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About a week before writing this article, I received word that "Uncle Bob" van Heekeren died on 11 September 1974. I had heard earlier in the summer that he was sick and in the hospital, but I had not realized how sick he was. I had hoped that I would be able to have this review article out so that we could talk it over, but that was not to be. I had a good visit with him in Jakarta in June 1973, and I remarked to him that he looked and acted just like he had when I first met him in Manila in November 1953 at the time of the Fourth Far-Eastern Prehistory Congress. But appearances are deceiving, and now we have lost the last, to my knowledge, of the archaeologists who were active before World War II and who returned to activity after the war.

In this article I will review both the book and Bob himself, who, if not one of the world's best archaeologists, was one of its finer men. In recognition of his many qualities one book in his memory has already appeared. Edited by Gert-Jan Bartstra and Willem Casparie, it is titled Modern Quarternary Research in Southeast Asia. I review it elsewhere in this issue.

I reviewed the first edition of The Stone Age of Indonesia in AP III for 1959 (p. 37). It had appeared in 1957 as volume 21 of the Verhandelingen. . . . The use of the same title but a different volume number in the new edition may cause some problems to librarians, but there is no confusing the two editions, either in size or in content. The first edition contained 141 pages, had 24 text figures and 47 plates. The second edition has 247 pages, 51 text figures, and 105 plates.

Because van Heekeren was a friend and a sensitive person and because from a strictly archaeological point of view the first editions of his The Stone Age of Indonesia and The Bronze-Iron Age of Indonesia were not very good, I reviewed them myself rather than having another archaeologist tear them apart (1959a–b).
I feel rather strongly that many of the shortcomings of the earlier editions were not van Heekeren’s fault but rather were due to the quality and kind of prehistoric archaeology that had been done before World War II. While many American archaeologists and anthropologists were highly critical of Heine-Geldern’s methods of prehistoric reconstruction, I believe they felt him to be well organized and scholarly, and his reputation among many of the archaeologists of the rest of the world was high. Yet you look at his presentation of the research done on Indonesian prehistory (1945) and you are left with much the same feeling that you have after reading the first editions of *The Stone Age of Indonesia* and *The Bronze-Iron Age of Indonesia*. The words of Alfred Smith, in describing van Heekeren’s *Bronze-Iron Age* as a “laundry list” (1959: 335), are not far off the mark. Again, I do not think this was van Heekeren’s fault. Virtually all of the work that had been done must be treated as if it had to do with surface finds. The one archaeologist in Southeast Asia with the greatest reputation not only as a prehistorian but as a scientific excavator, Stein Callenfels (Beyer 1951), in his Indonesian work disregarded his own excavation data and interpreted the artifacts he found typologically rather than stratigraphically (Mulvaney and Soejono 1970: 166). Data of this quality, spread very unevenly over the tremendous area and distances of Indonesia, with its great variability (though with single threads stretching the whole way across and through), defy anyone to present a coherent, strongly data-oriented picture of Indonesian prehistory.

Only recently has the situation begun to change with respect to the lack of trustworthy data. It had changed very little for Indonesia by the time Heekeren had completed this revision. In a rather extensive bibliography he listed 203 items from 1957, the year of the first edition, to 1969, the latest year included. Of these items about 19 were on general topics not concerned with Indonesia, 126 were generally archaeological but not concerning Indonesia, 7 had to do with Sarawak, 20 were on paleontological subjects involving Indonesia, 16 had to do with fossil man in Indonesia, and 15 focused on aspects of archaeology or ethnography in Indonesia. Six of the latter were his own papers. In spite of the scantness of this addition to the data on Indonesia archaeology, this book is much better than the first edition. Why? For one thing, relatively speaking, Heekeren had available considerable new data on paleontology and fossil man which strengthened his section on Pleistocene prehistory. Unfortunately, this was the section that increased the least in size, from 54 to 78 pages. His section on the “Mesolithic” (which expanded from 42 to 75 pages) presented many more data on the flake and blade industries, including finds from new localities. His third section on the “Neolithic” had the greatest change, from 18 to 44 pages, much of this because he included in this “stage” a considerable amount of material that he previously had included in *The Bronze-Iron Age of Indonesia*. This change was due not to new data or better dating, but to a change in his own concepts stemming from the wider reading that he had done after completing the first edition. The improvement in the second edition over the first was not nearly so much improvement in the data available as it was improvement of van Heekeren’s archaeological knowledge.

