"EARLY FORMATIVE PERIOD OF COASTAL ECUADOR": WHERE IS THE EVIDENCE?

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> "If you keep discouraging me, I'm going to become an archaeologist. You know what that is? It's a science where you dig up something old and make up a story to go with it." (Gordon and Gordon, 1966, p. 33).

Introduction

> The primary duty of the excavator is to record facts. If he does not publish the measurements of the structures he has found, the nature of the strata covering and filling them, and descriptions of the finds which they contain, he fails in the responsibility which is implicit in the act of excavating. Such a man has no right to excavate, still less to foist upon the public interpretations unsupported by evidence and incapable of being checked independently. (Atkinson, 1946, p. 165; emphasis in the original)

This statement is one with which most archaeologists would probably concur. Because we commonly take for granted that an excavation report will present evidence, we seldom question if it does until we find it necessary to utilize the data it should contain. It was the efforts of Hill (1975) to make use of the report authored by Meggers, Evans, and Estrada (1965; henceforth referred to as MEE)[1] that drew my attention to the problem of the difference between evidence and interpretation in archaeological publication.

When it first appeared, most reviewers hailed "Early Formative Period in coastal Ecuador" as a proud beginning to a new series. Whether or not they agreed with the rather startling suggestions of the authors about the origin of pottery in the New World, the reviewers spoke highly of the lavish illustration, mass presentation of data, and ease of cross reference. However, in a penetrating review, Henning Bischof noted the lack of detailed profile and section drawings (Bischof, 1967, p. 217), as well as of provenience for the illustrated artifacts (1967, p. 219). Also, rather than using the term "data," Bischof refers to, "Classification, description, and statistics of the materials recovered" (1967, p. 217). Bischof was correct in so doing, for the publication under consideration is most noteworthy for the presentation of interpretation unsupported by evidence.

Why then, did some reviewers refer to the abundance of data? I suggest that the array of tables and the 196 beautifully reproduced photographic plates, as well as the ease of reference from table to

figure, to Type description, to plate, led the reviewers to suppose that they were being provided with the data they expected. Since none of these reviewers tried to use any of the "data" they did not discover that the data were not there.

Archaeological Evidence

> Archaeological evidence is of two kinds, which we can call monuments and associations. Monuments, in this special archaeological sense, are objects surviving from the past which reflect human activity in some way, having been made, or used, or merely transported by man. Objects which were made or modified by man are commonly called artifacts by archaeologists; they are one kind of monument.... Natural or unmodified objects are not artifacts, but they may be monuments, if they can be shown to have been used or even simply transported or moved by man.... Associations are the relationships of monuments to one another and to natural features in the ground. (Rowe, ms.)[2]

It cannot be sufficiently emphasized that these are the only two kinds of archaeological evidence. All the rest is interpretation.

In any site report there is a basic minimum that must be reported in order to present the evidence.

1. How the evidence was found, i.e., the actual mechanics of excavation and recording.

2. What were the associations, i.e., accurate description of the location of monuments, and section and profile drawings.

3. What were the monuments, i.e., adequate descriptions with reasonable illustrations, and reference to their associations.

I will discuss the report on the "Early Formative Period of coastal Ecuador" in terms of these three kinds of information. In this discussion I will refer primarily to that portion of the report dealing with the Valdivia Phase, but the evaluation is applicable to the rest of the report as well.

How was the evidence found?

In the section entitled, "Description of sites and excavations," (MEE, pp. 15-25) the authors state that they examined 8 of 10 known Valdivia Phase sites, that the soil and contents of these sites were similar, and that "no evidence was found of walls, floors, or other kinds of structures, and no significant natural stratigraphy could be identified at any of the sites" (MEE, p. 15). Each of the 8 sites is then described, but only on page 87 are we told that only 5 sites were excavated. There

are "sketch maps" of 4 of the 5 excavated sites (MEE, figs. 3-5, 7, pp. 14, 17, 18, 23). Each "sketch map" is provided with a scale, a north arrow, contour lines, and shows the location of excavation units. The maps do not entirely agree with the text, especially regarding preliminary tests and extensions to some of the excavation units which are mentioned but not always drawn. The fifth excavated site, G-84, is described as having been excavated by Estrada in 10 cm. arbitrary levels with Valdivia Phase refuse extending from 0.4 to 1.3 m. under Jambelí Phase refuse. "No further details are available on the site area, nature of the deposit, or size of the cut" (MEE, p. 21).[3]

