Thoughts Towards A User's Guide to Ceramic Assemblages, Part I: Lumping Sites into Mega-assemblages by Those That Cannot Tell Time.

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What constitutes meaningful data from excavated ceramic assemblages and the scrutiny to which it is subjected is a gray area in the archaeological literature. There seems to be an unstated assumption that once an assemblage has been described and quantified, it is as good as the next one as a building block in reconstructing and understanding the past. Quantified assemblages are presented as representing the whole period of a site's occupation with few questions asked in terms of what the assemblage does or does not represent. This paper is meant to raise issues for further discussion that will hopefully lead towards the establishment of some guidelines for the evaluation of ceramic assemblages. This essay will start with what seem to be the most common problems and the unstated assumptions that they represent.

The most common format encountered in dealing with site reports is the phenomenon of lumped assemblages in which all of the ceramics are combined into a single laundry list that is presented as representing the whole period of the site's occupation. This technique, or one should say lack of a technique, no doubt goes back to our origins as prehistorians where the ability to "tell time" is limited compared to what is possible with the chronologies available to historical archaeologists.

Archaeologists have established basic standards for dealing with excavated collections, which, at a minimum level, involve recording the grid and level locations for excavated artifacts. This time-consuming process is one of the things that makes archaeology so expensive. Why bother to expend all the time and energy used to establish and maintain locational and associational data if the artifacts are just going to be lumped together in one assemblage whose only context to be considered is the site? Is there a difference between a lumped collection from a carefully excavated site and a pot hunter's collection where the only provenance is the site? Clearly, the lumping of excavated collections, particularly in sites occupied for long periods of time, is a cop out on one of the main objectives of archaeological research which is the study of changes through time. Instead of dealing with change within sites, much broader lumped periods are being used to study change in which sites are used as the basic building blocks.

There are sites for which lumping is a very appropriate approach, for example, those which were occupied for periods of less than 10 years and have not produced enough artifacts for meaningful breakdowns. Even for sites occupied for short periods of time it is sometimes possible to break the collections into sub-assemblages. For example, Meredith Moodey was able to use locational data in combination with information on cross-mends and ceramic chronology to segregate a plowzone collection into three sub-assemblages representing the sequence of deposition at the Franklin Glass Works, which

was only occupied for eight years. This breakdown establishes a sequence of acquisition for the site's eight short years of occupation which enabled Moodey to examine the impact of economic stress of the failing glass works on ceramics purchases (Moodey 1988). Moodey clearly demonstrated that it is possible to break down a plowzone assemblage from a site only occupied for eight years. That level of control of our data is necessary to deal with questions of acquisition and deposition of ceramics, and of course the site formation process will be better understood by establishing better control of the element of time.

Lumping of archaeological collections carries with it some implied assumptions which need to be examined. Perhaps first and foremost is the assumption that they are representative of what was on the site. Time and again, the ceramics from an excavation are presented without any discussion as to how representative they are of what was on the site. Rarely is there any indication of what percentage of the site was excavated, or an estimate by the person presenting the collection as to what percentage that they think the collection represents of what was deposited on the site. There seems to be an assumption that as long as the number of vessels recovered is fairly large then they are representative of what was on the site. Looking at the distribution of vessels from 19th century sites that have been occupied for a long period of time, one is frequently struck by how skewed the assemblage is in representing only a segment of the time that the site was occupied.

John Otto's *Cannon's Point Plantation, 1794-1860: Living Conditions and Status Patterns in the Old South* illustrates some of the problems in the lumping of ceramic assemblages, the implicit assumptions involved and how they do not hold up to close scrutiny. Otto set out to excavate assemblages from the households of the plantation owner, the overseer, and a typical slave cabin. This he accomplished; however, his control of the data beyond that has several major problems which were compounded by the lumping of his ceramic assemblages and assumptions as to what they represent in time periods.

