

since they are used expressly for the purpose of explaining and analyzing the real data, and are constantly under modification themselves. This should be the cardinal principle of the typological method.

We wish to re-assert our adherence to Colton's principle, and hope that these additional remarks will serve to clarify some of the fundamental issues involved in his statement.

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SOME NOTES ON THE USE OF TOBACCO AND CANE PIPES
BY THE PIMAS OF THE GILA VALLEY

In reference to a statement in Dr. Castetter's excellent summary of *Early Tobacco Utilization and Cultivation in the American Southwest* (AMERICAN ANTHROPOLOGIST, Vol. 45, no. 2), there is an earlier reference to the use of tobacco by the Pima than J. Ross Browne's mention of tobacco under cultivation on the Gila in 1864.

The California Historical Society reprinted in its Quarterly for July, 1922, a short article entitled *Adventures on the Plains*, by Charles Cardinell. The article, which describes Cardinell's hardships while on his way to the goldfields, originally appeared in three installments in the San Francisco Chronicle on January 21, February 5, and February 16, 1856.

During October and November, 1849, Cardinell was stranded at the Pima villages on the Gila and one night witnessed a curing ceremony. He said of the ceremony, "One Indian was smoking tobacco through a piece of reed, blowing the smoke with all possible force through his nostrils upon the bare back of the sick Indian, at the same time pressing it with a cloth as if to smother the smoke upon his back to prevent its escape. He continued this process for about fifteen minutes, when another Indian took his place, going through the same process. During this time all, except the one engaged in smoking, were singing at the top of their voices a song to the tune I had heard at Santa Rosa. The performance was kept up all night. I could not sleep for the noise until daylight, when they all dispersed except two or three, who lay down to sleep."

In addition to the specific mention of tobacco, Cardinell's account is also another reference for Pima use of the cane cigarette, or pipe. There are few records of the cane pipe as used by the Pima, for during the second half of the nineteenth century the Pima stopped using them. In 1907 J. W. Fewkes wrote (*Excavations at Casa Grande*, Smithsonian Miscellaneous Collections, Vol. 4, part 3, p. 317), "In the memory of several of the old Pimas, similar reed cigarettes were smoked when they went to war." By 1935 I could find no Pima who could remember any use of the cane tubes.

I do not believe that such a complete stop to the use of the cane pipes can be charged to the cultural change which the Pimas underwent during the second half of the nineteenth century. There is a physical reason available. The stands of phragmites which formerly grew along the Gila have been replaced by mesquite thickets and tamarisk. I suggest that the Pimas stopped using the cane pipes because there was no longer a supply of canes readily available. I write this with some misgivings, for I do not know when the canes disappeared; but it probably occurred as soon as settlers on the upper

Gila diverted water for their farms, that is, during the 1870's. The flow of the river was then curtailed, the sloughs dried up and the phragmites died.

There is another question concerning the cane pipes. How long were they in use by the Pima, and their predecessors, the Hohokam? The pipes are of fairly common occurrence in prehistoric sites located along the Gila River and in the mountains to the north. They are found in village sites and in small caves which seem to have been shrines. To the best of my knowledge no reed pipes have been found in a pure Hohokam site; the custom of smoking them seems to have been a Salado trait. If this is correct, cane pipes arrived in the desert with the intrusive Salado population about 1300 A.D.

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NOTE ON THE AGE OF THE GALILEE AND MT. CARMEL SKELETAL MATERIAL

The following note will in no way detract from the interest of Dr. Weidenreich's article *The "Neanderthal Man" and the Ancestors of "Homo Sapiens,"* vol. 45, pp. 39 ff. of this journal. For the record, however, it might be wise to observe that Dr. Weidenreich gives more surety to the chronological position of the Galilee and Mt. Carmel physical remains than present evidence would allow. On p. 42 (*op. cit.*), he writes:

The "homogeneity" of the just mentioned "Neanderthaloids," namely of the skulls of Galilee, Ehringsdorf, Skhül V and Steinheim, and their divergence from the classic type is of the greatest importance. For in spite of the incontestable fact that they represent phylogenetically more advanced hominids than the classic Neanderthal type does, they all belong to the Last Interglacial Period of the Pleistocene and, therefore, are geologically older than the classic type which is characteristic of the Last Glacial Period.

As there is not yet a reliable geochronological correlation between Palestine and Europe, I doubt that the evidence allows such certainty as regards the Palestinian material. Archaeologically, a rough relative chronology may be constructed between western Europe and Palestine, but this remains a typological construct alone. Moreover, Miss Garrod writes (*Stone Age of Mt. Carmel*, I, p. 120) that:

The Levallois-Mousterian (in which the skeletons were found) of Palestine, with its remarkably uniform development over a long period of time, does not correspond so closely with western Europe, where levels of classic Mousterian type alternate with others of Levallois affinities.

She considers (*op. cit.*, pp. 121 ff.) the possible correlation between Miss Bate's scheme of the Wady el-Mughara faunal sequence and Picard's Jordan valley "pluvials" as only in part "reasonably certain," and ends on a note of expectation that further work in Pleistocene geochronology may make matters more clear. M. Raymond Vaufrey (*Revue Scientifique*, 77 (1939), pp. 390 ff.) offers an alternative interpretation of Miss Bate's Mt. Carmel faunal scheme, which would correlate Tabūn Ec with the approximate peak of Würm I in western Europe. Vaufrey's scheme would thus place Tabūn C-B and Skhül B (the levels which contained the skeletons) in the first Würmian interglacial, rather than in "the Last Interglacial Period of the Pleistocene" which implies the Riss-Würm interglacial.

That we may not agree with all of M. Vaufrey's arguments does not detract from the fact that another interpretation of the Wady el-Mughara evidence has been of-