

**SMOKESCREENS IN THE PROVENANCE INVESTIGATION
OF EARLY FORMATIVE MESOAMERICAN CERAMICS**

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We are glad that Sharer et al. (2006) have dropped their original claim that the INAA data demonstrate multidirectional movement of Early Formative pottery. Beyond this, however, they offer nothing that might enhance understanding of Early Formative ceramic circulation or inspire new insights into Early Formative cultural evolution in Mesoamerica. Instead, their response contains fresh distortions, replications of mistakes made in their *PNAS* articles, and lengthy passages that are irrelevant to the issues raised by Neff et al. (2006) . We correct and re-correct their latest distortions and misunderstandings here. Besides showing why their discussion of ceramic sourcing repeatedly misses the mark, we also correct a number of erroneous assertions about the archaeology of Olmec San Lorenzo. New evidence deepens understanding of Early Formative Mesoamerica but requires that some researchers discard cherished beliefs.

Nos agrada que Sharer et al. (2006) hayan abandonado su afirmación inicial de que los resultados del INAA demuestran un patrón de circulación en múltiples direcciones para la cerámica del Formativo Temprano. No obstante, no ofrecen nada nuevo que pudiera mejorar el entendimiento de la circulación de la cerámica para este momento y tampoco proponen nuevas perspectivas sobre la evolución cultural en Mesoamérica. En cambio, usan una retórica de poca relevancia o patentemente falsa para desviar la atención y negar los resultados que no son de su agrado. En el presente texto corregimos sus últimas distorsiones y malentendidos. Además de mostrar las razones por las cuales su discurso sobre la determinación de las fuentes de cerámica fracasa en muchos puntos, también corregimos las numerosas declaraciones erróneas sobre la arqueología del centro olmeca de San Lorenzo. Las nuevas evidencias proporcionan un entendimiento más profundo del Formativo Temprano de Mesoamérica aunque requiere que ciertos investigadores abandonen algunas de las ideas más cercanas a su corazón.

We thank Sharer and his colleagues for responding to our discussion of what the results of INAA imply about interregional movement of ceramics during the Early Formative Period. Yet much of their response restates mistaken assertions from the original *PNAS* articles. It also offers irrelevant claims and imputes views to us that we do not, as a group, hold. We believe that scientific discourse should be structured as an argument, not as rhetoric.

Sharer et al. (2006) draw on prior conviction, a set of beliefs about the study region and this period of Mesoamerican antiquity. Because of this stance, they reject the results of Blomster et al. (2005), Neff and Glascock (2002), and Neff et al. (2006) and seek to discredit any evidence that contradicts their closely held beliefs. The authors do not want to see valued goods moving from the so-called Olmec “heartland” to regions they have regarded as primary producers and compeers with sites like San Lorenzo. To achieve that end, they present results from a limited and problematic petrographic study and two flawed statistical analyses. In the latest essay and its precedents, they emit a rhetorical smokescreen to cover their main objection and to shield problems in their analyses. There is, however, no empirical support for the latest salvo. The newest claims are wrong.

Sharer et al.’s first mistaken claim is their opening assertion that all 13 coauthors of our article failed to understand why the *PNAS* pieces were written. The purpose of the *PNAS* articles was hard to miss: The essays were written in an explicit attempt to “overturn” results of a large study that demonstrates lopsidedness in the circulation of certain ceramics in Early Formative Mesoamerica. Even the title of Stoltman et al. (2005)

betrays this purpose: “Petrographic evidence shows that pottery exchange between the Olmec and their neighbors was two-way.” Their current article continues to press this argument, so we are confident that our original understanding is correct.

Another claim in Sharer et al.’s (2006) introductory section is that the *PNAS* articles by Stoltman et al. (2005) and Flannery et al. (2005) were peer reviewed and thus carefully checked for quality and fairness. We checked with Daniel Salsbury, managing editor of *PNAS*, about the peer review process for papers submitted with an Academy member as an author and received the following statement:

An article submitted to *PNAS* with an Academy member as an author is considered to be ‘contributed’ to the journal. *PNAS* editorial policy requires that the paper be submitted along with the comments of a minimum of one expert reviewer and the authors’ response to the comments. All papers are submitted to the Editorial Board for a final round of review before they can be accepted for publication in the journal.

The *PNAS* peer review policy for “contributed” papers, then, is atypical of refereed journals both in the small number of solicited reviews (one) and because it is the article author rather than the editor who selects the reviewer. Perhaps the peer selected by Flannery et al. (2005) and Stoltman et al. (2005) to review their papers was expert, but he/she certainly was not careful. Even a cursory review should have detected glaring errors like the misuse of discriminant analysis by Stoltman et al. (2005).

Ceramic Sourcing

First, we are pleased that Sharer and his colleagues have dropped the claim that the MURR INAA data demonstrate “that most regions received pottery from several other areas” (Stoltman et al. 2005:11216). But considering this shift in perspective together with the additional expertise added to their list of co-authors (especially Price and Prudence M. Rice) we might have expected a more measured, thoughtful discussion of ceramic sourcing methodology. The time allotted for response was short and may not have been sufficient for careful reflection. However, this does not excuse the basic mistakes in the rebuttal. Each of these must be met, clarified, and rejected. We proceed in roughly the order in which they appear in Sharer et al.’s article.

Common Sense

Sharer et al.’s (2006) advocacy of a “common sense approach” to ceramic characterization is partly valid. Most scholars agree that specimens should come from well-documented contexts. But Sharer et al. are wrong about the universality of a recipe for simple-to-complex analytical techniques in ceramic characterization. The usefulness of an analytical technique depends on whether it measures what the archaeologist/analyst wants to measure, not on whether it is higher- or lower-tech than some other technique. A 10x hand lens is certainly useful for basic temper/texture discriminations (so is 1x examination by the human eye, especially by younger people), but it would be unwise to depend on these low-tech tools for most source discrimination. Moreover, with subsidized instrumental analysis programs like MURR, INAA and other “high-tech” analyses may actually be *less* costly than laborious but “lower-tech” techniques like thin-

section petrography. In the Stoltman et al. (2005) study, for instance, the expense of petrography limited the sample size to approximately 20 sherds, whereas the INAA database was larger by more than an order of magnitude and included hundreds of raw material analyses. To reiterate a point made repeatedly in our earlier article, we would not argue for the superiority of INAA over petrography, only that it is the source-discriminating power of the techniques in *specific, real-world situations* that is important, not whether one is “lower-tech” and therefore closer to the top of the hierarchy advocated by Rice (1987). Petrography does not deserve precedence simply because a petrographic microscope is cheaper than a nuclear reactor.