Neither the size of the excavation units nor the method of excavation is given in all cases. The size of most of the cuts can be ascertained by measuring them on the "sketch maps" although there is, of course, no way to learn the size of the tests and extensions that are not included on the maps. The method of excavation in the "stratigraphic" excavations is, in all cases, that of arbitrary levels of varying depth. This fact, although nowhere stated in the report, can be ascertained from various tables, as can the depth of the levels used in any given cut, with a few exceptions. For example, it is impossible to find how cuts B, C, and D at site G-25 were excavated, since all the excavated material from this site is lumped in a single unit as if it were surface material. Numbered cuts are generally referred to as "stratigraphic" excavations, and we can probably infer that, unless otherwise indicated, the rest of the excavations, tests and extensions, were not dug "stratigraphically." Such an inference is supported by the authors in discussing the "two trenches and a test pit," at G-54: "Only rims, decorated sherds and artifacts were saved from these excavations as the digging was not controlled by levels nor the refuse sifted" (MEE, p. 19). This sentence is also the only place where it is noted that a screen was not used. The use of a screen is only specified, however, for the Valdivia levels of Cut J at G-31, Cut 1 at G-54, Cut 2 at G-88, and "earlier excavations at G-31" (MEE, p. 41), and the mesh size is never given.[4]

Equally scattered, and sometimes contradictory, information is available on what was saved from the excavations. At G-31, "Everything recovered from Cuts A, F, and H was saved for analysis and samples of shell and animal remains were kept from other excavations" (MEE, p. 17).[5] The contents of the samples from other excavations are not included in the report. At G-54, in Cut 1, "only large, fire-cracked rocks and unworked shells were discarded in the field" (MEE, p. 19). And later, "All bone from G-54, Cut 1, and G-88, Cut 2 was preserved and identified" (MEE, p. 25). Regarding G-84, and Cut J at G-31, statements are not so clear. We find a reference to "the failure of nonceramic refuse material to be saved from excavations at G-84, which represents Period D, and from G-31, Cut J, Sections D and E, which provided the best pottery samples for Period A" (MEE, p. 34). Since this statement occurs in a subsection entitled "Chronological distribution of stone artifact types" we might infer that the authors refer not only to nonartifactual remains, but to any nonceramic remains, although the phrasing is not entirely clear. A glance at Table 1, however, will show that several stone artifacts and some shell, as well, were recovered from Cut J. And we are also told

that the sifting of the Valdivia Phase deposit of Cut J, "produced a tremendous quantity of sherds as well as shell, stone and other kinds of refuse" (MEE, p. 17). In contrast, we are told that no bone (or shell?) sample was saved from G-84 (MEE, p. 25).

As far as field recording is concerned, we are even more in the dark. If profile and section drawings were made of the excavation units they were not published, although some references are made to observed soil differences and other features (e.g., MEE, pp. 19, 24-25). That such profiles were not made in all cases is suggested by the following statement: "Three of the G-31 excavations (Cut A, Cut J-D and Cut J-E) are adjacent and depths are more or less equivalent, although their exact correlation was not recorded during the fieldwork" (MEE, p. 149).[6]

It is also not at all clear who was actually present during the various excavations. From Estrada's early publications, one can infer that Félix Martínez was his foreman during the excavation of Cuts A and B at G-31, and from then on Julio Viteri was his field assistant. We are told specifically that Viteri supervised the excavation of Cut J (MEE, p. 17). The real question is when and where were Meggers, Evans, and/or Estrada actually on the spot and supervising the excavation, and those data are not given. The reason this question becomes important is that there is some internal evidence in the report suggesting that the authors were not always pleased with what was done in the field. The question is, who was responsible?

Although several burials were excavated, especially at site G-54, no drawings are published, and the only photographs are those of burials 1-7 at G-54 that were excavated by Viteri (see note 3).

An inconsistency in dates referring to excavations at G-31 and at G-54 seems to indicate poor recording. Aside from tests, the only excavations at G-31 were Cuts A through J. Cuts A through I were excavated in 1956-57 (MEE, p. 16), and Cut J was excavated in January of 1961 (MEE, p. 17). However, "In December of 1960, during large scale excavations at G-31,...Julio Viteri...discovered and excavated a group of burials [at G-54]" (MEE, p. 18). While on the following page we find that "Seven burials were excavated in February 1961 by...Julio Viteri" (MEE, p. 19). The last two citations both refer to burials 1-7 at G-54. Thus, we have a major excavation at G-31 not otherwise mentioned, and Viteri excavating the same burials at G-54 at two different times, two months apart.