The change in van Heekeren from 1957 to the day he ceased to be active was a sign of the man. He had an open mind. After returning to the Netherlands in 1956 he quickly realized that archaeology had changed a great deal during and after
World War II but that his knowledge of archaeology, other than on his own work in Indonesia, was about as it had been in 1941. He learned from his colleagues, he learned from his students, and he learned from his reading. His archaeological knowledge continued to increase until the end. His mind was somewhat like a sponge and he soaked up a great deal. Unfortunately, and unhappily, he did not continue with us long enough to reach a fully critical stage in his thinking.

The two main criticisms of the first edition of The Stone Age of Indonesia were its focus on data when there were very few reliable data, and the attempt here and there to say a bit about the prehistoric cultures which the artifacts represented, presenting this material as if it were known fact when actually it was reconstruction based on outdated anthropological concepts. In the new edition only a bit of this second flaw is left, and the first flaw is not nearly as obvious, as van Heekeren has been able to bring in more context and thus change a listing to a somewhat disjointed story. I feel the major shortcoming of this edition results from his open mind. He has accepted almost without question my suggested reconstruction of Southeast Asian prehistory (1969) and has tried to fit Indonesian data into that reconstruction. That van Heekeren would trust my thoughts so much I consider a great compliment. I do not trust them that much, however, and it would have been better if he had not done so. I still believe in my reconstruction, the latest version, that is, as each time I present it, it changes a bit. However, it is made up of a number of broad generalizations that cover a very wide area where there is much variety. Like most generalizations it probably will not apply well in local, specific situations. New data should be used to test it and to suggest possible changes or refinements, and should not be interpreted automatically as fitting.

The Stone Age of Indonesia is divided into two parts, the first on the “Cenozoic” (pp. 5–78), and the second on the “Post-Glacial” (pp. 79–206), followed by a 41-page bibliography. The first part (59 pages) is primarily concerned with Java, including coverage of fossil man, plus one section of 9 pages on Celebes. The second part has two major subdivisions, “The Mesolithic Stage” (pp. 79–153) and “The Neolithic Stage” (pp. 154–206). The “Mesolithic” includes a brief general discussion of the “Bacson-Hoabinhian” (pp. 83–85) of Mainland Southeast Asia and “The Hoabinhian in Indonesia” (pp. 85–92), then “The Sampung Bone Industry” (pp. 92–106), followed by two sections on flake and blade industries which are predominantly reported from Eastern Indonesia. In summarizing specific sites, Heekeren describes not only the site itself and its contents, but also the geological situation of and the identification of faunal material from the site. He starts out the “Neolithic” with a five-page reconstruction of the life of the people in Southeast Asia. He then presents a typological description of 14 different stone and shell artifacts, including where they have been found (pp. 159–170), and a very brief account of two neolithic manufacturing traditions still practiced (pottery and bark cloth). Accounts follow of excavations in Kendeng Lembu, Java (pp. 173–184), on the Karama River, Sulawesi (pp. 184–190; “Contents” mistakenly has this section starting on page 182), and the Melolo urn field, Sumba (pp. 191–198), and of surface finds of factory sites (pp. 198–199). He concludes with a summary of reconstructions by Heine-Geldern and others of the “Neolithic” of Southeast Asia (pp. 199–202) and then of recent evidence that does not fit these reconstructions (pp. 203–206).
Heekeren’s discussion of the “Lower Palaeolithic” has improved considerably as well as expanded (from around 9 to 15 pages). In the first edition he did not question the typological approach to the stone tools as the proper method of dealing with the data. In the second edition he becomes critical of this typological approach. In discussing the relationship between the Patjitanian and the Chopper-Chopping-tool tradition of Southeast Asia he says, “It is true that typologically the general aspect of the assemblages just mentioned seems comparable with that of the Patjitanian, but there is a complete absence of geological and/or palaeontological data. Assignment to the Patjitanian would therefore be based on no more evidence than the notoriously uncertain one of typology” (p. 46). In his discussion of the sites and tools of the Indonesian “Lower Palaeolithic,” he neglected the use of flakes which were indicated in the sites, while continuing the traditional emphasis on core tools. He noted that in Movius’s study of the Patjitanian tools, “More than 50% of the assemblage is formed of flakes showing signs of use” (p. 37). In the collections made in the Tabuhan area, one of the new areas for Patjitanian-like tools (discovered by Soejono in 1953), he remarks that “The flake-tools which do not always exhibit either a bulb of percussion or a striking platform, outnumber the pebble-tools by far” (p. 43). He then goes on to list the general types of tools found in the area; approximately 73% are flake-tools. These flake-tools and used flakes should be studied not only because of their apparent importance in the Patjitanian stone industry, but also because of their likely relationship to the important flake industries that follow in the Upper Pleistocene and Holocene of East Java and eastern Indonesia. Heekeren’s closing summary of this first part of the book, titled “In retrospect: the Pleistocene” (pp. 73-78), is a good presentation of what was known at the time of writing and of possible major changes in the picture which could come with future work.