Sharer et al. tout the virtues of “intimate familiarity with Woodland pottery” (Sharer et al. 2006: 5) and state that we disparaged such familiarity. We did not, nor would we. Intimate familiarity is one excellent source of *hypotheses* to test with materials analysis. Indeed, such familiarity with pottery from Oaxaca and the Gulf Coast might have helped Stoltman et al. (2005) avoid some of the more fallacious claims in their *PNAS* paper. And, presumably, Stoltman and Mainfort (2002) were testing the hypothesis of a local Pinson Mounds origin, derived at least partly from intimate familiarity, when they did their thin-section analysis. But if two materials analysis techniques yield conflicting results, as Stoltman and Mainfort (2002) claim for the Pinson case and as Stoltman et al. (2005) assert for the Olmec case, resolving the difference by appeal to “intimate familiarity” is circular. It is more productive to probe incompatibilities between the two data sets through a civil collaborative venture. Unfortunately, in the Olmec case, a search for reasons for possible incompatibilities is precluded because Sharer and his

colleagues have not accepted Glascock's offer to analyze the petrographic samples by INAA at MURR expense.

Chemical Groups

Sharer et al. attach some importance to the fact that INAA only creates chemical groups and does not “reveal sources” of pottery, but this is true only in a limited and very technical sense. To be *entirely* accurate in a technical sense, one would say that INAA only records gamma-ray emissions. But the gamma spectra are then processed to yield elemental concentrations; the concentrations are then processed to yield chemical groups; and the chemical groups are then linked, with varying degrees of confidence and with varying bridging arguments, to geographic locations, zones, or regions. In the Olmec study, the last step – one leading to an inference of source location – derived from comparison with raw materials sampled in Mazatan, the Valley of Oaxaca, the Valley of Mexico, and the San Lorenzo region. The number of raw material analyses included in this study is nearly unprecedented, and makes for exceptionally strong source-zone inferences. True, the “source” regions thus identified are regions where the raw materials were procured, not necessarily zones where the pot was made, as Sharer et al. (2006:7) note. But for this observation to undermine the results of the Olmec study Sharer et al. must affirm that potters from Oaxaca, Mazatan, Tlapacoya or Etlatongo traveled to San Lorenzo to obtain raw clays, then carried them back to distant workshops. This alternative scenario is implausible in the extreme, and its implausibility reveals this whole line of argument as a smokescreen.

The smokescreen billows when Sharer et al. praise “Old World practitioners,” who are “candid” about the limitations of INAA, then go on several sentences later to repeat Stoltman et al.’s (2005) criticism of Neff and Glascock (2002) for the same candor. Neff and Glascock (2002) are again rebuked for noting the similarity between *some* Oaxaca pottery (Late Formative) and Early Formative San Lorenzo pottery, as if doing so amounted to a stunning revelation. But, as we explained at length (Neff et al. 2006), evaluating the precision of one’s measuring instrument is *part of good science*; Day et al. (1999) recognize this and so do we. However, as we also explain (Neff et al. 2006) , *careful evaluation of the discriminating power of INAA in this application also allows us to rule out the possibility that there were any misattributions of Early Formative pottery derived from Oaxaca clays to the San Lorenzo group.*

Sharer et al. (2006) continue to replay claims from their *PNAS* articles, making the obvious and gratuitous point that INAA (like other bulk techniques) “cannot determine whether the source of a given element comes from the clay, the temper, the substances cooked or stored in the pot, or the soil in which the sherds lay buried.” Later, they circle back to the issue of diagenesis, stating that Blomster et al. (2005) ignored it. This is not true. As we explain at length (Neff et al. 2006), the possibility that diagenesis might be creating or destroying structure in the data *is always considered* in the bulk-chemical provenance investigations we know. In the case of Olmec pottery, there are multiple lines of evidence (similarity of groups to raw materials, lack of an association of chemical groups with burial context, etc.) demonstrating that diagenesis is *not* responsible for the group structure reported by Neff and Glascock (2002) and Blomster et al. (2005). We discussed these issues at length (Neff et al. 2006) and are baffled that

Sharer et al. again commend Day and his colleagues (1999) for considering diagenesis, while disregarding our extended discussion of precisely the same issues.

False Analogies

The extended discussion of Day et al.'s (1999) study of Minoan pottery is used by Sharer and his colleagues to thicken the blanket of smoke. To them, the Minoan study shows how, at Aegean Bronze Age palatial sites (analogous to San Lorenzo in Sharer et al.'s discussion), the frequency of occurrence of ceramics indicates consumption patterns but may not necessarily indicate what pottery was produced there. Yet Sharer et al. seriously misrepresent the intent and results of the Minoan work. The main motivation for combining chemical and petrographic approaches on Crete was the difficulty of making key interregional discriminations with chemistry alone (Day et al. 1999:1028). In Early Formative Mesoamerica, interregional chemical discrimination is not the issue at all: Early Formative ceramics produced in the various sampled regions are easily and unambiguously discriminated from those produced in other regions. Furthermore, most of the Early Formative compositional groups, including the two San Lorenzo groups together with the Tlapacoya, Oaxaca, and Mazatan groups, are tied to locations on the ground not just by frequency of occurrence but by comparisons to analyzed raw materials. In short, the analogy with Minoan Crete is false, and using it to attack the Olmec INAA study is a diversion.

The Minoan study is, however, relevant to the current debate in another way. Day et al. (1999:1026) observe that "thin section petrography is also unable, in isolation, to

distinguish between the products of different sources.” In other words, Day and colleagues explicitly evaluate the limitations of thin-section petrography as a source-discrimination tool on Crete, exactly as they do for INAA, and exactly as we recommend should have been done by Stoltman et al (2005) in the Olmec study. In contrast, Sharer and his colleagues, in response to our recommendation that the source-discriminating power of petrography in the Olmec study should have been evaluated by sampling the range of petrographic variation in San Lorenzo and Oaxaca pottery, state “[t]his is, of course, a red herring” (p. 8). Of course it is *not* a red herring, and we believe that Day and his colleagues would agree with us. There is a vast gulf here between the positions favored by ourselves and Day et al. (1999), who believe that scientific measuring instruments should be evaluated carefully and their limitations acknowledged, and the position of Sharer, Stoltman, and their colleagues, who reject any scrutiny of petrography and label evaluation as “a red herring.” It is thus not only diversionary but also quite ironic for Sharer et al. to rely for validation so heavily on the work of Day et al. (1999), which exemplifies a very different, more skeptical, approach to petrographic data.