Some of this confusion is resolved, however, by reference to a work by Estrada written in either late 1960 or early 1961, prior to the arrival of Meggers and Evans in Ecuador for the 1961 excavation season (Estrada, 1961). In this work, Estrada describes his excavation of Cut J at G-31, this cut being a trench running east-west, composed of 5 m. square sections designated A to E (Estrada, 1961, p. 9). He also mentions plans to extend that excavation at G-31 as well as to study a cemetery found a short distance away [an obvious reference to the burials at G-54] as soon as Meggers and Evans arrive (Estrada, 1961, p. 7). It

is now clear that Cut J was dug partly in 1960 and partly in 1961, and that Viteri discovered and excavated burials 1-7 at G-54 in 1960. Apparently, then, when Meggers and Evans arrived in Ecuador, the sixth section of Cut J was excavated, not in line with the existing trench, but abutting the northwest edge of Estrada's section E. For some reason, after Estrada's publication, his sections were redesignated, and his section E was changed to F, while the sixth section was designated E as it appears in the "sketch map" of G-31 (MEE, fig. 4, p. 17). This change resulted in two serious apparent misprints:

> [Cut J] consisted of five squares...designated as Sections A-E [sic].... A sixth 5 meter square (Section E) was added at the northwest side of Section F in the hope of recovering additional fragments of a castellated rim vessel (pl. 103a, b) found near the northwest side of Section E [sic]. (MEE, p. 17)

If we realize that these sentences reflect a confusion of the original nomenclature of Estrada's trench, with the changed nomenclature, presumably of Meggers and Evans, this seeming nonsense is immediately clarified, and we find that Cut J consisted of Sections A-D and F, with a sixth square (Section E) added at the northwest side of Section F to get more fragments of a vessel found near the northwest side of Section F. We also now understand why the sections of the original trench were designated in such an odd fashion--they were relettered.

If we are told little about field recording, we are told almost nothing about laboratory recording and control. The only way in which we know that the materials were marked is by the acknowledgements and a few numbers that can be seen on nonceramic objects in the photographs (MEE, p. xii [vii] and pls. 15, 19, 20, 22, 25). No numbers are visible on any of the pottery, although the word "Valdivia" may be discerned on a Machalilla stirrup spout (MEE, pl. 155h). The numbers that can be seen are obviously keyed to some sort of catalog, since the numbers 868, 869, and 870 are applied respectively to a piece of worked shell from G-31 (pl. 22e), a stone ax from the same site (pl. 19o), and the T-shaped stone ax from G-54 (pl. 19r). How the system works is not at all clear, however, since in plate 20, several fishhook reamers are seen to bear the same number.

What were the associations?

In his review Bischof stated, "Excavation by artificial levels was dictated by the nature of the deposits" (1967, p. 217), but he has since demonstrated this statement not to be true (Bischof and Viteri, 1972; Bischof, 1973). He further remarks that disturbances and stratification can at least be recorded in detailed profiles, referring to Meggers, Evans, and Estrada's figure 6 as a "schematic profile drawing" (Bischof, 1967, p. 217). Hill has discussed some of the features of this profile and their implications for the interpretation of the occupation at G-54 (1975, p. 23). I will discuss in more detail the situation in figure 6, and the excavations of Cut 1 and the northeast extension of that

cut at G-54, as an example of the sort of stratigraphic problem encountered throughout this work.

It must first be understood that the profile represented in figure 6 (MEE, p. 20) is not only the only profile in the report, but is not directly related to any excavated unit. It represents the north bank of site G-54 which was cleaned off to show the profile, or, "to reveal the distinct natural stratigraphy, corresponding to the sterile upper and lower layers and the refuse deposit" (MEE, p. 19). In fact, this profile reveals a cut some 5.18 m. long, with the refuse deposit divided for its entire length into two distinct strata, presumably corresponding to those described for Cut 1 (MEE, p. 19). Shown in the profile, however, is a third stratum separating the two main strata, for a distance of 3.76 m. from the east edge of the profile, with a maximum thickness of some 15 cm. It is on top of this middle stratum that there lie the yellow sand lenses to which Hill refers (1975, p. 23). The authors do mention a "vertical column of sterile dirt 56 cm. in width, joining the upper and lower sterile layers near the center of the cleared zone" (MEE, p. 19), accounting for this feature as a post-Valdivia Phase disturbance.

This profile is separated by only about .50 m. from the north wall of the test trench which abuts the northeast extension of Cut 1. It is, then, only 2.5 m. from the north wall of Cut 1 (see MEE, fig. 5, p. 18). Although no profile is provided, the authors describe the soil changes in that cut in some detail (MEE, p. 19). They mention that the base of the second stratum (the first of undisturbed refuse) occurs at a depth of 100 cm. on the south side of Cut 1, and at 85 cm. on the north side. Interestingly, when the northeast extension was made (abutting the "north" wall of Cut 1), it was excavated in two levels--0-85 cm. and 85-120 cm.--although we are not told why (MEE, p. 19). This division probably represents an endeavor to separate the observed strata, but it seems likely that the two levels were excavated horizontally rather than by following the natural strata, which would again result in mixed material. From the profile, we can see that the bottom of the upper stratum of undisturbed refuse occurs at a depth of only 70 cm. on the east edge of the profile, and at about 75 cm. at the east edge of the prehistoric (?) "disturbance," while below that point not just one, but two strata are depicted.