Heekeren presents his “Mesolithic or sub-Neolithic Stage” (p. 79) without definition until he reaches the end of this section. He agrees that for the cultures in Southeast Asia to which the term “Mesolithic” has been applied, the term has lost much of its meaning. He feels, however, that in the absence of a better term, what is needed is a redefinition of “Mesolithic” as it fits Southeast Asia. He then proceeds to define it as including “a primitive horticulture,” “a shifting horticulture” as part of the economy, and the beginning of pottery manufacture and ground edge pebble tools (p. 150). These particular elements of his “Mesolithic” are so different from the elements usually attributed to the “Mesolithic” that it is difficult to equate the two. I do not feel that his use of the term is a satisfactory alternative.

Introducing his section on the “Mesolithic,” Heekeren talks of new races colonizing “... Indonesia by a series of successive waves of races of Homo sapiens” by boats or rafts (p. 79). The concept of “waves of migration” has been dropped for some time in hypothesizing population expansion and cultural spread and evolution in Southeast Asia. I think that this concept is no longer viable.

Before starting to present the Indonesian “Mesolithic,” Heekeren gives a brief summary of the “Bacson-Hoabinhian,” which he apparently equates with the “Mesolithic” in Mainland Southeast Asia. He says that “The Hoabinhian is definitely pre-ceramic or non-ceramic. ... Only in the upper layers are potsherds and polished axes sometimes found mixed with Hoabinh tools” (p. 84). Later he states that “Pottery, mostly bearing cord impressions and associated with crude
Hoabinh tools... made its appearance as early as 10,000 B.C.” (p. 149), and follows with the definition of “Mesolithic” which includes pottery manufacture. He nowhere talks of a second widespread culture in Southeast Asia and/or South China, contemporaneous with at least the latest levels of the Hoabinhian that had pottery, though perhaps this was what he was thinking. He also says that “The bearers of the Bacson-Hoabinhian throughout the Far East were not a physically homogeneous race, but Palaeo-Melanesoids were predominant” (p. 84). I would agree with the first part of this statement, but feel that we do not know enough of the skeletal material from Hoabinhian sites in South China to say definitely that Palaeo-Melanesoids were predominant.

The so-called Toalean Culture of southwest Sulawesi has one of the best known cultural sequences of Indonesia. Heekeren presents a detailed account of the work done there by Stein Callenfels and others, including himself, in his “Mesolithic” section (pp. 106–125). He disagrees with Callenfels’ classification of the culture with an Upper and Lower Toalean and presents his own division into Toalean I, II and III (Upper, Middle, and Lower), with a figure showing the respective tool types (pp. 113–115, Fig. 24). The Toalean is of particular interest as it is one of the few cultures in Southeast Asia with nicely flaked stone points and well made small flaked tools. Some of these tools are much like distinctive tool types in Australia. In 1969 John Mulvaney, of Australia, and Soejono investigated a number of the Toalean sites with the question in mind of the possible relationship between the Toalean tools and the similar ones from Australia. In a preliminary report on this work they said that “It is now an open question whether the Proto-Toalean or Lower Toalean is a viable cultural entity. . . . We note that van Heekeren (1949: 94) voiced doubts relevant to this issue. Further experience in the Maros region, where other Toalean ‘type specimens’ occur in unexpected distribution and assemblages, makes us urge caution in the use of the present concept of the Toalean culture, particularly regarding its stadial differentiation” (Mulvaney and Soejono 1970: 169). While this information was known to Heekeren before publication of this book, it came too late for him to make revisions of this section.