There is comparable irony in Sharer et al.’s approving reference to the study by Adan-Bayewitz and Wieder (1992) of pottery from Roman Galilee. In this study, thin-section analysis and other techniques were compared to *INAA*, *which was taken as the arbiter of valid group discriminations* (note that this is the *opposite* of Stoltman et al. [2005], who view petrography as the ultimate arbiter). Adan-Bayewitz and Wieder (1992:191) state, “...it was decided to employ neutron activation analysis for the provenience work. This technique, although costly, features sensitive differentiation and accurate measurement of a wide array of chemical elements in pottery and clays, and is

therefore particularly suitable for local provenience studies that require high resolution.” After conducting the comparison, they conclude (p. 203) that “[thin-section analysis] was found to be an effective means for classifying collections according to major fabric categories, which agree with those defined by neutron activation analysis” and they point out that thin-section analysis and INAA usefully complement one another, in accord with our own views (Neff et al. above).

As an aside, we agree with both Sharer et al. (2006) and Day et al. (1999) that bulk elemental characterization techniques (like petrographic analysis) can be and have been applied to ceramics inappropriately, such that they have led to erroneous conclusions. Some previous studies by Sharer’s coauthors provide illuminating examples of such mistakes in the New World (Burton and Simon 1993 [see Neff et al. 1996 and Triadan et al. 1997]; Rice 1977, 1978 [see Neff et al. 1990]). And some of us have also published conclusions that were later changed (see, e.g., Neff et al. [1999], which modifies conclusions of Bishop et al. [1986], Neff et al. [1990], and Neff et al. [1994]). But it is simply fallacious to suggest that, because the *potential* for mistakes exists, they must have been made in the Olmec case. It is the *specifics* of the Olmec study, not false analogies with Bronze Age Crete or cautionary tales about the potential limitations of INAA, that are the only relevant issues here.

Pinson Mounds

Sharer et al. assert that we misrepresent Stoltman and Mainfort’s (2002) petrographic study of pottery from the region of Pinson Mounds, Tennessee. We do not.

The confusion lies with the continued assertion that this study definitively refutes the usefulness of INAA as a sourcing tool. It is useful to outline the history of this work. The petrographic study was purportedly undertaken because Mainfort was skeptical of INAA results (Mainfort et al. 1997), especially those that showed that Swift Creek complicated stamped pottery, assumed by Mainfort to be imported to Pinson Mounds from Georgia, fell within the range of chemical variation of presumed local pottery from the Pinson region. However, all of the sherds with “non-local” surface treatments turned out to be sand-tempered, like local Pinson pottery, and the only evidence produced by the petrographic study that might “overturn” the attributions based on INAA were slight quantitative textural differences between three of the presumed imports and the mean textures of Pinson Mounds sherds. We stand by our characterization of this evidence as a feeble basis for claiming to have “overturned” the INAA study.

Sharer et al. (2006) also mention “a minimum of five” other Pinson vessels that were not suspected to be imports, but which had “exotic” tempers, such as bone, grog, quartzite, and limestone. First, it is worth pointing out that three of these vessels had already been identified in the INAA study (Mainfort et al. 1997) as having anomalous temper, so the petrographic study (Stoltman and Mainfort 2002) contributed no new information. Second, as in the Olmec case, Stoltman and Mainfort (2002) simply assert, rather than document, the range of petrographic variation expectable in the Pinson region. Grog (crushed sherds) and bone are certainly available anywhere, including the Pinson region. The diagnostic value of common geological materials like quartzite and limestone is also limited (though Stoltman and Mainfort [2002:9] claim they do not occur within 50 km of Pinson Mounds). It might have been interesting to explore the possibility that

variant tempering practices sometimes cropped up, like mutations, in the practices of Pinson region potters; that foreign tempering preferences were sometimes transmitted into the Pinson area; that “exotic” tempers were sometimes carried into and used in the Pinson area; or that one or more of the INAA groups might represent a source area large enough to encompass neighboring regions with “exotic” tempers (the 50 km-distance to the presumed non-local rocks [Stoltman and Mainfort 2002:9] is only about two-to-three times the distance from Pinson Mounds to neighboring sites whose pottery falls within the range of variation of Pinson pottery both according to INAA [Mainfort et al. 1997] and according to petrography [Stoltman and Mainfort 2002:9]). As we discuss (Neff et al. 2006), INAA and petrographic analysis usefully complement one another, and this study presented an opportunity to capitalize on this complementary relationship. Stoltman and Mainfort, however, chose not to discuss (or even disclose) their results to Glascock, Neff, and their collaborators, opting instead to market the results (like the Olmec results) as a definitive demonstration that bulk elemental analysis is unreliable as a ceramic sourcing tool. Even cursory evaluation of the petrographic studies, however, reveals their claims to be exaggerated and unsupportable. We have more to say on Olmec petrography below.

Statistics

As we said previously, we applaud Sharer et al. for recognizing the statistical fallacies in Stoltman et al. (2005) and dropping the claim to have demonstrated that the INAA data themselves support multi-directional ceramic circulation. At the same time, misrepresentation and distortion of statistical issues continue to be problematic. . To

begin with, Sharer et al. (2006) display surprising ignorance when they contrast Mahalanobis distance (MD) with discriminant analysis, as if they are two distinct approaches. The allocatory results of discriminant analysis (e.g., Tables 1 and 2 in Stoltman et al. 2005) involve calculation of MDs. Clearly, Stoltman et al. (2005) did not understand what their black box (discriminant analysis) was telling them. As we explain in our article above, it is not the use of discriminant analysis (a powerful technique when used appropriately), but the *misuse* of discriminant analysis (by defining the groups to be discriminated solely on the basis of archaeological context, so that they subsumed multiple compositionally distinct sources) that leads Stoltman et al. (2005) to unsupported conclusions about interregional ceramic circulation.

