

SOME THOUGHTS ON THE BACKGROUND OF EARLY MAN,  
HRDLIČKA, AND FOLSOM\*

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A persistent feature of discussions dealing with the history of early man studies in the New World has been one which casts Aleš Hrdlička as a conservative villain (Judd 1967:70; Jennings 1968:34), and which spotlights the Folsom discoveries of 1926 as the "turning point" in a progressive battle for recognition of man's considerable antiquity in America (Wilmsen 1965:179-180). This is an interesting, and rather uncritical view, and is of particular interest, first, because Hrdlička's published estimates for the duration of man's occupation of the New World would allow almost twice as much of a time span as has been attributed to the Folsom finds (Graham and Heizer, 1967; Hrdlička 1925:491), and, secondly, because most of Hrdlička's objections to various claims of antiquity were based on interpretations of skeletal morphology and stratigraphy, and were not concerned with artifact discoveries such as Folsom (Hrdlicka 1907, 1918, 1937; Stewart 1949). Hrdlička, in fact, never questioned the association of human artifacts with extinct bison bones at Folsom, but did maintain his position against the concept of morphologically early man in America long after the Folsom finds had been generally accepted (Hrdlička 1937). Although more than one contemporary scholar has defended Hrdlička and pointed out that he cannot be considered entirely villainous (cf. Krieger 1964:24; Mason 1966:194; Graham and Heizer, 1967), the notion persists that he stood as a fortress against progress in early man studies.

As anthropology becomes more self-conscious as a discipline, and

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becomes more interested in its own history (Hymes 1962), such problems take on a heightened importance. From the point of view of intellectual history, it is unfortunate that no adequate history of American archaeology is available to set the problem into a proper sociological perspective (Braidwood 1959:82). This requires each investigator to discover and reconstruct for himself the intellectual climate and theoretical currents of the time with which he deals. In this regard, an abbreviated history of American archaeology in general up to the time of the 1926 Folsom finds will perhaps assist the reader in providing an historically-oriented view of Hrdlička's stance on some of the issues of his time. While the following discussion may by no means be considered comprehensive, it is felt, nonetheless, adequate to provide a background against which Hrdlička's views may be appraised with more objectivity.

In Europe, despite a Renaissance origin for general fascination with antiquities (cf. Heizer 1962; Daniel 1967; Lynch and Lynch 1968), archaeology as a science had a prolonged birthdate of between 1840 and 1870 (Daniel 1950: 54-121). Prehistoric archaeology, to which American early man studies were roughly equivalent, had its "coming of age" about 1870 (Ibid.:111). Developing rapidly in technique and sophistication, European archaeology, including prehistoric and even Paleolithic studies, reached the level of a mature science between 1870 and 1900 (Ibid.:122-151). These European developments are worth noting because of the interesting parallels in some aspects of archaeology in America which may be drawn from them.

In the United States, although there had been a certain interest in archaeology since Colonial times (Wilmsen 1965:172-173; Griffin 1959: 379), most of this interest was a result of white curiosity about American Indians, and was rather more anecdotal and incidental than it was scholarly. Very few entire works were devoted to serious archaeological inquiry (Atwater, 1820, being the one notable exception), and Jefferson's few pages (Heizer 1959:218-221) in the lengthy Notes on the State of Virginia are probably typical of the manner in which pre-white remains were regarded in the early decades of the Republic.

