

COMMENTARY ON W. R. COE AND ROBERT STUCKENRATH'S REVIEW OF
EXCAVATIONS AT LA VENTA, TABASCO, 1955

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A play-by-play commentary on the Coe and Stuckenrath review (1964; cited hereafter as C&S) of our monograph (Drucker, Heizer, and Squier 1959; cited hereafter as DHS) is called for in addition to Heizer's preliminary discussion (1964) not only because of the fact that data from La Venta derived from the report published in 1959 have been drawn on widely in interpretations of Mesoamerican culture growth, but also because in their review C&S try to develop the theme that the work at La Venta was much more clumsily done than was actually the case and that a rather different interpretation of the data is called for. We make no pretense at having presented the perfect report on the perfect investigation; we acknowledged in that report that we committed certain errors. Our intent here is to clarify the category of error--whether in field method, in interpretation, or in exposition. Our purpose is to clarify, not to defend or excuse. There is, may it be noted, an additional potential error in communication of scientific data over which we admit no responsibility, that is the failure of the reader to get the clearly presented point.

While any archaeologist should be able to defend anything he writes, he cannot always do so after a lapse of years. We claim an "intuition," or perhaps better, a feeling about the La Venta site which C&S clearly lack, though this in no way diminishes the propriety of some of their criticisms. We expect that if we were to sit down and pick apart one of the still to be published detailed Tikal reports we might be able to ask some pretty tough questions. Partly this would be for the reason that Coe's intuition or feeling for Tikal is better than ours. Any detailed archaeological report is probably open to points of challenge. However, since we feel that we have been rather more right than wrong, we will try here, a full ten years after the excavation, to answer the main points raised by C&S.

The first consideration of the reviewers of the DHS report (after some preliminary generalizations as to the significance of La Venta) is that of the physical stratigraphy of Complex A, from which we derived our conclusions as to sequences of building activity. The C&S estimate of this phase of the report, to state it bluntly, is that the work of excavation was incompetently done and incorrectly interpreted. When they finally conclude in this section of the review that our conclusions as to the sequence of major construction phases were apparently correct, the implication is that we were pretty lucky. C&S have assured us, both in person and in letters, that their review of our work is not to be taken as a personal criticism. This we accept, and wish to respond by stating that our remarks here are directed solely to their review.

For discussing the problem of field method, the beginning is the best place to start. When we planned the 1955 investigations at La Venta we knew we were tackling a difficult project, one for which there was no developed Standard Operating Procedure. Earth and clay mounds have been excavated before, but a mound or pyramid, by its external form, provides guides as to possible approaches. One may sink a shaft from the summit, cut a cross trench,

dig a tunnel, drive in against a vertical face, drive in across a corner, or strip it down in layers--in each case measuring one's capacities in terms of time and labor against a roughly calculable cubic yardage of ancient construction. This was not so at La Venta. Complex A in our time was essentially subterranean and of unknown dimensions. Our only important clues derived from Wedel's work in 1943. That scholar worked under considerable difficulties, including those of limited time and budget, so that although he demonstrated the fact of a structural complexity of Complex A he was not able to define either the horizontal or vertical extent of artificial construction. Wedel (1952) makes clear that he did not have sufficient time and labor to determine where or whether he had reached the natural subsoil underlying the man-made structures.

C&S, in an effort to stress the great amount of work expended in La Venta investigations, include mention of Stirling's 1941 work (incorrectly cited in C&S, p. 1, as of 1940) and that by Drucker in 1952. Stirling's work in 1941 was purely surface reconnaissance plus the scraping away of mostly post-La Venta soil depositions around stone monuments for purposes of photography. Stirling has nowhere implied that it was anything else. Drucker's (1952) probes into structural masses--made in the course of a few days with a tiny crew--showed only, as he then tried to make clear, that the incomplete quadrangles of basalt columns projecting above the mostly sterile aeolian sands were the superficial markers of probably elaborate archaeological features.

An additional consideration was that none of us were prepared to make a career of excavating La Venta. Our personal circumstances, institutional and financial support, were such that we had to program our efforts toward accomplishing as much as possible in a single season. As a consequence we went prepared to modify and improvise our field methodology as circumstances demanded. Our original plan of attack involved stripping the drift sand overburden from at least one-half of what we assumed was the Court area, then peeling away layers of whatever structural formations we might encounter in the classical onionskin method. This, we found, did not work. First, we learned that the sheer mass of the sand overburden, greater than we had anticipated, was more than we could handle in our one season with our labor force--at least if we wanted any time left for actual archaeology. Second, clay materials of the structures did not peel away like onion layers as the book says they should. On horizontal exposures under the dry season sun the only cleavage planes they developed were amorphous mainly vertical ones similar to the columnar soil cracking of natural clay soil formations. We learned, the hard way, that the only practical approach was that of trenching to develop, analyze, and record vertical profiles. Therefore we laid out trenches to section the complex.

We also became involved in the dissection of the Southwest Platform. This work we began by working against the outer faces of the feature. The north and east faces were retained so that their inner profiles might serve as control sections. In addition we retained a north-south central control section. These are shown in DHS (1959:p1. 14). We did not, and indeed could not, anticipate how large a subterranean feature the Southwest Platform would turn out to be in terms of excavation and interpretation. The result was

that roughly half of our labor force was devoted to this single feature for most of the digging season.

As the work progressed and the importance and utility of vertical profiles became more and more apparent, we arranged for the mechanical cutting of the east-west trench by bulldozer. We should add that the Pemex Company kindly made available the bulldozer to us for two days, mainly for the purpose of removing backdirt along the sides of the deep main north-south trench since we were concerned about the possibility of a cave-in that might bury our workmen. The dozer was also used to set Stela 3 upright. When these housekeeping tasks were done, there was still enough free time to cut the east-west trench that provided us with a useful section across the east half of the Court. While neither of us then considered, nor now considers, a bulldozer as a useful tool for excavation, we nevertheless took the chance of destroying something important in order to have a continuous section of the Court exposed, which would have otherwise been impossible to secure. The basalt column tomb (Monument 7) we did not feel authorized to dismantle. The result of our failure to excavate beneath this construction accounts in part for our incomplete understanding of the constructional history of Mound A-2.

This brief account of our work is aimed at bringing out the fact that the problems of excavation at La Venta Complex A were not routine, and that we had to modify and adapt our methodology to cope with the special conditions. A very important point, too, and one that we neglected to stress in the original report, was that at the beginning there was no way to foretell just what sort of structure we were excavating. This fact has been obscured by the terminology "Ceremonial Court," "Court Plaza," etc. introduced in the earlier publications because of the enclosure effect produced by the tops of the rows of columns projecting above the drift sand. As our work advanced it became plain that these early designations had luckily correctly described the nature of the main feature, and so we retained them.

It also became obvious that the Ceremonial Court actually was the principal structural feature to which all other components of Complex A were adjuncts or appendages. C&S's complaint that "a mound in practice at La Venta was not treated as a 'growth unit' to be analyzed and periodized initially in its own terms but rather as a source for data to fatten Complex A phases" (C&S:4) indicates that the reviewers do not understand the physical proportions of the structures. Perhaps they did not bother to note the dimensions of the various mounds. The La Venta Complex A "mounds" were not towering, majestic masses like the pyramids of Tikal and Piedras Negras. Rather they were little bumps in and around the Court. The Northwest and Northeast Platforms could not be discerned as elevations prior to removal of the drift sand overburden. The Southeast and Southwest Platforms similarly did not appear to be elevations prior to clearing. Mound A-2 was, when Drucker first deforested the tract, a very inconspicuous little knoll. Actually the term "mound" is misleading. The DHS designation of "platform" is preferable (although we did refer to A-2 as a "mound," committing a small sin against terminological consistency). Had we called these features simply "substructures" this "independent giant mound" (C&S) idea might have been avoided--but then some careless reader probably would have inferred that they were underneath, not on top of, the Court. To the careful reader our coded designations, A-1-c through A-1-g, make clear the subsidiary nature of these features.

To repeat, the Court proper, by which we mean the fills, floors, and enclosing wall, was the primary and important structure. The platforms were subsidiary in terms of architectural layout as well as cubic yardage. As work progressed it became plain that there was no warrant for handling the platforms independently of the Court. They were built, refurbished, enlarged, etc. in connection with similar Court operations; therefore we handled both excavation and exposition from that approach. Our use of the Court structural phases as the temporal reference base of the complex was neither the result of lack of discernment on our part nor of slothful workmanship. The constructional sequence of the Court and its appurtenances--platforms and other subsidiary features--emerged as a result of analysis of the data revealed by excavation. Had such data indicated that the platforms were the features of primary significance, we would have directed our major efforts to them rather than to the Court as a whole.

In passing it may be remarked that C&S neglect to note that we began to excavate one of the subsidiary features, the Southwest Platform (A-1-e), precisely as they insist that it should have been done--"to be analyzed and periodized initially in its own terms." By great good luck (or perhaps by some awareness of problem on our part), we foresaw the necessity of retaining the sections that gave the significant tie-ins with the major Court structure, and consequently modified our approach to leave the interior faces of northern and eastern walls. The profiles of these were insurance against possible loss of data for relating the "growth unit" to the major architectural feature of which it was a part.

This brings up a point of order, as it were, of broad significance in regard to archaeological methodology and valid criticism of same. We consider it proper to stress the matter for its relevance to the critique here considered. The essential point is that there is no single "correct technique" in archaeology. The fabled camel's-hair brush-and-grapefruit knife method may be highly effective under certain circumstances, but it would yield no practical results if applied literally to the excavation of a massive deposit of low per-yard artifact yield and minimal architectural content like, let us say, a San Francisco Bay shell mound. By the same token, the "independent growth unit" approach may yield important results when applied to certain types of sites but that does not imply that it is necessarily the only proper approach at La Venta. The real task of the archaeologist in the field is to devise methods which will extract the greatest possible amount of meaningful data from the archaeological materials under attack. Significant results are the pay-off, regardless of whether obtained through use of a camel's-hair brush or a bulldozer, or through concentrating on minor features or major ones. No critic has the right to insist that the approach and method that served him well in one site must be applied to all other excavations.

The constructional sequence of the Court proper and its appendages (platforms, etc.), which provided us the base for the "phases" which we interpreted as of temporal significance, are treated by C&S, but with a certain constant factor of miscomprehension that provides the reviewers with a source of ammunition for criticism which is not in every case pertinent. Since careful rereading of DHS indicates that neither was our methodology faulty on these points nor our exposition unclear, apparently we have to do here with a breakdown in communication due to reader misunderstanding.

Our basic interpretation of the evidence revealed by excavations was this: Complex A was originally laid out and built according to a definite plan; it was not a helter-skelter, fortuitous, opportunistic, or accidental melange of elements. Evidence for this is the thousands of cubic yards (thousands of tons, if you prefer) of material used for the preparation of the base and the overlying "floors." Subsequently, in conformity with the generally recognized Mesoamerican pattern of refurbishing, modifying, and elaborating, and with the additional effect of gross enlargement, additional masses of fill and finishing materials were applied and subsidiary features were similarly added or developed. The evidence of the profile sections was very clear that such subsequent major enlargements were made three times. We nowhere imply that the final Phase IV form was envisaged at the inception of construction, but we do believe that each major enlargement was in itself a planned, not a haphazard, operation. Evidence for this we shall bring out--or rather redevelop, since we said it before in DHS--in subsequent paragraphs.