Sharer et al. now disown the conclusions that Stoltman et al. (2005) drew from their discriminant analysis, contending “[the results in Tables 1 and 2 of Stoltman et al. (2005)]... are heuristic devices which simply ‘tested the hypothesis that archaeological context correctly describes the place of origin of a sherd’” and “the Discriminant Function Analysis leading to those tables could not in reality identify places of origin.” Although we are glad they now agree with us on this score, readers should be aware of revisionist history. The written record of the claims that were made (Stoltman et al. 2005:11216) includes assertions that the discriminant analysis shows that “...most regions received pottery from several other areas,” and “...that pottery was exchanged among highland valleys and between the highlands and the lowlands.” Flannery et al. (2005) likewise accept the discriminant analysis results as “[tending] not to support a model of Olmec ‘one-way’ trade...”

Some statistical background is required to expose the fallacy behind the assertion that MD “is not a ‘generalization of the univariate z-score’.” Let us therefore consider the well-known standardized or z-score measure of distance of an individual datum from the mean of a distribution:

$$z = \frac{x - \bar{x}}{sd_x}. \quad (1)$$

Now here in matrix notation is the Mahalanobis distance of an individual datum from the centroid of a distribution:

$$D_j^2 = (X_j - \bar{X})' V^{-1} (X_j - \bar{X}). \quad (2)$$

In equation (2), the term inside the parentheses is a vector of differences between the values for all elements on a particular sample (X_j) and the centroid values for the group (\bar{X}), i.e., it is a generalization of the numerator in equation (1). (Note, however, that this term is squared in the second case, which is why the symbol D_j^2 is used for the MD; the “ $'$ ” notation indicates that the first parenthetical term is a transposed version of the second, i.e., the first is a row vector and the second is a column vector). What the z-score (equation 1) does, however, is to put the distance measure into standard-deviation units by dividing through by the group standard deviation (which is an indicator of the group spread). How can this be generalized to the multivariate case, when the group is spread along multiple dimensions? The multivariate generalization of the standard deviation (or, more precisely, the variance, i.e., the square of the standard deviation) is a matrix of variances (on the diagonal) and covariances (the off-diagonal elements). To generalize the z-score to the multivariate case, then, one divides the squared distances from the centroid by the variance-covariance matrix. However, there is no such operation as

matrix division, so instead one multiplies by the inverse of the variance-covariance matrix, V^{-1} in equation (2). The inverse of matrix \mathbf{A} is the matrix \mathbf{A}^{-1} such that in the operation $\mathbf{A}^{-1} \mathbf{A} = \mathbf{I}$, \mathbf{I} is the identity matrix, with 1s on the principal diagonal and 0s in all off-diagonal positions (Van de Geer 1971). This comparison shows how MD is a generalization of the univariate z-score.

Sharer et al. also say that [MD] “is computed using a matrix of correlations among variables viewed as z-scores,” a statement we find difficult to interpret. If the original data were first turned into z-scores before calculating the variance-covariance matrix, then the latter would in fact be a correlation matrix, with 1s on the diagonal and Pearson correlation coefficients in the off-diagonal elements, but none of the matrix elements themselves could be viewed as z-scores. We are left scratching our heads, but one thing is clear: Their discourse on statistics is part of the overall effort to impede understanding, so that readers forget the fact that the INAA study produced clear, unambiguous ceramic-provenance results that Sharer and his colleagues refuse to take seriously.

We are also puzzled by Sharer et al.’s statement that Blomster et al. (2005) “only rarely find composition groups from unknown sites,” because they also chastise us, both here (Sharer et al. 2006) and elsewhere (Flannery et al. 2005; Stoltman et al. 2005) for leaving some analyses unassigned. There is a logical contradiction here. As we explain (Neff et al. 2006), the unassigned specimens are presumed to include some specimens that “may pertain to sources sampled so sparsely that they were not recognized as distinct groups.” That is, we *do* find “composition groups from unknown sites,” but we usually cannot characterize them well (and neither could anybody else) if they are represented by

only a few samples. Admittedly, with the dense rhetorical smokescreen created by this point in their critique, it is easy to understand how Sharer et al. became lost in the twists and turns of their own discourse.

One group, White-2, is abundant enough in our database to identify it as a distinct group but not abundant enough to be well-characterized. . While Blomster et al. (2005:1070) noted that its nine members were all from the Valley of Oaxaca, they were reluctant to conclude that it came from the Valley of Oaxaca because over half of its members showed above 1 percent probability of membership in the San Lorenzo White group. Sharer et al. remark, derisively, that “BNG found the possibility that Oaxaca produced its own kaolin ware so unpalatable that they ignored their own data and declare that White-2 ‘cannot be associated with a specific region.’” Perhaps unsurprisingly considering their other statistical misunderstandings, Sharer et al. fallaciously consider the 1 percent probability as “meager” evidence of a possible San Lorenzo origin. While Blomster et al. (2005) certainly did not consider it definitive (hence their reluctance to assign White-2 to San Lorenzo or any other region), the 1 percent probabilities are not “meager,” but rather constitute evidence that the White-2 and San Lorenzo White groups overlap at their edges; in fact, one of every 100 members of the San Lorenzo White group would fall as distant from the San Lorenzo centroid (in MD terms) as many White-2 group members. In light of this compositional ambiguity, one plausible hypothesis that perhaps merits further testing is that White-2 might derive from slightly anomalous clays of the San Lorenzo region. Sharer et al. deride this, and claim that “petrography confirms that this group was made in Oaxaca,” yet they cannot possibly have tested this because they did not analyze any of the INAA samples included in the White-2 group. We agree

that the hypothesis of a Oaxacan origin for the White-2 group members is viable, because all of them were found in Oaxaca. Blomster et al. (2005:1070) raised this as a possibility, noting that White-2 “could represent a kaolin source in the Valley of Oaxaca.” Yet, based on the chemical data, another viable hypothesis is that the group derives from the San Lorenzo region. We even admit that the contextual evidence makes us (like Sharer et al.) favor the Oaxacan-origin hypothesis, but we cannot reject the San Lorenzo hypothesis in light of the chemical evidence. Both hypotheses deserve to be tested further. The bottom line here, however, is that, if White-2 is a Oaxacan product, it provides additional evidence that Oaxaca did not export pottery, whereas, if it is a San Lorenzo product, it provides additional evidence of the magnitude of San Lorenzo’s exports; *either inference strengthens the overall conclusion* of Blomster et al. (2005) and Neff and Glascock (2002), which is that San Lorenzo exported lots of pottery and other regions exported very little, if any, to San Lorenzo.¹