There are several disturbing things about this stratigraphic situation. We have no statement about the extension of the post-Valdivia "disturbance" back from the face where the profile was drawn. From the profile, it is clear that this "disturbance" was an excavation, either a hole or a trench, which was subsequently filled either naturally or deliberately. It is most probable that the excavation was filled deliberately, since the profile does not show the rounded margins that would occur in the case of erosional fill. The authors refer to the "disturbance" as a "column of sterile dirt," but this assertion seems unlikely. If the "disturbance" represents an excavation that was deliberately refilled, the only way the fill would be sterile would be if the fill were brought in from off the site, or if the excavated soil had been screened to remove all cultural material before refilling. If the excavation were filled either naturally or deliberately with local material, it would contain at least some mixed cultural material, since

there is no way in which a hole could be dug through more than a meter of rather dense refuse as well as some 30 cm. of "semi-sterile" wash (providing the latter existed at the time of the excavation), without completely mixing the contents of the excavated soil. Also disturbing, is the question of the middle stratum in the profile, which has outlines suspiciously like a shallow house pit. We have no statement about the extension of this feature back from the face where the profile was drawn, either. If either of these two features extended into Cut 1 or the northeast extension thereof, then all the arbitrary levels from these excavations may contain mixed strata. If not, we are still not in a position to reconstruct the contents of the levels, since we are deprived of one significant datum, namely, from what point the measurements of the levels were taken.

Did the authors take all their measurements from the surface, thus varying the depth as the surface varied; or did they take all measurements from a single point, thus assuring a horizontal floor to each arbitrary level? Again, we are not told. Meggers, Evans, and Estrada's plate 9c and 9d, however, shows us two views of the eastern corner of Cut J, section F, at G-31. Along the southeast wall of the cut there is a downward slope of the surface toward the east, and on the northeast wall there is an even more marked downward slope of the surface toward the north. The excavated level of section F appears to be horizontal, sloping neither to north or east. In plate 9a we can see that the same is true of all of Cut J. Although we still do not know from what point measurements were taken, we can infer that it was some fixed point and not the sloping surface. It is probable that, if one major cut was excavated in this fashion, so were all the rest.

It is evident that there was considerable horizontal, as well as vertical, stratigraphy at some of the sites. In discussing site G-88, the authors note variation in the depth of the sterile layer in two of their pits, in one case from 50 to 85 cm. in a 3 m. square. They also consider Cut 3 at that site to contain Period A material, while Cuts 1 and 2 contained Period B only (MEE, fig. 52).

Site G-31 also has a highly variable base level as well as indications of a complex history of occupation. Although the "sketch map" of the site shows a continuous area of Valdivia occupation partially overlaid by a Guangala occupation (MEE, fig. 4, p. 17), the excavation findings tell a somewhat different story. Cut E, some 20 m. southeast of Cut J, contained only Guangala refuse. Cut D, which is only 2 m. from Cut J, section F, is characterized as shallow and contained mostly Guangala remains, whereas section F reached a depth of 4.40 m. (MEE, pp. 16-17). If we accept the authors' chronological attributions, then Cut F, near the extreme east side of the site, is all Period B; Cut H, near the center of the site, contains refuse pertaining to Periods B and C; and only those cuts near the northwest edge of the site (A, B, and J) contain refuse from Periods A through C, although surrounded by primarily or wholly Guangala bearing cuts (C, D, and E). A vivid illustration of the uneven nature of the pre-occupation surface can be seen in the depth of refuse over the 25 m. length of Cut J, by 5 m. squares: Section B is 30 cm. lower than A; C is 70 cm. lower than B; D is 90 cm. lower than C; but F is only 20

cm. lower than D, which is adjacent (see MEE, p. 17).[7]

In addition to the highly uneven bases upon which the cultural remains were deposited, and the apparently shifting nature of the occupation within the sites, there is evidence of considerable erosion at all the sites for which there is information (G-25, G-31, G-54, and G-88). At G-88, Cut 1, some 6 m. below the summit of the site, had 30 cm. of sterile wash overlying the refuse which varied from 50 to 85 cm. in depth. Cut 3, however, on the summit of the site, had only 20 cm. of refuse. It was also Cut 3 which contained Period A materials. In a situation with this sort of erosion, one would expect the condition of the summit of the site to be similar to that of a blowout site, i.e., having the soil and light weight material washed away leaving the heavier remains to settle little by little, gradually mingling earlier and later materials. There are two indications that such a process did operate on the area of Cut 3. First, the matrix containing the sherds is described as "lumpy, medium-gray clay" in contrast to Cuts 1 and 2 where the sherd bearing levels contained powdery, light gray dirt (MEE, p. 21). Second, rather than saying that Cut 3 contained early material, the authors state, "the sample [from Cut 3] included types characteristic of the earliest part of the Valdivia Phase" (MEE, p. 21; emphasis mine). Nowhere, however, do the authors suggest the possibility of such artifical mixture in this cut.

