

SOME INTERIM REMARKS ON THE COE-STUCKENRATH REVIEW

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On January 15, 1963, W. R. Coe (one of the authors of the preceding paper) sent to me the manuscript published here, asking for comments and possible corrections. I was on the point of leaving on a six-months sabbatical leave, and in fact sailed from San Francisco on January 28. I sent a copy of the manuscript to R. J. Squier who had participated in the 1955 excavation of La Venta and who was one of the authors of the Drucker-Heizer-Squier report on these investigations published as Bulletin 170 of the Bureau of American Ethnology (referred to here as DHS), and suggested to him that in August 1964 we spend some time working together in Berkeley with the original field notes and prepare an answer to the questions raised by Coe and Stuckenrath in their detailed review of the report on the 1955 excavations and to publish the papers together. Coe, incidentally, was agreeable to this procedure. Squier and I did work together in August in Berkeley and drafted an outline of our answer, but for one reason or another we have failed to get this into finished form at this time (December 1964). Accordingly, since the Kroeber Anthropological Society is willing to print the Coe-Stuckenrath paper alone, the Squier-Heizer rejoinder will have to be printed separately--we anticipate its appearance in the next number of the Kroeber Anthropological Society Papers.

These following remarks are provided in the hope that they will aid the reader in acquiring some perspective. First, both Squier and I are flattered that two such able workers thought it worth their time to read and analyze with such care a report which we had helped write; and second, we were somewhat disappointed to learn that the report contained so many obscurities, uncertainties, and lapses as to require 75 pages of typescript to present them. While we were fully conscious that there were deficiencies of data, we were unaware that these were sufficient to allow the reality of the four major building phases to be questioned seriously. Complex A at the La Venta site was large, and the supervisory crew consisting of Drucker, Squier, and myself were kept very busy keeping an eye on the excavating (done by local workers), drawing profiles, surveying, making notes, taking photographs, and engaging in practically continuous verbal analysis of the cumulative picture as it emerged from day-after-day work. We tried not to neglect observing and noting things which seemed important, and as the work continued we moved small gangs of laborers to this spot or that spot to check points on what we believed was the pattern that was emerging. Of course, we did not do enough of this, but even if we had had twice the funds and twice the labor force, the three of us probably could have done not much more than we actually managed to do in the spring of 1955. We did feel when we finished that we understood the main history of the site itself, but realized only too well that there were many things about which we had learned little. For example, we were unable to assign any of the numerous sculptured monuments to specific building phases, much less even to such vague time divisions as "early" or "late" in the site's history. But these failures were not the result of lack of awareness of the problems but, as in the case of the sculptured monuments, to the absence of the kinds of data needed to elucidate them. The physical tracing of floor series so that each structure could be lineally and actually connected with at least one other was not done by us in a systematic fashion. We realized then, and

are even more conscious today, that this was important and that only by doing this could the excavation be said to be well done. But we did not do this in all cases, partly for the reason that we were short-handed, partly for the reason that individual floors often thinned out and were replaced by another of different color and thickness, partly because they were often interrupted by intrusive offering pits, and partly because we often did not link up one structure with another with an excavation which provided a continuous section. We saw, rather, a series of main flooring or surfacing layers which occurred pretty much throughout the site in the same sequence, and came to regard these as being the stratigraphic-constructional-functional equivalents of each other as they appeared in their expected sequential order as the excavation proceeded. The fine matchings and tying-in that would be expected of well-trained and competent archaeologists we seem not to have accomplished. This lapse is one of the criticisms which Coe and Stuckenrath have made. Some of their questions we can answer; others we cannot since we do not have the information.

Coe and Stuckenrath pay a good deal of attention to the series of nine radiocarbon dates (M-528 to M-536) and raise a number of questions about our interpretation of the nine dates which we intend to discuss at length in our paper now in preparation. It may be of interest for the reader to know that the nine Michigan dates probably do not contain serious laboratory errors. In 1955 when Drucker returned to Washington he divided two of the charcoal samples and submitted these two to the USGS radiocarbon laboratory. The remaining halves were dated by Michigan and designated M-528, M-533. In December 1964, Dr. W. F. Libby and Dr. R. Berger were kind enough to date the two samples originally submitted to the USGS laboratory and which Dr. Myer Rubin returned at my request. The results are as follows:

M-528	2560 \pm 300 yrs. B.P.	= UCLA-902	2940 \pm 80 yrs. B.P.
M-533	2130 \pm 300 yrs. B.P.	= UCLA-903	2460 \pm 80 yrs. B.P.