The INAA Sample Revisited

As we explain at length (Neff et al. 2006), the INAA sample was selectively designed (1) to provide an assessment of variation expectable in local ceramics and raw materials in the various sampled regions and (2) to maximize our chances of detecting any interregional movement. Sharer et al.’s (2006) charge that the INAA sample is “biased” apparently relates to the second criterion. Yes, there was a “bias” in favor of finding exports by sampling white and carved-incised pottery in the regions for which collections were available for sampling. This was part of the research design, for the

reasons explained in our paper (Neff et al. 2006). But Sharer et al. are simply wrong to accuse us, again, of deliberately inflating the probability of finding imports at the highland sites while minimizing that probability at San Lorenzo. In any case, a new, larger sample to be published in the near future (David Cheetham personal communication 2005) confirms the absence of imports at San Lorenzo. Of course, Sharer et al. scoff at any additions to the INAA sample, saying “Neff et al. clearly seem to feel that if they run additional INAA samples, they will overturn the petrographic evidence,” which makes us wonder if anything could ever convince them that our results deserve to be taken seriously.

We also explain here (Neff et al. 2006) and elsewhere (Blomster et al. 2005; Neff and Glascock 2002) how ranges of compositional variation were estimated in Basin of Mexico and Oaxaca pottery, so Sharer et al. are making a false allegation when they accuse us of having “no interest” in estimating the ranges of variation in these regions. Considerable effort has been expended sampling pottery and raw materials from all of the regions in the INAA study in order to estimate ranges of compositional variation. Consider the Basin of Mexico, from which the number of pottery plus raw materials in the MURR database now exceeds 4000 analyses; from the southern Basin (where Tlapacoya is located), we have 76 raw clay samples, plus hundreds of Formative through Aztec period ceramics of various textures. Ironically, it was a student of Joyce Marcus, Mary Hodge, who initiated the Basin of Mexico compositional study, so at least one of Sharer’s coauthors knows of the comparative database from this region. What more could we do to demonstrate our interest in sampling ranges of elemental variation expectable in

ceramics from this region? We hope readers will refer back to our discussions of sampling issues in order to evaluate these false allegations by Sharer et al.

Renewed disparagement of our “nebulous mention of a ‘reference collection’” ignores the lengths to which we went (Neff et al. 2006) to describe our sample of both raw materials and ceramics and to direct readers to the relevant passages in our previous publications (e.g., Blomster et al 2005:1069). Some MURR data used in the Olmec study (and in other studies, such as Sharer’s Copan study [see below]) remain unpublished and thus were not made available as supplementary documentation with the *Science* article, but this does not make our reference to them “nebulous.” MURR’s record of publishing thousands of raw data speaks for itself and compares favorably with Stoltman’s record of publishing raw petrographic data.

Sharer et al. also castigate us for sampling pottery from only seven sites, apparently excluding some of their favorite places. This is sheer smokescreen. As an examination of the data in Table S2 of Blomster et al. (2005) will reveal, the database includes pottery from 16 Early Formative sites: Abasolo, Altamira, Aquiles Serdan, El Varal, Etlatongo, Hacienda Blanca, Huitzo, Laguna Zope, Paso de la Amada, Saltillo, San Carlos (Canton Corralito), San Isidro, San José Mogote, San Lorenzo, Tierras Largas, and Tlapacoya. Every sample has limitations, and one of the limitations of the Olmec INAA sample is that it does not include some highland zones, such as Morelos, Puebla, and Guerrero, where fine white and carved-incised pottery occur. This does not, however, invalidate the exceptionally strong pattern in the 944-specimen data set already analyzed, as Sharer et al. (2006) and Flannery et al. (2005) want readers to believe. A future study of Early Formative pottery focused on highland zones not included in the

present study would be of tremendous interest. We encourage any of the coauthors of Sharer et al.'s response to submit samples from these regions for INAA at MURR.

Sharer et al. continue by dismissing the sample analyzed by Blomster et al. (2005) on the grounds that “little information about the archaeological context” of the pottery was provided. They even refer to the samples as coming from “uncontrolled sherd collections,” thus implying that there *is* no contextual information for the sherds in the sample. Readers who may be tempted to believe this should check Tables S2 and S3 in Blomster et al. (2005), where the vast majority of samples have detailed, specific contextual information provided in the recording system of the excavators. The documentation is far more extensive than that provided by the *PNAS* authors; Table S1, for instance, provides references to direct readers to where illustrations of the analyzed sherds have been published. Readers may have to consult original site reports to interpret the context codes. For Etlatongo, for instance, detailed context information is available in several sources (Blomster 1998, 2004). This is a far cry, however, from depending on “uncontrolled surface collections.”

Sharer et al. hold the Copan study up as a model because of its use of ceramics from secure, primary contexts with well-documented associations. While the Copan vessels themselves were from well-controlled tomb contexts, the reference collections used by Bishop (Reents-Budet et al. 2004) to identify possible zones of origin included many of the same regional Mexican databases utilized in the present study! This irony escapes Sharer et al. Not only that, the Maya area database that was searched in order to identify vessels similar in composition to the Copan tomb vessels is dominated by vessels

from museum collections, which most often have little more than site or even regional contextual information!

The fact is, however, that Sharer et al.'s whole discussion of the importance of contextual information is diversionary rhetoric. Whether a sherd came from a numbered elite house in Oaxaca is not germane to the issue of whether the chemical data identify that sherd as a local Oaxaca product or an import from San Lorenzo. The scale of the archaeological question here only demands contextual information at the site or even the regional level. As archaeologists, none of us would question the "vital need to control the contexts and associations of ceramic collections," but it is nonsense to assert that detailed information about associations is relevant to every possible archaeological question. It is nonsense, and it is part of a smokescreen.

The Petrography of Early Formative Pottery

While Sharer et al. (2006) appear to have tempered their claim about the utility of INAA, it is clear that they favor only the technique – petrography – that, at least according to Stoltman et al. (2005), gives them their desired result. The *PNAS* authors often refer to how a handful of sherds analyzed by thin sections "overturned" the large sample analyzed in the INAA study, yet, by inverted logic, Sharer et al. have decided that chemical composition data can "not overturn petrography." The reasoning is unspecified, but let us assume for the sake of argument that it is correct and that we should confine ourselves only to petrographic evidence.

