



# A few kernels short of a cob: on the Staller and Thompson late entry scenario for the introduction of maize into northern South America

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## Abstract

In recent *JAS* articles, Staller and Thompson (*J. Archaeol. Sci.* 29 (2002) 33) and Staller (2002) claim that phytoliths recovered in food residues from the Late Valdivia site of La Emerenciana represent the earliest remains of maize in Ecuador. These authors also dispute the utility of a technique developed by Pearsall and this author which identifies the remains of vegetative structures of maize retrieved from archaeological and paleoecological sediments. Staller and Thompson, in arguing that maize phytoliths occur only in the latest sherds recovered from La Emerenciana, ignore evidence strongly suggesting that maize is also present in the earliest ceramics at the site, despite the very small sample of pottery sherds from this time period that was analyzed. Their criticisms of the corpus of evidence identifying maize use during the late preceramic and early ceramic periods in northern South America, remains of which age they did not study, are based on inaccurate depictions of the methodology and interpretations relating to these accumulated data, and on unsupported assumptions concerning the integrity of this well-dated, multiproxy collection of information. © 2003 Elsevier Science Ltd. All rights reserved.

*Keywords:* *Zea mays*; Ecuador; Phytolith analysis; Multiproxy data collection

## 1. Introduction

A paper published recently in this journal [35], and a response to a rejoinder by Pearsall (2002) by one of the authors of the original report [34] argue for a much later (post-4000 BP) introduction of maize into South America, specifically Ecuador, than is evidenced by phytolith and pollen records from earlier ceramic cultural occupations and lake cores (all dates in this manuscript are in uncorrected <sup>14</sup>C years). Unlike earlier objections from paleoethnobotanists specializing in macro-fossil plant remains who demand the recovery of directly dated macro-fossils as proof of maize use [10,11,31,33], the argument is based on a phytolith analysis of food residues in late Valdivia-period ceramics. In the second edition of his widely read popular summary of agricultural origins, Smith [33, p. 159] refers to the study of Staller and Thompson [35],

then unpublished, accepting it as proof of prehistoric maize occurrence in South America, although macrobotanical remains of maize have not been reported from the site. Smith also used the study to further press his belief that maize arrived in South America sometime after 4000 BP (in his discussion, Smith refers to the site of La Emerenciana, but since there is no archaeological site with that name of any age in Ecuador, I assume he is referring to La Emerenciana).

Thus, for a number of reasons it becomes important to carefully examine this food residue evidence to establish the degree to which it contradicts an earlier entry for maize. A late preceramic (ca. 7000–5000 BP, depending on the region) maize dispersal into southern Central American and northern South America is supported by a substantial body of evidence from phytolith, pollen, and starch grain studies of archaeological and paleoecological sites (reviewed in Refs. [18,25,27,28]), and is accepted by many Latin American scholars [1,3,6,29,30].

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Staller and Thompson [35] also criticize a phytolith method for identification of maize leaf phytoliths developed by the author and Deborah Pearsall. Pearsall (2002) has already replied. Herein, I comment on additional points not specifically addressed by her commentary or subsequently raised by Staller [34] in his reply to her.

## 2. Staller and Thompson's evidence from La Emerenciana for a late introduction of maize

Staller and Thompson [35] studied the phytoliths occurring inside of food residue from a total of two pottery sherds from the initial Valdivia IV–VI (Stratum 3) occupation and seven pottery sherds from the terminal Valdivia (Stratum 5) occupation at the site of La Emerenciana, southern Ecuador. No radiocarbon determination from Stratum 3 was reported; carbon-14 dates from food residue in Stratum 5 pottery sherds and associated contexts were from ca. 3800 to 3400 BP. In some Stratum 5 pottery sherds, Staller and Thompson identified phytoliths that phytolith analysts agree can identify deposition from the chaff and/or cupules of maize cobs [2,15,18,28]. Earlier, Stratum 3 pottery sherds were said to contain no evidence for maize.

The sample of pots examined from Stratum 3 is very small, particularly in light of the fact that cob phytoliths were found in only three of the seven pottery sherds examined from Stratum 5. Nonetheless, as Pearsall (2002, p. 54) also points out, Staller and Thompson isolated an assemblage of 17 grass phytoliths from one of the two Stratum 3 pots that is nearly identical in composition to the assemblage they identified as maize in the Stratum 5 food residues and on human teeth (see Ref. [35, p. 42, Table 6]). They state, however, that in Stratum 3 pot "... grass inflorescence material is not incorporated into the food residues" [35, p. 43]. While the sample size is not as large as from some of the later pottery sherds where maize was identified, an even smaller sample of 12 grass phytoliths isolated from one of the dental calculus samples was interpreted as being consistent with maize presence [35, p. 42]. The food residues in the Stratum 3 ceramic vessel containing the maize-like signature did not have carbon isotope values clearly indicating C<sub>4</sub> grass presence, but neither did the isotope values in residues from most Stratum 5 pots containing maize, probably indicating that C<sub>3</sub> plant foods were cooked in these vessels more so than was maize.