Heekeren was careful to acknowledge the work and publications of other people on specific archaeological sites, and he gave the references for the information he used. He was not as careful about references in his introductory and summary sections. At times he made a mistake in interpretation or fact taken from other sources, and it is impossible to check his source as he did not reference them. This is a problem that is certainly not Heekeren’s alone. Over a period of time when you have accepted another person’s thoughts and ideas on a subject that you are working with and thinking about, the thoughts and ideas of the second person become so entwined with your own thoughts that you are no longer able to distinguish the original source. In the process, statements of the other person that are presented as assumptions or in conditional terms sometimes come to be thought of as fact. In his retrospect on the “Mesolithic” (pp. 149–153), Heekeren begins a statement on early agricultural activities in Southeast Asia by saying that “It appears . . .” that various things were so. A few lines later in the same paragraph it is no longer expressed as a probability as he says, “Indeed, forest clearing and shifting cultivation took place here earlier than anywhere in the world, i.e. at the onset of the Post-Glacial approximately 9000 B.C.” (p. 149). We do not yet know that this is so.
I, and some others, think that it probably was so, and there is suggestive evidence for a date even earlier than 9000 B.C. However, that evidence is not strong enough to say more than "It appears to me. . . ." A non-referenced error in fact occurs in the paragraph following the one alluded to above. Heekeren reports a 10,000 B.C. dating for ground-edge pebble-tools in northern Australia (pp. 149-150) when it should be around 18,000 B.C. (White 1967).

Van Heekeren's "Neolithic Stage" is on shaky ground. In his first edition he had about 4 pages in this section on archaeological sites and 14 on a typological description of "neolithic" artifacts from surface finds. In this edition the typological description is about the same length, while there are about 26 pages on sites, including numerous illustrations and tables. None of the sites, however, are definitely "Neolithic." In the first edition there was no definition of the term neolithic but its use appeared to be largely based on a stone tool typology and the absence of metal. There is some discussion of agriculture but this was not presented as a definite part of the Indonesian "Neolithic." In the second edition, Heekeren does give a partial definition of "neolithic proper . . . with intensified destruction of virgin forest and intensified agricultural activity, practiced in a village context" (p. 154). This very tentative definition continues with a brief descriptive reconstruction of the culture of these village-oriented people. He said nothing about the dating of the "Neolithic" in the first edition, but in the second he says it started 3500-2500 B.C. (p. 154). None of the sites he describes as "neolithic" can definitely be said to fit within his definition or description of the "Neolithic." I shall turn to these sites momentarily.

Following his descriptive reconstruction of "neolithic" life he very briefly presents (without reference to sources) three "neolithic" traditions found in Mainland Southeast Asia. His one reference on this page (p. 158) for the tradition preceding the others is to Chester Gordon (1964). There is no such reference included in his bibliography, so he is probably referring to Chester Gorman. The date is not 1964 but 1969, listed as "1969(?)" (p. 225). The "(?)" can be removed as this was the year in which this report appeared. The three traditions he presents are the Non Nok Tha culture, Lungshanoid, and Geometric-stamped pottery (p. 158). Referring to the Non Nok Tha culture, he places the date 1968 in parentheses. I have no idea what this could refer to. He says it has "... a great number of burials, dated 2500-2000 B.C., including a burial in the lowest level with a bronze socketed axe and moulds" (p. 158). The many burials from this site dated from before 3000 B.C. to about A.D. 200. Bronze axes and moulds together first appeared in the fourth level from the bottom, and from that time until around A.D. 200 the culture was what Heekeren in Indonesia would have called a "Bronze Age Culture" (Bayard 1972). The Lungshanoid and Geometric-stamped pottery traditions are both found in southeastern China. Heekeren consistently includes South China, of the Stone Age, with Southeast Asia but does not explain or reference the reasoning for this. While his description of the Lungshanoid is reasonable, he closes by saying, "This culture was brought around 2000 B.C. from the north by brown-skinned Southern Mongoloids" (p. 158). The recently announced C-14 results from China date Lungshanoid cultures in South China back to the 4th millennium B.C., while the Lungshanoid culture in the north that was hypothesized "... as the ancestral culture of all other Lungshanoid cultures" (Chang 1973: 527) has
a date of 2310 ± 95 B.C. (Chang 1973; Meacham 1975). Of course, Heekeren did not have these dates. The dating for the Geometric-stamped pottery, given by Heekeren as 1500–700 B.C. (p. 158), has also been pushed back to 2335 B.C. for a site in Kiangsi province, with the end of the "neolithic" portion of this tradition coming around 1500 B.C. when bronze becomes a part of the tradition (Meacham 1975: 211–212).

