probable paths of diffusion. In view of our imperfect knowledge of the prehistory of most of the world, this still seems to me the most scientific procedure for my purpose. If anything, it should tend to cause more variability in the results and hence, as a procedure, to weigh the results against the regularity I had hypothesized. Map distances were used in my paper as an approximate measure to a probability defined chain of interpersonal communication along which cultural information was presumably carried. The routing of such communication in the modern world is far too intricate to be traced in most cases. I find the suggestion that we wait to study diffusion until we can reconstruct such routes for some prehistoric circumstance simply fantastic. So, apparently, does Narr, whose advice seems to be that we might calculate diffusion rates only after we know everything worth knowing about routing, about migrations, and about diffusion itself in each separate area of the world. Even then he would appear to be saying it would be a bad idea.

Other commentators on my paper have raised similar objections. I think I am charged fairly with questioning certain widely accepted ideas, notably those relating to an "organic" conception of cultures or culture types—conceptions to which I think the "generations of archaeologists" mentioned have become unduly attached. If there is such a thing as "Neolithic culture" or "Danubian culture," I believe I have shown cause for questioning whether pottery as such has any role in it—except as a fortuitous and unpredictable introduction from outside. (Specific types of pottery or styles of manufacture and decoration may well be a quite different phenomenon with different diagnostic significance). The argument of ein Volk, ein Topf which is implied by the migration interpretation is certainly sustained only rarely in documentable history, and while I am prepared to accept it in the Danubian case, it is risky to generalize it.

On the Method of Studying Ethnological Art

by Ronald Cohen

Montreal, Quebec, Canada. 9.29.61

I would like to emphasize William Fagg’s comment that Haselberger ("Method of Studying Ethnological Art," CA 2:341–84) treats "as established facts propositions which are at best reasonable hypotheses; ..." Kanem may have been an important "disseminator of culture traits," (p. 354), but Haselberger’s estimation of the size of the Kanem state, even at its zenith, sounds more like a myth than a fact. I would also caution readers to treat the Sao culture with suspicion, at least as reported by Haselberger. I have seen some of the sites south of Lake Chad, which I suspect to be early Kotoko, or pre-cursor to Kotoko, and they are not "cities." Local legend has it that the great Sao culture underlies that of the Kanuri. In 1957, Mr. A. Rosen, his wife, and I and my wife dug a few pits at Birni Ngazargamo, the second great Kanuri capital (circa 1450-1810 A.D.). Under the Kanuri material we found only sterile earth.

by Henri Lavachery

Brussels. 12.18.61

Of the comments on Haselberger’s "Method of Studying Ethnological Art" I was particularly interested in Gilbert Archeys. I quote the following sentence in order to append some personal remarks which may be of interest to students of so-called "primitive art."

Archeys writes, "Haselberger’s remark that the optical concept of form, that is, form seen at a distance is not found among ethnological peoples immediately brings to mind the stone statues of Easter Island."

It happens that I spent several months on Easter Island (Franco-Belgian expedition to the Australian Pacific, 1934-35) and that I camped for several weeks at the base of the native quarry. It seemed to me incredible that the synthetic, grandiose style which marks these long countenances was not intended by the artists. I saw the statues under all kinds of natural lighting conditions; in the slanting rays of the rising sun; in the full light of day; on dull, rainy afternoons; by the light of the moon; and by starlight on moonless nights. Never have I seen statuary better suited to the outdoors. This simplification of volumes cannot be but intentional. It is sufficient to see, on the other hand, how these same Pascuan sculptures decorat forming portable statuettes of mimosa wood designed to be fondled by their owners or exhibited to the admiration of all at the feasts of Aho; the same care in the use of expressive detail, intended to be appreciated at close hand, is found in the hundreds of petroglyphs, light bas-reliefs or in tags, divinity masks, bird-man figures, and animal figures uncovered by our expedition. The contrast in style between these petroglyphs on the one hand and the great stone figures on the other is sufficient to demonstrate that the Pascuan artist knew how to adapt his conception of form to the conditions under which his work would be viewed.