Phase I was identified as a building stage begun by preparation of what was essentially a foundation. C&S's statement (1964:5) that the water-sorted floors "ultimately overlie sterile base drift sands of the island but may also at times rest on either specially prepared pre-water-sorted sand and/or clay fills or on occupation fills" is incorrect and confuses the picture. We specified not once but many times (DHS 1959:27, 37, 61, 70, 77, 121-124) that the complex was constructed on a natural formation consisting of a low, irregular ridge of massive clay capped by a similarly irregular layer of aeolian ("drift") sand. Our evidence is that the base structure of Complex A derives precisely from the fact that the rather straggling natural formation, sloping gently eastward from its crest and dipping abruptly in some places to the westward, was carefully graded by removal of drift sand at the high points and laying down of massive artificial fills in low areas to provide the flat base for the Court. Given the irregular form of the natural ridge and the fact that we were not able to excavate the entire structure, we cannot offer precise figures on volume, but the clear probabilities are that several thousand tons of material were moved in preparing this foundation. There is also the evidence of careful grading, possibly for drainage purposes (DHS 1959:27). The C&S reference to "occupational fills" is incomprehensible. We found no recognizable occupational (i.e. midden or trash) deposit at any point beneath the structural elements, nor did we make any statement that could be so interpreted. What we did find at certain points were laminated fragments of clay of various colors, indicating that they had been plastered, one layer over the other, precisely as certain well-defined "floors" or surfaces of subsequent levels of the Court and its platforms. We pointed out the possibility that these floor-shards may have come from pre-Court (i.e. pre-Phase I) structures demolished in the quest for fill for the foundation mass. We have no clue as to the location of these pre-Court structures and so state. We surmised that they may have occurred in the vicinity of the Court, but in point of fact they may have originally been situated off the island of La Venta. The interesting fact is that the remnants we found indicate that this same architectural finishing or surfacing technique was in vogue somewhere in the region before Complex A was begun, and we were at pains to point this out in our report (DHS 1959:38, 44, 124, 298). A hint, but nothing more than that, to indicate the source of these earlier constructions as located at La Venta, comes from the casual report by Piña Chan and Covarrubias (1964:16-24) of their stratigraphic excavations outside the main

site area and in which they establish three periods, two of which predate the La Venta site. No architecture is reported as associated with these earlier occupations, but there is a possibility that this exists, or did exist. Our implicit assumption that the Northeast and Northwest Platforms were each altered on the same occasion may not be correct since there are surely sequential differences in minor improvements made on one or the other. But four major time stages for the two structures can still be read and these four are at least roughly contemporary. The most difficult thing to interpret is what C&S label in their Figure 2 as Pre-I in the Northwest Platform--the clay block underlying the water-sorted floors (DHS 1959:64-65, Fig. 20). In all probability we made some faulty stratigraphic observations here to the extent that we were in 1955, and are still in 1965, unable to satisfactorily explain what purpose the clay block (labeled "p" in DHS 1959:65, Fig. 20) served. It may have been a pre-Phase I platform remnant, or (as suggested in DHS 1959:65 as the sheerest guess) a pedestal for a monument, or a grading device similar to one which Wedel (1952:67) found earlier, though none of these possibilities appeal to us and we admit our inability to suggest anything more definite. Perhaps we should have allotted this clay block with its "painted" (i.e. colored clay plaster) sloping front more importance than we did, but we did not want to establish a pre-Phase I stage in the site on such slender and fragmentary evidence. To have done so would have laid us open to challenge of why we established a constructional phase with so little basis. It was, we recall, this kind of thinking that we indulged in at La Venta in 1955, and we resolved the problem by interpreting the clay block beneath the water-sorted floors as a puzzling element of the pre-Phase I leveling fills in the low area in the western half of the Court. Perhaps, as we have suggested above, the deep leveling fills took longer to lay down than we estimated, and some sort of clay platform was built and used for a period of time. If there had been any very substantial number of clay platform structure remnants (such as the pre-water-sorted floor clay block beneath the Northwest Platform or the Phase I platform-facing or flooring fragments such as found below the Northwest Platform, discussed in DHS 1959:67, or as found in layer j-3 in Mound A-2, discussed in DHS 1959:37-38) we would have granted more formal terminological recognition to an earlier site construction phase. But we did not read the scanty indications in this way and still see no warrant in doing so. La Venta, with its big pyramid and Complex A lying to the north, still appears to us to be a single unit with an internal coherence. We do not doubt that there were older (pre-La Venta in the terms just stated) occupations of the La Venta Island locality, but what bearing these may have had upon the major site which is called La Venta and which we believe was built and then underwent three major rebuildings, we do not know.

What we designated as "water-sorted floors" were laid over this foundation. If the laminated clay fragments previously mentioned were actually from floor surfaces rather than platform facings, and thus functionally comparable to those of later Complex A phases, the poorly consolidated sandy water-sorted floors may have represented an architectural experiment, one which perhaps did not work out too well. The clear evidence of washing and puddling, indicating that the entire enclosed area filled with water during rainy periods, plus the fact that there was a return to use of more compact surfacing layers subsequently, hints at this. Throughout the period of fieldwork we found the water-sorted floors difficult to interpret, and a great deal of discussion over the several months we were at La Venta failed to give us an

adequate answer. We made no secret of our difficulty in understanding these layers when we wrote our report (cf. DHS 1959:56, 100-101).

In any case the basic consideration here in connection with the C&S critique is that we stressed our interpretation that the tons of fill loaded in to provide a level foundation for the Court and the water-sorted floors comprise a structural unit. There are indications that this Phase I work may have been accomplished fairly rapidly. At no point where our trenches transected the Phase I fill used to level off the ridge were there any signs of erosional surfaces such as might have been formed had incomplete areas of fill lain exposed to the heavy regional rains for a very long time. The one bit of evidence to the contrary depends upon interpretation. If the sheared-off clay block (DHS 1959:65, Fig. 20, items "p" and "o") is actually a remnant of a small platform, and not a grading device, the implication would be that it was erected and utilized during the laying down of the basic fill, suggesting less rapid progress in the foundation building.

This temporal connection between foundation and prepared surface (fill and overlying water-sorted floors) is the essential issue in C&S's questioning the time-relevance of carbon-14 sample No. M-534 from the foundation fill supporting the water-sorted floors beneath the Northwest Platform. The recovery of this sample from a point about four feet below the floors does not dissociate it from the floors aside from the time interval, probably short, during which the Phase I work was accomplished. If charcoal suitable for collection had been noted twice as many feet deep to the westward (where the mass of the fill increased rapidly due to the sharp dip of the prestructural natural surface) it still would have referred to Phase I in regard to deposition time, at least as long as it came from the artificially formed Court foundation leveling-fill overlying the original natural surface of the ridge. The criterion here is not absolute depth from the horizontal datum plane but the lack of any indication of a major time break during construction of the fill. C&S's failure to understand DHS's basic profile-derived hypothesis, that the floors alone were not the real structural units but that the fills and directly overlying floors were so in fact, is the source of many of the errors in their critique.

The placing of the construction of subsidiary features becomes pertinent here. At the completion of the basic fill which leveled off the site preparatory to installation of the first floors, there were, in addition to the low enclosing red clay embankment, two subsidiary elements built prior to the laying of the floors--the original Northeast Platform (A-1-f) and the base of Platform A-2. In continuation, the water-sorted floors were laid. We have made manifest our interpretation (DHS 1959:27-28) that the areas we excavated suggest that at this stage the Court was a large enclosed rectangle with little relief, more like a parade ground than the usual known Mesoamerican plaza complex.

In the field we considered the possibility that the base structure of the Northwest Platform (A-1-g) may not have been a final development of Phase I construction built directly on the water-sorted floors. The facing covers of this component where we profiled them clearly belonged to the subsequent phase of construction, so that one would have to assume that if it was a Phase I platform base, erected at the close of activities of that epoch, it

must have been heavily modified to the extent of removal of its original surfacings, at the onset of Phase II activities. We intended to present that possibility but in the course of revising, rewriting, etc. we lost it. The potentially significant factor is not the argument offered and then dismissed as "specious" by C&S (1964:13) but the fact presented in the original report (DHS 1959:Fig. 20) of the clearly defined horizontal difference in material between structural depositions A-1-g-l and A-1-g-k. Here, then, is one of our errors in exposition. We shall attempt to redeem it by revising our originally published (1959) appraisal to indicate that while the weight of the evidence relates the earliest platform construction to Phase II, there is a possibility that it was begun in the later aspects of the Phase I construction.

There is unmistakable evidence that some Phase I construction activity continued after the deposition of the water-sorted floors: the initial construction of the South Central Platform (A-1-c-p). This was a clearly defined low platform which had no detectable distinctively colored finish surfacings and which was completely encapsulated by Phase II construction work.

Why should the A-1-g structure have been begun at a later date (either late Phase I or Phase II) than its "mate" (A-1-f)? There is no decisive answer. One must speculate. All evidence points to the fact that the Northeast Platform (A-1-f) had some very highly specialized function; the numerous offerings placed within it make this clear. The symmetrically located Northwest Platform (A-1-g) manifestly differed in use. Therefore there is a distinct possibility that ceremonial patterns were adapted to the architectural pattern by building the Northwest Platform as a complement to A-1-f, dedicating it to special ceremonial usages.

The foregoing leads us to C&S's point (1964:7) regarding refinement of the site chronology. On the one hand they argue for reduction of the Complex A time span, and on the other for an elaboration of phases into sub-phases. It would have been possible for us, on the basis of material at hand, to present the data relating to sequence as a series of substeps of the major divisions. For example, in the discussion just offered of Phase I we could have offered a presentation more or less as follows (reading, in geological fashion, from top backward in time):

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| | subphase e - Features A-1-c(p) (A-1-g(<u>1</u>)?) |
| | subphase d - Water-sorted floors |
| Phase I | subphase c - Features A-2(<u>j3</u> , <u>j4</u>)
A-1-f(<u>g</u> , <u>p</u>) |
| | subphase b - Features "grade stakes"
A-1-f(<u>s</u>), (A-1-g(p)?)
A-1-a(<u>q</u>) |
| | subphase a - Massive fill foundation leveling
of natural surface of ridge |

Our decision on the matter, rightly or wrongly, was that such a breakdown would have been ideal had we been able to excavate Complex A completely. However, in view of the fact that there were extensive unexplored areas which might contain additional minor features whose placing might affect the minutiae of such a breakdown, we decided against it. Not only did we regard such a breakdown as needlessly complicated, but we wanted to avoid the more serious fault of presenting, or seeming to present, a more complex sequence than our basic data warranted. Some very elaborate sequences, structural and culture-stratigraphic, have been presented on some rather skimpy data. That, or even the suggestion of that, we wished to avoid. Hence our presentation in terms of the four major construction periods, Phases I-IV.

The four Phases are demonstrably real and significant. There is nothing simplistic about using them as bases for temporal comparisons within or beyond the site. Our original, and present, position is that it is preferable to rely on a clearly defined series of major developments rather than on a breakdown into minute, possibly incomplete, substages until such time as the site can be completely dissected. When one is dealing with a familiar or predictable kind of archaeological site, one can establish a reference terminology for all of the variables--one which is open-ended in the sense that new assignments can be made as new finds come to light. This is what Coe and his colleagues have done at Tikal (Shook and Coe 1961); but this procedure is only feasible under circumstances where one is dealing with known types of sites. Uaxactun, which lies a few miles from Tikal, would have provided a model or guide. At the La Venta site we could not do this in advance, and rather than completely revise our field designations when we were writing up the report we tended to retain these and to accept or incorporate earlier designations for features recorded by Wedel and Drucker. Further, while we had no plans for returning to La Venta in the immediate future, we believed that additional work would be carried out eventually--an anticipation which, unhappily, is now impossible.