It is unfortunate that van Heekeren did not have longer to assimilate and work over the new reconstruction of Southeast Asian prehistory. His thinking on the subject had not yet become consistent. He realized that the reconstructions of Heine-Geldern, Beyer, and Stein Callenfels, based to a considerable degree on the distribution patterns of stone tools found out of context, were not to be trusted (pp. 158–159), but he still used some of their routes of migration, calling them "ways of diffusion" (p. 158). The first of these, he said, "runs via the Malay Peninsula to Western Indonesia" (p. 158). Other than the considerably earlier probable spread of the Hoabinhian from the Malayan Peninsula to northern Sumatra, possibly during the Late Pleistocene, what evidence there is that I know of for diffusion between the Malay Peninsula and western Indonesia suggests the opposite direction for that diffusion, from western Indonesia to the peninsula. The only evidence for the other direction is based on Heine-Geldern's distributional studies of long ago.

The first 'neolithic' site reported on by van Heekeren is Kendeng Lembu in Java. He was excavating here when World War II started in the Pacific, and he lost his notes and the collections during the war. No report was made of the site. Soejono excavated there in early 1949, and van Heekeren's account of this site was taken from Soejono's notes (pp. 173–184). The collections had not been analyzed nor had a report been published. While I think it reasonable to expect the lower layer of this site to be 'neolithic,' there is no specific evidence presented by van Heekeren that would place it within his definition or description of the "Neolithic."

The next two 'neolithic' sites presented are on the Karama River in west central Sulawesi (pp. 184–190). I would say the same about these two sites as I did about Kendeng Lembu. That is, I suspect that at least portions of both sites are 'neolithic,' but nothing that has been presented on these sites would clearly include any portion of either of them within van Heekeren's "Neolithic." I will return to these sites below. The final 'neolithic' site is the Melolo Urn field on Sumba (pp. 191–198), formerly considered by van Heekeren as "Bronze Age." He moved this site back in time (?) to 'neolithic' because no metal had been found. I suspect that he is correct, but again there is no direct evidence to fit this within his definition of the "Neolithic."

In his "In retrospect: the Neolithic Stage" (pp. 203–206), van Heekeren again presents a number of generalizations and summary statements without indication of sources for these statements. I would certainly agree with him that "our knowledge of neolithic Indonesia is vague and rudimentary. At present it raises more problems than it solves" (p. 203). He has changed the early date for edge-grinding of stone tools in Australia to 13,000 B.C., still several thousand years too young (p. 205). He mentions 6000 B.C. for the beginning of pottery manufacture on Mainland Southeast Asia as well as in New Guinea. I do not know of a site on New Guinea with pottery that early. Regardless of the mistakes, his discussion at least
makes its readers realize that little has been settled on the Southeast Asian "Neolithic," and that for Indonesia in particular there are many surprises yet to come.

I have not attempted to go through the "references" and "selected bibliography" carefully, but I did notice two items that should be changed. On page 207, the paper by Almeida on the palaeolithic station in Portuguese Timor is incorrectly listed; it should be listed as Correa, Almeida, and Cinatti (1953). While this appeared only in abstract, a more complete paper on the same general subject by Almeida and Zbyszewski appeared in 1967. Twice Chang is alphabetized under his first name Kwang-chih (p. 215 and pp. 240-241) rather than his family name.

Heekeren included data from Portuguese Timor and Sarawak in this edition. It is unfortunate that he did not include more from Sarawak, where there has been considerable archaeological work done and published during the last 20 years which does have relevance to Kalimantan and the rest of Indonesia.

Other reviews of *The Stone Age of Indonesia* have appeared, the most detailed of which is by Wolfgang Marschall (1974). I would like to review it briefly, as part of Marschall's review is concerned directly and indirectly with my proposed framework of Southeast Asian prehistory. It is quite reasonable that Marschall examines my framework in a review of van Heekeren's book, as Marschall feels that some of the difficulties and problems that van Heekeren has in his treatment of the "Neolithic" and the "Mesolithic" are due to van Heekeren's "intent to somehow comprise the prehistoric material from Indonesia into the wider framework of Southeast Asian prehistory" (Marschall 1974: 80) as presented by myself and others. As I have indicated earlier, I am in general agreement with this criticism. Before concerning myself with Marschall's examination of the framework of Southeast Asian prehistory, I would like to examine a few other points which he makes.