One of the first widely read books on American archaeology, and one which still retains considerable scholarly value, was the famous Incidents of Travel in Central America, Chiapas, and Yucatan by John Lloyd Stephens, first published in 1812.<sup>1</sup> The book contained abundant facts of great academic interest, was illustrated with the unsurpassed drawings of Frederick Catherwood, and was replete with enough tales of hardship, dangerous encounters, sacred wells and their sacrificial victims to excite the curiosity of the average layman, or the romantically inclined armchair adventurer. It dealt, of course, with the spectacular ruins of the Maya civilization, and is of historical importance and interest for at least two reasons. First, it appeared at a time when archaeology as a science was being born in Europe and was a reflection of a parallel interest in America.<sup>2</sup> Both Stephens and Catherwood had visited the spectacular ruins of the Middle East (von Hagen 1950:42; Ceram 1958:280), were familiar with European archaeological thought, and from the excellence of their work in Yucatan, it is not unreasonable to assume that they anticipated a more rigorous scholarly treatment of the Maya ruins than of the curiosities of antiquity which had previously been commented on in America. Secondly, due to its popularity in America within academic as well as popular circles, the work apparently stimulated and led to a wide general interest in spectacular sites in North America. Stephens' writings and Catherwood's illustrations had impressed many readers with the monumentality of the Maya sites, and it is tempting to speculate that the inaccessibility of, and prohibitive expense of travel to, the impressive ruins of Mexico and Guatemala led most researchers in the United States to seek a similar or surrogate monumentality and grandeur in the ancient remains at home. Attention was centered on the large, and often elaborate, earthen mounds which abounded in the forests of the Eastern United States. Thus, we find such titles as Ancient Monuments of the Mississippi Valley (Squier and Davis 1848), Aboriginal Monuments of the State of New York (Squier 1849), and Antiquities of Wisconsin (Lapham 1855) to be indicative of the trend of orientation which followed the

work of Stephens and Catherwood, and which held the prime archaeological interest of America for some thirty years.

The tenor of the trend during this period (i.e., 1840-1870) was in descriptions of the more grandiose aspects of the monuments. Very little attention was paid to the artifacts associated with these mounds. "What few artifactual details made their way into print did so purely incidentally and secondary to the prevailing concern with monumental and architectural features." (Taylor 1948:22). Speculations were made concerning the origins of these mounds. Many of the speculations were outrightly ludicrous and unscientific, and consisted of such theories as proposing a vast antediluvian continent of Atlantis (Donnelly 1882), which provided the necessary link to connect the people of the Mississippi Valley to the Chaldeans (Ibid.:139-140), and the "Mound builders of Michigan" to the "man of Cro-Magnon and the ancient inhabitants of Wales" (Ibid.:273). In most cases, Biblical explanations were invoked (Griffin 1959:379-380). The Mound Builders often were linked to the "lost tribes of Israel" (Holmes 1919:14; cf. Randall 1908; Wauchope 1962). In all, a great deal of time and research in archaeology was "devoted to efforts at harmonizing the results of the rising science with the biblical Genesis" (Mitra 1933:81-82).

Shortly after 1870, and without doubt gaining a stimulus from European interest in "Stone Age" archaeology (Daniels 1950:111), American students began to turn their studies toward early man investigation. Such titles as Stone Age in New Jersey (Abbott 1877), Pre-Glacial Man in Ohio (Wright 1888), and Man and the Mastodon (Putnam 1885) are typical of the American parallel to European interest at the time. With this interest in early man, an important shift in the orientation of American archaeology may be noted. This is the concern over artifacts and their association. Much of this interest was generated by an insistent U.S. Congress, a Congress whose members "were less interested in Indian languages, customs, and governments than in specimens for the National Museum -- specimens that would be seen by visitors to the capital city" (Judd 1967:18).

It was not long before this interest in artifacts had reached the point where "the digging of sites for specimens and other information had become an end in itself" (Taylor 1948:23). These objects were, of course, worth little to scholars as mere objects, and it was their interpretative significance, rather than their public appeal, that became of paramount importance.