Consider the main plantation house. A fair amount of historical research established this basic outline of the plantation's history. Cannon's Point Plantation was built by John Couper in 1794 to take advantage of a strong market and high prices for long-staple cotton. Previously he had been a merchant. John was born in 1759, married in 1792, and moved his family to the plantation in 1796. The initial phase of the plantation was very successful, and by 1804 Couper had built two cotton gin houses and owned several hundred slaves. However, there were setbacks. In 1804, a hurricane devastated the cotton crop. Shortly after that the Embargo of 1807 followed by the War of 1812 put further economic strain on the operation which caused Couper to mortgage 290 of his slaves to borrow $100,000. During the War, 60 of his slaves were carried off by the British. The market for long-staple cotton recovered after the War briefly but then fell during the panic of 1819 and remained low through the mid-1820s. In addition, another hurricane devastated the cotton crop in 1824, and in 1825 a crop was lost to caterpillars.

In 1827 John Couper declared bankruptcy as the price of cotton fell and took with it the value of his slaves and land. While Couper lost another plantation and other investments, he came out of his bankruptcy still owning his Cannon's Point plantation and 100 slaves. By 1828, Couper was on the way to recovery, and prices for long-staple cotton rose to a peak in 1837 of between 45 to 50 cents a pound. After the panic of 1837 set in, the prices fell to a low of 18 cents a pound in 1842. John Couper retired to Hopeton Plantation in 1845 at age 86. Five years later John Couper died, and the ownership of Cannon's Point passed to his son James. "From 1845 to 1861 the only year-round white residents of Cannon's Point were the hired overseers who supervised the slave force . . ." (Otto 1984:124). The management of the Cannon's Point Plantation was taken over by John's son, James Couper, who managed it from his residence on another plantation. After 1845, according to Otto's research, the Couper family used Cannon's Point as a summer home, residing there during the malarial season. In 1862, Union Army troops occupied the area. The plantation house was described in 1864 by a Union naval surgeon, who included the following statement:

> In the basement, large quantities of [fossil] bones and minerals of all sizes and kinds are scattered around the floor. Broken furniture, dilapidated paintings, and broken crockery by the boat load are strewn around the rooms (Otto 1984:30).

John Otto pulled together all of the above data, presented it, and then ignored its potential for providing insights on his excavated assemblages. From the historical synopsis, it is clear that John Couper set up his household after marriage in 1792 and moved his family to Cannon's Point in 1794. He appears to have done very well until the hurricane of 1804, the embargo of 1807, and the War of 1812. The fall of cotton prices following the Panic of 1819 and losses of the 1824 crop to another hurricane and the 1825 crop to caterpillars led to a bankruptcy in 1827. In the 1830s the plantation seems to have flourished. One would guess that the ceramics from initial setting up of the household would be quite different from those in use three decades later after the plantation had recovered.

Instead of using this information, Otto presented a lumped assemblage from the excavation of the midden associated with the Couper's plantation kitchen as being representative of the whole 66 years of the plantation's occupation. Several aspects of the ceramic assemblage and its associations indicate that this assemblage probably represents a period after the hurricane of 1824 until the 1860 abandonment of the plantation. One is that almost 80 percent of the 935 nails recovered from the kitchen were machine cut and headed which places them no earlier than the 1820s. John Couper lists several outbuildings and 12 slave cabins being lost in the 1824 hurricane, suggesting the kitchen midden with which the ceramics are associated probably began accumulating in the 1820s after the kitchen was built.

Supporting this supposition is the makeup of the ceramic assemblage. Otto presented the 1242 sherds recovered from the planter's kitchen as representing the whole

period of the Couper family's occupation of the site and came up with a ceramic mean date of 1818 for the assemblage. Otto states that 1818 is a good fit for the site because he feels that represents the main period of the site's occupation which was from 1796, when John Couper moved his family to Cannon's Point, to 1845, after which it was just occupied during the summer season. The median of this period would be 1820.