There is no space here to discuss, even briefly, the problems raised by Coe and Stuckenrath about massive offerings, mosaic pavements, burials, small dedicatory offerings and stone monuments. I can say that when Squier and I publish our comments on the present paper, we will reaffirm our belief that there were four main and distinguishable building phases (Phases I-IV) at the La Venta site, and that we will defend most of the assignments of small and large offerings to the phases originally specified by us. Where we have queried the phase attribution of an offering or construction feature we will explain why we did so. In some instances we can now be more certain of the stratigraphic placement of buried materials about which we were earlier doubtful.

The reader will learn that the major purpose of questioning the intra-site sequence is to cast doubt upon the placement in time of items which can be identified as done in the Olmec style. Coe and Stuckenrath apparently wish to have anything that is specifically Olmec to be very late in the history of the La Venta site and a substantial part of their paper is aimed at trying to interpret all Olmec traits as belonging to Phase IV. For large sculptured monuments it is not possible to say that any can be proved beyond doubt to have been made and placed before Phase IV times, so that we cannot cite these as evidence of the continuity of Olmec culture through the history of the site. The same is not true of many of the small lots which were designated as "dedicatory caches" or "offerings," nor for some of the large "pavements"

which include both the solid and unpatterned layers of rectangular serpentine blocks as well as the "mosaic masks" composed of patterned layouts of shaped serpentine blocks.

The two so-called "mosaic masks" or "jaguar mosaic masks" which were found in the Southwest and Southeast Platforms are of significance in several regards to the site's building history and its interpretation. Coe and Stuck-enrath have queried our conclusion (actually an assumption since we did not tie the two platforms together with a continuous stratigraphic trench) that the two platforms were built at the same time. Our belief that the two are so closely coeval in time as to be contemporaneous was based on what we felt in 1955, and still believe, to be practical identity between the two features. Wedel, in excavating the Southeast Platform in 1942, did not do as complete a dissection as we did in the Southwest Platform in 1955, so we cannot compare all of the elements of each construction. Superficially the two platforms are twins--they occupy positions equidistant from the centerline on the southern margin of the Court; each was evidenced on the surface as a low adobe brick platform whose border was ringed by a rectangle of upright basalt columns; with the two sub-brickwork fills were cruciform celt caches; beneath the brickwork in each was a fill of the same color, consistency, and thickness; below this fill were almost identical "mosaics" of serpentine blocks (one of 485, one of 497 blocks), and; beneath the mosaics was a thick "foundation" or bed of rough serpentine quarry blocks set in clay mortar. If one compares the two mosaics (DHS:Fig. 29; Wedel 1952:Fig. 20) he will see that they are so much alike that they may be called practically identical. If the two mosaics are separated in time as regards their deposition, their remarkable similarity speaks for an uninterrupted and highly specific formal continuity. While this is not impossible, it is difficult to believe that two features so nearly similar could be separated in time by as much as a year or ten years or a generation or whatever brief span of time might be assumed necessary for exact details of a complicated buried feature to be forgotten. These were our thoughts in 1955, and they remain ours in 1964.

As for the mosaic which is generally similar that was found by Wedel in the centerline trench in 1942 and called by him Pavement No. 2, we have guessed that it dates from Phase IV, but we were and are at the moment not certain of this attribution. As excavators of the site, we believe it most probable that it is assignable to Phase IV, and if this be so, its differences from the similar mosaics in the two platforms can be looked at as evidence of stylistic change over time. We will discuss this in detail later.