Sharer and his colleagues (especially, Stoltman et al. 2005) base their identification of Oaxacan imports at San Lorenzo on the “gneiss” in “Leandro Gray” vessels from San Lorenzo, which supposedly identifies them decisively as Oaxacan imports. We suggested (Neff et al. 2006) that Stoltman et al. (2005) should have provided some estimate of the range of petrographic variation of San Lorenzo ceramics, a suggestion that Sharer et al. label a “red herring.” Let us swim after that red herring for a moment.

Does “gneiss” temper preclude a lowland origin? Well, Winter has noted that in the southern Isthmus region, less than 5 km from the important Early Formative site of Barrio Tepalcate and less than 100 km from San Lorenzo, there are outcrops of metamorphic rock. Further, Weber and Hecht (2003) show that metamorphics are present in the inner Isthmus and headwaters of the Río Coatzacoalcos. So much for Sharer et al.’s claim that “the nearest natural occurrence to San Lorenzo of gneisses is several hundred kilometers away in the highlands of Mexico.”

Even more telling is a petrographic study conducted of San Lorenzo pottery by María Eugenia Guevara (2004). Guevara’s work overturns Stoltman et al.’s (2005) claim that inclusions in San Lorenzo pottery are exclusively calcareous sand. In her analysis of 30 sherds representing 14 local San Lorenzo types (substantially larger than Stoltman’s sample of one presumed local San Lorenzo sherd), Guevara found the following minerals, not necessarily co-occurring: mica (biotite), plagioclase (albite) and volcanic glass splinters, all of which are fairly common. Less common minerals include amphiboles, chlorite, zircon, epidote, pyroxene and calcite (also not necessarily co-occurring). Guevara’s petrographic analysis shows that bentonite, a weathering product

derived from volcanic ash, likely was incorporated in the ceramic paste during manufacture. Polycrystalline quartz is found alongside monocrystalline quartz in San Lorenzo pastes, suggesting that co-occurrence was in the clay source and may be due to the fluvial transport of minerals. Volcanics have been claimed as “proof” of Oaxacan products at San Lorenzo, as has gneiss, a metamorphic rock composed of materials such as quartz, feldspar, biotite mica and muscovite – most of which have been demonstrated to be present in San Lorenzo clays by Guevara’s study (2004). In light of Guevara’s study, it would be difficult to maintain that Stoltman’s analysis of thin sections demonstrates that any of the San Lorenzo sherds he examined contain inclusions that are unique to Oaxaca. Thus, while conceding the *possibility*, we doubt that Stoltman et al. (2005) identified any Oaxacan imports in the San Lorenzo collection, despite their self-assured claims based on a handful of thin sections.

Interpretation

If we strip away the rhetoric, Sharer et al. (2006) may not actually lie too far away from our own point of view. They admit comfort with our basic findings and with the inference that “San Lorenzo traded a lot of pots, a possibility that requires further testing.” We would only modify this statement to read “San Lorenzo exported a lot of pots, many more than it imported based on current evidence, and these patterns in the existing data require further testing.” Ours is a more accurate summary of the findings of Blomster et al. (2005) and Neff and Glascock (2002), and it is these findings that we

believe should be taken seriously by anyone interested in understanding the nature of Early Formative interaction patterns.

As Sharer et al. are in basic agreement with the views expressed by Neff et al. (2006), and because our article was intended to correct distorted and incorrect assertions about the Olmec INAA study (Flannery et al. 2005; Stoltman et al. 2005), most of the discussion of “interpretation” is misplaced. Much of their interpretation section is not even a response to Neff et al. (2006) but rather a renewed assault, complete with new distortions, on the *Science* article of Blomster et al. (2005) and the accompanying perspective piece by Diehl (2005). Some of us believe, like Diehl, that many traits of Mesoamerican civilization crystallized in the Gulf lowlands between about 3150 and 2800 B.P. and that Olmec iconography was a coherent iconographic system; other authors of this paper would state things differently. Some of us would deny an overriding interest in “[knowing] the level of sociopolitical complexity reached by the Olmec” or their contemporaries on the grounds that *explanatory* rather than *interpretive* goals should guide archaeological research. Our diversity makes it difficult for us to respond as a group to Sharer et al.’s discourse on interpretation. The main issues that prompted our joint participation in Neff et al. (2006) were (1) the misrepresentations by Stoltman et al. (2005) and Flannery et al. (2005) of the strong pattern revealed by the INAA data and (2) the inappropriateness in scientific discourse of arguments by framing, such as those used by Flannery et al. (2005).²

While our own diversity of opinion precludes us from addressing many of the issues of “interpretation” raised by Sharer et al., we all agree that misrepresentation of basic facts by Sharer et al. should be noted and corrected. The following paragraphs

correct problems ranging from mistakes about basic chronology to misrepresentation of the archaeological findings at San Lorenzo.

Chronology

Throughout the *PNAS* articles, and now in Sharer et al, the authors confuse the San Lorenzo horizon with the later La Venta horizon. They discuss a pot from Tlapacoya (Sharer et al.2006:Figure 3) designated “Paloma Negative painted” that they claim shows “Olmec-style” designs not made at San Lorenzo. In fact, however, the design on the vessel aligns it with incised designs on portable carved objects of the La Venta horizon, so the vessel probably postdates the decline of San Lorenzo. Also worthy of remark is the fact that the pot they illustrate is in a private collection and thus devoid of the basic context information that they consider vital and that might resolve its questionable chronological placement. Because of the chronological ambiguities, the assertion by Sharer et al. (2006) that “pottery of this type [Paloma Negative] was traded at least as far as the Valley of Oaxaca,” even if true, may have no bearing whatsoever on patterns of ceramic exchange during San Lorenzo’s apogee.

Sharer et al. also criticize us for not considering other widespread media for Formative symbols, such as carved reliefs. They specifically mention the reliefs at Chalcatzingo, which, as they must know, are later than San Lorenzo and thus have no relevance to the current discussion. Indeed, they contradict this statement by noting that, since Early Formative sculpture does not appear outside of San Lorenzo, there must be a significant error in our reasoning. An example, however, of monumental sculpture

appears in the Soconusco area, where a fragment of a large Olmec-style sculpture from Alvaro Obregon (4 miles north of Cantón Corralito) has been published (Clark and Pye 2000:Fig.6).