While the date generated from Stanley South's formula is within a couple of years of the median of peak occupation period, it appears to be far too early for the ceramic assemblage. Just over 2 percent of the sherds from the Planter's Kitchen were creamware, which seems far too low for an assemblage that began accumulating in the 1790s. Pearlware, on the other hand, made up almost 86 percent of the ceramics, while whiteware only accounts for 4.5 percent of the sherds. Those proportions suggest a site that was occupied for a short period in the early 1820s. A more plausible explanation is that Otto's definition of pearlware was too broad and that any trace of blue in the foot ring would have cast the vessels into the pearlware category. When dealing with blue printed wares this can be a problem as the definition of what constitutes pearlware is an arbitrary one. Given that whiteware began showing up in American assemblages shortly after the War of 1812 and had become very common by the 1830s, particularly with the growing popularity of red, green, brown, and purple printed wares during that decade, one would expect a much higher proportion of whiteware in the Planter's Kitchen assemblage. Unfortunately, Otto does not mention the colors of the printed wares he is dealing with.

The Planter's Kitchen assemblage is presented as one lumped context. However, in Otto's earlier article in Stanley South's *Research Strategies in Historical Archaeology*, Otto presented the kitchen assemblages broken down into "zones" as shown in Table 1.

These zones appear to have integrity as a meaningful time sequence. In this table, the whiteware category includes the decorated as well as undecorated whitewares, whereas the printed category are those listed as printed pearlwares by Otto. Assuming that these "zones" represent layers, the TPQ artifact in layer III would be the white granite wares, suggesting that the level was accumulating material until at least the mid-1840s. Level II therefore must postdate ca. 1845, which would have been a period of just summertime occupation of the plantation. Given that sequence of events, I doubt that the printed pearlware made up 64 percent of the sherds. There must be more printed whitewares than Otto has identified.

Again, looking at the above levels, they seem to suggest that shell edge may have been the site's earliest tableware, which was probably replaced by printed wares after the War of 1812. However, to confirm that assumption one would need to see the shell edge and printed wares. Why Otto chose to lump these three zones in his book is not clear, nor is there any information presented on what was found in Zone I. On page 66 of his book, Otto discusses changes in the styles of transfer prints which he summarizes as follows. The earlier decades of production were dominated by oriental patterns, which were replaced by English, American, and Near Eastern scenes that remained popular to the 1840s, which were then replaced by floral patterns. Unfortunately, Otto did not use this information to provide his readers insight to the dating of his assemblages that would

have gone beyond the ceramic mean dates that he generated. None of the transfer printed patterns are broken down into pattern styles.

In summary, Otto segmented his historical research from his archaeological analysis and limited the dating of his assemblages to what could be derived from Stanley South's mean ceramic date formula, which for the 19th century does not work very well. Despite the evidence of the predominance of machine cut and headed nails from the kitchen which suggests a building date sometime after the early 1820s, Otto concludes that the ceramics associated with the kitchen's midden represent the whole period of the site's occupation by the Couper family, i.e., from 1794 to 1860. From the minimal information presented in the book, I would estimate that the assemblage represents a collection generated from the mid-1820s to the early 1840s. One of the variables that Otto claims to have under control is the element of time. Lumping is not a way to control the element of time as it masks changes that took place, which is one of the major subjects of archaeological inquiry.

Even if Otto's assemblage was representative of the whole period of the occupation, how meaningful would it be? Let's assume that the ceramic assemblages from slave cabin and the overseer's house were also representative of the same 66 year period of occupation. What would the samples tell us? The period from the 1790s to the Civil War saw a major decline in English ceramic prices (Miller 1991:1-4). Ceramics have a flexible demand curve, which means as they got cheaper people adjusted their consumption patterns accordingly. This is seen in two ways: one was that more ceramics were consumed, and the second was that more decorated ceramics were used as they became cheaper. The 1790s was a period still dominated by plain creamware with some shell edged tableware and painted teas.

After the War of 1812, plain creamware began to be replaced by decorated wares, including shell edge, dipt, painted, and printed wares. In addition to decoration becoming more common, the variety of forms and quantity of ceramics being purchased and used in households increased. When one lumps ceramics from such a long period as the first half of the 19th century, it would be very difficult to come to meaningful conclusions about the differences in consumption patterns. The one place where Otto's argument holds up is in the area of vessel forms, i.e., bowls versus flatware. These differences can be seen through time. In short, lumping obscures the process of change that we are trying to observe as archaeologists.