The authors challenge our identification of the mosaic in the Southwest Platform, which was excavated in 1955, as representing the jaguar face, as well as our interpretation of the several features such as eyes, nose, mouth, and central notch. Here we are relieved to be able to say that the mosaic "masks" from the first constituted for us a difficult problem of representational intent by their makers. Heizer and Squier preferred, as the merest beginners in Mexican archaeology, to leave this matter to our senior colleague, Drucker, who had thought more about the Olmec art style than we had. Drucker also was puzzled by the mosaic masks, but because we all saw or sensed the La Venta site as a manifestation of the continuity of the same group, and because the Olmec art style was abundantly evidenced, we perhaps too easily assumed that the mosaics were representations of the jaguar. In 1942 Wedel had guessed (Wedel 1952:56) that the Southeast Platform mosaic

represented "apparently, a conventionalized jaguar mask." This suggestion, however tentatively stated by him, may have influenced us and may have been the source of our conclusion that the mosaics portrayed the jaguar. Now we can say that we modify the unqualified opinion that these are Olmec jaguars and take the position that they may be, or may not be. It was obvious to Coe and Stuckenrath that we had failed to examine the question of continuity of Olmec art style evidence in the site's history, and this we also admit. But, on reviewing this matter, we find some quite obvious stylistic connections between the mosaics with designs on engraved celts from the La Venta site, and by this means believe we can show that the unique mosaics are linked and contemporaneous with undoubted examples of the developed Olmec art style.

Figure 1 a shows the layout of the mosaic from the Southwest Platform. It is shown here in reversed position to that in which it was presented by us in publication (DHS:Fig. 29) and in its present position we believe it to be correct. Here we are probably in agreement with Coe and Stuckenrath, as well as with M. Coe (n.d.). First, the notch should be a forehead, and not a chin cleft; second, the "headdress" or "plumes" (DHS:93) or "appendages" (Drucker 1952:58-59) look better as pendant decorations than as upright "plumes." Indeed, these four elements strongly suggest a series of stone beads, longitudinally drilled, strung together with cords. If this be their stimulus, they probably hung around the neck, and this suggests that the mosaic is in fact some sort of facial representation, though not necessarily, we repeat, that of the jaguar. However, the forehead notch suggests a feline and therefore the jaguar, which is usually represented in Olmec art as having such a forehead notch or cleft, is not ruled out.

Now let us turn to certain items from the La Venta site that appear to us to show stylistic similarities to the three mosaic masks (Pavement No. 2; Southwest Platform mosaic; Southeast Platform mosaic). These items are the five stone celt engravings shown in Figure 1 b-f. Figure 1 b is from Mound A-2, Offering 1942-C (Drucker 1952:165; DHS:213-214) and here one sees the frontal notch or cleft, the vertical "lineal" nose, and four circles or discs whose identification is doubtful, but may be suggested as eyes (the two upper circles) and mouth or discs (labrets?) affixed to the face near each end of the mouth (cf. DHS:Fig. 43 a,b--the two lower circles). Above the frontal cleft or notch is a flower-like element which may be a forehead or head decoration, but whose exact identification is unknown. Figure 1 c also from Offering 1942-C is basically similar. This offering dates, we believe, from Phase IV. Figure 1 d comes from Offering No. 2 (DHS:141) and is again similar, but has two engraved elements which may represent eyebrows, or a halved or divided element equivalent to the inverted U below the forehead notch of the mosaic mask. Finally, Figure 1 e also from Offering No. 2, shows a cross-legged and masked(?) man whose head is shown in profile. On the side of the face are two discs or circles, and on the forehead, probably to be interpreted as part of a mask, is a band whose lower border bears notching. The engraving is certainly no great work of art, but because of its similarity both to the design shown in Figure 1 d and to the three mosaic masks, deserves to be considered as stylistically related. Now, an interesting association in Offering No. 2 with the engraved celts shown in Figure 1 d, e, is the engraved design on another celt shown in Figure 1 f and which is surely in the best tradition of Olmec style (cf. DHS:Fig. 36). We do not feel that we are pressing too hard to argue that the Phase II mosaic masks in the Southwest and