Phase II was characterized by the deposition of what we designated the "White Sandy floor series" in the Court. This set of surfacing layers represent, apparently, a return to the inferred pre-Phase I pattern of surfacing structures. The white sandy floors consisted of laminated layers of compact clays mixed with a high percentage of sand. The layers were actually of various colors alternated with layers of a clear, bright white material which, in the profiles, gave the horizon its distinctive color cast. It was not possible to determine if the white layers were actually used as surfaces so that at one time the general effect of the Court was a large expanse of white, subsequently given some other color, then whitened again, as though the white material was a sort of primer coating or backing for the colored layers. In any event, there was clear evidence of purposeful selection of these surfacing layers. The white layers themselves were of a clear, unstained white very rare in local natural clay formations noted by us. Its sand content too was probably intentionally added, using specially selected white sand. Natural clay formations at La Venta--those forming the subsoil of most of the island--contain small amounts of very fine, powdery sand, but nothing in amount or texture like that of the white sandy layers.

The colored layers of these surfacings also gave clear evidence of special selection. Like the white layers, they were of clays with considerable

sand. Most were browns and tans in color, but some of them, in certain areas, were very unusual. There were areas--horizontal extent and pattern unknown--in which dark purple clays were used. We also noted and commented upon the occurrences, at two points intersected by our trenches, where the principal constituent of the surfacing layer was coarsely ground green serpentine. These expanses of serpentine fused with bright yellow facings on certain platforms so that when freshly applied the Court must have had a rather gala, not to say gaudy, appearance. The detailed color combinations of Court floor and platforms were not worked out by us. This we should have done, but it would have necessitated taking time we could not afford from direct supervisory chores or training of an unskilled crew in the tracking of thin layers with trowels. Without any intent to apologize or depreciate our efforts, we remind the reader that the entire 1955 La Venta dig was overseen by only three archaeologists, two of them tyros in Mesoamerica.

The white sandy floors, like the "Old Rose" floor series of the succeeding phase (Phase III), were encountered at the same vertical level (allowing for some intentional grading in its deposition) in all the Court profiles, except underneath platforms and where patently removed by ancient pit intrusions. Having seen several hundred feet of it, we then assumed, and still do, that it was the diagnostic surfacing layer associated with the second major constructional phase.

Two things must be noted here. The first is that in areas where distinctive and unusual colors were used (the purples, green serpentine, etc. previously mentioned), we believe for special decorative purposes, the underlying companion layers were the usual white-brown-white etc. found elsewhere at the same level with the same distinctive angular sand additive, so that identification was no problem. The second point, one stressed by C&S in attacking our identification of this surfacing series, is that while single individual surfacing layers could at places be traced without a break for many feet along a profile, some dwindled away at certain points or were replaced by others of different color. At some places the series was five layers thick, at others six or eight. Such variations were clearly due to wear and restoration. It is not at all difficult to see why there should have been irregular wear on horizontal surfaces; erosion during heavy rains must have caused some removal despite the carefully planned drainage system. Anything less than perfect grading would leave occasional slight depressions where water would stand and discolor the clay surface. Human traffic as well must have had effect. We have no clues as to just what activities were carried out on the Court surfaces, but it is reasonable to expect that there may have been enough of them to leave some marks on certain floor areas. The indications of erosion and wear of the floors and on parts of platforms as well, and the signs of patching and repair, were the real bases for our speculations as to frequent if not continuous maintenance activities of the Court and its features.

The white sandy floors lay directly over a great part of the Court, on the surfacings of the preceding phase, with no intervening fill. However, considerable construction activity preceded the laying of these floors. This took the form of the enlargement of the enclosing wall through addition of a solid adobe brick structure, enlargement of existing platforms with quantities

of clay fill prior to resurfacing, and the construction of those strange features, the Southwest and Southeast Platforms.

C&S make a very determined attack on the asserted contemporaneity of the twin structures as part of their attempt to destroy the picture of systematic construction of the Complex. They have few logical grounds for so doing. One point they hammer at is the "partial" excavation of the Southeast Platform. This is irresponsible criticism. Wedel did not "partially" excavate the main portion of the structure, he excavated it quite thoroughly, down to and including the serpentine mosaic. He did not dig out the huge "basement" under the mask. We made a serious omission in not tying in our Court sections with his excavation to nail down the Phase II dating. But nonetheless, comparison of the two excavations shows such a detailed correspondence of structural detail that it becomes ridiculous to question that the two platforms were built according to the identical plan--therefore as companion structures (as accords with their architectural situation in reference to the Court) and therefore at the same period (cf. Heizer 1964 for further discussion). Both were capped with large rectangular blocks built of adobe bricks laid in heavy clay mortar. The type of adobe was the same in both cases, as was the fact that mortar of contrasting color (to the adobes) was used. This adobe brick in clay mortar of differing color construction is, incidentally, identical to the Phase II adobe construction of the Court wall. Beneath the adobe brickwork in both instances was a clay fill. This fill overlay, also in both cases, was a clay of different color which enveloped (i.e. served as base as well as covered over) a remarkably intricate serpentine block mosaic. Be it noted that the vertical measurements of these elements in both structures correspond very closely. As to the mosaics themselves, whether they are interpreted as depictions of jaguars or not, they are so nearly identical in size, form, and material (as Heizer 1964 has noted) that it is inconceivable that they were not designed and executed simultaneously. Wedel's brief check of structural materials beneath the mosaic revealed a base of heavy clay and stone, indicating that in all probability a similar massive stone and clay fill exists in the Southeast as in the Southwest Platform. About 1958 the mosaic in the Southeast Platform (which had been left intact after exposure by Wedel) was removed and transferred to an open-air spot in the Parque Olmeca in Villahermosa. About the same time the Southwest Platform mosaic, which had in 1955 been shipped by truck to Mexico City and reconstructed in the patio of the old Museo Nacional on Moneda, was shipped to Villahermosa and installed in the Parque Olmeca. Both are now in a very bad state of preservation. We assume that the removal of the mosaic in the Southeast Platform was done solely for the purpose of recovery and that no detailed observations were made of the relationship of the pit's constructional elements to layers northward in the Court or westward to the centerline.

C&S accept our demonstration that the Southwest Platform belonged to Phase II so that there is no question about that period attribution. In suggesting a different phase allocation of the Southeast Platform, they overlook an additional very important lead: that the Phase II Court wall construction also involved use of large quantities of the identical kind of adobe brickwork. The phase attribution of the adobe wall is not subject to question either. We thus have three major features in which this adobe brick in heavy mortar construction was used. To date this technique is unique in Mesoamerican archaeology. Two of these features are Phase II beyond a shadow of a

doubt. Unless one is deliberately trying to prove something else regardless of evidence, it would be reasonable to accept the adobe brickwork as a diagnostic trait of Phase II even without the virtual identity of detail of the Southwest and Southeast Platforms.

Phase III involved deposition of a quantity of fill over the Court itself and, in addition, the rebuilding of most of the subfeatures. The old rose floor series sealed over most of the Phase III construction of which it was a part. The remarks made as to identification of the white sandy floor series apply to the old rose series. There is no room for doubt that it was correctly identified wherever it occurred. We made note (DHS 1959) that the critical distinguishing feature of this floor series was the unusual shade of the layers (which alternated with white clay layers) that we refer to as "Old Rose." We have never seen, then or since, in exposures of clay deposits in the region any clay material at all similar. It seems most probable, after consideration, that the old rose layers were not a natural material at all, but rather a specially prepared pigment. We noted in the original report, and here emphasize once more, that this detail of coloration was very plainly an important aspect of Complex A architecture. Even fill material was obviously selected and constituted of special soils, principally undisturbed ones. No midden refuse has been noted as used to add bulk to other fill materials at any point observed by us. The old rose floor series, like the earlier one, provided evidence for the interpretation of fairly regular maintenance and repair.

The final, Phase IV, structural epoch at La Venta began with the application of a renovating fill of red clay over the Court floor, walls, platforms, etc. The material of this fill was remarkably uniform in color wherever found in the Complex, which means that it also was carefully selected. C&S profess not to understand the nature of this layer. We made this easy for them by retaining a field designation for the layer of fill that we had acquired the habit of using: "the red clay cap." However, we defined explicitly that the evidence pointed to the fact that this layer had been a carefully deposited fill, over the Court and its features, which raised their level somewhat and must have been finished off with specially prepared surfacing materials. The critics' picturesque reference to the layer as "a great red blanket pulled over the site" (C&S 1965:34) is, to be blunt, expressive imagery but at the same time uninformed criticism. We stressed time and time again (DHS 1959:55, 60, 78, 108, 117) that we believed this fill had originally been deposited in orderly fashion, level and smooth over the Court floor, with a regularity that produced well defined horizontal and vertical (and/or regularly battered) surfaces on platforms, the Court wall, etc. At the time we encountered this layer, however, its entire exposed surface had been heavily eroded. Anyone taking the trouble to check on the average annual rainfall in the southern Veracruz-Tabasco region (cf. Sanders 1953:41) can understand how this came about. It was only after the aeolian sands gradually came to cover the site that the erosion which had demolished the upper and outer surfaces of the red clay fill--those giving, we assume, regularity of form to the architectural features--was finally stopped. This aspect of the red clay cap that C&S treat as a mysterious phenomenon made clear to us that the ancient La Ventans must have been almost constantly occupied with maintenance and repair to preserve the clean, uneroded surfaces of the Court and its adjuncts.

C&S deplore the fact that there is little said specifically about "architecture" of Complex A (cf. Westheim 1952). Our rejoinder is, "What more can be said about it?" La Venta architecture cannot be compared with Egyptian, Graeco-Roman, or even Lowland Mayan. There were no columns, arches (corbelled or other), plinths, lintels, or clerestories. There was only what we constantly emphasized in DHS (1959): a fairly large, enclosed rectangular area, laid out according to plan and containing a few very low but neatly built platforms, simple or stepped, some of which had more subterranean than above-ground mass. The architectural niceties of these constructions we brought out clearly: the large, overall symmetry, not only of Complex A but of other La Venta components (a point which C&S deny because it does not fit their theories); the evidence for synchronic elaboration and modification of structures; the evidence of use of special techniques for leveling and/or grading; the carefully designed and well executed drainage patterns within the Court; the use of specially selected earth materials both for fill and surface finishings; the rather strange use of carefully prepared imported stone; dressed stones set in courses for decorative, not architecturally functional, effect; and the similarly nonfunctional use (in the architectural sense of support of integrated structural masses, the exception being Feature A-2-a) of the heavy imported basalt columns. This was La Venta architecture. We presented all the detail we could define on these points in our original report. We did not assemble this material in a single chapter under the heading of "Architecture," but, after all, one expects the scholarly reader to do a little work.

C&S, in their effort to demolish La Venta as of moderate antiquity and as a manifestation of a particular Mesoamerican culture, deny the possibility of cultural continuity from Phase I to Phase IV of Complex A. Be it noted that they do not disprove that continuity existed--they simply deny it.

One of our reasons for presenting (or rather re-presenting, since aside from a few minor flubs we spelled it all out in DHS 1959) these data on Complex A structures and phases is exactly this, to present the evidence for continuity of structural patterns. Such continuity we interpret as indicative of cultural continuity on the grounds that construction and development of the architectural features was not aimless but rather was purposefully oriented activity in the sense that the end result in the form of the Court and its subsidiary features had some specific purpose which we postulate as ceremonial. A cultural change of significance should consequently have left its marks in the form of either (1) abandonment of the site, or (2) drastic modification of original plans to adapt the locale for the new and different ceremonials.