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Thoughts Towards a User's Guide to Ceramic Assemblages, Part II: What Does This Assemblage Represent?

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In Part I of this series on evaluating archaeological collections, the problem of lumping site collections into mega-assemblages was discussed in light of what they may or may not represent. The issue of what an assemblage represents should be one of the starting points in the analysis process: i.e., what am I looking at and what does it mean?

If someone presented you with a list of ceramics which they said are a sample pulled from a 19th-century account book, that would immediately generate a series of questions. Among those questions would be how was the sample selected, what percentage does it represent of the whole account book, and what time periods are represented? Beyond that, is the sample representative of what is in the account book? However, when presented an assemblage of vessels from a site, rarely does anyone ask these very basic questions. Through a leap of faith, most archaeologists assume that an excavated sample, if it is "large" enough, is representative of the archaeological population. Archaeologists have varying definitions for what constitutes a "large" sample, which can range from 100 sherds to a minimal vessel count of at least 50 vessels.

As was demonstrated in the discussion of Otto's Cannon's Point site, excavated assemblages may or may not be representative of the whole period of a site's occupation. This is particularly true with sites which have long occupation periods (Miller 1991). How does one begin to go about assessing an assemblage, and what are the questions to be addressed? One way to think about this question is to consider the information one would like to have available in an ideal world. Clearly, it would be great to know what percentage of the archaeological population had been recovered. Second, is the collection representative of the whole occupation period of the site? Third, what were the major changes during the site's occupation that impacted what was used, broken, lost, and

discarded there? This list could be expanded considerably; however, these questions will be enough to occupy our time for the present. While the above questions cannot be answered, consideration of them will bring to mind some of the types of data that can be assembled in a consistent manner that would be useful for comparison and examination of assemblages.

The question of what percentage of the archaeological population has been recovered is a difficult one to answer. A simple model for estimating the size of the vessel population on isolated rural sites has been published in *Historical Archaeology* (Miller and Moodey 1986). This model is based on one developed by biologists for estimating the number of fish in a pond through capture and recapture of a tagged population. For urban sites with dumped-in fill and organized garbage removal, establishing an estimate of the size of the vessel population becomes much more complex and may be impossible to answer. Given the problems associated with urban sites, this discussion will be limited to rural sites where there are limited amounts of intrusive materials.

What types of information can help in gaining a handle on how representative an excavated sample is from a site? Often site reports contain information that is ignored when assemblages from sites are brought together for comparison. For example, a simple estimate of what percentage of the site was excavated would be a starting point. While it is true that different parts of a site have different intensities of use, archaeologists have a tendency to excavate the more intensely used areas such as around structures and where there are concentrations of artifacts. In other words, if a report provides an estimate that roughly a fourth of the site was excavated, then there is a good chance that probably more than 25 percent of the ceramics were recovered.

This rough estimate, of course, can be influenced by the way in which the site was excavated. In plowed shallow sites such as 17th century post-in-the-ground structures of the Chesapeake or log cabins without minimal foundations, most of the artifacts will be found in the plowzone. If the archaeologists have bulldozed the plowzone to get down to features and postholes, most of the collection will be lost from ever being recovered. In sites where the whole plowzone has been bladed off into oblivion to expose all of the features, the resulting sample will clearly be a fairly low percentage of the archaeological population.

The house area of the Franklin Glass Works site in Portage County, Ohio, can shed some light on what can be lost if the site has been bulldozed. A total of 1330 square feet of domestic area of the site was hand excavated and screened. This area represented between 15 and 25 percent of the house area of the site (Miller and Moodey 1986:61). Six small trash pits were exposed below the plow zone. Sherds to a minimum of 141 vessels were recovered from the plowzone and trash pits. All of these vessels had sherds from the plowzone, whereas only 28 percent of them had sherds from the features. In other words, bulldozing would have blown away 72 percent of the vessels and greatly limited what could be done with the recovered sample. Using the biologists' model for

estimating population size, it was possible to generate an estimate of between 144 and 152 vessels in the archaeological population for the house area. This suggests that the excavated sample represents between 93 and 98 percent of the archaeological population (Miller and Moodey 1986:62). While there can be some doubt as to how well the formula borrowed from biology works on archaeological populations, it still provides a starting point towards understanding what one is looking at and a better handle on quantification of data. Quantification in archaeology and history seems to have different meanings. In history they are generally dealing with known quantities, whereas in archaeology we are more often dealing with ratios and samples from populations where the size is not known.

