

# Robert von Heine-Geldern:

1885–1968

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**P**ROFESSOR Dr. Robert von Heine-Geldern, born 16 July 1885 in Grub, Lower Austria, died in Vienna the morning of 26 May 1968 while, as usual, he was getting ready to go the Institute of Ethnology.

Like most of his contemporaries in Austria, Heine-Geldern's grounding was classical. His first scientific interests were in the field of art history, which at the time included archaeology. To understand and appreciate the contributions of this erudite scholar, one may wish to recapitulate some of the key concepts and methodological principles that guided him throughout his scientific life.

## METHODOLOGICAL BACKGROUND

The science of archaeology can be considered as consisting of two sets of complementary activities: art archaeology and scientific excavation, of which the latter overshadows the former not only in the popular estimation, but also in the opinion of many modern archaeologists. However, that art archaeology represents the primary and characteristic quest of archaeology is indicated by the fact that even within the discipline of prehistoric archaeology, which is predominantly attuned to excavation, the principles of comparative typology are derived from art archaeology (cf. Potratz 1962).

In the absence of scientific and stratigraphic excavations—and let it not be forgotten that there were very few in Southeast Asia throughout the lifetime of Heine-Geldern—art archaeology, supplemented by ethnographic reference material in methodologically secure contexts, was in fact the only legitimate means of archaeological research in that area.

Archaeology as a historical science offering evidence on ancient man deals with specific source materials, namely, material relics of the past. The total cultural approach to these relics practiced by modern archaeologists replaced a prolonged preoccupation with works of art and applied art, including tools and ceramics.

An intrinsic quality of art is the insights it offers into the changing world views held by mankind in different epochs of history and different regions of the world. This reflective

quality causes considerations of form and style to take precedence over discussions of the material content of artifacts in scientific analysis. However, clear discernment of the process of the genesis of art (particularly of the factors time and space) is the precondition for deductive abstraction. Although the faculty of artistic expression appears to be an inherent human communicative attribute, it does not follow that the pattern of this expression is uniform all over the world. Thus, it would be a mistake to consider the change from sensorial (palaeolithic) to abstract (mesolithic) art a necessary step in human development. It stands to reason that art developed particular styles of expression according to the particular way of life valid in each historical region, and that the genesis of art cannot be conceived to be any single, uniform style developing unilinearly in stereotyped stages all over the world. Every historical region must have created the artistic form which most adequately expressed its own particular world view, insofar as man can express only what is prescribed by his own cultural makeup, that is, his historical position in space. Only in that perspective can we comprehend the breadth of variation seen in the artistic expression of the past. Artistic expression is thus not a biological function, but a conditioned response to concepts formed within the intellectual habitat of one's given social environment. It is a cognitive mode of giving material form to information by means of optical signals—signals that are not, however, universally valid, either in form or content.

Thus, art archaeology proceeds from the valid facts of spatial group orientation and temporal context within the intellectual climate of a given epoch, determines the formative principles for a given region, and abstracts valid theories. The form or historical structure of a given art style can be reliably fixed within a temporal sequence by absolute dating of some characteristic fossils.

This methodological framework devised for art archaeology is considered to be equally applicable to tools and ceramics. Although the tectonic elements of a ceramic vessel are necessarily prescribed by function, its artistic differentiation largely depends on the artistic traditions by which the individual potter is bound. However, the typological inventory of a given tradition appears to be restricted, often limited to several traditional patterns. Isolation and paucity of outside stimulation postulated for the very distant past must have accented this conservative trend. Thus, man was content with a very restricted inventory of types and very gradual typological changes over prolonged periods of time.

However, form in this methodological framework is assumed to be unique in the temporal and spatial context of its creation. The validity of this basic principle is, of course, annulled by extreme simplicity of form, or limitations imposed on form by certain materials. Very cautious acceptance of this principle and its limitations, which in a different verbal formulation constitutes the "criterium of form" proposed by Graebner and subsequently incorporated into the methodological system of the Vienna School of Historical Ethnology, is one of the few points of agreement between Heine-Geldern and the tenets of this school of thought, although in Heine-Geldern's case the formulation of the principle was not the result of speculative thinking, but of abundant experience in comparative typology.

Although form is unique in its temporal and spatial context, it does not remain unchanged forever. Changes of form may result from external influences or internal developments. In the first case, changes tend to appear spontaneously; in the second, innovations can usually be traced through a series of developmental links. The consecutive stages of a developmental process are then considered to constitute a typological series. Within this typological series only consecutive sequence, and not developmental direction, is certain. Thus, every

serviceable typological series has need of at least two chronological anchors to determine the direction of development. Among typological criteria, the so-called technical ornaments (ornaments derived from technical details) contain a very useful chronological element, since they are obviously chronologically younger than the technical device from which they are derived (cf. 1954a: 358). There is also no generally valid norm for the speed of development. Periods of developmental acceleration are as clearly recognizable as periods of stagnation, but developmental speed cannot be adduced by purely typological features. A number of chronological fixed points are required to determine direction and speed of development within a typological series.