On the basis of what I have just presented, it might appear that the authors do provide evidence with which the reader can reconstruct associations. However, I have discussed the cases where the maximum evidence is presented, and it is not sufficient. In view of the inconsistency of the authors' reporting, we cannot assume that because they do not mention an uneven sterile base, the base was really relatively level. Nor, because they do not mention changes in the soil within a cut, can we assume that the deposit is really uniform, as can be seen by Bischof and Viteri's work at G-31 and the photographs of Cut J in the report itself (Hill, 1975, fig. 2; MEE, pls. 9 and 10). And what does it mean to say that the depth of refuse in Cut 2 at G-88 varied from 50 to 85 cm. if the authors were measuring their arbitrary levels from an unknown fixed point and we already know that the site sloped steeply? Only detailed section diagrams with superimposed level lines would tell us anything about the stratigraphic associations of the arbitrary levels, and this evidence is not provided.

In order to establish associations, we need to know not only the stratigraphic situation, but what objects were found in what strata. There are, of course, degrees of association which provide varying amounts of information. The most informative is an association, such as a burial association, where we can be sure that all the items found together were buried at the same time. In a well controlled stratigraphic excavation, occurrence within the same stratum, when there is no evidence of disturbance, is a very good association. In a situation with considerable horizontal stratigraphy, even data on the excavation unit from which objects come can provide useful evidence. And finally, site provenience can be useful if the occupation at the sites is of relatively short duration, or if there is likely to be regional variation. In any case, the provenience must be given for specific objects, not for typological

abstractions, or it does not constitute evidence.

In Table 1, I have tabulated all the objects from the Valdivia Phase excavations for which Meggers and Evans provide specific provenience. In order to qualify for inclusion in the table, the object must be illustrated, described, and the cut and level where it was found must be specified.

TABLE 1

Valdivia Phase monuments with cut and level provenience

Specimen	Site	Cut and Level	Illustration	Description
stone bowl	G-31	J-D 270-300 cm.	pl. 16a	p. 26
stone bowl	G-31	J-B 120-150 cm.	pl. 16b	p. 26
stone ax	G-54	Burial 1	pl. 19r	p. 28
cut shell	G-31	B 300-320 cm.	pl. 21o pl	pp. 37-38
cut shell	G-31	B 340-360 cm.	pl. 21p	p. 38
shell object	G-31	J-E 270-300 cm.	pl. 22 e	p. 40
figurine stool	G-31	H 120-130 cm.	fig. 63d-f	p. 101
figurine stool	G-31	F 20-40 cm.	fig. 63a-c	p. 102
deer antler tip projectile point	G-54	northeast extension of Cut 1, 85-125 cm. [sic]	pl. 25e	p. 41

There are other objects for which we have cut provenience: the stone figurine from G-31, Cut J (MEE, pp. 100-101 and fig. 61); the castellated rim vessel from G-31, Cut J (MEE, p. 17 and pl. 103a, b); all Valdivia Cord Impressed Type pottery from G-31, Cut J (MEE, pp. 54-56 and pl. 51); all Valdivia Embossed Type pottery from G-31, Cut J (MEE, p. 57, pl. 57, fig. 30); three polished stone axes from G-31, Cut J (MEE, p. 28, pl. 19o-q).[8] And finally, there are a few items for which we have site provenience: the deer antler awls from G-54 (MEE, p. 41 and pl. 25a-d); some Machalilla Double-line Incised Type pottery from G-54 (MEE, pl. 138); Machalilla Punctate and Red Zoned Type pottery from G-54 (MEE, pl. 148t-u); Machalilla Red Banded Type pottery from G-54 (MEE, pl. 153). Although initially the shell pendants described on page 38 appear to have provenience, we find that there is no reference to specific illustrations.

If we are not given provenience for the monuments, we cannot establish associations, and that line of evidence is closed. In this

case, even with provenience, because of the excavation in arbitrary horizontal levels in sites with sloping natural strata, we would be hard pressed to establish any associations.

What were the monuments?

Apparently, most of this report is devoted to the description of the material objects recovered. There are three sections in the report where one would seek evidence on the Valdivia Phase monuments: (1) the section entitled "Description of artifacts" (MEE, pp. 26-107); (2) Appendix I, Tables 1-7 and 9-14 (MEE, pp. 183-213); (3) the plates of photographs (MEE, pls. 15-126, 158, 160-170, 172, 174-185, 187, 189-190). There are also a number of drawings of artifacts in the artifact description section.

