and proto-handaxe, general affinity with the Lower Palaeolithic assemblages of South and East Asia is implied by the investigators. Serizawa in particular has argued for close affinities of the Japanese assemblages with certain Lower Palaeolithic assemblages of North China, on the basis of frequency distribution of major classes of implements, relative frequencies of core and flake tools, and flaking techniques. According to Serizawa (1968b: 55-60, 1970b, 1971b), similarities to the lower strata of Locality 1 of Choukoutien are found in the Sozudai assemblage of north Kyushu and the lower strata of the Iwajuku "Zero" Horizon; certain characteristics of the upper strata of Choukoutien Locality 1 are found in the upper strata of the Iwajuku "Zero" Horizon and the Cultural Horizon 8 of the Hoshino site; and the Locality 15 assemblage of Choukoutien appears to resemble the Cultural Horizon 4 of Hoshino. Kato (1967, 1970), on the other hand, looks farther north on the Asiatic mainland and suggests that Cultural Horizons 3 and 4 of Hoshino should be traced to the Ust'Kanskaiya Cave in the Altai Mountains, and the Sozudai assemblage to the Filmoshki and Kumary collections in the Amur-Zeya basin.

For later development of the Palaeolithic industries in Japan, Serizawa and his associates emphasize technological continuity. Thus Serizawa has argued for the indigenous development of the classic blade technique in Japan (1970a) and attaches special importance to the occurrence of the knife-shaped and *kiridashi*-shaped implements below the Iwajuku I horizon at the Mukoyama site in northern Kanto (1971b). Similarly, Hiraguchi (1972) sees the "prototype" of the blade technique within Japan, and Yokoyama (1970) attempts to demonstrate the evolutionary trend of flaking technique from the "Early Palaeolithic" assemblages of Sozudai, Amidachi, and Hoshino to the late Palaeolithic assemblages, including those with parallel-sided blades and side-blow flakes.

Certain other archaeologists, especially those who are unwilling to accept the "Early Palaeolithic" of Japan on the basis of currently available evidence, feel, however, that the Late Palaeolithic assemblages based on blade blanks are traceable to the Upper Palaeolithic of Siberia (Oi 1965, 1968; Kato 1968). These assemblages, according to these authors, include not only those with regularly shaped, parallel-sided blades, but also such assemblages as Ioyama and Iwajuku I, where what certain other authors call "elongated flakes" occur with partially ground ovate tools. Oi (1965, 1968) argues that there existed parallel traditions in Japan during Late Palaeolithic times: the northern tradition with blade tools, and a tradition of Southeast Asian origin where tools were made of side-blow flakes. Sato (1970), on the other hand, argues for Southeast Asian origins for all the non-ceramic assemblages except those with microblades, which appear during the final phase of his "non-ceramic" period, and traces the origin of the Iwajuku I assemblage to the "Hoabinhian." All the backed blades in Japan, according to Sato, are derived from

the Toalian, and therefore they should be younger than the age assigned for the Toalian in Southeast Asia.

The "isolationist barriers" which Mulvaney (1969: 26) attributed to Australian archaeology up to the 1940s may be said to have existed for the Palaeolithic archaeology of Japan during the first decade after the excavation of the Iwajuku site in 1949. Initially, Japanese prehistorians even refrained from applying the universalistic term "Palaeolithic" to artifacts clearly recovered from Pleistocene formations, preferring "pre-Jomon," "non-ceramic," and later, "pre-ceramic." Appeals and initiatives to adopt a comparative perspective came from abroad (Befu and Chard 1960: 842-843; Maringer 1956*a*, 1956*b*, 1957*a*, 1957*b*), but Japanese investigators maintained that the most important task then was to establish a relative chronology of non-ceramic artifacts within Japan without preconceived notions derived from chronological sequences elsewhere (Serizawa 1955*a*, 1955*b*, 1958).

In contrast, Palaeolithic archaeology in Japan during the 1960s appears to have suffered from an overabundance of non-isolationist approaches, and I do not deny that I have had my modest share in this trend (Ikawa 1964; Serizawa and Ikawa 1960). This was expressed in attempts, some of which were mentioned earlier, to extend abroad the time-space correlation of Palaeolithic assemblages. As some of the authors admit (Sato 1970: 52; Oi 1968: 87), such correlations were not always based on firm evidence. Even when the comparisons present an appearance of precision, such as frequency distribution of major classes of implements or flake removal techniques as reconstructed from illustrations and verbal descriptions, I am doubtful that these could be taken as the valid evidence of "genealogical" relationships. It would be necessary at least to present non-random clusterings of attributes within major classes as evidence of similarity, but Palaeolithic archaeologists have not devised a method to distinguish the similarity due to genetic relationship from similarities which may be due to a variety of other reasons. Fascinating as it is to search for the ancestral homelands, I am afraid such a search will remain unfruitful for many years.

