

en who do not fight. The Zuni kachina who was captured in the mythical Ky'ana-kwe fight was made to put on a woman's dress in order to break his spirit—he was rebelling against taking part in the celebration of his capture. He was in Zuni theory the first transvestite. In another Zuni version the first transvestite was the elder sister of the Koyemshi clowns. In other words in Zuni opinion the practice had some association with war and with clowning.

There is a general Pueblo war-clown complex which among Tewa emphasizes change of sex. Also the Tewa hunter outdone by a girl in pursuing a rabbit had to exchange garments with her until he gave her another rabbit (Bandelier). These rabbit hunts with the girls are generally directed by the clown societies and, among Keres, at the request of the masked dancers. Possibly hunt and war transvestism was derived from Plains Indians; and possibly it was carried along into the late dance cult that derived in part from hunt or war dances, the masked dancer or kachina cult. Passing from an antifeminist culture like the Plains into a feminist culture like the Pueblo the trait of exchanging sex would tend to lose punitive character or disappear as it does in the Keresan rabbit hunt with the girls. In Bandelier's novel the Keres travellers from the south who come upon the disguised and penalized Tewa rabbit hunters can interpret their woman's dress only as clowning.

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#### NOTE ON RICHARD SCHULTES' "THE APPEAL OF PEYOTE."

Richard Schultes' paper on "The Appeal of Peyote," in the recent *ANTHROPOLOGIST*, is ably argued, but contains some misconceptions, I think, which should be pointed out. Mr Schultes' general thesis is (p. 704) that "the peyote vision has been incidental while the medicinal reputation of peyote has been fundamental in the establishment, spread, and, to some extent, in the maintenance of the peyote cult in the United States."

In section IV, paragraph 4, he argues that even in Mexico the medicinal virtues of the plant over-topped the importance of the visions. This may be so, yet the preceding paragraphs of evidence he cites for this do not appear to me to support the thesis, for it was the *visions* which accounted for the *use* of peyote in prophecy etc. (compare the "death vine," datura, cohoba snuff, yahé etc. in native America). Indeed, even in Mexico some of the authorities Schultes cites mention the vision. Further, Lumholtz' term "curing" in Mexican peyotism is used in a special sense: it is a *prophylactic* "baptism"-ceremony, rather than a *therapeutic* medicinal cure, a ritual to protect a group, not to cure an individual invalid. Furthermore (p. 706), the use of peyote in battle is surely as much for its supernatural vision-produced "power" as for its "stimulating" properties.

Again, Mr. Schultes argues (p. 707) that the healing power of peyote was the most important element in enabling peyotists to sweep aside conservative tribal opposition, and cites Radin to this end; but Radin mentions *in the quotation* "the glorious visions" of peyote-intoxication also. I think, indeed, a mere new plant medi-

cine, without the authority of the visions peyote produces, could not have made its way against such resistance (the Kiowa, I believe, are typical for the Plains in saying, "We don't get medicine power from any plants except peyote.") A new plant remedy, of which the Plains Indians already had many, did not crush this opposition; the supernatural authority of the visions produced by peyote appealing to an older vision-valuing culture did. In the Plains, as is well established, the epistemological authority for behavior is the supernatural vision-experience, quite as the experimental laboratory method is for our scientific culture. It is, I repeat, this element, at least in the Plains, which overcame individual tribal resistance, and which indeed convinced them it had medicinal curing-power! Schultes' own quotation of Petrullo (p. 708) notes that the devotees "hope to win the attention of the spirit-forces and their intercession for the sick person." *Via the vision!*

As for the argument that the new plant medicine succeeded chiefly on the basis of its medicinal virtues, Schultes quotes John Wilson to the effect that "as the peyote worshipper progressed in knowledge, he could ignore the effects of the native native pharmacopeia and effect his cures upon himself and others by the sole use of peyote." *Why* could one plant cause the casting-out of use of all other plants, if not because of its *additional* vision-power? And whence the "knowledge" if not through the vision-experience?