I think the opposite may be said of the statuary of the Marquesas. I have observed in their natural setting the stone tikis of one of the sanctuaries (the one farthest from the sea, in the mountain) in the Typee Valley at Nuku-Hiva, romanticized by Melville. The Marquesan style is above all graphic. Whether it is a question of sculpted bones, stones, or tree trunks, the definitive characteristic of the image scarcely penetrates the material. But this style adapts itself equally well to the object designed to be viewed at close hand and to the statue played upon by sun and shadow in some mysterious underbrush. The sculptor has not had to change his style to accommodate the locale.

The adaptation of forms to environment may, doubtless, be verified by comparing the great wooden statues of Hawaiian sanctuaries with small-scale images. The former are grossly hacked, as with a bill-hook; the latter are finely polished and detailed. But the comparison is more difficult, for these are now works of art having been transported from the open air to the neutral atmosphere of museums. It is undoubtedly for this reason that men of good will have been misguided in their appraisal of the "optical concept of form" of "primitive" peoples.

by Suzanne W. Miles

Madrid. 12.12.61

The discussion and argument about "ethnological," "primitive" and even "anthropological" art seem curiously anachronistic. It appears clear that what constitutes an art object is destined to endless debate, but nowadays one appropriately discusses art in general, the arts, and the art of a named category—be it geographic, style, period, school or the product of an individual—regardless of whether anthropologists have studied or are studying the society, or not. Some of the geographic terms still in use are so general as to be nearly meaningless—"African art" is a good example. As soon as study of a new area or period is in the least sophisticated, regional and period styles are named and even "schools"
may be sketched. Anthropologists should be the last to insist on a terminol-
ogy which implies fundamental differentiation between art tradition-
ally studied by art historians untrained in anthropology and the art of peoples
studied by anthropologists (what then becomes of the art of India and China
longs of interest to both groups of scholars?)

In terms of Haselberger's actual contribution, the article would have
been far better titled "Method for the Anthropological Study of Art," or,
since the vast amount of archaeological
material was not really considered,
"Method for the Ethnological Study of Art."

Further, the statement that "style
phases based on ideal-type methods
cannot reach the individuality of the
historic facts, is a magnificent under-
statement, or, perhaps, a conscious
archaism. Indeed, as Haselberger surely
knows, style phases defined by such
methods may be entirely false.
Definition of style-phase is extremely
important but is only possible with a
minimal knowledge of which art objects
are contemporaneous and which later.
Otherwise the definition is merely a
construct based on the ignorant gues-
ses and concepts of the interested stu-
dent. Once an historical frame is de-
fin ed, through archaeology or records
(inference may have to be rather ex-
tended for areas of poor preservation),
"fossil" objects of unknown age can be
roughly placed, although, indeed,
the "individuality of the historical
facts" is definitely lost. In situations
in which an historical frame cannot be
ascertained, it is far better to treat
such art in terms that ignore historical
background and development, than to
set up a probably erroneous series
which will become dogma for the un-
trained.

There is a point to be added to
Haselberger's discussion of local art
styles—their evolution and decline.
They may not decline at all, but simply
disappear. This apparently has
happened historically a large number
of times, so that it is often futile to
look for anything resembling the cus-
tomary "Western" three part evolu-
tionary (and spurious, for other situa-
tions) "historical" developments in
style.

Since so much of the art that con-
cerns anthropologists is archaeological,
Haselberger's sections on description
and iconography should be especially
emphasized. Though many of the bi-
polar concepts useful to art historians of
Western tradition are of less than
zero value in the study of other tradi-
tions, Haselberger's careful exposition
should lead to the development of
multidimensional concepts and tech-
niques for handling the archaeological
materials she excluded from discus-

sion. And, one might hope, Carpenter's
marvellous report on Eskimo approach
to art and life would be kept in mind
to remind the historian of his loss.

by Fabrizio Mori

Rome. 1.12.61

I read Herta Haselberger's article
titled "Method of Studying Ethno-
logical Art" in the October 1961 num-
er of CURRENT ANTHROPOLOGY with great interest. She honored me by
citing my work in the Acacus for
which I am grateful, but I feel it my
duty to voice my opinions in these
references. Some scholars, in fact, may
have based ill-founded convictions
upon the opinions I expressed regard-
ing Saharan rock-art in general and
upon the rock-art of the Acacus in
particular. Such errors would assume
greater importance in proportion to
the delicacy of the problems involved.

p. 353: Through Mori's work we have
a new C-14 date, 3500 B.C., for one of
the rock-shelters with "Negro" and "Egyp-
tian" pictures.