Unfortunately, some of the interpretations which were brought forth were not particularly credible, and, like some of the explanations for the large mounds in the eastern U.S., did a good deal to tarnish the image of archaeology as a science (cf. Willey 1968:30-32). Encouraged by the fact that in Europe the battle for Pleistocene man had been fought and won, and excited by human bones and implements turning up in America in apparent association with Pleistocene faunal remains, some American archaeological workers enthusiastically assigned great age to the tenure of man's occupancy in the New World. A damaging degree of recklessness was often shown. One skeletal "discovery" in California would have required "a human race older by at least one-half than Pithecanthropus erectus" to adequately explain it (Holmes 1919:61), while a human pelvis found in New Orleans (cf. Lyell 1863:200-205) was said by Dowler to have an age of at least fifty thousand years (Young 1882:184). Even such well-known scholars as F. W. Putnam and Thomas Wilson casually referred to man's existence in America as stemming from Pleistocene times or "possibly still earlier" (Griffin 1959:382). At the same time, regardless of how obviously wrong such claims now appear to us, it should be remembered that the chronology of the Pleistocene was not at all understood, and no person knew how old man was on the earth. In 1860 with J. D. Whitney's acceptance of the Calaveras skull as a Pliocene representative of man, there was really nothing in the way of evidence to say he was wrong.

At this point, it must be observed that the men who advanced these opinions could not have been expected, in most cases, to do otherwise, considering the intellectual history of their times. American

archaeology, it must be remembered, had scarcely been defined as a science. Very little formal training was at that time readily available. Although a few museums maintained loose connections with universities, archaeology was not a classroom topic, "and neither museum curators nor anthropologists looked to the graduate school as the proper place for anthropological research, training, and publication" (Freeman 1965:81). Therefore, "many of the leading personalities in archaeology of the pre-World War I era had little or no formal training in archaeology" (Griffin 1959:387). Very few had specific knowledge to apply to particular American problems (Holmes 1919:75). Many others were self-styled adventurers, more interested in enjoying travel, romance, and pseudo-scientific controversy than in attempting to establish archaeology as a science. Even to the present day, archaeology has been plagued by the problem of outside imposters. As Braidwood has noted:

Upon their own declaration, the public and its press readily accepted as archaeologists well-meaning Boy Scouts, bored farmers, clerics, and country doctors; ladies-club lecturers, journalists, and publishers who knew a good bet when they see it; and even oil-lands promoters. The exasperating complications to -- and even blocking of scholarly research which some of these self-designated archaeologists cause are well known to us all. There is probably no other profession so vulnerable to the poseur and the charlatan. (1959:77)

And indeed early American archaeology suffered greatly from the acts of charlatans, crackpots, and hoaxters. Combined with the taint of over-enthusiastic, dubious interpretations which had been made concerning serious finds, the work of tricksters tended to make much of archaeology as exercise in exposing farces (Holmes 1919:70). One perhaps typical example is the following:

In the Grave Creek Mound is said to have been found a small oval disk of white sandstone, on which there were engraved twenty-two letters. I mention it because it has been the subject of much discussion, but it is now generally admitted to be a fraud. It is inscribed with Hebrew characters, but the forger has copied the modern instead of the ancient form of the letters. (Lubbock 1906:221)

Other problematical discoveries were less easy to reject, and the controversies over such finds as the Calaveras skull (cf. Holmes 1901, 1907) and the Quaternary cave finds in California (cf. Putnam 1906) went well into the twentieth century.

Thus, archaeology, due to over-enthusiasm, lack of scientific methodology, and embarrassments caused by hoaxes, was not a very confident, or respected science by the year 1900. Only as a reaction to this state of affairs can Hrdlička's attitudes of the early 1900's be completely understood.

By 1900, for a number of reasons, archaeology in North America had turned sharply away from early man studies (Wilmsen 1965:178-180). That is, practicing scientists who considered themselves to be archaeologists were no longer positively active in this field of research. They were, on the other hand, often active in a negative manner, attempting to disprove and depreciate the few so-called early man finds, which were turned up by paleontologists and geologists. (The fact that these disciplines had "grabbed the ball," as it were, may help to explain the interest of such men as J. C. Merriam and W. H. Sellards in the early man problem. Merriam is remembered in connection particularly with the La Brea skull, and various California cave explorations, and Sellards is associated with the controversial Vero Beach material.) Bryan has written:

For many years there had been one attempt after another to prove great antiquity for man in America. One alleged find after another had been made and many, alas, had been demonstrated as mistaken or even fraudulent. As a result, archaeologists became expert in discrediting "finds" purporting to show the existence of ancient man. The rejection of any such claim became more or less automatic. Opinion was crystallizing into a dogma to the effect that man is a very recent immigrant to North America. (1937:137)

The persons usually held responsible for these dogmatic attitudes were William H. Holmes and Aleš Hrdlička of the Division of Anthropology, U.S. National Museum.