Although there are indications of a new "isolationist" mood in some recent works (e.g., Oda and Keally 1973*a*: 21), I hope Japanese Palaeolithic archaeology in the 1970s will see the development of a non-isolationist approach of a different order, one in which Palaeolithic remains are seen as the products of human activities and are compared with other such products elsewhere. There are some promising indications.

#### STUDY OF SPATIAL DISTRIBUTION

One of the encouraging signs is the growing interest in reconstruction of pre-historic human behavior from spatial distribution of artifacts and raw materials. I will mention three aspects of this interest: (1) spatial distribution of artifacts within a site; (2) spatial distribution of assemblages with different contents; and (3) distribution of lithic material from a given source.

Research concerning (1) and (2) above is referred to in Japan as the study of "settlement patterns," using the English term. The authorities cited in support of research goals and methodology by Oda and Keally (1973*b*) include Binford (1962, 1964), Binford and Binford (1966), Chang (1958), and Trigger (1968). The com-

bination is interesting, and Japanese workers, who did not have personal involvement in the processualist controversy of the 1960s in North America, might develop a comprehensive strategy, incorporating without prejudice useful methods and concepts from either side of the controversy. In any case, "settlement archaeology" in Japan appears to encompass a broader spectrum of methodology than what this term usually denotes in North America.

At the moment, only tentative attempts have been made, and they tend to be the application of the typological approach which characterized the study of artifacts in the preceding decades to spatial units delineated by concentrations of artifactual remains. Thus the investigators of the Nogawa site in Chofu City, Tokyo, believe that the study of settlement pattern should be based on typology of the units of artifactual distribution (Kobayashi and Oda 1971: 269) and propose for the site six types of such units (Kobayashi et al. 1971: 239-240):

- Type A: Units containing a large number of varied artifacts, suggesting a long period of occupation or concentrated occurrences of a variety of activities;
- Type B: Units containing certain types of artifacts only (less than one third of the total range of artifacts), suggesting a short period of occupation or occurrence of specialized activity;
- Type C: Units characterized by the predominance of cores and flakes, suggesting a workshop from which finished products have been removed;
- Type D: Units containing grinding stones, pounding stones, and anvils, and associated, in some cases, with fireplaces, suggesting cooking activity;
- Type E: Units containing large number of cores and flakes, suggesting production of special tools;
- Type F: Units containing a small number of artifacts and/or flakes, where specialized activities may have taken place, but which may simply be locales where these items were accidentally discarded.

The investigators then proceed to describe the assemblages obtained from ten preceramic horizons from the Nogawa site, with reference to the number and the type of units present. For example, the assemblage from Level VIII, the earliest occupation level, consists of one unit of Type E only, while the assemblage from the next level (VII) contains two units each of Type D and Type E. The same procedure is used in the discussion of the Musashino Koen site in the same Nogawa drainage basin (Oda and Keally 1973*b*).

It is clear neither to me nor to the critics of the Nogawa site study (Hayashi 1971*a*, Serizawa and Yokoyama 1973) how these "units" were delineated, how these units were reduced to the six "types," and how these "types" were applied back again to actual data. From available accounts, it seems that units were isolated by inspection, and the unit types are intuitively constructed ideal types. It also seems that this unit-type concept directly transforms actual segments of space, presumed to be activity areas, into "factors" representing specific human activity, bypassing the statistical procedure employed by Binford and Binford (1966). Furthermore, the activity areas as spatial subdivisions of a site, on the one hand, and the artifact groups

obtained from such units, on the other, are not always clearly differentiated by the authors conceptually: for example, Kobayashi et al. (1971: 239) and Oda and Keally (1973b: 45) use the term "unit," referring to a segment of space, interchangeably with "artifact group" or "artifact unit," a subdivision of an artifactual assemblage. It seems to me that these shifts in the levels of conceptual abstraction invite the confusion of issues evident in some of the critics (e.g., Hayashi 1971a: 259). Hayashi states that Type F could not exist because it is outside the definition of a "unit." A locale where only scattered artifacts occur surely does exist in actuality, and would form a spatial unit within a site: what is untenable is that this unit could not be reduced to a "factor," because human activities which may have taken place at such a locale are very difficult to infer and probably diverse in character.

By extending the unit-type concept to a number of sites in the Nogawa drainage, and controlling the approximate contemporaneity by geological horizon markers, Oda and Keally (1973b) made a tentative suggestion of a settlement system consisting of sites occupied for different purposes. Kato and his associates (1970, 1971), on the other hand, attempted to identify the differences and similarities in the nature of the sites along the Tokoro River in Hokkaido, through, among other things, microscopic examination of use-wear patterns and comparison of end-scraper attributes.