As a matter of fact, however, I believe the difference between Mr Schultes and myself derives from the ambiguity of meaning of the familiar term "medicine power" in Plains ethnography. To be sure in peyotism supernatural power vouchsafed in the vision-experience often is *power to cure* illness; yet the term "medicine power" encountered in the literature should by no means be read always as "power to cure illness." It is far more generalized, as I think I have shown in "The Peyote Cult," to mean on occasion power over the enemy in war, witchcraft prophylaxis, power to prophesy, to be clairvoyant, to find lost articles, etc. I think Mr Schultes is mistaken to equate "medicine-power" with *medicinal*-power, so far as peyote is concerned. And I think he is surely mistaken in believing that in Oklahoma there is a "disinterest in the peyote vision" or that there is "no indication of the pursuit of visions during peyote ceremonies." (p. 711). I collected 75 or so visions in the field and mentioned in the literature; Sunday forenoons after meetings, indeed, are usually spent in *telling about* ones peyote-visions!

Mr Schultes (p. 709) states that "The importance of the curing ritual in the peyote ceremony has been completely overlooked by those who have written on the subject." This statement has the appearance of being gravely unfair to the extensive discussion of curing and doctoring by peyote which I undertook to make in "The Peyote Cult." The facts are otherwise. The manuscript for Mr Schultes' article was prepared at a time when "The Peyote Cult" was unavailable to him, being then in press; this appeared before the publication of Mr Schultes' article, but subsequent to his absence on a field trip to Oaxaca, Mexico.

Incidentally Mr Schultes' authoritative work on the botany of peyote has been capped on this field trip by his discovery in Oaxaca of the *mushroom* "teonanacatl," which Safford erroneously identified with peyote. My own critique of Safford was

based entirely on textual criticism, which could only have become conclusive through Mr Schultes' authoritative botanical evidence.

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NORTHERN ARIZONA POTTERY: REPLY TO A  
REVIEW BY DR PAUL REITER

On pp. 489-491 of a recent issue of the *ANTHROPOLOGIST* (40: 3, July-September 1938) there is a review by Mr Paul Reiter of the *Handbook of Northern Arizona Pottery Wares* by H. S. Colton and L. L. Hargrave, recently issued by the Museum of Northern Arizona, Flagstaff. Certain statements, but more especially the general attitude of the reviewer, call for discussion.

Taking specific statements first, the one most needing comment is that "The authors consider their organization to be the synthesis which 'is only as accurate as the analyses which preceded it'." Now, this is simply not true. The *Handbook*, like most of the work of the Museum of Northern Arizona, is purely *analysis*, with synthesis only by implication. This detailed classification of pottery types is an attempt to make analysis sufficiently accurate that, later, syntheses can be made accurate. The statement in the introduction, misinterpreted by Mr Reiter, that "Synthesis is only as accurate as the analyses which preceded it"—a statement which might well be inscribed in gold on the walls of archaeological laboratories and class-rooms—is made as a criticism and a warning and is immediately followed by the statement: "Many students think that southwestern archaeology is a finished study because works of synthesis have been published, but the analysis that preceded this synthesis is, in many cases, insufficient for the conclusions drawn." The last is only too true.

Dr Colton and Mr Hargrave do not feel that their work is a synthesis or is in any way final. They are attempting to set up a uniform but usable system in southwestern prehistoric ceramics.

The modifications by Roberts and Kidder of the Pecos Conference *terminology* are not at all radical changes, but simply replacing of one set of names by others. Of course, however, dating the line between Pueblo IV and Pueblo V at A.D. 1600 instead of circa 1700 is a serious slip.

The implication by Mr Reiter that there is no valid difference between Kayenta black-on-white and Betatakin black-on-white is easily answered not only by handling the material but even by simply reading carefully the descriptions of these types in the *Handbook*.

The destruction of "Winona red-on-tan" is justifiable only on theoretical grounds; if Mr Reiter were to see that one sherd and realize its individuality he might feel differently. The immediately preceding type, "Winona black-on-red," is a more promising candidate for demolition, as it is based not only on one single sherd but on a sherd which certainly seems to have been altered by re-firing. This "type" must indeed be held in abeyance, at least.