It is to be noted that we shall not discuss here types of offerings since C&S have challenged the positioning of certain of these. This problem will be discussed subsequently.

One significant construction trait consists in the building, with selected materials, of foundations and/or fills with systematic grading as provision for drainage. All fills and their floor series showed this feature, except of course the heavily eroded Phase IV fill (the red clay cap). The meaning, of course, is that the fundamental plan from the beginning is that the Court was intended to be an enclosed area except for drainage outlets. While no original Phase IV surfaces remain, the elaboration of the wall

(including the setting of rows of basalt columns) demonstrates perpetuation of this basic plan. Hence the significant point here is that from first to last the Court was designed to be an enclosed area. Engineering skills were required to keep the structure from being a fishpond for half of each year.

A matter whose complete import is not clear is the obvious insistence throughout use of Complex A of especially selected materials. Clean, sterile clays and sands were invariably used although this by ordinary horse sense must have meant expenditure of more labor in searching out and transporting the material. This is so well defined a pattern at La Venta that in the few instances in which a mixture such as charcoal, etc. occurs in the fill it becomes reasonable to assume that such material was intentionally deposited. (The fragments of surfacing materials, i.e. "floors," from pre-Phase I included in some phase structures form an exception of sorts but nonetheless are remnants of ceremonial structures, not ordinary occupational levels.) This stress on use of selected materials would seem of scant value were it not for the fact that throughout Mesoamerica the usual custom seems to have been to collect fill from the nearest and/or easiest-to-dig deposits. Consequently most Mesoamerican mounds and pyramids provide abundant quantities of earlier sherds and other cultural debris. Why there should have been this emphasis on use of clean, sterile soils is a mystery, but it was a pattern noted from Phase I to Phase IV; in fact, in the final phase the concept seems to have been carried to the extreme by bringing in fill of a single specific color. What occurs to us both, who are familiar with the distinctive personality of the Indians of northwestern California, is a possible parallel in outlook between these recent peoples and the La Ventans in terms of attitudes toward contamination and ritual purity. We do not insist upon the parallel (the interested reader can learn the details for California in Kroeber 1959, and Goldschmidt 1951), but the basic ideas of hard work as purificatory in a religious sense, and clean fills as more desirable than refuse deposits for use in a sacred place does suggest the possibility of a similar motivation for these peculiarities of La Venta construction. Lest the reader try to read something we do not intend into this, we hasten to specify that we are not trying to establish cultural tie-ins between La Venta and the North Pacific Coast--what we stress is the fact that elsewhere in the world the human mind has conceived of a correlation: hard labor + cleanliness = sanctity.

We emphasized in the original report (DHS 1959), and repeat in these pages, the hitherto unique stress on use of specially selected, often vividly colored surfacing materials for covering floors and platforms. This pattern, interestingly enough, can be noted from pre-Phase I times in the surfacing fragments found at certain points in Phase I fill. We cannot demonstrate it in Phase IV due to the eroded condition of the fill (the red clay cap). In view of the marked emphasis during Phase IV activity of utilizing clay of one certain color only, we seem justified in assuming that Phase IV surfaces were similarly decorated.

Use of imported stone (basalt and serpentine and various metamorphic types) occurs consistently from Phase II through Phase IV. There were a variety of forms, that is to say intentionally worked types--the neatly finished basalt facing blocks, the "chipped" blocks, the carefully ground and polished serpentine blocks, and so on, up to and including the partially worked basalt

columns. The mere statement that stone was used is not very impressive. What is impressive is that all this stone, and the quantities ran into the thousands of tons (cf. DHS 1959:97, passim), was imported over long distances. Our authority, G. Curtis (1959:284ff.), cites a rough figure of 100 kilometers (60 miles) from La Venta to the nearest possible source of serpentines and other metamorphic rocks. Bitter experience has shown us that the minimum factor for converting airline to trail distance in this region is x3 (by water, in the meandering coastal streams, it is x5) which gives at least 300 kilometers (180 miles) of trail distance for transportation of this material. The source of the basalts is now known (Williams and Heizer 1965). The significance of this is that it means (apart from the inferences as to social structure of the La Ventans) that there must have been communication, trade, etc. with the same distant regions.

In resumé, there are a series of aspects of Complex A construction that indicate continuity of purpose and of external relationships that cover the period of structural activity at Complex A, so that it is far more reasonable to assume that there was cultural continuity throughout the Phase I - Phase IV period of La Venta.

C&S devote considerable space to the suite of nine radiocarbon dates derived from wood charcoal. These were originally published by us in Science (126:72-73, 1957) and subsequently in DHS (1959:264-267). First, we should say that we collected the charcoal samples in 1955 with what we thought was reasonable care, and interpreted the dates as objectively as we could. It is scarcely necessary to point out that we were ourselves surprised at the age determinations since they were rather older than we had guessed they might be. However, we assumed that a series of nine dates derived from wood charcoal collected during a single excavation and run at the same time by one laboratory, which exhibited reasonable internal consistency and conformity to their stratigraphic position, were acceptable. Without going into details, we can say now, ten years later, that the La Venta dates (samples M-528 through M-536) fit quite well with a number of non-La Venta Middle Preclassic radiocarbon dated culture levels in Mesoamerica. They seem to be, in other words, of the correct order of magnitude, and thus have not been shown in the past decade to be markedly out of line with what would be expected.

We do not, of course, think that the M-528/536 radiocarbon dates accurately pinpoint specific events in the history of the site. What we have already written demonstrates that we believed only that we were dating the site as having been built and used during the first millennium B.C. When we came to the moment of truth and decided that Complex A dated from 800 to 400 B.C., we were only trying to make a reasonable decision based upon a series of dates which (according to statistical probability having one chance in three of being incorrect) ranged from 1454 B.C. to 126 A.D. (DHS 1959:264-267). C&S themselves accept (1964:20) the dates (assuming as we did that the laboratory determinations are reliable) as indicating "that construction occurred at La Venta during the first millennium B.C., if not earlier and later." If we went further than C&S were willing to go in deciding that La Venta history was probably intra- rather than pre- or post-first millennium B.C., then that is how we saw the dates. It has already been pointed out (Heizer 1964) that additional portions of samples M-531¹ and M-533 were dated in early 1965 at UCLA (Berger, Fergusson and Libby 1965:345-346), and that

the ages, while not the same, are not so different as to encourage us to deny the conclusions reached by us earlier as to the radiocarbon-derived age of La Venta. Sample UCLA-902 is part of sample M-531. Sample UCLA-903 is part of sample M-533. The ages are shown below.

<u>Sample</u>	
UCLA-902	2940 \pm 80 (3020-2860 B.P.; 990 B.C.)
M-531	2560 \pm 300 (2860-2260 B.P.; 610 B.C.)
UCLA-903	2460 \pm 80 (2540-2380 B.P.; 510 B.C.)
M-533	2130 \pm 300 (2430-1830 B.P.; 180 B.C.)

These ages and dates of these two sample sets are all different, it is true, but we feel that they all support the main conclusion of La Venta's floruit as being in the first millennium. Actually, we are ourselves unable to compare the two pairs of dates in any meaningful way since we know nothing about the preparatory cleaning procedures, instrumentation, or computation employed at Michigan in 1957 or UCLA in 1964. In 1964 Squier collected charcoal samples from a test pit in the occupation deposits outside and beyond (and therefore unassociated with) Complex A. From a "burned soil zone" which he equates with the La Venta Complex A period, an age of 2560 \pm 240 B.P. (700 B.C.) was determined (sample UCLA 788B). While it is tempting to accept this date as supporting the M-528/536 and UCLA 902/903 dates, two stratigraphically inferior samples collected by Squier gave impossibly old ages of 1810 B.C. and 7800 B.C., so there is a good chance that Squier's 1964 sample series is badly fouled up.

We will not attempt here (though we may later make the effort) to discuss in detail the questions which C&S raise with reference to each of the samples collected in 1955. A few points may be important to note, however. Sample M-535 was taken, according to our notes, from all three levels shown in DHS 1959, Figure 26 (cf. C&S 1964:8). The wood charcoal constituting sample M-535 was in the form of small, sharp-edged bits which we supposed came from small wood such as limbs or branches or medium-sized stems. We assumed that the charcoal came from recently cleared and recently burned areas of second growth, and was reasonably contemporaneous with the laying of the water-sorted floors; we cannot prove this, but this is what we felt was probably the situation. We have, admittedly, no control on the age of the wood, but the same can be said about almost any charcoal in any site. There is always the possibility that the charcoal we found came from wooden objects made a thousand years before, kept as sacred heirlooms, then burned and mixed with floors or fills--but we think the possibility is a remote one. The preservation of soft wood lying on the ground at La Venta is a very short-term process since rot and termites attack it immediately. We would guess that one or two years would be the average lifetime of fallen wood lying on the ground. Fallen woods, on the other hand, may resist rot, termites and ants, and weather for up to 50 years, but this would probably be the maximum. Preservation for really long periods of time in the form of architectural beams or lintels, as at Tikal, seems improbable. If protected under a thatch roof and kept reasonably dry such woods might last indefinitely. Wood in this form is also attacked in the La Venta area, and there is no evidence for ancient architecture employing such beams. It is not impossible that timber-

built "temples" existed at La Venta, but we found no evidence of any sort which could be so interpreted. All in all, our guess that the charcoal in fills was fairly contemporaneous with the time of placement of those fills strikes us as a good one, though as C&S say (1964:20) such speculation soon becomes a pointless game. The matter boils down to their guess as against ours. In Mesoamerica, radiocarbon dating seems not to have worked as well for clarification of archaeological time sequences as in some other parts of the world. Thus the large number of C-14 dates secured from Cuicuilco and Teotihuacan have not provided us with a good chronology, and at Tikal a great many radiocarbon dates have proved to be quite unhelpful for purposes of cultural sequencing. Perhaps La Venta is another of these great sites for which we have a number of dates but no clear guidelines as to how they should be interpreted. Only further excavation, charcoal collecting, and its dating from La Venta, Tres Zapotes, and San Lorenzo will give us the tertium quid which is required to permit a sound and defensible radiocarbon-dated chronology for the Olmec area.

The queries about sample M-529 we believe have been largely answered by our remarks on the corrections of Figure 18 in DHS (1959). Most simply, Offering 15 has nothing to do with the pit in question, and we now say that we believe that M-529 does date from Phase I at La Venta. With reference to M-534 (C&S 1964:10), we cannot answer the many questions which C&S pose, but we believe the leveling fills beneath the Northwest Platform immediately preceded the laying of the water-sorted floors. Assuming that the charcoal is not ancient material whose existence long predated the act of loading in the fills, the charcoal would not be "significantly earlier" than the Phase I floors. C&S may be right, but so also may we be correct. The reader will have to make his own choice. Unfortunately the La Ventans did not build large fires in the Court, cover the coals, and thus provide us with indubitable phase-connected carbon samples. Sample M-532's description is admittedly confusing and we must explain that the charcoal from layers j-5 and j-3 (DHS 1959:Fig. 10) were lumped and that this was not fully shown in the figure referred to. Both, in our opinion, are from Phase I depositions. Sample M-530 was of wood charcoal (DHS 1959:264) and not anything else, despite our rather confused description (DHS 1959:68). The pits in the Northwest Platform were difficult for us to interpret, both as to age (i.e. phase) and function, and we believe that a Phase II attribution is a probable one but cannot be certain. Figure 24 in DHS is faulty in not showing the presence of a very thin layer of clean sand under the charcoal layer lying on top of the Phase IV red clay. On the spot in 1955 we agreed that some interval of time had elapsed since the abandonment of the site and the building of the fire as evidenced by the eroded red clay surface and the thin sand layer. We were willing to hazard the guess that abandonment had occurred about 400 B.C., but would agree that the date might well be earlier, though hardly later, as judged from M-528.