The metrical data presented by Kato et al. (1970) on 147 end-scrapers from three components in the Tokoro River settlement system with respect to eight attributes are most useful for comparative purposes, but the single between-attribute association analyzed by the authors (use-wear direction and the orientation of the tool) may indicate only that these tools were used at different sites for a similar purpose. If one is interested in demonstrating the existence of "communication between human groups" (p. 231), an operation beyond simple comparison of individual attributes is obviously needed. The existence of such a communication could conceivably be inferred from statistically demonstrated clusterings of a number of attributes. Some of these attribute clusters may be due to stylistic similarity, rather than functional or mechanical contingencies, although interpretation of the attribute clusters is not an easy task. In any event, the recommendation to use accumulative graphs (Hayashi 1971b: 8) for the purpose of explaining the inter-site variability along the Tokoro River is clearly misplaced. Accumulative graphs are nothing more than visual representations of differential frequency distributions of intuitively established tool types, and do not explain the nature of differences or of similarities. The potentials, as well as the limitations, of attribute analysis are yet to be fully explored and understood.

Reconstruction of "paleocommunication" is the stated objective of Suzuki's study (1973b: 242) based on physicochemical analyses of some 2000 obsidian specimens from a large number of sites in central Honshu. Suzuki determines the eruption ages of the volcanic rock by fission-track dating and measurements of uranium contents, and archaeological specimens are referred to the known sources with matching eruption ages. The time of utilization by prehistoric men is determined by Suzuki by the obsidian-hydration method and stratigraphic contexts at archaeological sites. In the case of the Nogawa site, where obsidian occurred in eight preceramic horizons, Suzuki (in Kobayashi et al. 1971: 252) observes that in early horizons (up to 16,000 B.P.), obsidian from the Mt. Hakone area (about 65 km

to the southwest) was utilized, while in horizons between 16,000 B.P. and 12,000 B.P., obsidian from several sources in Nagano Prefecture in the central mountainous area (about 140 km to the northwest) becomes predominant. After 12,000 B.P. into Jomon times, obsidian from the Hakone area gains predominance again.

The information provided by Suzuki (1973*a*, 1973*b*; in Kobayashi et al. 1971) is most tantalizing, because the implications of the "paleocommunications" have not yet been fully discussed by the author. Suzuki's findings are particularly interesting, because Kobayashi (1971: 308-309) states that the increasing use of the obsidian from the Nagano sources begins with the appearance at the Nogawa site (and probably also at the Tsukimino site in Sagamino Upland) of small backed blades. It seems to me that the shift in the sources of obsidian for the prehistoric inhabitants of the southern Kanto Plain (where both Nogawa and Tsukimino are located) may be interpreted not only as a phenomenon of "trade," as Suzuki (in Kobayashi et al. 1971: 250) suggested, but also with reference to adaptive shifts, climatic change, and changes in the patterns of seasonal population movement, population density, and extractive efficiency. Many questions come to mind, and problem-oriented research in the 1970s may provide some answers.

A trading network could certainly have been present, but if so, I would like to know what was being given in return for the obsidian. It also seems possible, however, that both the high mountains of Nagano Prefecture and the plain of South Kanto, 140 km away, were covered by the same population during a seasonal movement. To test this hypothesis, we would need to compare the assemblages from the Kanto Plain and Nagano with particular attention to functional differentiation and seasonality. I am also curious to know whether the increasing use of the lithic material from Nagano was in any way related to the rise in temperature during the Late Pleistocene, which would have made the mountainous regions more accessible to man and animals. To address both of these questions, we would certainly need more information about paleo-environment than is available today. If the increasing importance of the resources of the mountains was a factor, then I would like to know whether it was the populations primarily based in the Kanto Plain who expanded into mountainous areas for the newly available resources, or the populations who had lived as small groups in the mountains who expanded into the Kanto Plain. For this, we need to know more about stylistic continuity and temporal depths of archaeological assemblages in both regions, and the temporal depths, for such purposes, must be based on non-typological, independent data, if we are to avoid a tautological argument. I also wonder if the appearance of backed blades at this time could be interpreted as the indication of extractive efficiency, and if the increased extractive efficiency was a cultural response to changes in population pressure (Smith 1972: 10). We would first have to solve the long-standing problem over the function of the backed blades: whether they were "projectiles" (extractive tools) or "knives" (maintenance tools). I would then wonder why the closer source of obsidian in Hakone was utilized more often after 12,000 B.P., and speculate if the shift is related to smaller size of territorial range, increase in sedentism, and greater population density. These are the kinds of questions I wish to ask in the 1970s.

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