Holmes, as chief of the Bureau of American Ethnology, bluntly placed his convictions on record in 1907:

The Bureau of American Ethnology, from its foundation has taken a deep interest in all research relating to the antiquity of man in America, and its attitude in considering the various questions that have arisen has been conservative. In the earlier years of the investigation there existed a marked tendency on the part of students, and especially on the part of amateurs and the general public, hastily to accept any testimony that seemed to favor antiquity, and the conservative attitude of the Bureau was emphasized by a desire to counteract and correct this tendency. (Holmes, cited in Hrdlička 1907:3).

When we consider the intellectual history of American archaeology for the thirty years prior to this remark of Holmes', it seems to be the most appropriate approach which could be taken. Holmes' attitude should be seen, not as a dogmatic one which served to frighten scholars away from the topic of early man, as is sometimes suggested (Wilmsen 1965:179; Roberts 1940:52), but as one which sought to warn against or correct the uncritical acceptance of unsupportable claims of earlier years.

Hrdlička, commenting on the same topic, is not posing obstacles to early man research, but is instead calling for more rigorous methodology, one which can stand up under close scrutiny.

As chances of peculiar associations of human bones or human artifacts are infinite, anthropology in this country must expect to be called upon again and again to pass on alluring claims to the antiquity of such objects. But the burden of proof of the antiquity of such finds lies, and will always lie, with those who urge such claims. They must show clear, conclusive, full evidence acceptable to anthropology; and no beliefs, opinions, or "convictions," even though advanced by men otherwise highly deserving, can ever take the place of real and sufficient evidence. (Hrdlička 1918:60)

Noting that the idea of early man in America was not unacceptable, Hrdlička demanded that its acceptance be based on "indisputable evidence" (Hrdlička 1907:13), and rejected earlier claims only because they were supported by "not a single fact" (Hrdlička 1918:38).

Realizing, quite correctly, that archaeology had developed very little methodology of its own, Hrdlička suggested that it institute some, and enlist the aid of other disciplines as well. "Finds of osseous

remains suggesting man of other than the recent period should be photographed in situ," he wrote, "and should be examined by more than one man of science, including especially a geologist" (Hrdlička 1907:11). He later suggested again "that geology and especially paleontology can be, on occasion, of the greatest aid to anthropology in determining the age of human remains" (Hrdlička 1918:35). Here again Hrdlička may scarcely be accused of intellectual obstructionism. He is, however, asking for certain methodological improvements which were very badly needed in the archaeology of that time.

Archaeology, not only because of its earlier ineptitude in critically handling early man problems, but as a sub-discipline in general, was apparently widely regarded as hanging on the coat tails of anthropology. Holmes felt that "archaeology stands quite apart from. . . the science of man" (Holmes 1919:2). E. B. Howard, as late as 1936, still regarded archaeology as a separate field from anthropology when man's antiquity in North America was to be considered (Howard 1936:395). Of course, the unique development of state and local historical societies, and their intense preoccupation with American Indians (Boyd 1934:10-37) tended to draw archaeological interests into an historical frame of reference. But most anthropology in America between 1880 and 1920 was inspired by North American Indians (Mead and Bunzel 1960), so this cannot be offered to explain why prominent (and, incidentally, historically-oriented) anthropologists looked upon the practice of archaeology with disrespect. Franz Boas, for example, is reported to have stated with acidity, "If a man finds a pot, he is an archaeologist; if two, a great archaeologist; three, a renowned archaeologist" (Mason 1943:58). Moreover, Boas, in his years as professor at Columbia beginning in 1889 (Kardiner and Preble 1961:120) expressed his disdain for archaeology and "was never compelled to take it within his purview" (Mason 1943:65-66). As curator at the American Museum of Natural History he also left archaeology rather strictly alone (Ibid.).