In several places in his review, Marschall expresses the opinion that pottery found in association with various early cultures is extraneous (1974: 75, 76) or that it was found in a lower level because a sherd or two "sank down" from a higher level (1974: 75, 76; 83, n. 6). The idea that potsherds will "sink" in a site does not seem reasonable to me. Quite commonly in a disturbed site, sherds will have been moved up onto the surface of a living level and thus become deposited in that level, while sherds from a living level may well become incorporated in a disturbance. If excavation is properly done, the disturbance will be noted and the material from the disturbance (pit, etc.) will also be noted so that it is not confused with earlier levels. There are two cases where I can conceive of sherds moving down: an occasional sherd in an animal burrow, falling in after the burrow is no longer in use, or, in a cave with rock falls where there are crevices and areas of loose soil among the fallen rocks (or in a loose rock wall or foundation), a sherd may fall between the rocks and, as a result of earthquakes or water movement, may move farther down among the rocks. I cannot accept this as a common happening. While I would not trust a single sherd reported by Stein Callenfels to be the only one from a layer as proof that pottery was associated with the other materials he reports from that layer, I would accept the report of Mulvaney and Soejono on pottery dating from Toalean sites (Marschall 1974: 76) until I can see their final report and judge for myself. If I know an archaeologist is a good excavator I will trust his or her
preliminary reports until the final report can be gone over. In other words, Marschall does not accept the presence of pottery reported by van Heekeren from several sites, while I will accept it until I can see final reports on these sites and then make up my own mind.

There is a major problem in the dating of the site of Kalumpang in western, central Sulawesi. The site was excavated by Stein Callenfels (Heekeren 1972: 185) in 1933, and further excavations were made by van Heekeren in 1949. A soil scientist who examined the site has said that the soil formation could not be more than 1000 years old (1972: 188). Callenfels reported that the grandfather of the headman of that locality when he was there had used the site as a rice field. The top layer, in which all the artifacts were found, varied from 15 to 45 cm in thickness and the yellow loam below this layer was sterile (Callenfels 1951: 83). I suspect that the disturbance has some effect on the dating of a disturbed soil formation. No metal was found in the site. I am willing to accept the dating of the soil scientist for the soil of the top layer, but I question that the disturbed artifacts in that layer are dated by the soil formation. Only if we find dates for Minanga Sipakko, a similar site nearby, to be less than 1000 years old will I accept this dating for Kalumpang. Unlike van Heekeren, who says that “This site is bound to be much older than the Kalumpang site” (p. 189), I do suspect that the two sites are close to each other in dating. Marschall accepts the dating and interprets the site as “... a metal age site yielding objects of a group of persisting traditions, which somewhere and somewhen belonged to a neolithic stage” (1974: 78).

Marschall is unhappy, as I am, with van Heekeren’s use of the term “Mesolithic.” Marschall (1974: 79) suggests in place of it “Epi-Palaeolithic, “Post-Pleistocene Palaeolithic,” or “Upper Palaeolithic.” I do not care for any of these terms and for different regions within Indonesia would prefer the use of type culture names for specific culture sequences within this period. In the last paper van Heekeren presented before he died (1975: 51), he called this period from 5000–10,000 B.C. the “Epi-Palaeolithic.” I am willing to go along with this terminology for now.

In a footnote, Marschall (1974: 75, n. 4) says “I assume that the immigration of Indonesia population to Indonesia is of relatively recent date.” Later in his review, Marschall says that “All post-Pleistocene traditions in Indonesia up to the beginning of the quadrangular-adze tradition are associated with Melanesid populations” (1974: 84). This statement cannot be supported, because no human skeletal material has been recovered from most of the pre-quadrangular-adze sites in Indonesia, and so we do not know what the associated population was like. On the following page (85) Marschall states that all of the oldest group of traditions of the Upper Pleistocene and Holocene were traditions of Melanesid populations. We just do not have data to say that. Further, he says that pig breeding was probably not a tradition among the early New Guinea population and that “Pig bones from two sites in New Guinea dated at 3,000 B.C. do not present evidence for this tradition in this area” (1974: 85). He does not present an alternative explanation for the presence of pig bones in New Guinea at this date.

Jacob, summarizing the human variation in Indonesia during the Epi-Palaeolithic, has said that the Austromelanesoid element was strong in the northwest though some Mongolid elements were present; Mongolid elements were not present in the south, either east or west, while in northeastern Indonesia Mongolid elements
were much stronger (1974: 393). Jacob does not here present his data, or a source, to support this statement. I have proposed that northeastern Indonesia and southern Philippines is the source area of Austronesian languages (1974) and I would suggest here that this area was of major importance in the ancestry of the peoples living in Indonesia and the Philippines today. Whether Marschall would consider that the Epi-Palaeolithic is of recent date I do not know, though I doubt it.