With the exception of the artifacts noted in the preceding section, however, we find that all the descriptions, tables, and photographs are useless. They are not even necessarily an accurate representation of the kind of material that is found on coastal sites in Guayas Province, since the stone figurine illustrated in plate 118g is obviously the same specimen as that represented by Estrada with a caption stating that it came from La Ponga (Estrada, 1958, fig. 12, no. 9, p. 32). La Ponga is located in the very near vicinity of the town of Loma Alta (personal communication, Hill), and not on the coast at all. That the material in the photographs does not necessarily represent artifacts recovered from the excavated units referred to in the tables, or even from excavated units, can be seen by comparing the figurines in plates 120-124 with those published by Estrada with exact provenience (1956, lâm. 3) from Cuts A, B, and the surface at G-31. The plates are not intended to provide evidence, but simply to illustrate Type specimens. And Types are the result of interpretation, they are not evidence.

The artifact descriptions likewise do not refer to specific specimens, but to Types. The reconstructed vessel shapes, although said to be reconstructed from sherds can be demonstrated in at least one case to go far beyond the evidence provided by the sherds. In many cases the description of the base is "probably slightly flattened" or "probably rounded or slightly flattened" (MEE, p. 54), but all reconstructed forms are reconstructed with bases, thus providing a false idea of the completeness of the data. The rim profiles mean little if not associated with the specimens from which they are drawn, and even the groupings are frequently meaningless. For example, of the 5 rim profiles associated with figure 42, no. 1 (MEE, p. 77), only the 3 on the right fit the description, while the 2 on the left rather than having what Hill called a recessed rim, have a broad incision or groove below the rim.

The tables are of little more use than the photographs and descriptions, although they overpower one with the quantity of numbers and percentages presented. As before, those tables supposedly referring to artifacts refer not to monuments, but to typological constructs, and so are meaningless as evidence. Even the tables showing the distribution of faunal remains are less useful than they should be because we lack associated data. For example, is the greater amount of bone at G-54 correlated with a lesser quantity of shell refuse? We cannot tell for

unmodified shell from that site was not saved. Table 1, "Frequency of species of mollusks in levels in G-31, Cuts A, F, and H of the Valdivia Phase" is basically useless, since it was stated that Cut H is the only one of the three from which everything was saved, so the samples are not comparable (see note 5).

Even were they to represent data rather than interpretation, the tables are so inconsistent and have so many faults that they would be highly suspect. For example, why is Period D represented in Table 11 and not in 12? If Cut 3 at G-88 is listed in Tables 10 and 12 as Cut 2, what other misprints are there that are not so easily detectable? If 25 sherds is a sample too small for valid percentage calculations (MEE, p. 210), why is the same not true for 23 sherds (MEE, p. 214)? Why are the two test trenches and the northeast extension of Cut 1 at G-54 noted as having selected samples (MEE, p. 202), but no such notation made for the test at G-88 (MEE, p. 205)? Table 5 is an excellent example of the sorts of internal and external inconsistencies found in the tables. Nine deer antler awls are tabulated, but on page 41 the text indicates that only 8 were found. In addition to the "stratigraphic" cuts which are generally the only ones represented in the tables, Table 5 includes not only the northeast extension of Cut 1 at G-54, but also one of the Test Trenches at that site and the "Bank Excavation" which is mentioned nowhere else. The "Bank Excavation" must refer either to the results of pothunting activity by the local inhabitants in the north face of the site, or to the results of cleaning the north face (MEE, p. 19). Whichever of these alternatives may be correct, the reason for including this anomalous material in the table is clear when one notes that 5 of the total 8 or 9 deer bone awls come from the "Bank Excavation." Not only are these uncontrolled excavations included in the table, however, they are interdigitated among the arbitrary levels of the "stratigraphic" Cut 1, as though they had a seriated position within the sequence, something not suggested elsewhere in the report.

The position of a given excavated level within the seriated sequence is also subject to some confusion. This confusion is especially disturbing when it affects the placement of the levels within the authors' Periods, since frequent reference is made to such-and-such a feature or Type occurring in Period A, B, C, or D levels. For example, Cut J, section D level 150-180 cm. falls in Period B according to figures 50 and 51 and Table 9, but in Table 2, the same level is down toward the middle of Period A. Section E of Cut J is worse still, since not one but two levels are involved. In Table 2, both the 240-270 cm. and the 270-300 cm. levels of this section are in Period A, whereas both are in Period B in figures 50 and 51 and Table 9.