Olmec Style

Sharer et al. claim that Blomster et al. (2005) “stack the deck” by their use of the term Olmec style, but in fact Blomster et al. (2005:1068) carefully distinguished between Olmec and Olmec style, noting that “[n]ot all features referred to as the Olmec style may actually be linked with the archaeological Gulf Coast Olmec.” There are obvious problems with the term Olmec style, but it is used by the majority of scholars in the field; suggested replacements such as “X-Complex” (Grove 1989), or pan-Mesoamerican (Flannery and Marcus 1994), while well-intentioned, never caught on, and there seems to be no interest in a revival at this date.

One could even argue that Flannery and Marcus (1994) are the ones who “stack the deck” by their promiscuous labeling of symbols as “pan-Mesoamerican” (read “Olmec”) style. Thus, when Sharer et al. say that Flannery and Marcus (2000) have already “indicated” that San Lorenzo had a smaller repertoire of these symbols, they are only highlighting the fact that Flannery and Marcus (1994) were overly inclusive in what should fall into this category in the Valley of Oaxaca. A similar problem attends Flannery and Marcus’s (2000) argument that a larger number of pottery types in Oaxaca with Olmec symbols indicates that Oaxaca potters were involved more heavily in creating these symbols. This pattern is merely an artifact of classification: Ceramics from San

Lorenzo excavated by Coe and Diehl (1980) were too eroded to determine surface color, so their classification is very different from that devised by Flannery and Marcus (1994). Comparing how many pottery “types” (a creation of the archaeologist) at San Lorenzo and San José Mogote have Olmec designs is meaningless.

Finally, Sharer et al distort the views of Blomster et al. (2005) regarding the relationship of designs on pottery to political institutions. Blomster et al. (2005) did not claim that the pots themselves somehow disseminated sociopolitical institutions, only that a link between processes that spread Olmec motifs and processes that might have spread political institutions had been suggested and that the motifs may reflect basic concepts of Olmec religion and ideology. Thus, Sharer et al.’s discourse on “reading” Olmec institutions in pottery is pointless, and in one case – the sunburst (Coe and Diehl 1980:Fig. 140f) misidentified as an eagle - is based on an obvious typographical error: the vessel with an eagle is Figure 140g. Coe and Diehl plead guilty to failure to catch the typo 25 years ago and apologize to the ornithologist whose time was wasted by Sharer et al.

Hyperbole and the Nature of Olmec San Lorenzo

Sharer et al. (2006) repeat the claim of the *PNAS* authors (Flannery et al. 2005) that Blomster et al. (2005) are guilty of “hyperbole” in their discussion of the Olmec at San Lorenzo. The charge of hyperbole implies that Blomster et al. (2005) made patently exaggerated claims about the Olmec. While the patterning in the INAA data favors a view that may contrast with that of the *PNAS* authors, we dispute the charge of hyperbole

Sharer et al. suggest that the scant mention of the San Lorenzo Olmec in Neff et al. (2006) implies we have nothing to say about them. Although we do not have a united viewpoint about Olmec complexity or even about what questions might be most important to address, we do have a united viewpoint about representation and misrepresentation of patterning in data. Misrepresentation of the patterning in ceramic data is one thing that concerns us; and we have not “given up” on more “time consuming” research, as Sharer et al. suggest. Thus, we turn to a brief consideration of what the time-consuming fieldwork of the recent past shows about the archaeology of San Lorenzo.

Sharer et al. take issue with the statement in Blomster et al. (2005:1068) that “we simply note the more complex level of sociopolitical organization achieved by the Olmec,” chiding Blomster and his colleagues for not determining which evolutionary category – chiefdom or state – the Olmec best fit. Blomster et al. (2005) were not asking readers to accept greater Olmec complexity as “an article of faith,” as Sharer et al. assert; instead, their statement was an abbreviated but accurate summary of what recent fieldwork shows about the scale of population aggregation and monumental construction at San Lorenzo.

Sharer et al. (2006) and Flannery et al. (2005) selectively deploy San Lorenzo data to downplay its scale and complexity. They prefer Coe and Diehl’s (1980) original estimate of about 53 ha for San Lorenzo to the estimate of 500 ha provided by the current San Lorenzo Tenochtitlán Archaeological Project (SLTAP), directed by Ann Cyphers (Symonds et al 2002). They ignore the most recent SLTAP work in which the 500 ha size is supported by uninterrupted subsurface deposits encountered in transect excavations in

the spaces between what Lunagómez (1995) interpreted as separate or “outlying” sites in the settlement survey reported in his undergraduate thesis. SLTAP researchers have defined six permanent types of sites around San Lorenzo, with an organizational hierarchy of three levels, clearly atypical of Early Formative Mesoamerica. Integration and control was facilitated by placement of secondary centers at river confluences (Symonds et al. 2002:125-126). In Oaxaca, such a complex settlement hierarchy would almost certainly be called a state (Marcus and Flannery 1996).

Flannery et al.’s (2005:11222) second example of “hyperbole” concerns the supposed lack of domestic architecture in the Gulf lowlands, a “modest wattle and daub structure” at La Venta being mentioned as the only “confirmed Olmec residence”. They assume that the SLTAP produced evidence for only one structure at San Lorenzo, referred to as the Red Palace in preliminary reports (Cyphers 1996, 1997). This structure proves especially vexing for Flannery et al. (2005), and they dismiss it as hematite-stained “patch” of sand underlying a basalt column. While they hold out the possibility that it may be a temple, they assume that in order for it to be an elite residence, such as a palace, it should have a floor plan similar to that found over 1,000 years later in the Zapotec center of Monte Albán (Flannery et al. 2005:11222). In their brief dismissal of the Red Palace as an elite residence, they fail to mention the basalt used as step coverings as well as a drain/aqueduct. Additionally, the Red Palace is associated with a sculpture workshop (Cyphers 1997, 2004). Based on these data, the excavator of the Red Palace has referred to it as “clearly elite” (Cyphers 1999:167; Symonds et al. 2002:127). Recently, Cyphers has concluded that the “amorphous” red floor is somewhat larger than a patch, as it is estimated to cover some 400 m², a calculation based on 127 m² of

extensive excavation, trenching and interval tests (Cyphers 2005). In the final report of the SLTAP excavations, as yet unpublished (Cyphers 2006), the evidence indicating its residential function will be provided along with comparative data from another 13 domestic structures that have been excavated.