Southeast Platforms can be stylistically affiliated with the engraved designs from Offering No. 2 which we have attributed to Phase III. A complex series of additional resemblances could be pointed out, but we believe enough evidence has been given to show a connection between the Phase II mosaic masks and the Phase III and Phase IV engraved celts. Our argument here on continuity, we should remind the reader, is based upon our continued adherence to the four-phase sequence interpretation of construction periods. When we have the Olmec style occurring in Phases II, III and IV, we therefore refer to this as evidence of the Olmec continuum at the La Venta site. We still believe it probable that the jade "maskette" from Offering No. 7 (DHS:Fig. 43b, p. 171) dates from Phase I, and that the two figurines from Offering No. 3 (DHS:Pl. 26, p. 146) date from Phase II. These pieces support the view of Olmec occupation through the whole period of use of the La Venta site. I cannot refrain from digressing to refer (Fig. 1 g) to the "votive axe" illustrated by Covarrubias (1957:Fig. 32) since it is so similar to some from La Venta, and to point out that this piece provides a direct link with the La Venta period and the votive axes which do not occur at La Venta. When Coe and Stuckenrath question either or both the four building periods (our "phases") and the correctness of the phase attribution of the various offerings, their argument can then take the direction of questioning whether there is anything that is undoubtedly representative of the Olmec style earlier than Phase IV. It is on this issue that the matter rests and until Squier and I provide our counter-argument which will generally take the form of a defense of our position published in Bureau of American Ethnology Bulletin 170, the reader must choose between our earlier published statements and the Coe-Stuckenrath alternatives.

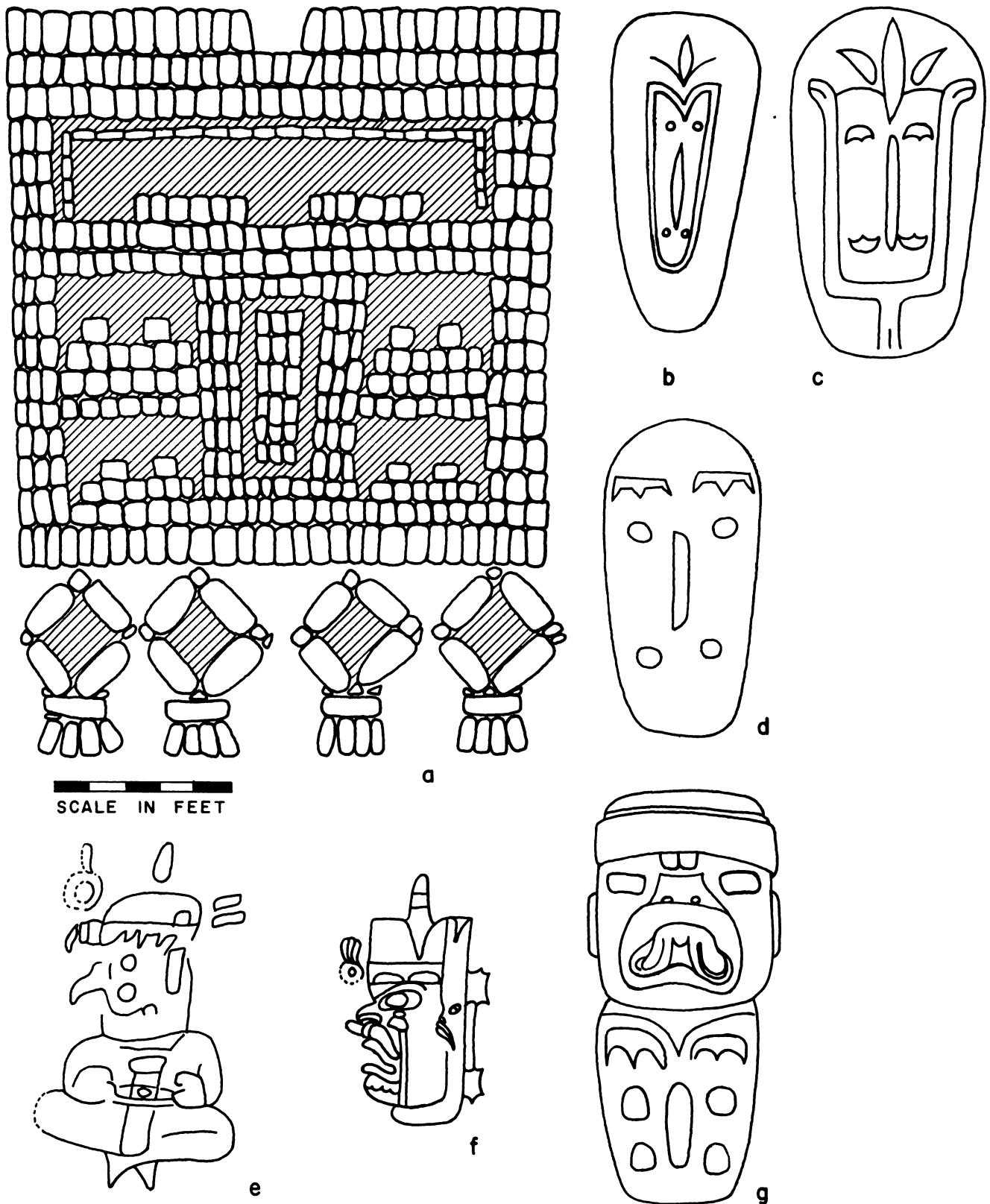
Since I am taking the responsibility for this preliminary statement, it provides me with an opportunity to clarify a matter which I am alone responsible for. This deals with what I had in mind in writing the three papers mentioned by Coe and Stuckenrath (Heizer 1960, 1961, 1962) and described by them as "infectious re-creations" and which concern my efforts to reconstruct the socio-religious-economic structure of La Venta culture. These articles were attempts to push beyond the facts of archaeology in order to try to get some idea of the organization and motivation of the La Venta population. I tried to approach this subject with the words of my teacher, Alfred Kroeber, in mind when he wrote (Kroeber 1923:125):