a) In the first place, there are two
dates obtained by the C-14 method
from the Un Muhuggig deposit
evidently referred to. The most
recent of these, 3500 B.C. was obtained
from the antelope skin in which the mummy
was wrapped; the second, 5500 B.C.,
was obtained from carbon taken from
the layer in which the mummy itself
was buried. A difference of 2000 years
is a good deal and, from material col-
lected during my last mission 1960-
1961, much of which is still being
examined, I believe that the earlier
date is very probably to be considered
as the most normal in connection with
the best production of Saharan rock-
art.

b) As Anatoly rightly stresses, in his
reply to Haselberger's article, it is not
certain that any relation exists be-
tween the more recent date obtained at
Un Muhuggig (3500 B.C.) and the paint-
ing in the shelter itself. I have pointed
out the possibility of a connection existing between this
date, obtained from the mummy, and
the painting representing a cadaver
wrapped in bands and perhaps mum-
mified; but I do not hold this to be
certain and irrefutable. After due
reflection on the discovery, I am be-
coming more and more convinced
that the painting in question must be
older than the anthropological find.

On the other hand, it must not be
forgotten, as I have already said, that the
earliest date obtained in the same
deposit is 5500 B.C. Apropos of this,
it is well to point out that, during
evacuations of the last mission, in the

same deposit, numerous elements were
collected which will oblige us to revise
some ideas from the chronological,
paleontological and paleoanthropological
points of view and, in particular,
about the phase of domestication.

C) "Negro and Egyptian pictures."
I cannot believe that my comments
on the paintings from the Un Muhuggig shelter could have created
such an opinion. The walls of the shelter are covered with at least fifteen
styles belonging to very different
ever; only one post-pastoral (and
therefore very recent) scene reveals
characteristics with a strong resem-
bance to Dynastic iconography. No
"Negro" paintings exists and I have
never affirmed that it did: only the
long file of women (pastoral period)
carrying baskets on their backs pres-
sents some affinity with pictures of
Negroes bearing tributes to the Pha-
raohs (XVIII Dynasty). But here also
I pointed 2 that "sufficient evidence
of phynochnical data to permit us to
judge the morphological and racial
type of the figures in question is lack-
ing," but the little that remains of
figures that remains reveals orthogon-
athic rather than prognathic charac-
tistics, if this one element racial in-
duction may be drawn. From this to
assume that I wished to attribute
negroid characteristics to the individuals
in the paintings of "round-heads" de-
picted in other strata on the same wall
would be erroneous, and I am wronging my idea. Later I will explain why...

... it also enables us to demonstrate
a continuity of Negro art styles for
over 5000 years...

If my opinion has any weight in
this particular problem, I will put on
record once more that I have always
maintained the material impossibility,
up to now, of attributing negroid charac-
tistics to the prepastoral "round-heads" paintings. There is not
a single phynochnical element, as
on the contrary occurs with the Medi-
terranean shepherd peoples, that per-
mits a judgement in one sense or the
other. As Dr. Fagg rightly points out,
and as I myself maintained in the
short paper discussed at the Sym-
posium "The Chronology of Western Mediterranean and Saharan Prehis-
toric Cave and Rock Shelter Art," organised by the Wenner-Gren Foun-
dation at Burg Wartenstein in July
1965, (p. 2–3), the presence in some
layers of the pictures of "round-
heads," of masks, costumes and orna-
ment characteristic of some Negro
peoples of Africa today, cannot dem-
strate scientifically the negroidism of
the individuals represented in the
paintings themselves. Such an element
could also have precisely the opposite

---

1 V. Mori and Ascenzi: "The child
mummy of Un Muhuggig—Anthro-
logical observations," in RIV. dell'Istituto
Italiano di Antropologia, XLVI, 1959.

2 "Catalogue of the Exhibition Pre-
historic Art of Libyan Saraha" Ed. De-
Luca, Rome, 1959, p. 45.

212

CURRENT ANTHROPOLOGY
The Editor wishes to acknowledge with thanks the receipt of the publications listed below. In order to keep the list current, we include only material published within the last 3 years. To make our listings more accurate and serviceable, each publication sent to the Editor should include on the cover or title page complete information (with date, city, etc.), translated into English, and, where necessary, transliterated into Roman script.

**Books and Offprints**