The decision to ignore the phytoliths in the Stratum 3 context, and to claim that the pot contains no grass inflorescence material at all leads to an incongruity of major proportions between the data reported in Table 6 and discussion in the text. This, combined with the fact that materials from earlier ceramic occupations were not

considered by Staller and Thompson, indicate that the study has little relevance to the question of when maize first arrived to Ecuador.

## 3. Staller and Thompson's critique of the Pearsall/Piperno maize identification method

Scholarly disagreement plays an important role in the scientific process, and good debates often lead to the development or refinement of new techniques with which scientists can evaluate questions that fall outside the purview of more faddish types of analyses. A requirement of authentic debate, however, is that the content of the research under critique is discussed and evaluated in the way that it was originally presented to the scientific community. On this point, Staller and Thompson [35] and Staller [34] fall considerably short of minimal scientific standards, as they routinely portray inaccurately or misrepresent published findings by the author, Pearsall, and other scholars relating to the evidence for a pre-4000 BP maize presence in northern South America. Some of these instances have already been noted by Pearsall (2002). I discuss a few others in more detail below.

For example, in regard to the evidence for maize presence in an eastern Ecuadorian lake core, Ayauchi<sup>i</sup>, reported in Ref. [5], Staller [34] states that I relied on the recovery of a single maize phytolith to argue for maize cultivation by ca. 5300 BP. I made no such argument from a single phytolith, stating that the first occurrence of *Zea mays* was demonstrated by "pollen and *phytoliths*' (emphasis mine) of maize [5, p. 304]." In the longer paper dealing with the phytolith record [20], a separate and more detailed graph of the phytolith results showed that maize phytoliths accounted for 3% of the phytolith sum at 5300 BP. Anyone who has worked with phytoliths in such kinds of samples from the tropical forest is aware that such a percentage represents a very significant amount of decay from cross-shaped producing grasses, in this case maize. The sample size of cross-shaped phytoliths from the 5300 BP-dated level that was counted, measured, and identified as maize is 31 (D.R.P. laboratory notes, dated January 4, 1988).

Staller [34] also states that Bush et al. [5] reported and discussed stable carbon isotope data from the Ayauchi<sup>i</sup> sediments, and interpreted them as a changeover from a C<sub>3</sub> to a C<sub>4</sub> dominated ecosystem at about 3500 BP, far later than the earliest pollen and phytolith evidence for maize at the site. We neither reported nor interpreted such data, but rather discussed how a rise of pollen and phytoliths from a variety of early successional woody and herbaceous plants coincided with the earliest appearance of maize [5]. Carbon isotopes from the dated sediment samples were published in the longer account of the pollen record by Bush and Colinvaux [4], showing no trend as described by Staller. The sample that

returned an age of 3310 BP had a  $\delta^{13}\text{C}$  of  $-25.2$ , reflective of contribution from mostly  $\text{C}_3$  taxa, and such  $\text{C}_3$ -dominated values were characteristic of earlier samples below it. Bush and Colinvaux made no attempt to interpret the carbon isotope ratios from Ayauch<sup>1</sup> because ratios in lake sediments are likely to be considerably influenced by changes in lake productivity and a loss or gain of arboreal and herbaceous plants of the lake edge, which may produce either a  $\text{C}_3$ - or  $\text{C}_4$ -trending signal, or no trend at all.

With relation to the Pearsall/Piperno method of maize identification using cross-shaped phytoliths, found mostly in leaves and also in husks, Staller and Thompson [35] and Staller [34] claim that investigators within the phytolith community find replicating our technique to be problematic, by reference to a highly selective citation list that includes a paper by Doolittle and Fredericks [41] and various manuscripts, published and unpublished, by Irwin Rovner. In point of fact, Doolittle and Fredericks did not carry out a study of cross-shaped phytolith size and three-dimensional morphology because they mistakenly concluded that the maize leaves they analyzed did not contain any such phytoliths (see also the discussion in Ref. [21, pp. 428–433]). Examples of research by other investigators considerably more experienced in phytolith studies than were Doolittle and Fredericks at the time of their analysis, and that successfully used the Pearsall/Piperno method to determine whether maize leaf decay occurred (e.g. [15,37]) go unnoted by Staller and Thompson [35]. Other successful applications are reported more recently [12,40].

Staller [34] also claims that Thompson found the Pearsall/Piperno technique to be “unimpressive, unreproducible, and contextually problematic...”, without providing a shred of supporting data. The scientific community cannot be expected to take these kinds of unsubstantiated comments seriously.