The significance of the typological method lies in its transmissibility. Formal types excavated at different sites or regions may thus be dated relatively according to their position within their typological series. However, application of this method requires cognizance of formative principles and form genesis, even though ultimate criteria of typological comparison may defy mechanical measurement and call instead for experience, intuition, and integrity—qualities Professor Heine-Geldern possessed in unique measure.

Although the methodological framework indicated emerges clearly from the direction and the scientific procedure of Heine-Geldern's *oeuvre*, in all the years I knew him, I never heard him expound theory for theory's sake. He once told me jokingly that one's methodological grounding ought to be like a foundation garment, ever present, but never to be seen.

This conceptual orientation and an early journey through Southeast Asia determined the direction of Heine-Geldern's lifework. It was concentrated on culture contacts and reflected his conviction that art styles are the products of particular world views and are thus never transmitted as isolated culture elements (cf. 1930, 1957f). Another of his prominent interests was the dynamics of culture change.

#### HEINE-GELDERN'S OUTLINE TESTED

Archaeology in Southeast Asia has in recent years developed into a battlefield where an ever-expanding inventory of revolutionizing data, allied with an evermore precise technical apparatus, forces our conventional conceptual system into a last-stand retreat. In the light of new discoveries even time-honored archaeological concepts—and some of them like "neolithic" used to belong to our basic "mental equipment"—require essential reformulation, or they have become obsolete. In spite of this situation, the outline of culture history written by Heine-Geldern almost a lifetime ago against tremendous handicaps still provides the only framework against which to test our results (1931b). His Southeast Asia *oeuvre* still remains our only frame of reference, as Donald D. Tugby demonstrated in his recent paper on ethnological and allied research problems in Southeast Asia (Tugby 1970).

Heine-Geldern never knew of the date "before 9700 B.C." for plant domestication in Southeast Asia (see Gorman 1971). He would probably hide guarded excitement behind his favorite comment "*Ich bin skeptisch*," but the skepticism would be limited to the very early date, because the possibility of early plant domestication in Southeast Asia was strongly advocated by him in many a stimulating seminar.

Abstracting his early overall vision of culture history in Southeast Asia from a laborious comparative typology of stone tools known by 1930 and their respective distribution patterns, we find that Heine-Geldern was sufficiently farsighted to accommodate most of the

constituent elements known today. Archaeological terminology has changed. What Heine-Geldern called "culture," we call "tool tradition." While he could not differentiate some finds for lack of material, we can supply some typological links. However, his emphasis on the strong regional character of local variants of these at first fairly undistinguished cultures sharing an economic base, but getting increasingly adapted to their specific ecological environments (1932a: 550), has been upheld by newer research, as has his suggestion that chopper-chopping tool industries and blade tool industries may have existed side by side in space rather than in temporal sequence (Heine-Geldern 1932a: 555-556). He assumed a continuation for these industries until very recent times in some local contexts (1932a: 556; cf. Sung 1969), but their currently accepted lower limit of around 30,000 B.C. far exceeds his original expectations.

Some of us tend to look upon Heine-Geldern's first full neolithic "culture," his Walzenbeil tradition, as an indigenous evolutionary end product of an increasing use of polishing techniques on previous stone-tool forms (Solheim 1969: 133). Although this is possible, it should lend itself to typological demonstration. Besides, if so, the problem merely shifts from the provenience of the Walzenbeil tradition in Southeast Asia to that of the stone-polishing technique.

There is some doubt about the "Schulterbeilkultur" as a separate tool tradition (culture complex in the former terminology) and its bearing on the Austroasiatic problem, but to explain shouldering as a functional feature developed for purposes of better hafting for some kinds of work (Solheim 1969: 133) does not, although it is indisputably correct, advance our understanding of the problem a very great deal. Do the varieties of shouldered adzes known to date constitute an independent tool type, being links of a typological series? What is the known distribution of the type as such and of several diagnostic subtypes, and does it afford any insights into the cultural environment in which this tool type developed, and how it spread? Is it possible to pinpoint developmental direction and speed? What is the relative stratigraphic position of shouldered and quadrangular adzes throughout representative sites? And finally, what, if any, bearing has this tool type on the Austroasiatic problem?