Nor did A. L. Kroeber have much praise for American archaeology

of this period.

Incredible as it now may seem, by 1915-1925 so little time perspective had been achieved in archaeology that Wissler and I in trying to reconstruct the native American past, could then actually infer more from the distribution and typology of ethnographic data than from the archaeologists' determinations. Our inferences were not too exact, but they were broader than those from excavations. (Kroeber 1952:151)

This lack of time perspective can hardly be thought of as surprising when we consider that at this time archaeology had not yet adapted use of stratigraphy and seriation to determine chronologies, the relative age of the eastern mounds was still thought of as limited, and ancient man was no longer a topic of investigation (cf. Dixon 1913). The archaeologists of the time "saw their archaeological pasts almost completely flat" (Kroeber 1952:151). Archaeology, as a science, was "little better than ethnology gone cold" (Amsden 1931:46-47).

When compared to the attitudes of such figures as Boas and Kroeber during this period, and when the objective status of archaeology at that time is recalled, Hrdlička's remarks seem to fall on the benevolent side, and his methodological suggestions take on the character of much needed constructive criticisms. And indeed, there were developments taking place within archaeology that ultimately were to make it into a successful scientific endeavor. What were needed to make archaeology a reputable science were techniques such as seriation, taxonomic schemes such as are used in typological concepts, and the use of stratigraphical methodology. This would allow for chronologies to be developed so that prehistoric time could be determined, at least on a relative basis (Griffin 1959:389). As early as 1902, crudely applied stratigraphic techniques were being used by Uhle on the Emeryville shell mound on San Francisco Bay (Uhle 1907:1-84). This approach, and its results, were disregarded at the time by Kroeber as "insignificant" (Rowe 1962:399). Later in his career, however, Kroeber changed his mind, and "became one of the leaders in the movement to introduce time into American archaeology" (Rowe 1961:2).

Nels Nelson, whose work was later heralded as responsible for bringing stratigraphy into use in American archaeology, was, likewise, conducting limited stratigraphical experimentation in his excavations on California shell mounds. The Peabody Museum workers at the Whaleback Shellheap, in fact, as early as 1886, had been aware of depositional units of a physical nature (Hodge 1910:937-938).

Franz Boas, in spite of his disparaging attitudes towards archaeology, was another early figure to attack the question of "how to determine the chronological sequence" of archaeological remains "by observations on geological sequence of strata" (Boas 1915:385). As director of the International School of American Archaeology and Ethnology during 1911-1912, he carried out research in the Valley of Mexico. "He at once realized the importance of geological aspects of the work and secured the collaboration of Dr. Jorge Engerrand, then primarily a geologist, to aid in their solution" (Mason 1943:61). It is of interest to note that Boas had, like Kroeber, undergone a change of mind concerning use of such methods. In 1902 he had written an article making no mention of stratigraphy, but instead had suggested the approach of connecting "ethnological and linguistic methods" with a vague archaeological practice (Boas 1902:1-6). While in Mexico, after noting that ceramic differences existed, he began to tie together the ideas of culture change and stratigraphy (see Gamio 1959:117-118).

It is tempting to postulate the influence of Engerrand, who "was freshly arrived from Europe with wide experience in geological stratigraphy and the current field work being carried out in European Paleolithic archaeology" (Graham 1962:25), upon Boas at this time. Boas had not mentioned the idea in his previous writings and, after working with Engerrand, had gained such a high "estimate of the value of geology that he urged and arranged the appointment of Engerrand as his successor as director" (Mason 1943:61). It would certainly seem that the Boas-Engerrand acquaintance resulted in a healthy exposé of techniques for archaeology, even though it did not gain wide acceptance until years later.