Flannery et al. (2005:11222) assume that public space at San Lorenzo should be organized in a manner similar to later Central Mexican and Zapotec states. The SLTAP has documented extensive monumental constructions, including large earthen mounds, causeways that served as dikes and docks (one measures 600 m long by 75 m wide), and a buried architectural complex composed of a sunken patio surrounded by low earthen platforms measuring approximately 75 by 75 m (Cyphers 1997:106; Cyphers et al 2006.). Recognizing that monumental activity at San Lorenzo does not necessarily conform to expected patterns, Cyphers (1997:112) characterizes construction as transforming the landscape itself into monumental architecture. Noting that their data do not easily fit the model of a complex chiefdom, SLTAP researchers have concluded that San Lorenzo was an incipient state of modest size and success (Symonds et al. 2002:128). While the authors of the present piece do not necessarily agree about this specific interpretation, we do believe that, like the ceramic INAA results, the massive scale of construction activities at San Lorenzo revealed by recent archaeological fieldwork deserves to be taken seriously in future debates about cultural evolution in Early Formative Mesoamerica.

Finally, we must question the relevance of the comparison by Sharer et al. between Olmec colossal heads and Easter Island *moai*. Following Flannery and Marcus (2000), they note that some *moai* are larger than Olmec heads. First, this comparison is misleading because the lightweight stone used on Easter Island, volcanic tuff and scoria,

is local and reasonably easy to carve with basalt adzes (Van Tilburg 1995, 1996), whereas the dense basalt for colossal heads was non-local and quite difficult to carve. Experimental studies have shown that a relatively small number of individuals could move the *moai*, rather than large groups of organized labour once considered necessary (Love 1990; Van Tilburg 1996). Creation of Olmec colossal heads involved coordination of land- and water-borne transport of dense basalt across some 80 km from the Tuxtla Mountains to San Lorenzo, and subsequent carving, flaking, and grinding of the hard, dense stone.

No one disputes that monumental sculpture (and other art) can be created by people in societies that are organized in different ways. But the relative sizes of *moai* and colossal heads does not undermine the hypothesis that the San Lorenzo Olmec eclipsed contemporary highland groups in organizational complexity. In much the same way, Athens cannot be discounted as a city state because its people carved statues smaller than *moai*. In prehispanic Mesoamerica, monumental art that involves portraiture is often associated with the institution of kingship (among the Classic Maya for example). Thus, in the Mesoamerican context, it is reasonable to view the expensive monumental stone portraiture combined with the monumental scale of construction at San Lorenzo as evidence that the power of certain Gulf lowland individuals or factions surpassed that of contemporary Zapotec leaders at San José Mogote, who could only muster sufficient labor for relatively modest investments in monuments and public buildings.

Conclusion

While we have devoted most of our comments here and in our previous article (Neff et al. 2006) to issues surrounding the provenance investigation of Early Formative ceramics, we recognize that there are larger underlying issues. This is why we summarize some of the recent archaeological findings from San Lorenzo in the previous section. The INAA study shows that the San Lorenzo Olmec invested more heavily than other groups in exporting pottery with “Olmec motifs” to other regions, and the excavations at San Lorenzo appear to document higher population densities, greater investment in monumental art and architecture, and other ways in which San Lorenzo eclipsed contemporary centers in the highlands and lowlands.

The “mother culture” label may be outmoded and not “analytically useful,” as we said before (Neff et al. 2006), but it is certainly possible to compare Early Formative archaeological sites in terms of scale of population aggregation and monumental construction. At least in principle, the hypothesis that San Lorenzo is many times larger and more complexly organized spatially than San Jose Mogote is testable. If this pattern, which appears to hold based on Cyphers’ recent work (see above), bears up under further scrutiny, it casts doubt on the viability of the notion that Early Formative Mesoamerica consisted of roughly equivalent chiefdoms. As scientists, we should welcome this outcome. Science advances when we seek and find evidence that allows us to discard ideas, and it stagnates when we cling to ideas in which we have invested heavily, as if we will be disgraced by evidence that calls them into question. Recent ceramic provenance work (Blomster et al. 2005; Neff et al. 2006) and recent archaeological work in the Olmec heartland (Cyphers 2005; Symonds et al. 2002) call into question some cherished

ideas about the Early Formative, but this only frees us to start exploring new, more productive research questions.

How and why did San Lorenzo and other later, large centers arise? Why were they innovative in comparison to contemporaries? How did their innovations spread internally and to other parts of Mesoamerica? This exchange with Sharer and his colleagues will be useful if it encourages others to take up the challenge of discarding non-viable ideas and defining new and more profitable research questions about the nature of Early Formative Mesoamerica.

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Notes

¹Winter recently reexamined portions of some of the sherds sampled by Sergio Herrera and reported by Blomster et al. (2005:Table S3). Samples SLN251-SLN255, classified in the table as Xochiltepec White, were assigned to White-2. The paste of these samples is white but the sherds are thicker than the Xochiltepec White sherds from Etlatongo and elsewhere. They appear to come from bowls that are similar to local Oaxacan grayware forms. All five sherds are from Barrio del Rosario Huitzo and are probably Guadalupe phase or Rosario phase, that is, Middle Formative and later than the time when San Lorenzo imports were reaching the Valley of Oaxaca. While it would be interesting to know where the raw materials come from and where the vessels were made, a Middle Formative chronological placement would mean they have nothing to do with the Early Formative or with exchange during San Lorenzo times. Winter suspects that some other examples sampled by Herrera are later than San Lorenzo horizon. Possible misclassifications such as these would reduce the number of “Olmec” sherds (e.g., Xochiltepec White) attributed to production centers other than San Lorenzo and would further strengthen the conclusions of Blomster et al. (2005).

² Our concern actually goes beyond the use of inappropriate rhetorical tactics in the *PNAS* articles. Framing, distortion, and misrepresentation have long been used by several of Sharer’s coauthors (e.g., Balkansky 1998; Flannery 1997; Flannery and Marcus 1990; Marcus and Flannery 1990) to discredit their opposition. We are not the first to note these tactics (e.g., Coe 1993; Cowgill 1992; Dunnell 1992; Hardy 1999; Joyce et al. 2000).