Marschall's critical examination of my reconstruction and of evidence said to push back the dates in Southeast Asia for various cultural elements makes up about one-third of his review (1974: 80-87). He begins with an accurate time chart of my Lithic to Conflicting Empires reconstruction (p. 81). He approves of the idea of "... a new terminology avoiding the Near-Eastern terms" (p. 81) but doubts the usefulness of the terms I suggest as they are not selected on the basis of one criterion. I was avoiding the use not only of Near-eastern terms but also of terms commonly employed for American cultural periods, as both sets of terms are in effect concepts that have established meanings that are not directly applicable to Southeast Asian prehistory. Marschall proposes the term "Formative" in place of my "Crystallitic" (p. 81, n. 11). When I was first setting up names for the different periods, I used "Formative" for the "Crystallitic," but because of its use in New World reconstruction, where it has definitions that do not fit Southeast Asia, I decided I could not use it. For a time I tried to find terms based on a single criterion, but virtually all we know about the "Lithic" is the stone-tool technology, while the technology of the tools, stone or whatever, is not distinctive for the last three periods because of the great variety in cultural levels, or economic bases, found until today in Southeast Asia.

Marschall brings up several questions about the dating of horticulture in Southeast Asia much earlier than was previously accepted. He says that "... several of these dates are wrong, others misleading" (1974: 82). He first criticizes Chang's suggested dating of 9000 B.C. for horticulture by the people of the Corded Ware Culture in Taiwan. I will agree that several of us too quickly referred to that date without question. However, the faulty method by which that date was arrived at does not invalidate Tsukada's pollen analysis, which shows that there was an abrupt change in pollen at about 11,000 B.C. with a continuation of the new pollen types suggesting burning of the forest, as you might have from slash-and-burn horticulture (Tsukada 1966: 543; Chang and Stuiver 1966: 542; Chang 1969: 192–194, 217–218).

Next Marschall says that my dates of 13,000 or 10,000 B.C. for plant domestication in Southeast Asia are wrong (1974: 82). They are not wrong, they are hypothesized. Without question it is my fault that I have not consistently made clear both of the points that I am hypothesizing in this situation. First, I am hypothesizing that some of the plants recovered by Gorman in Spirit Cave, found in Layer 4 and above, were domesticated. As Gorman states (1970: 102), "The leguminous plants are tentatively considered to represent early domesticated varieties." Our thoughts on these plants have not changed. We still do not know whether they were domesticated or not. Gorman has found more specimens of the peas in later excavations but no more beans. Douglas Yen, who has been working with the plant materials from Spirit Cave and other sites, and who has himself worked in the area in 1974, will shortly be publishing on this matter. Second, I am hypothesizing that one or more
Asian Perspectives, XVIII(2), 1975

of these plants that may be domesticated were domesticated as early as 13,000 B.C. or earlier. In one paper in which I included the plant question, I was unfortunately too positive (1970). In my abstract I said “domesticated plants before 9700 B.C.” and in summarizing the finds at Spirit Cave I said “... that somewhere in this general area domesticated and tended plants were being grown ...” (p. 145). The only qualification was given in my summary of Southeast Asian prehistory, in which I stated that “Parts of the following framework are conjectural” (1970: 149). In an article a year later, I was more careful to say that “... Southeast Asians may have been the first ...” (1971: 331), and “Material excavated and analyzed during the past five years suggests that men were cultivating plants there ...” (1971: 330). In the time chart presented in this paper my conjectured datings are in red, clearly distinguished from actual datings in black (1971: 332-333). Finally, returning to plant domestication and other points, I said that “Most of the ideas I have proposed must be labeled as hypothesis or conjecture. They need a great deal more research to bear them out—or refute them” (1971: 339).

The only specific date from Spirit Cave that Marschall mentions is the one Heekeren used. “Van Heekeren ... mentions domesticated plants from Spirit Cave dated as early as 9200 B.C., which correctly should read 9180 B.P. or about 7000 B.C.” (1974: 82). The 9180 is the earlier of the first two published dates from Spirit Cave. The C-14 dates from Spirit Cave go back to 11,690 ± 560 B.P. (half-life 5730) (Gorman 1970: 99), and it is from this that I use a rounded 10,000 B.C. date. This also makes van Heekeren’s date of 9200 B.C. not as far off as the 7000 B.C. that Marschall rounds it to. Shortly following this Marschall says that my “... Upper ‘Crystallitic’ ... or Late Hoabinhian ... characterized by cord-marked pottery ‘or the domestication of one plant or animal’ simply does not exist at 13,000 B.C.” (1974: 83). This statement is incorrect. At this time we do not know whether it exists or not, though I am hypothesizing that it does.