This acceptance may be noted in the famous stratigraphical work of Nels Nelson (Nelson 1914, 1916, 1917). Nelson has acknowledged that he utilized stratigraphic techniques learned while working in Europe at Castillo Cave (Woodbury 1960:98), where such methodology was in more common practice. His efforts were immediately noticed and borrowed by such scholars as Kroeber, Kidder, and Leslie Spier (Woodbury 1960:401; Spier 1917, 1931). (It is difficult, in this respect, to understand why Haag feels that Nelson's methodological suggestions were "not completely overlooked or ignored, but very nearly so" [Haag 1959:92]. Perhaps he is implying that the many dilettantes of the time were not interested in stratigraphy.)

However, the important point here is that, despite the fact that they were unknown to and unused by most archaeologists, these important research techniques were being developed and made workable for field practice by several of anthropology's foremost scholars. These tools, once perfected, were to provide American archaeology with the methodological equipment it needed to assume a responsible scientific position in the study of New World prehistory, especially early man studies. All that was needed now was for archaeology to be forcefully set to its task. This, in effect, is what occurred as a result of the discoveries at Folsom, New Mexico, in the summer of 1926.

The importance of the Folsom find is not that it "disproved" Hrdlička, because, as shown above, such was not the case. What is important is that the discoveries at Folsom did prove positively the value of rigorous methodology of the kind that Hrdlička had called for again and again. In the face of the predominant skepticism of the time when the find was made, the careful work and clear associations in situ made believers of the most skeptical. This is best seen in a brief review of the circumstances surrounding the discoveries.

In the summer of 1925, a Negro cowboy discovered bones projecting from the sides of an arroyo on a ranch in Union County, New Mexico (Sellards 1952:47). Word of the find was sent to Jesse Figgins of the

Colorado Museum of Natural History in Denver, and the following summer he sent a paleontological field party to work the area (Roberts 1935:4). The party observed clear associations of extinct bison bones with man-made objects of flint. Excited by the discovery, a large block of earth containing one of the flint points and the bison bones was cut out and sent to the Denver Museum. "Dr. Figgins was so impressed with the find and was so thoroughly convinced that it was of importance to students of American archaeology that he took the points with him that winter when he visited several of the large eastern museums on paleontological business" (Ibid.). However, the dogmatists were hard to convince, and as a result, "the information was skeptically received in most quarters and he was given little encouragement" (Roberts 1937:154). Many archaeologists went so far as to challenge Figgins' ability as an archaeologist (apparently in ignorance of the fact that a careful paleontologist employs the same excavation techniques as an archaeologist), and expressed doubt concerning "the trustworthiness of the association" (Roberts 1935:4). However, Figgins was undaunted and sent another party to the site in the summer of 1927 (Figgins 1927). Additional positive associations were unearthed, but this time work was halted at the moment of encountering another example of a flint artifact in direct association with the bison bones and "telegrams were sent to various institutions inviting them to send representatives to view the point in situ" (Roberts 1937:154). Two scholars responded: Barnum Brown (paleontologist) of the American Museum of Natural History in New York, and Frank Roberts (archaeologist) of the Bureau of American Ethnology. The two men viewed the evidence and decided that "there was no question but that the association was authentic" (Ibid.). It is worth pointing out that in situ verification, and stratigraphic reliability were exactly the demands that Hrdlička had so often made. It was only studies which lacked methodological rigor and objectivity which he relentlessly attacked.

Roberts then went to the nearby town of Raton and sent a telegram to A. V. Kidder in which he "urged that he also visit the site"

(Roberts 1937:154). Two days later, Kidder arrived and investigated the excavation. He too was convinced, and after the whole situation had been carefully studied, it was agreed that "the association could not be questioned" (Roberts 1935:5).