The reference by C&S (1964:15) to "examples of 19th century burning of Classic wooden beams" at Tikal must be in error since the passage referred to skips about between alternative possibilities to account for beams which are, themselves, missing and no evidence is given of chicleros having been the pyromaniacal culprits. We feel that burning of ancient beams to account for La Venta dated charcoal is not a "reasonable probability." In the case of M-528, a "reasonable possibility" is that some milpero here burned a pile

of cut wood preparatory to planting a field of corn in the abandoned Court area. Sample M-536 tells us, we acknowledge, practically nothing. It dates some event, chronologically unplaced, in the history of the La Venta pyramid. Our guess, quite unwarranted at the time we sent the samples to the Michigan laboratory, and commented on by us in Science (126:72-73, 1957) was without any foundation--in fact, it was a bad guess. We tried to make amends for this in a later publication (DHS 1959:265-267), but our critics have not ignored the opportunity to belt us a good one for having changed our minds. As to why we submitted the sample in the first place, we were flying blind in 1955 as regards chronology and we naturally submitted a carbon sample from the pyramid because we were curious about anything which might help to orient us, in any part of the site, in time. C&S, being naturally more analytical and self-critical, would have suppressed their curiosity and (assuming that they found out no more than we did about the pyramid) would have (also assuming, of course, some curiosity) felt self-righteous in their self-denial in not submitting the sample for dating. But we were, on the other hand, anxious to learn anything we could find out, and did not feel that we were imposing upon our sponsors in submitting the sample for age determination.

Finally, when we come to the tabulation (C&S 1964:17) of what we or C&S think about the La Venta radiocarbon dates, the reader may want to know that we are not here defending our opinions (DHS 1959:264-267) as to phase attribution of any of the nine samples as inalienable, and that we concede we could have done better in providing discussion of alternatives of interpretation. However, aside from our original error with regard to the pyramid sample (M-536) we see a relatively good correlation between our stratigraphic-phase placement of samples and their ages (cf. DHS 1959:Fig. 79), and warn the reader that obfuscation, rather than enlightenment, awaits him if he tries to make sense out of C&S's reinterpretation of the dates.

Massive Offerings

What we designated as Massive Offerings (DHS 1959:128ff., passim) present something of a problem of interpretation since so far as we know they are unique. C&S (1964:21-22) call for redefinition of the term. We feel that our definition (DHS 1959:128) was as clear and as specific as our data would permit. "The characteristics of these massive offerings . . . seemed to be that large pits were dug to receive them, and the offerings themselves consist of very great quantities of stone." Even after ten years of reflection there seems to be little we can add to this definition. C&S deliberately confuse the picture by supposing a distinction between the mosaics and the massive offerings. This means they did not read, or misread, our clear statements in reference to feature A-1-e, to the effect that the mosaic and the underlying twenty-eight layers of stone were constructed as a single operation and consequently were regarded by us as a single unit: MO 1 (DHS 1959:102, 128). We have no basis for suggesting, since there was no evidence to that effect, that large depositions of stonework inevitably must be accompanied by mosaics. Our MO 2 and MO 3 differed considerably in detail from MO 1 (the Southwest Platform mosaic and underlying layers of stone). In point of fact, MO 2 and MO 3 could themselves be referred to as mosaics--layouts of shaped stones (in both cases serpentine) carefully set in something approximating orderly geometric patterns. MO 2 comprised a single layer of such stones, MO 3, six layers. The essential classificatory principle for grouping these

varied depositions together had nothing to do with the absolute tonnage of mineralogical material nor the particular figures it formed as deposited, however. The determinant was expressed in our definition that a large pit had been dug, a considerable quantity of rock was promptly deposited, and then the whole partly-loaded excavation was immediately filled in.

Our mistake in this regard was in terminology; we neglected to clarify the nomenclature matter, to include the mosaic of A-1-d with the stone-laden base that presumably underlies it as MO 4, and to specify that the mosaic designated "Pavement 2" by Wedel was to be called MO 5. We indicated, at least to fair-minded readers (DHS 1959:128), that we intended such a synonymy but did not spell it out.

Different among themselves as MO 1 and MO 3 are, they are all large quantities of imported stone, carefully arranged, not dumped, in a special large, deep pit, then promptly covered up. Two of these features and the matching partner to MO 1 (the 1943 discovery, MO 4) had obviously been placed with relation to them in the pitfill cruciform celt offerings. Drucker found two lots of three serpentine celts each (DHS 1959:273, 1942-E). He definitely recalls that the two lots were at approximately the same level and not far apart. They could not have been far apart and have been encountered in the small test pit. He did not recognize the two lots as being in a pattern relationship at the time. If they were, they could have been only two arms of an incomplete and small arrangement, not entirely comparable to the celt layouts associated with MO's 2, 3, and 4. Nevertheless, we regard this detail of supplementary celt offerings (in three out of the four cases elaborate cruciform patterns) as confirmatory of the basic unity of type of the four Massive Offerings.

All four of these peculiar depositions are precisely placed in our site chronology. MO 1 and MO 4, bases of the twin Southwest and Southeast Platforms, refer to Phase II. MO 3 is Phase III, and MO 2 is Phase IV. There is no question at all about these phase assignments. Even C&S cannot attack the contemporaneity of the Southwest and Southeast Platforms, and thus MO 1--MO 4, except by illogical questioning, previously discussed, and by their effort to confuse the situation (cf. C&S 1964:Fig. 2) by attempting to distinguish between the temporally united mosaics and the subterranean stone layers of these same features.

An additional mosaic, MO 5, discovered in 1943 by Wedel and referred to as Pavement No. 2, cannot be dated with any reliance. We suggested (DHS 1959:123) the possibility of a Phase IV dating. Both in form and deposition this feature is slightly aberrant. Here, as archaeologists familiar with the site, we made a guess. It is only an inference, but we think it one of high probability. The four "pendants" or decorative features on the lower margin of the Southwest and Southeast Platforms mosaics do not seem to be duplicated in engraving or sculpture at La Venta, and these elements do not occur in Wedel's Pavement No. 2 (also referred to as MO 5) which is somewhat different in other respects from the platform mosaics. The central vertical rectangle in the platform mosaics (perhaps to be interpreted as the nose if these mosaics do portray faces) is missing in Pavement No. 2, and there is no evidence in the published illustrations that would lead one to suggest that such a rectangle once existed but had been filled in with stone blocks; it simply

lacks the feature. A further difference is that the four small crenelated elements are made of blocks, while the equivalent elements in form in the mosaic in MO 5 are formed by the space left vacant by the blocks; in crude terms, one is the negative of the other. By and large the MO 5 mosaic is simpler and more crude than the almost identical pair of mosaics in the Southwest and Southeast Platforms. We do not draw any conclusions from this, but merely indicate awareness of these differences while at the same time reaffirming our belief that the MO 5 mosaic shows a non-accidental and strong stylistic connection with the two platform mosaics. These differences are of such magnitude that some time difference probably is involved in their deposition. Since the mosaics in the Southwest and Southeast Platforms are Phase II, the MO 5 mosaic could be Phase III or, as we have already opined, Phase IV. We still feel that Phase IV is the best guess for reasons already set forth (DHS 1959).

The Massive Offerings, it may be noted, contribute substantially to our hypothesis of cultural continuity of the Complex during the Phases II to IV. We found no comparable feature referable to Phase I, although we suggested (DHS 1959:39-40, 131) the possibility that a large quantity of reused serpentine blocks, found redeposited around MO 3, may have represented an early (possibly Phase I) offering intruded upon by the later deposition. We are not going to belabor this hypothesis, however. We feel that to do so is unnecessary. Phase-Massive Offering tie-ins are unquestionable during Phases II through IV. It is reasonable to accept the possibility that the obviously superfluous stone blocks reinterred around MO 3 very likely came from an earlier Massive Offering deposition, and that such offering may have derived from Phase I times.

Burials and Small Offerings

C&S are rather critical regarding our discussion of the data on "burials," "tombs," etc. at La Venta. They have a right to be, for we made some blunders in presentation of data. We disclaim responsibility for errors presented in Stirling's early popular articles and the like, but we should have cleared up these points in our 1959 report. Furthermore, we made at least one major foul-up on this topic in the report; we still cannot understand how it survived revision, checking, and editing processes. This is our statement (DHS 1959:127, cited by C&S 1964:23) in regard to the existence of "great tombs" in Phase IV. We offer apologies to our critics and to anyone else we may have confused on this point (cf. Heizer 1964).

There is no point at all in trying to explain how we made this particular error in our presentation. We shall, rather, set the record straight as to the actually observed situation at La Venta.

First, in the course of the various investigations at La Venta on which data are available to us (Drucker 1952, Wedel 1952, DHS 1959), no, and we repeat no, primary interments have been found in relation to or apart from offerings. The only evidence of relationship of human skeletal remains with offertory materials was that found in 1942 in "Monument 7" in connection with the offering tabulated in DHS (1959:272) as 1942-A.

Offering 1942-A was collected by Stirling, who took direct personal charge of operations at that time. The pressure of time (Stirling visited

the site en route to a Mesa Redonda conference) hampered excavation. Drucker, as an onlooker, cannot recall any significant details as to arrangement, etc. of the specimens except for one thing--the two lots of obviously human osseous remains were definitely within the lens of cinnabar dust that contained the jade artifacts. They were not, repeat not, "at one end of the tomb chamber" (C&S 1964:23). Drucker subsequently referred to the two lots of bony materials as "bundle burials" with no intent to confuse; he assumed that almost everybody understood what the term means--a secondary deposition (not primary interment) of osseous materials from which most or all of the soft tissues have been removed by weathering or by chemical or mechanical means (or any combination thereof, e.g. hot sun and buzzards) so that the bones were obviously not in articulation or even in normal order when uncovered. The two lots of human bones in question were obviously remnants collected and redeposited after some such cleaning process. They were very incomplete, and consisted mainly of long bones, ribs, and the like, arranged compactly with their long axes parallel, manifestly having been tied or packaged for convenience in handling.

It was not possible to determine whether the osseous remains were the primary or secondary components of the offering. The fact remains that the two bundle burials (we decline to change this designation) represent the only instances of deposition of human remains encountered in the total excavations at La Venta. Let it be noted on the record that this statement includes Drucker's scattered but fairly extensive tests of occupational deposits as well. In no other instances--popular articles and our own unfortunate flub to the contrary--was there the slightest trace of human remains associated with, or separate from, any other feature or subfeature.

At this point Drucker wishes to note that he has had the opportunity before 1955 to observe several interments dating from archaeological times that had been made in the highly acid clay soils of the region. The osseous material is attacked by soil acids until it becomes putty-soft, but it apparently never completely dissolves to the point of total disappearance. Harder areas--the surfaces of long bones and portions of the skull--even retain a sheen. Tooth caps are practically indestructible. If osseous remains had been deposited, enough evidence could have been noted of them to affirm the fact even in these tropical soils, although the condition of such remains might make a physical anthropologist weep.