It is distressing that, in spite of all the illustrations, tables, and ready cross referencing, it is not possible to find out one simple but important piece of information, namely, how many vessels with castellated rims were represented in the excavation? We know that section E, Cut J at G-31 was excavated "at the northwest side of Section F in the hope of recovering additional fragments of a castellated rim vessel (pl. 103a, b) found near the northwest side of Section [F]" (MEE, p. 17). The caption to plate 103 indicates that these fragments have been assigned to the

Valdivia Red Incised Type. The Type description (MEE, p. 81), under "Vessel shapes reconstructed from sherds" states, "3. Jar with rounded belly, wall sloping inward and upward to direct rim, flattened lip. Rim diameter 28-34 cm. (fig. 29-3 top). One example has a castellated rim (pl. 103a, b)." There is a reconstructed vessel shape of the castellated rim vessel which goes far beyond the evidence provided by the sherds, since it indicates a vessel with a slightly flattened base, and four evenly spaced castellations (MEE, fig. 29, middle row right, p. 56), whereas the number of castellations is not obvious from the fragments, and there is no base whatsoever on the fragments. There is one other fragment of a quite different castellated rim vessel (MEE, fig. 101b, p. 164) which apparently did not come from the analyzed excavation units, since there is no mention of vessels with castellated rims in any Type description other than the one cited above. We are not given provenience for this second fragment, nor are we told whether additional fragments of the first vessel were found in Cut J, section E. There is, however, some evidence available on both of these points. Estrada published photographs of two fragments of the first vessel and of the single fragment of the second vessel (Estrada, 1961, lâm. V, figs. 2-4), stating in the caption that they all came from Cut J. By comparing Estrada's illustration of the larger fragment of the more complete vessel (lâm. V, fig. 2) with the same fragment published by Meggers and Evans (MEE, pl. 103b), we can see that the second version contains 3 more sherds than did the first. It is possible that these additional sherds were present in Estrada's excavated sample and had simply not yet been recognized, but since one of them is a fairly good-sized rim sherd, this possibility seems fairly unlikely. Since the authors did not see fit to consider the castellated rim vessel a special form, or even to include Valdivia Red Incised Form 3 in their combined vessel shapes (MEE, Table A, pp. 91-92), we have no chance of ascertaining the exact provenience of these 3 sherds that are probably included in their tabulated levels. Nor can we ascertain the number of vessels with castellated rims that were represented in the excavations.

So we see that even a form which is highly visible, rare, and important to the argument about Japanese contact, cannot be traced through the mazes of tables and charts. The situation is still worse with the anthropomorphic faces represented on plate 41. These particular faces have been assigned by Meggers and Evans to Valdivia Broad-line Incised Type, Motif 7. However, there are no examples of the faces in the seriated levels (MEE, p. 105 and Table 7, p. 206), so we cannot say whether such faces occur at any of the excavated sites except for the rather aberrant example reproduced in plate 41a, which probably comes from Cut B at G-31 (compare Estrada, 1958, fig. 15, no. 4, p. 35).

Conclusions

This discussion has not been exhaustive. I did not ask why the authors combined the 10 cm. levels used by Estrada in his excavations into 20 cm. levels in Tables 7, 10, 11, and 14, but not in Tables 6 and 13. Nor will I inquire as to how the authors can compare the relative proportions of kinds of shell at G-54 and G-88 (MEE, p. 21) and G-54 and G-110 (MEE, p. 110), when the shell was not saved at G-54. There are equally puzzling anomalies on every side. I did not intend to point out every

contradiction, misprint, or failure to provide specific evidence, although I hope that what I have provided will be of use to others who try to use this volume in the future. The point I have attempted to make and document is that, in spite of an imposing façade, the volume is empty. We are provided with no evidence regarding the nature of the Early Formative Period of coastal Ecuador.

Although I disagree with the theoretical approach of the authors, I must point out that there is nothing intrinsic in such an approach that prohibits the presentation of data. The classification of sherds into pottery types does not preclude providing provenience data for the illustrated type specimens. Although the type-frequency approach to archaeology does not necessitate excavation in arbitrary levels, the two tend to go together. But even excavation in arbitrary levels does not preclude the provision of measured profiles. The provision of reconstructed shapes for categories of vessels does not require that such shapes be complete when there is no evidence for the whole shape. And contradiction is contradiction whatever one's theoretical stance.

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NOTES

[1]This report was actually written by Meggers and Evans, since Estrada died unexpectedly in November 1961, "shortly following the final season of fieldwork" (MEE, p. viii). While it is true that he would undoubtedly "have loved to see the verification of his correlation...between Valdivia and Jomon" (MEE, p. viii), I am not at all sure that he would have been as pleased with other aspects of the report. The hypothesis of contemporaneity between Valdivia and Machalilla, for example, does not agree at all with his published interpretations. Since we can only evaluate Estrada's position on the basis of his earlier publications, I feel that for the purposes of this essay, the authors of the study should really be considered to be Meggers and Evans.

[2]An earlier version of this statement of Rowe was paraphrased by Patterson (1973, pp. 2-3).