Now and then it seems permissible for the student to leave off his daily association with specific facts and rise above them on the gyroscope of his imagination to discover if a broader view may not give him new insights into their relations, or alter his conception of their setting in the larger landscape of nature as a whole. Such flights indeed appear almost incumbent on him at intervals if his occupation with his materials is close and unremitting. The requirement which integrity imposes on these ventures is that knowledge and fancy, fact and fabrication, be kept as distinct as possible, lest one come to pass for the other.

I now see in the cold glow of criticism that which I did not in the warmth of enthusiasm, namely that I failed to observe the proper measure of "integrity" which Kroeber recommended in not making it sufficiently clear where I was quoting fact and where I was interpreting it. Thus, by treating as literal fact the calculated floruit of the La Venta site as 800-400 B.C., and dividing this 400 year span by the number of building periods (four), a nice round number of 100 years per phase was secured, and from this flowed

Legend for Figure 1

- a, mosaic mask in Southwest Platform, La Venta site (after DHS:Fig. 28). Hachured areas are unfilled; open areas are stone blocks.
- b, engraved celt from Offering 1942-C, La Venta site.
- c, engraved celt from Offering 1942-C, La Venta site.
- d, engraved celt from Offering No. 2, La Venta site (after DHS:Fig. 35c).
- e, engraving on celt from Offering No. 2, La Venta site (after DHS:Fig. 35b).
- f, engraved design on celt from Offering No. 2, La Venta site (after DHS:Fig. 35e).
- g, "votive axe," unknown provenience (after Covarrubias 1957:Fig. 32, lower right).



rather too easily the suggestion of calendrically-derived periodic renovations. In fact, these are the sheerest guesses, and they all rest upon shaky assumptions--they should not be considered as anything else. On another matter I am surely guilty of stretching what could be at best called only possible evidence into what the uncritical reader would take to be archaeological fact in referring to "tombs" at La Venta. In the DHS report we were not only vague about whether there were actual tombs in the site, but on a few occasions we were inconsistent in assuming that there were (DHS:127) and arguing in other places that there may not have been any. These confusions were due simply to the nature of the evidence which allowed one to argue that some features were actually burial tombs, and others were "surrogate interments." I am only trying to make it clear here that the three papers referred to are properly to be looked upon as essays in looking beyond the mere facts, and that they were written because I had thought about the subject and believed, not that they were accurate reconstructions, but that such attempts had value in bringing the facts of the dead past into an animated view of people living and doing things. If my imagination was too lively, or if I treated obscure evidence as fact, then that is a reflection on my judgment, but for using my imagination I do not make any apologies.

My remarks, already running to greater length than I had anticipated, will close with the reminder that the defendants have not yet submitted their testimony to the court.

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