It was, in part, the foregoing information that led Wedel to his hypothesis as to the "surrogate burial" pattern at La Venta. It might be noted in passing that the bundle burials of Offering 1942-A, whether of major or secondary importance in relation to the offering, suggest a primary burial pattern (e.g. exposure) that might account for the lack of burials at La Venta. C&S's suggestion regarding cremation can be dismissed on the grounds that enough survey and testing has been done at La Venta and other localities in the region so that, had this custom been² practiced, evidence for it certainly should have been found and none has.

In fine, despite the slip of the pen in our (DHS 1959:127) passage (properly designated by C&S as "incredible"), the only association of human remains with offerings, structures, or the ceremonial area in general was that of Offering 1942-A. In that lone instance it is impossible, partly

because of circumstances of excavation, to determine whether the bundles of bone were the central features of the deposit or were themselves the offerings. The "surrogate burial" or psuedo-burial pattern is demonstrated by enough examples to establish it as a definite pattern, but one whose meaning in terms of La Venta culture is unknown to us. There is no way to determine at present whether the peculiar surrogate burial depositions were meant to represent interments of certain individuals. For all we know, they may have been the manifestations of some memorial rite or the ceremonial interment of supernatural beings who were believed to "die" periodically to be resurrected, or they may have had some still more intricate meaning. What we do object to is the statement by C&S (1964:22) that "one would expect that human burials would have been found in association with ceremonial areas and architecture." This is an assumption made, in our opinion, without warrant. It does, however, demonstrate a bias on their part, as we shall point out, that motivates their attack on the La Venta work. We reported the facts as we found them; we were not forcing our data to try to "find" all features of Mesoamerican culture patterns at La Venta. In short, we were not out to present La Venta as the Mesoamerican "mother-culture." The fact is we specifically disavowed adherence to the current mother-culture cult (DHS 1959:262). Hence we were under no compulsion to force our data, on burials or anything else, into any preconceived notion as to basic Mesoamerican patterns. Rather we were impressed by the, to date, unique nature of a variety of La Venta features--architecture, buried stone "pavements," concave mirrors, and all the rest.

In their review of the small offerings C&S (1964:1) register their objection to our designation of the site, in its Phase I to Phase IV manifestation, as "Olmec." The word Olmec, as they point out with some justice (although the point is not original with them) has been used in a rather fuzzy fashion at times. Not by us, however. We have designated as "Olmec" one specific art style (long recognized as distinctive but first defined in precise terms by Drucker 1952) and other cultural materials--structures, ceramics, and all the rest--found in direct association with this style. There appears to be no serious logical flaw in this application of the term. C&S, however, attempt to whittle away the point of "association." That is, unable to offer a reasonable argument to the effect that materials unquestionably pertaining to "Olmec" artistic norms had no relationship to the La Venta structures, they attempt to reduce the art style-structural relationships to the final (IV) phase. Thus, by inference, La Venta began as a manifestation of some unspecified culture component which received an influx of alien Olmec traits--objects in the specific artistic tradition and concepts of the art style itself--in Phase IV. This is not only nonsense but it involves an un-scholarly attempt to misinterpret basic data.

The C&S review was good for us in the sense that it pointed out the error of our ways, which we shall endeavor to remedy from here on. One of these was false modesty, which led us to put a question mark after the Phase I designation of Offering 7 with its unquestionably "Olmec" jade maskette, and to phrase its situation by saying the offering "appears to have been deposited . . . during the Phase I construction." We seem to have been trying to indicate an icy impartiality, leaning over backwards to avoid the suggestion of forcing our material. Actually this was not proper presentation. By adopting the convention of adding a question mark we thought we were being objective. We now see that our attempt to short cut the obscurities by ques-

tioning the phase attribution has been misunderstood. What we should have done was to make a decision and explain in detail what reservations we had and what the data deficiencies were. We have both not only done a fair amount of archaeology but we have also seen a fair amount of the work that other archaeologists have done, and we know that all sorts of obscurities are presented which are glossed over as insoluble at the moment in order to proceed with the work at hand. Often these unsolved elements of the puzzle are ignored because the main outline comes through with satisfactory clarity. We were, in short, acting like a bunch of beginning students learning technique under the critical tutelage of a wise old professor who would stand for nothing less than perfection in whatever, or however much, was attempted. We do not have to explain further because we think that Coe is an archaeologist with enough experience to know what we mean. We will try here to indicate why we questioned the phase placement of offerings, since our critics seem to have taken our question marks as indications of a degree of uncertainty which we actually did not feel or intend to indicate.

In point of fact, there was no doubt at all as to the phase attribution of Offering 7. It was not in an intrusive pit; all that we saw allows us to say definitely that it was laid out during the construction of the Phase I fill of the Northeast Platform (A-1-f).³ It was then covered by unbroken and undisturbed (until we came along) masses of Phase I fill. No question marks were needed, or indeed had any place in our presentation of these facts. Offering 7, with its distinctively Olmec jade maskette (DHS 1959:P1. 40) was deposited on its prepared "bed" of orange colored sandy clay with very small amounts of cinnabar during the Phase I construction. There is, consequently, no question at all that objects of Olmec art style were in use at La Venta in Phase I times.

Other Phase I small offerings consisted of the pottery vessels contained in Offerings 15, 16, and 17. The phase allocations of these specimens, which were enclosed in undisturbed primary depositions of Phase I fill, is subject to no doubt. We ourselves are to blame for injecting doubt by placing the needless question marks--intended to indicate not real doubt but scholarly impartiality--to signify that we were not really squeezing or pressuring the data to get early associations. The honest fact is that we did not really comprehend at first the fact that there was a pattern at La Venta involving deposition of objects during the process of construction and elaboration of features such as the platforms and the like. We were looking for evidence of pits--and found some, including some tremendously big pits--but the evidence is clear that there were two distinct patterns for the deposition of "offerings" in the structures. One was that of placement on completion by means of an intrusive pit--a sort of post facto operation--and the other, the novel feature, that of deposition during the actual construction.

The Phase I ceramic materials provide a specific and definite tie-in with the La Venta pottery patterns described by Drucker (1952). Whether his descriptions of the 1942 pottery collections from occupation deposits (the gratuitous reclassification by C&S 1964:31 of these as "approximating occupational debris" is just that since there is no doubt whatsoever as to the fact that the 1942 strati-trenches were in accumulations of occupational refuse) may not be as refined as some might like, the hard fact is that the 1955

specimens (Offerings 15, 16, and 17) could be identified in terms of the 1952 descriptions of pottery types, thus definitely linking the ceramic sequence to the architectural features.

Phase II small offerings were few; that is, of course, those that were recovered in the course of the excavations. They consisted, in addition to Offering 3, of the probably incomplete celt deposition (DHS 1959:273, 1942E) and the cruciform layout of celts with a mirror found and collected in 1943 by Wedel. We have already clarified the illogically disputed question as to the temporal allocation of the Southeast Platform (A-1-d). There are no reasonable grounds for doubting that it was constructed in Phase II as was its twin feature, the Southwest Platform (A-1-e). The principal difference between the two that C&S can find to harp about is the difference in the offerings--that in the Southwest Platform being smaller (six celts rather than twenty), and was possibly incomplete. The well-formed cruciform celt layout of the Southeast Platform (A-1-d), with the mirror, establishes a very significant link between Phase II and Phase IV where two other cruciform layouts of celts accompanied by the technologically remarkable La Venta mirrors were found. This, we note in passing, is apparently why C&S strain the data as best they can to try to deny the obvious symmetry, structural equivalence, and contemporaneity of the Southeast and Southwest Platforms.

Offering 3 was also Phase II. C&S (1964:27) stress the "uncertainty" as though that quality had to do with phase allocation. Actually we have no doubts at all as to the phase of this lot of materials; the only uncertainty--loss of information if you please--concerns the in situ arrangement of the specimens. This, we regret to say was lost; the bulldozer took it out. However, this does not mean that there is any real question at all about the original location of the specimens. It is worthwhile to note two significant facts here. First, we did not leave the excavation of the "bulldozed trench" entirely to the untender mercies of the dozer jockey. One or the other of us walked back and forth during the three days of the machine operation, watching constantly. Because of this we were able to stop the bulldozer the moment after it rooted out the offering in question. The second point is, as anyone who has operated a bulldozer or worked along with one knows, in driving a one hundred foot trench through heavy soil, one does not drive the blade deep on each cut. Should he do so in a trench, where the blade cannot slough off load at the ends he would get stuck, even with a D-8 such as we used. A good operator will handle the blade to skim off about an inch or two of soil at the most on each cut. Therefore when the blue and green of the jade objects and the red cinnabar matrix rolled off in front of the blade the tractor was stopped and the source of the materials was determined. The offering had been sealed over with a thin but distinctive vivid yellow clay, chunks of which were mixed with the jade specimens and bits of which adhered to some of them. The remnant of this yellow clay cap appeared as a flattened ellipse in a section of the trench wall a short distance behind the point at which the specimens had been noted. This deposit had been unequivocally placed in a mass of Phase II fill, with no trace whatsoever (in the profile containing the remnant of yellow clay seal) of an intrusive pit. Consequently there is to us no doubt as to the temporal placing of Offering 3 as Phase II.

In Phase III both Offerings 5 and 6 occurred in pits whose points of origin lay within the Phase III fill of the Northeast Platform (A-1-f)

(DHS 1959:162-171). There exists no reason whatsoever to believe that these pits could have originated in the Phase IV layer, no matter how greatly C&S wish to alter their temporal position.

We do, however, concede the validity of C&S's complaint regarding DHS's Figure 18, with its clumsy attempt to superimpose the outlines of the Offering 5 and 6 pits on a diagram indicating the levels at which the North-east Platform offerings were found. These pit outlines injected an impression of a third dimension into a two-dimension diagram--bad presentation, we admit. The fact remains that reference to companion diagram Figure 14 (the horizontal of the platform) should make clear that, for example, Offerings 14 and 15 could not possibly have been contained in the Offering 5 and 6 pits, is not sufficient. C&S could not be expected to straighten this out for themselves. Our fault.

Another offering belonging to Phase III whose place is challenged by C&S is the remarkable figurine-and-celt assemblage designated as Offering 4. Here, once more, our own errors of presentation left us wide open to sharp criticism. We stand firm on the textual description of the stratigraphic situation of the offering: that it was deposited during the placement of the Phase III fill, carefully covered with a layer of distinctive, clean white sand that extended for some distance all around the offering, was sealed over by a brown sandy fill, and then by the old rose colored floor series. In late Phase III times, but prior to deposition of the Phase IV fill, an inspection hole was cut through the overlying old rose floors which at that time marked the Court floor, and then refilled.

This inspection hole, with its interesting implications of possibilities of some sort of record-keeping, or even accurately drawn plans of placement of this (and perhaps other?) offerings, is what gave us trouble. That, and our diagrams (DHS 1959:Figs. 16, 17, 39). The fact is that we relied heavily on our draftsman, who had participated in the excavations, to the extent that we believed that he understood the presentation problems involved. If he did not, as the drawings show, it was not his fault but ours. We should have proofread the diagrams with at least the care that C&S did. As a consequence we must specify here that DHS's Figures 16 and 17 are generalized presentations of profile sections, in the main correct, but their depiction of Offering 4 is inaccurate in precise detail.

DHS's Figure 39 is about as good a two-dimensional representation of a three-dimensional situation as could be drawn. The upper surface line of the white sand actually represents the line visible in profile behind the figurines. Precisely over them it was irregular, where the tips of the heads had been exposed for inspection. The lower margin of the inspection pit, as depicted, is a generalization also, for it was actually irregular, exposing the pits of the celts and the tips only of the heads of the figurines. The real pay-off is the photograph (DHS 1959:Pl. 31, left) which shows unequivocally the mounding of the white sand layer over the figurines and the unbroken continuation of this same layer, dipping but continuous, well out past the margins of the inspection hole. We have, we might add, other and unpublished photographs of this offering taken in situ.