Marschall also questions the dating of the pottery from Spirit Cave. He says that “The Cord-marked and burnished ceramics from this site stem from the surface of layer 2 and from layer 1. The most recent date from Spirit Cave, FSU 317, dates the bottom of layer 1, ‘just above the surface of layer 2’. ... If my understanding of this passage is correct, FSU 317 is from the surface of layer 2 and offers a date earlier than the time when pottery was deposited” (1974: 83). Marschall’s understanding is not correct. The charcoal analyzed as FSU 317 comes from just above the surface of Layer 2 and does not date that surface. “FSU 314 and GaK 1846 (corrected) both date the surface Layer 2” (Gorman 1970: 101). These dates are FSU 314—8142 ± 390 B.P., and GaK 1846—8806 ± 200 B.P. (both corrected for half-life of 5730) (Gorman 1970: 99). Marschall continues, “... I suppose that the pottery is confined to layer 1 and sank down to the surface of layer 2 ...” (1974: 83). While I do not agree that pottery will sink in soil, in this case sinking is not enough. “The surface of Layer 2 was compacted, and numerous potsherds were scattered about and pressed into the surface as if they had been walked on long enough to level them into the surface compaction” (Gorman 1970: 93). It was this surface which was dated by the two samples, and even if sherds could sink through an uncompacted layer, they could hardly sink into and become a part of a compacted living surface.

The next question taken up by Marschall is the dating of the bronze from Non
Nok Tha. From our first excavation at Non Nok Tha we recovered bronze from levels which were C-14 dated well back into the 3rd millennium B.C. (Solheim 1968). Donn Bayard’s second excavation at Non Nok Tha turned up a metal tool from an earlier level than any we had found during the first excavation. A metallurgical analysis made in Thailand indicated the tool was primarily copper, with no tin (Bayard 1970: 123, 136), suggesting that we had a sequence of metallurgy from copper to bronze (Marschall 1974: 81). Later Bayard was able to analyze a small fragment of metal from this same layer and found that there was indeed tin in it, suggesting strongly that the “copper socketed” tool was bronze and not copper (Bayard 1972). We no longer had a “copper phase” in our sequence, only a bronze metallurgy earlier than had been indicated from our first excavation. From this we cannot say, as Marschall does (1974: 82), that there was no “copper phase” in Southeast Asian prehistory, but only that we do not know what came before our earliest bronze. Marschall concludes that “The antiquity of bronze metallurgy in Thailand remains an unresolved problem . . .” and that “The traditional date for initial bronze metallurgy in Southeast Asia in the first half of the first millennium B.C. may still prove to be correct” (1974: 82–83).

In 1974 and 1975 considerable work has been done on bronze materials from Non Nok Tha at the University of Pennsylvania and by Garry Carriveau of Brookhaven National Laboratory. I will soon have a final report on this work, which I may include in the first volume of the final report on Non Nok Tha. One of the workers on the bronze, Tamara Wheeler, presented a paper in March 1975 at a colloquium held in England on metallurgy. In this paper she said, “Cast tin bronze is documented as a production of Thai metalworkers by 2800 B.C. while current excavation at Ban Chiang may present sufficient evidence to push the date of tin bronze manufacture back 800 years, thus antedating the advent of tin bronze in southwest Asia” (personal communication).

Would van Heekeren’s book have been better without his attempt to fit the Indonesian materials into a Southeast Asian context? I personally do not think so. Indonesian prehistory is a part of a much larger prehistory, and an attempt to view this prehistory in its larger context, even though faulty, is better than viewing it as if it were independent and complete unto itself. Many of the people reading this book will not have a good background on Southeast Asia prehistory. Nor will they get such a background from the book, but because of van Heekeren’s approach they will have a much better idea of Indonesian prehistory than if he had not tried to present it within a wider context.

It is easy to be critical of The Stone Age of Indonesia, but I doubt that there is anyone today who could do a better job of it. When a better book on the subject is written, it will be heavily dependent on the presentation made by van Heekeren. This is an important and useful book and a great improvement over the first edition. I only wish that the author could have lived to do a second revision.
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