In December, 1927, at Andover, Massachusetts, B. Brown, A. Kidder, and F. Roberts faced the American Anthropological Association and reported on the Folsom finds. As could be expected the report was received with "a general feeling of doubt" (Ibid.). "In spite of the convincing nature of the evidence, most of the anthropologists continued to doubt the validity of the discovery" (Roberts 1937:154-155). Some refused to accept the reality of the association, while others insisted that "there must be some mistake about the antiquity of the animal remains" (Roberts 1935:5). In spite of this, Barnum Brown personally returned to the Folsom site in the summer of 1928 as leader of an American Museum of Natural History field party. For the third consecutive summer positive associations of human implements with extinct forms of bison were uncovered. Telegrams were once again sent to a variety of institutions.

This time numerous specialists, archaeologists, paleontologists, and geologists rushed to see the evidence. The consensus of the informal conference held at the site was that this constituted the most important contribution yet made to American archaeology. Some of the most skeptical critics of the year before became enthusiastic converts. (Ibid.)

The confirmation of the Folsom evidence is interesting for the parallel it presents with the acceptance of the finds of Boucher de Perthes at Amiens, France, in 1859 by the English scholars Evans and Prestwich (see Heizer 1962:82-83, 93-94).

The progress of early man studies in America since 1928 is better known than is that from before the Folsom discoveries. Great strides have been made, and no one today questions the antiquity of man in the New World as demonstrated at Folsom. Roberts excavated the famous Lindenmeier site in Colorado a few years later, and from that time on

there have been so many finds that it is impossible to list references here. Haag states that "from 1934 until the beginning of World War II, American archaeology underwent a tremendous growth" (Haag 1959:93). Of course, this growth was enhanced by the use of stratigraphical and seriation methods which had by then gained acceptance, and which could yield valuable and scientifically defensible results when applied to much of the American archaeological material. The shift in technique was quickly noticeable. Wissler, in 1938, commented that "the object in archaeology has changed; every field worker is now intent upon stratification and sequence, and not so interested in horizontal distribution" (Wissler 1938:263). The finds at Folsom are, to a large degree, responsible for the changes noted by Wissler. At Folsom, archaeology was somewhat forcibly cured of its "flat past," and could thereby consider the problem of sequence. Also, the use of scientific field methodology, with associations and stratigraphic context taken into consideration, was shown to be as important as Hrdlička had insisted. Thirdly, the idea of typology became more meaningful after Folsom, since a distinctive type of point was noted as associated with a specific variety of extinct beast. Folsom points, in fact, are actually the first time or "horizon" markers to be recognized in America (Clewlow 1967:141-142).

The main point of this paper has been not to imply that it was due to Hrdlička that the Folsom discovery had the influence and far-reaching effect that it did. This would be foolish and unprovable. However, it is pointed out that the very improvements in archaeology which Hrdlička argued for are among the ingredients of success at Folsom and thereafter. While it is true that Hrdlička may have occasionally allowed his enthusiasm for proving a point to carry him across what are now accepted as ethical lines (Stewart 1946), he was nonetheless generally correct in his criticisms of early man studies until the late 1920's. It is suggested, then, that rather than blame Hrdlička for the fact that archaeology did not improve itself sooner (which is essentially what is involved in labelling him as completely against the ideas of early man

in America), it would be more accurate to judge his views in the more complex light of the status of archaeology of his time. Thus, present-day scholars might regard him with more of the intellectual respect which he is certainly due.

#### NOTES

1. It is of interest to note that the narrative of Stephens and the illustrations of Catherwood followed somewhat the same format as the travel descriptions of Dupaix and the fine drawings of Castañeda done in Mexico in 1805-1807, and reprinted in 1831 by Kingsborough (cf. Ceram 1958:272-277). These earlier works, however, never attracted the popularity associated with the Stephens and Catherwood efforts which in their day were "best sellers."
2. The deeds of Elgin and Belzoni were no doubt on his mind when Stephens purchased Copan for fifty dollars, hoping to transport the sculptures to New York as a New World counterpart to the Parthenon casts (cf. Stephens 1947; Ceram 1966).

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