C&S's suggestion (1964:28) that the figurines might have been pushed down into place through the undisturbed white sand layer is nothing else than silly. Neither you nor I nor the current Olympic shotput or weight lifter champion can push a two-inch-wide blunt object seven inches deep into compact, enclosed sand by hand. Almost anyone could drive such an object down out of sight with a longhandled maul, but the obviously careful and close placing of the specimens makes it highly dubious that such heavyhanded methods were used for positioning the figurines. Too, the celts would have had to have been placed bit, not poll, down to drive them in by this method.

The total result of this reappraisal is that Offering 4 was just what, and temporally where, we said it was: La Venta, late Phase III. Thus during Phase III there were not only various Olmec figurines (C&S can scarcely challenge their Olmec style characteristics) kicking around the site but among these were some old beat-up specimens. All of which makes C&S's thesis that Olmec art popped up suddenly at the end of La Venta, in Phase IV, hard to accept. If the archaeological expert who was not at La Venta in 1955 thinks he is a better judge of what we saw than we are, then he should be allowed to have his opinion. And that is where we will have to leave the matter of Offering 4.

There is one more point that must be noted in regard to pre-Phase IV art of Olmec type. Our critics agree with us (one of the few points on which they do) that Massive Offering 1, by which we mean both the mosaic and the tons of stone and clay substructure (C&S 1964:22 try to distinguish between the two) belongs to Phase II. What might be called a large figurine or small statuette was found in the same clay envelope as the mosaic, apparently an accidental inclusion (DHS 1959:95, 211-212, Fig. 63). The head had been knocked off long ago, so the object is not "pretty" and no one has paid much attention to it. We ourselves erroneously said that it was not placeable stylistically. That is not so. The posture, the smooth sweep of line of shoulders and thighs, the impressionistic but undistorted handling of the hands, the concave pit (for attaching a small copy of a mirror?) on the chest, the impressionistic modeling of the back (not shown in figures but mentioned in the text), leave no doubt that it is of the classic Olmec art tradition. That makes another Phase II piece of Olmec art. We apologize for not having made the point clear earlier.

The matter of temporal placing of the stone monuments merits little comment. Almost all that can be said about it has been said. C&S are correct when they point out that so far none of these objects has been placed as earlier than Phase IV. They are correct to date; that does not mean their statement will never be disproved. Since 1955 there has occurred at La Venta a treasure hunt of remarkable proportions. Perhaps our excavation encouraged the almost total destruction of Complex A by bulldozers, but the failure to protect the site from depredation can scarcely be laid against us. We excavated under terms of a contract with INAH, and it was the responsibility of that organization to safeguard the site after we left. In subsequent brief visits to the site, we have heard stories, perhaps exaggerated and perhaps not, of large numbers of jade figurines and stone sculptures pillaged and sold for large prices by treasure seekers. The Parque Olmeca in Villahermosa contains a number of sculptures reported to have been dug up at La Venta since 1955, and we believe this since none of these were either at La Venta

or Villahermosa in that year. We mention the rape of La Venta here to say that while there is a possibility that some undisturbed remnants of Complex A may exist, it would be a literally herculean project to remove the disturbed earth, relocate one's self, and salvage the still undug portions of the site. In the meanwhile the destruction proceeds, and we are increasingly unlikely to ever learn anything about the phase placement of the larger stone sculpture style at La Venta. A hint that firm association of monumental art in Olmec style with earlier phases may eventually be found is suggested by use of imported stone in the form of basalt "facing blocks" (as well as stone of metamorphic type from other sources) as early as Phase II times. This stone is significant for two reasons: (1) it demonstrates contact with regions at some distance where such basalts occur; and (2) it demonstrates developed technology in stone-working, in cutting and dressing this difficult material. We remind the reader of the Phase II basalt statuette mentioned in the last paragraph.

The matter of re-use, misuse, mutilation, etc. of monuments in post-La Venta times we leave to the dialecticians to speculate about. The only point to note here is that originally we differed on the matter of Monuments 25, 26, and 27. One of us believed that their positioning and reuse were post-Olmec on the grounds that the upside-down posture of Monuments 26 and 27 implied lack of understanding of the symbolism of the intricate low relief designs. We meant to offer this alternative but it got lost in revision and editing (DHS 1959:204-209).

The time seems to have come for summing up, drawing a few conclusions, and even for proposing an idea or two of our own.

First of all, we shall admit that we did not like the C&S review, but that it was probably good for us. It brought to light some serious errors we had committed. These were mainly in the area of presentation, but were nonetheless significant since proper communication of scientific data is as important as field and laboratory methodology and interpretation. To this extent we are grateful to our critics that they have shown us the error of our ways, although we feel they might have been at times more kindly about it.

Careful reappraisal, however, indicates that once our faults of exposition are cleared up, the La Venta work stands. The significant breakdown into four sequential constructional phases (Phases I-IV) cannot be challenged. If C&S would like to splinterize, that is a matter of taste; we did not feel that the data available warranted a breakdown into sub or sub-sub phases, or that anything of significance would emerge from such splinterizing. The cultural continuity of Complex A, a point we did not beat the drums for in the original report, also stands out clearly. Since C&S mounted a strong attack on this point, chiefly through attempts to refute allocations of objects referable to the Olmec art style to all but Phase IV deposits, it may be worthwhile to spell out evidences of this continuity. Since C&S have challenged the occurrence of Olmec style artifacts, surrogate burial, etc. in pre-Phase IV levels, we shall not use these items although we have demonstrated to our own satisfaction the critics' objections lack of validity.

	Phases			
	I	II	III	IV
Symmetrically designed enclosure	x	x	x	x
Fills of selected clean clays, sands	x	x	x	x
Planned leveling, drainage	x	x	x	?
Massive Offerings (deposited subterraneously)	-	x	x	x
Cruciform celt offering	-	x	x	x
Importation of large amounts of stone	-	x	x	x
"Facing blocks"	-	x	x	-
Facing and floor surfacing of selected materials	x	x	x	?
Low symmetrical platforms	x	x	x	x

This brief chart makes clear that there are a series of significant features, mostly related to the "architectural pattern" that C&S worry about so, that carry through all or most of the La Venta phases. The traits are of varying significance, and perhaps weights. The item referring to use of stone, for example, has special socio-political significance--it means not only use of stone per se but implies knowledge of distant regions, organized systems of transport, etc. The peculiar cruciform layout of celt offerings must have some ritual significance. Drucker has wanted for a long time to get his hands on a Nautical Almanac to see if the mean declination of the Southern Cross in the May-June evening sky does not tie in with the "Center Line" orientation not only of Complex A but also with the main features of La Venta. If someone wants to go way out on the limb to try to relate the cruciform celt pattern to the Southern Cross, conspicuous in the evening sky in May (i.e. at milpa planting time), we will cheer him on and be glad to hear if he finds a connection. In any case the cruciform layout of celts, presumably as offerings, carried on for a long time at La Venta.

The fact that this set of traits persisted at La Venta throughout the four periods, that is, was renewed on each restoration of the site (though each time with modifications), we regard as of more than just casual interest. We interpret this fact as of major significance, that is to say, that what it means to us is that the Court was originally constructed as the scene for the performance of certain specific ceremonials. There is no possible way to determine just what sort of rituals they were, but the fact is that the maintenance of the basic architectural pattern of a graded, well-drained enclosure containing various small platforms indicates that essentially the same rituals were performed throughout the use of the site. What the significance of changes in color scheme meant--when certain platforms were painted yellow instead of purple, or when the Court floor was done in old rose instead of white or green--we know not. But we can guess that the emphasis on elaborate color schemes, changes from one to another garish decor, had some purpose, and such purpose was in vogue throughout use of the site. This, to us, suggests that the ceremonial patterns, i.e. the manifestations of religious belief, continued from the inception of construction of Complex A until the site's obvious abandonment after Phase IV, when the constant maintenance program that had kept the features neat and orderly stopped, so that erosion took its toll of the final elaborate constructions, completely washing away painted surfaces, and washing and gullyng the fill, the red clay cap, into an irregular expanse

of hummocks and depressions. But from Phase I to the end of Phase IV there was obvious continuity. For this reason more than any other we feel obliged to resist C&S's effort to distort stratigraphic evidence to place all objects of Olmec type in Phase IV (cf. Heizer 1964). Our assumption is that these objects had some specific ceremonial significances and that, as the evidence really shows, they were part of the Complex A pattern from first to last (we have them present from Phase II through Phase IV, and extrapolate their presence in Phase I). We feel justified, therefore, in calling La Venta an "Olmec" site in the sense that it is one of the few places where objects pertaining to the Olmec artistic pattern have been found directly associated with architectural and occupational materials. The pottery offerings (such as Offerings 15, 16, etc.) clearly belong to categories of wares defined from the occupational deposits.

La Venta was an Olmec site during the time represented by Phases I through IV. We stand pat on that.

There was, throughout our analysis of the review, something that bothered us. The impression grew that the reviewers were trying to do something more than make a scholarly appraisal of the DHS report. They seemed to be trying to prove something all the time. So we went back and reread their introduction, and there we nailed down the basis of our discomfiture. The payoff is where, after listing some of the broad problems of the so-called "Olmec problem," they say (p. 1), "Such problems come to mind and stick there when one is engaged in the analysis of data derived from what amounts to a competitive site and culture. [*Italics ours.*] In this case we are thinking of Tikal, the major central lowland Maya site." This passage is remarkable in its implications.⁴ We are aware of no data, archaeological, traditional, or other, that indicates that there was any direct competition between ancient La Venta and ancient Tikal. DHS were not competing, either. We were not trying to find the oldest, or the prettiest, or the most of anything. Our goal was simply to find out as much as we could about La Venta.

As far as the "mother-culture" concept goes, C&S should have whetted their knife for someone else. We were on record (1959:262) as opposing the "mother-culture" idea.⁵ As much as we are reluctant to agree with our critics, we, as they, do not believe it probable that lowland Maya culture derived from La Venta or from Olmec in general. Looked at broadly, the "mother-culture" concept is decidedly simpliste. It compares to the long discarded view that Graeco-Roman culture was the source, the "mother-culture" if you choose, of modern Western civilization--a theory few culture historians will accept nowadays: doing a simple problem in arithmetic, remembering the day of the week, drinking a cocktail, consulting the calendar, eating a piece of toast, using a fork, smoking a cigarette, tying your shoelace, and recalling when you last went to church, among other things, will show you why.

Until someone comes up with a better hypothesis, we still favor the idea of Mesoamerican culture growth as derived from some ancient, simple, and widespread pattern centering on maize culture, religious beliefs centered on this economic mainstay, a pattern of preparing special places for the ceremonial observances, emphasis on ceramics, etc. that came to be widespread in Mesoamerica and which as well, as it developed, broke into two strains--Lowland and Highland. Localized proliferations of these rather simple and

generalized patterns, which are the only ones that could justifiably be referred to as "mother-cultures," and subsequent cross-fertilization, that is, diffusion and local re-elaboration, would have been the main factors in culture growth. That cultural cross-fertilization has occurred abundantly cannot be challenged. Why should the La Venta Olmec, in their stoneless land, have gone to the trouble of importing basalts and serpentines to make the "facing blocks" they set in their clay structures with no other function than that of adornment? This practice probably represents an attempt to copy stone masonry construction (though not necessarily, we hasten to add, that of Tikal). The Olmec doubtless contributed to various groups with whom they were in contact--lowland Maya of the Chontalpa region, Oaxacan, and Chiapanecan groups of the regions from which they got metamorphic rock and jade, and with the Valley of Mexico. The idea of Tlatilco as an outpost, or colony if you prefer, is not so hard to accept. There are abundant evidences in culture history of small groups splitting off from their congeners to migrate to distant lands: take, for example, the Huasteca, of Mayan linguistic affiliation; the various enclaves of Mexicano-speaking groups in Central America; the Oregon and northern California Athabascan-speaking groups; and on and on. As the result of such cross-stimulation and local elaboration, in our view, there developed the various major cultures of what we refer to as Mesoamerica--Highland Maya, Zapotec, Mixtec, Valley of Mexico, Lowland Maya, Olmec, Totonac, Huastec, and a host of minor patterns.