[3]Throughout the report we find scattered phrases of this nature, generally without further explanation. For example, "little of the material from...G-31, Cut J...was preserved for classification and analysis" (MEE, p. 41); "the failure of nonceramic refuse material to be saved from...G-84...and from G-31, Cut J" (MEE, p. 34); "the specimens were unavailable for rechecking" (MEE, p. 92); "No samples were available from Period D levels" (MEE, Table 3, p. 188); "Count unavailable for these

levels" (MEE, Table 4, p. 189); "separate count for each level unavailable" (MEE, Table 10, p. 207). We are not told whether the material was not saved on purpose or through error, not to mention what was saved, and almost never are we told whether the many things that were unavailable had ever been available, had been disposed of, were simply out of reach or anything else. This sort of problem is well illustrated by the case of burials 1-7 at G-54, excavated by Julio Viteri, Estrada's field assistant. We are told, "Details of position of the individuals and information on association is limited to what can be seen on the photographs" (MEE, p. 19). In plate 12a, however, we see that the area of the burials was carefully gridded with string, usually a preliminary to making a measured drawing. Were measured drawings of these burials made?

[4]Another disconcerting aspect of the report is the manner in which evidence on a single topic is scattered through the text. The evidence on screening is a good example of this disorganization. One would expect to find information on screening in the section entitled, "Description of sites and excavations," and it is there that we find that the Valdivia levels of Cut J at G-31 were screened (MEE, p. 17), and the information referring to site G-54 (MEE, p. 19). The information on screening in the "earlier excavations at G-31" and Cut 2 at G-88, however, is found in a subsection entitled "Chronological distribution of shell artifact types" (MEE, p. 41), not exactly where one would look for such information. It is also unfortunate that the authors are not more specific about whether all, or only some, of the earlier excavations at G-31 were screened, since their phrasing is ambiguous. Photographs of the excavations of Cuts A, G, and F do not show any screens (cf. Estrada, 1956, pp. 3, 5, 6, 8; Evans, Meggers, and Estrada, 1959, fig. 7a, b, p. 101).

[5]The wording of this sentence is very careful and, in the light of other versions, one should note the exact meaning of the phrase "everything recovered." A slightly different phrasing states, "All shell, bone and other organic remains from G-31, Cuts A, F and H were saved for classification and identification.... Samples of shell were retained from other G-31 excavations" (MEE, p. 25). The minor discrepancies between this phrase and the one on page 17 are not really important until we read the following statement published in an earlier report:

> Besides the sherds, each shell, bone, crab claw and rock from Cut H was saved and brought to the United States National Museum for detailed analysis. The majority of such objects were also recovered from the other excavations except in the lower levels of Cut A, where the conditions of the excavation and the workers' fear in the deep and dusty pit did not permit the recovery of more than a sample. (Evans, Meggers, and Estrada, 1959, p. 16; my translation)

This statement may be compared to the following: "The lower levels of G-31, Cut A were reduced in area because of excavation problems, and the sample from Period A is consequently very sparse" (MEE, p. 25), or, "The lower levels of G-31, Cut A, which extend into Period A, provided a very small sample because of constriction in the size of the excavation with

increasing depth as the walls were sloped to prevent collapse" (MEE, p. 34). Comparison of the different versions of what was saved from Cuts A, F, and H, suggests to me that the statement on page 17 is probably correct, and that whatever was found was saved. We are never told just which levels constitute the "lower levels" of Cut A.

[6]Even this statement does not really represent the true situation. Sections J-D and J-E touch only at one corner, while that portion of Cut A which is closest to J-E (also a corner) is at least 2.5 m. away (see MEE, fig. 4, p. 17). In view of the extreme irregularity of the original surface at G-31 (see MEE, p. 17; Evans, Meggers, and Estrada, 1959, p. 16), correlations of levels between sections without good section drawings are dubious at best.

[7]There is some discrepancy between the depths given for Cut J by Estrada (1961, p. 9) and those presented by Meggers, Evans, and Estrada (MEE, p. 17). Estrada states that the sterile base appeared at less than 2 meters depth in section A, and at 4.50 m. in section E (later changed to F), while Meggers and Evans give 2.10 m. for Section A and 4.40 m. for section F.

[8]Two of these axes are said to have come from Cut J, section E, 300-330 cm., while the third came from Cut J, section B, 270-300 cm. (MEE, p. 28). We find, however, that in section B of Cut J at G-31, refuse continued to a depth of only 240 cm. It is possible, therefore, that the third ax really came from Cut B at G-31, which reached a depth of 380 cm. (Evans, Meggers, and Estrada, 1959, p. 16). If this surmise is correct, then we do not have even cut provenience for the axes, since we are not told which of the three illustrated specimens came from which section.

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