I do not have the unpublished Rovner reference that Staller and Thompson [35] and Staller [34] cite with particular relation to phytolith three-dimensional morphology, but Staller [34], who has no experience with analyzing phytoliths or other archaeobotanical remains, does illustrate and discuss two photographs provided to him by Rovner of ‘the same particle’. These purportedly show how rotating a single phytolith artificially creates different types of three-dimensional forms, and thus confuses their identification. They also provide me with an opportunity to address again how three-dimensionality in cross-shaped phytoliths is determined. In many grasses, including wild and domesticated *Zea*, cross-shaped phytolith three-dimensional structure appears to be largely a product of where the phytoliths are formed in plant tissue. In non-bamboos, those located in the epidermal cells that lie over the leaf veins are often predominantly Variant 1 (mirror-image cross-

shape), while those formed in between the leaf veins are often of other types (e.g. Variant 2 = tent-shaped on one face; Variant 5/6 = trapezoidal on one face) (see Ref. [19, pp. 87–88]). It can also be seen upon inspection of phytoliths that they are transparent to translucent, so that regardless of which side is facing up to the investigator through the microscope objective, it is usually possible to see through to the other side and determine which three-dimensional characteristics they possess (see Ref. [21, p. 430, Fig. 21]).

Now, the top phytolith pictured in Ref. [34, Fig. 1] appears to be a bilobate, not a cross-shape, as it has the characteristic two lobes and two indentations that usually define bilobate forms. The rotated, bottom phytolith in Staller’s Fig. 1 has at least three indentations and very differently shaped lobes, and because of phytolith transparency the ‘ridge’ that is obvious on this phytolith should probably also have been apparent in the top photograph. I thus have serious doubts as to whether Fig. 1a and b are photographs of the same phytolith. In any case, since three-dimensional descriptions are based, in part, on how the upper and lower faces of a phytolith are structured and may differ (see also Thompson and Staller, 2002, p. 36, Table 3), Staller’s photographs showing how such characteristics of phytolith faces vary support the utility of this approach.

This brings me to Staller and Thompson’s criticisms of the Pearsall/Piperno three-dimensional criteria, initially developed for leaf and husk phytoliths [19]. Their disapproval is all the more difficult to understand in light of the fact, briefly noted above, that Thompson’s criteria for describing and distinguishing maize cob phytoliths depends heavily on the same kinds of three-dimensional attributes (e.g. differences in shapes and other characteristics of the upper and lower phytolith faces or top and base) [35, p. 37]. In a previous article, it is also stated that Thompson’s typology of cob phytoliths is ‘based on the three-dimensional morphology of opal phytoliths’ [36, p. 8]. Following Rovner, then, these features would also be artifacts.

Finally, a study carried out more than a decade ago [14] of phytolith size in *Arundo donax* (a giant Old World reed grass), said by Staller to invalidate Pearsall’s definition and use of cross-shaped phytoliths archaeologically, does no such thing. Mulholland used an older method for determining cross-shaped phytolith size that placed phytoliths into broad size categories, and no phytoliths as large as those that occur in maize were observed. Extensive studies carried out subsequent to Mulholland’s analysis on related and other grass taxa of the American tropics and subtropics, including bamboos and other grasses mentioned by Staller to be problematic (*Zizaniopsis* and *Leersia*), have shown they produce no cross-shapes, or smaller phytoliths than maize, and/or idiosyncratic

cross-shaped and other phytoliths recognizable by their morphology alone, and that should not be confused with maize in study regions in which they occur [12,17,21,26]).

#### 4. Arguments for and against a pre-4000 BP diffusion of maize into South America

I will not elaborate on Staller's confused discussion of maize evolution (see also Ref [39]), and of the significance of quite possibly being able to distinguish a harder- (i.e. more lignified) and/or larger-glumed *Zea* from a softer-glumed *Zea* in the phytolith record (memo to Staller: I have not argued for two separate domestications for maize, nor for teosinte presence in Panama and *all* varieties of teosinte have hard glumes, so your claim that pre-Guilá Naquitz maize varieties should have soft glumes makes little to no sense) (see Refs. [8,24,25, p. 223–225]). The same holds true for Staller's tangled comments on what actually is a consistent picture, though still yet preliminary, of the age and distribution of possibly harder- and softer-glumed varieties of maize recorded in the phytolith remains of maize cobs from Panama and Ecuador ([25, pp. 223–225, 28; Pearsall, 2002; 18]), and his claims that no maize race is supposed to have phytoliths with mean sizes greater than 15  $\mu\text{m}$  in width (see Ref. [19, pp. 76–77], for data contravening that statement and others concerning phytolith size in maize).