We have now come nearly, but not quite, to the end of our rejoinder to the review by C&S of our monograph. We take the opportunity to offer a few general remarks. C&S, we believe, did not write this piece to show us up as inferior archaeologists as much as to demonstrate that we were sloppy reporters and faulty interpreters. To us, these are all the same thing. We never implied either that we had performed a perfect archaeological investigation or that our report at the time, or in future, would provide all the answers to the archaeology of the La Venta site. We did the best we could with a small staff and under not easy conditions. If one surveys the literature on Mesoamerican archaeology, we believe that our report stands as one of the more conscientious and careful examples. The bogey-man which C&S raise about La Venta (or Olmec) being one of the earliest examples of the Classic type of culture is unfair--at the time we published our report, and still today in 1965, this is precisely where La Venta stands in the comparative picture of Mesoamerican cultural development. Whether it will continue to occupy this position we do not know, nor do we care, but their effort to deny our fundamental demonstration that La Venta is Olmec, that it dates from early in the first millennium B.C., and that it progressed through four stages of construction has, in our opinion, failed. C&S make it clear that they do not like to take for granted the conclusions we reached on the La Venta Olmecs. That is their privilege, and they are kept company by other qualified students such as Sanders (1962:34-43), but at the same time we also have our voluntary and independent adherents such as A. Caso (1965), M. D. Coe (1962), E. Wolf (1959: 70-71).

We have tried to be objective and forthright in this rejoinder to C&S, and in order that our present opinion not be misunderstood, we wish to say that we disagree with C&S's (1964:38) opinion that La Venta "was a fairly short-lived maverick site," but (granting a probable four century floruite for the site entitles it to be classed as long-lived) was, rather, one of a

series of regional sites of the Olmec culture. Even though we do not now know how to chronologically seriate San Lorenzo, La Venta, Tres Zapotes, Laguna de los Cerros, and a number of other Olmec sites, the fact remains that Olmec sites abound and some of them are Middle Preclassic in time. We are not impressed with any efforts made to date to seriate the major Veracruz-Tabasco Olmec sites on the basis of stylistic analysis of the art style.

The ultimate question remains to each student of Mesoamerican pre-history: "How do I decide where to place La Venta in the larger scheme?" The answer is simply this: "Take time to read Drucker (1952), DHS (1959), C&S (1964), Heizer (1964), and the present rejoinder, and make up your own mind." We believe that our description of La Venta archaeology is objective and factual. Where doubts occurred to us, we admitted these. We also made some errors, both of commission and omission, and we have tried here to point these out. We have not found it either easy or pleasant to dig through cold field notes which are ten years old, and it is perhaps only natural for us to feel that we were being put on the defensive in many instances where this was unjustified.

La Venta itself, except for the pyramid, is now so badly disrupted that it cannot be re-excavated. But there are other Olmec sites, and work is now beginning on these, so some of the questions which C&S raise will probably be answered by work still to be done. If our own work at La Venta in 1955, published in 1959, questioned in 1964, and defended in 1965 helps to illuminate the course of cultural development in prehistoric times in Meso-america, we ask no more.

NOTES

¹The sample referred to as M-528 by Heizer (1964:46) should have read M-531. The age of 2560 + 300 B.P. is correct for M-531 and not for M-528. Samples M-531, M-533, UCLA-902, and UCLA-903 are also discussed, with correct designations, in Berger et al (1965:345-346).

²Cremation at Tres Zapotes is demonstrably part of the quite late, intrusively post-TZ Soncautla period.

³Lest such a pattern of "offering" seem impossible, we point to a similar one common in our own culture in past decades, when the ceremony of laying of the "cornerstone" of some important edifice often involved deposition of various contemporary artifacts--a local newspaper of the date, and like mementoes.

Offerings placed beneath temple structures and other constructions and beneath stelae in the Maya area are well known, and the same situation has been observed in dedicatory caches beneath foundations of buildings in Egypt, Babylonia, and Assyria. Luckenbill (1924:100) in a translation of one of the records left by Sennacherib, writes: "A stela with my name inscribed I wrote and buried 160 tipku deep in the terrace, and left it deep down in the foundation for the days to come." We are, of course, not suggesting any historical connection between the La Venta Olmecs and the eastern Mediterranean civilizations, but cite these instances only because they suggest some

historically conscious motivation may have been involved in the Olmec offerings, both of small objects and of the "massive offering" variety.

⁴W. R. Coe (1965:18) has more recently disavowed his belief in any hypothesis that Preclassic Tikal was due to Olmec influence.

⁵Caso (1965:11, 44-48) takes issue with our crediting Covarrubias with the mother-culture idea (cf. DHS 1959:297) and indicates that he is the father of the proposal (Caso 1942).

REFERENCES

- BERGER, R., G. J. FERGUSON, and W. F. LIBBY
1965 UCLA Radiocarbon Dates IV. Radiocarbon, Vol. 7:336-371.
- CASO, A.
1942 Definición y Extensión del Complejo "Olmeca." In *Mayas y Olmecas, Segunda reunión de mesa redonda sobre problemas antropológicos de México y Centro America*, pp. 43-46. Sociedad Mexicana de Antropología, Tuxtla Gutierrez, Chiapas.
1965 Existio un Imperio Olmeca? *Memoria de el Colegio Nacional* 5, No. 3, for 1964, pp. 1-52. Mexico City.
- COE, M. D.
1962 Mexico. Praeger, New York.
- COE, W. R.
1965 Tikal, Guatemala, and Emergent Maya Civilization. *Science*, Vol. 147:1401-1419.
- COE, W. R. and R. STUCKENRATH, Jr.
1964 A review of La Venta, Tabasco, and its relevance to the Olmec problem. *Kroeber Anthropological Society Papers*, No. 31:1-44. (Cited here as C&S.)
- CURTIS, G.
1959 The petrology of artifacts and architectural stone at La Venta. In *Excavations at La Venta, Tabasco, 1955*, Appendix 4:284-289. Bureau of American Ethnology, Bulletin 170. Washington, D.C.
- DRUCKER, PHILIP
1952 La Venta, Tabasco: a study of Olmec ceramics and art. Bureau of American Ethnology, Bulletin 153. Washington, D.C.
- DRUCKER, P., R. F. HEIZER, and R. J. SQUIER
1959 Excavations at La Venta, Tabasco, 1955. Bureau of American Ethnology, Bulletin 170. Washington, D.C. (Cited herein as DHS.)
- GOLDSCHMIDT, W. R.
1951 Ethics and the structure of society: an ethnological contribution to the sociology of knowledge. *American Anthropology*, Vol. 53:506-524.
- HEIZER, ROBERT F.
1964 Some interim remarks on the Coe-Stuckenrath review. *Kroeber Anthropological Society Papers*, No. 31:45-50.

- KROEBER, A. L.
 1959 Yurok national character. In Ethnographic Interpretations: 7-11. University of California Publications in American Archaeology and Ethnology, Vol. 47, No. 3:236-240. Berkeley.
- LUCKENBILL, D. D.
 1924 The annals of Sennacherib. Oriental Institute Publications, Vol. 2. University of Chicago Press.
- PIÑA CHÁN, R. and L. COVARRUBIAS
 1964 El pueblo del jaguar. Mexico City, pp. 16-24.
- SANDERS, W. T.
 1953 The anthropogeography of central Veracruz. In Huastecos Totonacos y sus vecinos. I. Bernal and E. Davalos H., eds. Sociedad Mexicana de Antropología. México.
 1962 Cultural ecology of nuclear Mesoamerica. American Anthropology, Vol. 64:34-43.
- SHOOK, E. M. and W. R. COE
 1961 Tikal: numeration, terminology and objectives, report No. 5:1-14. In Tikal Reports, Nos. 5-10. University Museum Monographs, University of Pennsylvania. Philadelphia.
- WEDEL, W. R.
 1952 Structural investigations [at the La Venta site] in 1943. Bureau of American Ethnology, Bulletin 153:34-79. Washington, D.C.
- WESTHEIM, P.
 1952 La inexistente arquitectura de La Venta. Humanismo, No. 4:47-50. México.
- WILLIAMS, HOWEL and ROBERT F. HEIZER
 1965 Sources of rocks used in Olmec monuments. Contributions of the University of California Archaeological Research Facility, No. 1: 1-40. Berkeley.
- WOLF, E.
 1959 Sons of the shaking earth. Chicago, University of Chicago Press.

Comment by Coe and Stuckenrath written in November, 1965 after reading preceding typescript copy of Drucker and Heizer's Commentary.

The preceding paper by Drucker and Heizer has been read by us with care (and some disappointment), compared with our review (C&S) of the 1959 study of La Venta (DHS), and, finally, restudied in terms of DHS. There is so clearly nothing to be gained by further haggling. The true score is incalculable.

We do continue to hold responsible review, whatever its purpose, to be of value. The "bias" of our review, so underscored by Drucker and Heizer here, is, of course, exactly that. It is clearly conceded throughout our review: "Let us stop and look at the evidence once more, for the magnitude of the issues makes it worthwhile." Drucker and Heizer surely have the right to claim that we were at times intemperate, obfuscatory, and picayune--in fact, an inexcusable nuisance in our review. Moreover, we could not avoid implicit criticism (occasionally emerging as overt) of the excavations and interpretations without impinging upon the judgment of the participants. We regret only the allegations of irresponsibility in our review and leave the reader to find our criticisms real and founded, as well as to judge whether or not our purpose was as senseless and indiscrete as Drucker and Heizer protest.

Reviews and interplaying rejoinders inevitably become a round robin of wistful clarification by faithful reiteration. We will leave our comments at that, if only to avoid pages of confusing, cross-referenced "corrections" that really can be of no additional help to the reader.

Final comment (not to be taken as the last word) on the controversy, written by Heizer after reading Coe and Stuckenrath's comment printed supra.

Drucker and I agree that while the question and retort game could be played indefinitely, all parties presently concerned have had their say, and that not much is to be gained by continuing the argument.

What is most to be regretted, and this has nothing to do with the present exchange of opinions and views, is that the La Venta site which we left in May, 1955, could have been excavated again not only to learn new facts, but to verify or correct conclusions which we reached, were it not for the fact that the site has been so torn up by jade hunters that this is now impossible. The INAH cannot be blamed, and we state this lest our earlier remarks be so interpreted as to indicate that we hold this organization responsible. With such limited funds for scientific research and caretaking responsibilities, the Institute could not be expected to have guarded the site effectively. Perhaps one of the benefits of the C&S and D&H exchange will be to help make responsible parties aware of the vulnerability of these major sites, and the necessity for effectively protecting them for the future.

We agree with Coe and Stuckenrath that the reader should judge the matter on the central issues (i.e. excavation data and their interpretations) and not let overtones of bias, personality, emotional reaction, etc. influence his decisions.