

change as a function of geologic time. The result is that they assume and then assert as an axiom that Plateau culture has not changed.

A reference to the Goldendale site (p. 255) illustrates once more the problem of documentation in the Browman and Munsell paper: "However, a recent reanalysis of the Goldendale material has shown that there are no microcores present, and there are only two blade-like flakes. Because of the tenuous nature of these finds, as with the earlier report at Millard Creek, it does not seem reasonable to talk about the presence of microblades on the Plateau until our next cultural phase, Period IV." It is difficult to see how Browman and Munsell can be taken seriously when they pro-

ceed in this fashion. Microblades without cores may be common at some sites, but this should pose no difficulty in identifying a microblade. Who made the recent analysis? What criteria were used to establish that two microblades constitute tenuous evidence? Until Browman and Munsell document their case, the Goldendale site remains as published, not as they wish it to be.

There are so many instances in which published references are not used and no explanation is provided for opinions, that the paper offers nothing new in Plateau prehistory. To examine all the points will take another paper, but this may not be necessary until they choose to document their view.

### LETTERS

■ A criticism of Ritchie and Dragoo's (The eastern dispersal of Adena. NEW YORK STATE MUS. & SCI. SER., Bull. 379, 1960) hypothesis concerning a migration of "Adena" people into the northeastern United States by Donald K. Grayson appeared recently (AMER. ANT. 35:102-104, 1970). Grayson's criticism was with the statistical techniques employed by the authors in support of their hypothesis. I would like to offer a criticism based on more traditional archaeological methodology.

The authors offer a comparison of traits found at various northeastern sites with "diagnostic Adena traits" as well as with "less diagnostic Adena traits." The inclusion of traits in either of these two categories is based on earlier literature. It now appears that many of the traits in both of these categories cut across "cultural" lines and appear just as frequently in non-"Adena" sites as they do in "Adena" sites. The division of what was formerly considered a single archaeological unit into many geographically localized units now appears eminent. It appears that use of traits to indicate population movements must await the proper definition of that population.

A second criticism is based on the percentages of "Adena" traits at these sites as compiled by the authors. Grayson's statistical criticism notwithstanding, it appears that the figures offered by Ritchie and Dragoo for many of the sites are misleading. A restudy of several of the sites in detail by the writer indicates that a much greater number of local "run of the mill" artifact traits occurs on these sites than was previously realized. None of the sites used in

the study were excavated professionally and the inexperienced collectors often overlooked artifacts which were all too familiar to them. It was often not realized that these local artifacts could be associated with such spectacular and totally exotic "Adena" artifacts.

The writer has offered the suggestion that the exotic "Adena" artifacts found at these sites in association with local material and placed as grave goods in burials of local types can be explained as being due to the existence of a trading system. Evidence now exists that indicates that peoples of the northeast, and more particularly of the Middle Atlantic Coast, engaged in an extensive trading system that lasted, in varying degrees of elaboration, throughout most of the periods known as Early Woodland and Middle Woodland. Further study of this possibility is being conducted.

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May, 1970

■ I would like to bring two matters to the attention of the readers of *American Antiquity*.

First: in my paper "Engineering principles and the study of prehistoric structures" (AMER. ANT. 34:166-171, 1969), there is a transposition which already has misled researchers. A sentence reads, "The resistance of such a post . . . is roughly proportional to the product of its diameter squared and its depth of burial" (p. 168). The sentence should read, ". . . the product of its diameter and its depth of burial squared."

Second: an article, "A Hopewellian enclosure earthwork in the Illinois River Valley" (AMER. ANT. 32:391-393, 1967) by Patrick J. Munson, suggests that the Ogden-Fettie site in the central Illinois River valley is a geometrical earthwork. He bases his suggestion on aerial photographs taken in 1957 and 1963. This same earthwork shows on air photos taken by the Department of Agriculture on July 12, 1938, as very clearly non-geometrical. The photo numbers are RZ-9-792 and RZ-9-793.

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■ I regret that a factual error exists in the text, page 465, and in the bibliography, page 466, of

the obituary "Adan Eduardo Treganza, 1916-1968" (AMER. ANT. 34:462-466, 1969).

The official title of the museum at San Francisco State College is "The Adan E. Treganza Anthropology Museum," instead of "The Adan E. Treganza Museum of Anthropology and Archaeology," which appears in the article.

I accept full responsibility for the blunder, and ask that the corrected version be brought to the attention of the journal's readers, to whom I apologize for any inconvenience caused them.

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April, 1970