As Staller notes, we have, indeed, revised our chronology for the initial appearance of maize phytoliths at the Vegas type-site OGSE-80, based on carbon-14 dates obtained directly from phytolith assemblages in which maize phytoliths occur, as well as assemblages stratigraphically below them in which maize was not present. An age of  $7170 \pm 60$  BP was obtained on the earliest maize-bearing assemblage. This is why we said: "It is clear that maize is first present at the site shortly before the end of the late Las-Vegas phase occupation, which probably dates between 7000 and 6700 BP after correction for the reservoir effect (determinations on shell from the same stratigraphic cut yielding the maize)" [25, pp. 186–187]. The statements of Staller and Thompson and Staller, made persistently throughout their papers, that an 8000 BP or even mid-eighth millennium age is still ascribed to these remains are wrong.

Staller's and Staller and Thompson's critique of Pearsall/Piperno's and other scholars' data for a 7000 to 5000 BP dispersal of maize into northern South America does little more than belabour unsupported claims originally made by Fritz [10,11] and Smith [31,33] that there are 'contextual' or methodological problems with all of the considerable and well-dated archaeological and paleoecological phytolith, starch grain, and pollen evidence. The historical roots of Fritz and Smith's objections to a pre-4000 BP maize presence south of Mexico

have relevance here, made, as they were, as part of their argument at that time that agriculture did not emerge anywhere in the New World until about 5000 BP (see Ref. [21, pp. 433–435]). A 5000 year-ago start for plant husbandry in the Americas would have brought an independent origin of agriculture in eastern North America, which Fritz and Smith also strongly advocate, and on current evidence starts no earlier than about 4300 BP, much more into line with developments elsewhere in the New World. Agricultural origins in the New World, of course, did not get much younger than was originally believed (e.g. Refs. [27,32]).

Staller believes that the middle sixth millennium BP  $^{14}\text{C}$  dates on two maize cobs from Guilá Naquitz Cave [23] provide a chronology for the age of maize domestication as a whole. These cobs were two of a suite of four recovered from ephemeral occupations of the Cave starting at 6980 years BP that "were too brief to produce actual living floors, features, or well-defined activity areas ..." [23, p. 2102]. Thus Guilá Naquitz Cave was not a center of maize production when those four cobs were deposited, but the possibility is real that even earlier maize-growing than presently evidenced took place in that part of Oaxaca. And the fact of the matter is that we do not know when people in the Central Balsas River Valley, the putative hearth of maize [7,13] first took teosinte under cultivation and domesticated it because the requisite archaeological and paleoecological studies have not yet been carried out in that region of Mexico.

Interestingly, a recent study cited by Staller as supporting his views, estimates a ca. 8000 BP age for maize domestication on the basis of a molecular clock [13]. These molecular data also suggest that the oldest surviving maize is from highland Mexico, despite the fact that maize's wild ancestor is naturally distributed today at lower elevations in the Balsas River Valley. Presently available archaeological sequences from the highlands show no evidence for an exploitation of teosinte prior to the appearance of domesticated maize at Guilá Naquitz, as would be expected if maize was of highland derivation [22,23]. Hence, the oldest surviving maize varieties may inadequately reflect prehistoric maize diversity, or teosinte may have had a different distribution than today during the early Holocene.

These issues will not be resolved until archaeological and paleoecological data from the Balsas River Valley and other lower-lying regions of southern Mexico are available. Future work in the Mexican highlands will add to the seminal research already carried out by Richard MacNeish, Kent Flannery, and associates, and clarify the date of earliest maize use there. It is perhaps worth noting on this point that maize crops grown by rain-fed agriculture fail most years today in the Tehuacan and Oaxaca Valleys [9], and the climate in these areas between 8000 and 7000 radiocarbon years ago may have been even drier than at present.



## 5. Conclusions

The analysis of phytoliths in food residues found in pottery represents a valuable approach in tropical paleoethnobotany, with Staller and Thompson's study further underscoring how poorly macrofossils of maize are preserved in tropical sites, even in later, ceramic-period occupations dating to a few thousand years before the time of Christ. Despite the fact that no maize cobs or grains are reported from La Emerenciana, maize phytoliths were recovered in food residue from pottery sherds, sometimes in high numbers. Staller and Thompson badly overreached, however, in claiming that their phytolith and stable carbon isotope data are relevant to the earliest dispersals of maize into northern South America. More often than we are willing to acknowledge, our intellectual predecessors had it right. There probably *is* a kind of macro-fossil evidence for maize consumption during earlier Valdivia times, in way of maize kernel impressions in Valdivia III pottery (ca. 4300 BP) that were identified by Walton Galinat [38]. Pearsall's phytolith studies at Real Alto provide independent evidence to that effect, and starch grain, pollen, and phytolith studies from well-dated archaeological and paleoecological contexts, now including residues containing maize kernel starches and maize glume/chaff phytoliths from plant grinding stones of preceramic age [27,28], continue to support the original phytolith evidence that maize was introduced in late preceramic times (between ca. 7000 and 5000 BP) into southern Central America and northern South America.

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