Although the new dates (6800 B.C.; Gorman 1971) suggested for the third neolithic culture of Heine-Geldern, his Late Neolithic Vierkantbeilkultur (1932a: 566ff.), are in keeping with a general lowering of dates, they are much earlier than he expected, though they are already considerably modified from the still earlier date (9000 B.C.) suggested by Chang (1966: 542) for Taiwan. Even if one is prepared to consider that the rectangular adze developed somewhere in Southeast Asia—defined to include China up to the Yangtze River—around 10,000 B.C. within a cultural context including cord-marked pottery as suggested by Solheim (1969: 133) on the basis of finds in northwestern Thailand and Taiwan, one would like to be shown some very convincing typological evidence of how the quadrangular adze developed "presumably out of the oval edge-ground adze" (Solheim 1969: 133). Apart from the genetic problem, the main migrational routes of the quadrangular adze within Southeast Asia suggested by Heine-Geldern on the basis of comparative typology (1932: 571-572) have never been brought up to date, or seriously challenged by detailed refutation of Heine-Geldern's detailed evidence, an objection which, incidentally, also applies to the overwhelming evidence presented by Heine-Geldern for bronze and the Asiatic origin of metalworking in South America (1954a). It seems strange that of those of us who glibly criticize or lightly dismiss Heine-Geldern's work, particularly in the field of

pre-Columbian transpacific contacts, not one has made a sincere effort to disprove his evidence point for point.

#### EXCELLENCE REMEMBERED

Time and space permit only an arbitrary probe into a few aspects of Heine-Geldern's Southeast Asia work, a probe which is also handicapped by a *nolens volens* limitation to his 1932 "Urheimat" paper, because this is the best known and alas, through simplified translations, not rarely the only one known to many colleagues outside of the German-speaking countries. It is exactly for this reason that in his later years and particularly in his transpacific contact studies, hopefully expecting the response of Americanists, he increasingly took to writing in English. However, even so, the lesson seems clear enough. We who through providence are placed in a much more favorable research situation might learn to combine potentials with the solid, diligent, and scrupulous workmanship of Heine-Geldern, who refrained from proposing a theory unless he could demonstrate its feasibility with a reasonable degree of conviction, in some cases approaching certainty.

I had the privilege of witnessing Professor Heine-Geldern's working method for several years and know that it was a very systematic, laborious process involving countless checks and counterchecks for every element studied. He was always acutely conscious of the subjective limitations inherent in the "Formkriterium" and therefore rejected all but very complex and highly characteristic forms for comparative purposes. He would very often invite a debate on some new line of thought and listen very attentively to everybody's objections. Once he was convinced of the reasonableness of a hypothesis, he would stick to his guns and defend his position lucidly and with penetrating judgment. He was never dogmatic, and up to his very last day remained incredibly alert in his quest for new facts and lines of thought.

It is said that to understand the stimulating influence of Franz Boas, one had to be in day-to-day contact with him. The same observation could be made of Heine-Geldern. He was the most inspiring of our teachers, sparing himself no amount of trouble to provide illustrative material to show his students. In the years shortly after the war when the different institutes of Vienna University were widely dispersed, he was a familiar sight hurrying from place to place with a forbiddingly heavy suitcase that contained books and diapositives for the lectures of the day. I once mustered up courage to approach him on the street and offer my assistance, but he declined, smiling kindly: "Far be the day when a young lady carries my suitcase!" He was also the most exacting of our teachers, for nobody could satisfy him just by memorizing his lesson. What he expected and encouraged by his own living example was an independent scholarly mind, a scrupulous sense of moral responsibility, and a single-minded devotion to one's work.

The warmly human aspect of this truly noble scholar, known to all his students, is also shown in his initial appeal and energetic work for the scientific study of disappearing peoples and cultures throughout the world.

Another aspect of Heine-Geldern's work, probably the most hotly disputed, concerned pre-Columbian transpacific contacts. When he first broached the subject it was anathema, but thanks to the vast bulk of evidence brought forth in favor of such contacts by him and scholars encouraged by his work, the prejudices have decreased. Pre-Columbian contacts

can be studied now like any other historical subject and, especially in view of the lowering of dates in Southeast Asia, yield increasing confirmative evidence. About his principles, Heine-Geldern said (1960: 280): "There is no such thing as a moderate, or an extreme diffusionist. The only thing that counts is whether in a given concrete case a diffusionist explanation is scientifically reasonable or unreasonable." Nevertheless, to cite the German Americanist Paul Kirchoff (1969: 163), the indifferent if not downright rejective attitude of the majority of leading Americanists toward Heine-Geldern's transpacific contact studies is so unusual a phenomenon in the entire field of cultural anthropology that it calls for reflection. It appears to be the result of a widespread lack of interest in this problem—*pace* Alfred V. Kidder, who called it "the greatest single problem for the Americanist"—and of fear of the multitude of knowledge which is a precondition for work of this kind. Lack of response, and the almost incredible fact that a scholarly discussion or critique of his research methods and results does not exist, was not only a personal tragedy for Heine-Geldern, but remains fatal to many basic research problems.

Like Vinigi Grotanelli in his obituary speech before the Working Group on Urgent Anthropology at the Eighth International Congress of Anthropological Sciences in Tokyo (Grotanelli 1969), I am fully convinced that Heine-Geldern's work should be considered in prospect, rather than in retrospect. If somebody considers this position biased, may he forgive me, for like everybody in close contact with Heine-Geldern, I loved him very much.

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