Unfortunately, the information presented with minimal vessel counts rarely includes data on how much of the site was excavated, whether or not the site was hand excavated and screened or bulldozed down to the features. All of these factors clearly affect what was recovered, and how well that sample represents the archaeological population of the site. When extracting data on minimal vessel counts, one should make an effort to gather the information on how the site was excavated, and what percentage of the occupation area was excavated. Beyond these simple considerations, one can begin to look at the minimal vessel counts in relationship to the number of years that a site was occupied as a rough gauge of the significance of the sample. For example, the Franklin Glass Works was occupied from 1824 to 1832, after which the site reverted to agricultural land. The data from this site can be summarized as shown in Table 1.

This simple summary provides the reader some usable information about the quality of the data from the site and can be used to gauge this site against others in terms of how completed they appear to be. Let's now look at Table 2, which contains similar data recently published in Anne Yentsch's excellent article on "Minimal Vessel Lists as Evidence of Change in Folk and Courtly Traditions of Food Use" (1990).

Arranging the information in this format begins to suggest that some assemblages, i.e., those with a higher ratio of vessels to years of occupation, are more likely to be representative of what was discarded, abandoned, or lost on a given site. Again, one would like to have an estimate of what percentage of the site was excavated or, failing that, the types of deposits or at least the amount of square footage excavated. Some of the above sites, such as Pettus and Utopia, were partially destroyed by bulldozing away their plowzone layers to expose the features while others, such as the Van Sweringen sites, had all levels excavated and screened. Clearly the proportion of the population recovered in the latter sites would be much greater than that of the two bulldozed sites.

It is well known that the level of ceramic usage increased as ceramics became cheaper and replaced treen and pewter wares (Martin 1989). Therefore, one would expect more ceramics from 19th century sites than from 17th century sites. It is not that simple, however, because there will be a difference in ceramic holdings related to wealth of the sites' occupants. Pettus and Utopia would be a case in point. Both sites were from Kingsmill Plantation, occupied for roughly the same period of time, and excavated by

Bill Kelso (1984). However, Pettus was a house of a fairly well-off planter family, while Utopia appears to have been a tenant house which was only a fifth the size of Pettus. The difference in the quantities of ceramics could be reflecting either the socioeconomic differences or the differential proportion of the archaeological populations recovered from each site.

If it was known what percentage of the archaeological population had been recovered from each site, then one would know if the differences in ratio of vessels to years of occupation reflect different quantities in each household or a difference in the recovery of the artifacts. Fortunately, Henry Miller has analyzed the food bone from these sites to provide an estimate of the amount of meat they represent (Miller 1979). His meat estimates suggest that the sites may be very comparable in terms of the amount of population that was recovered. Consider the comparisons shown in Table 3.

Given that the house at Utopia was about a fifth the size of the house at Pettus and would have been occupied by a smaller household, that family probably consumed less meat. If that is the case, then the sample from the tenant site, Utopia, probably represents a greater proportion of the archaeological population than was recovered from Pettus, which has a much larger vessel population. While food bone or, more accurately, the meat they represent seems to work for this situation, it clearly is not a good solution because bone preservation varies considerably from area to area.

While the meat estimates suggest that the Utopia sample probably is better than the Pettus sample, there could still be a large time distortion when one is dealing with sites occupied for 60 years. Consider the data from Fraser Neiman's excavations of Clifts Plantation. The site was occupied from 1670 to ca. 1730, which is 60 years. From that site there were a minimum of 321 vessels, which works out to 5.4 vessels per year of occupation. However, Fraser was able to separate four distinct components of the site which had occupation periods ranging from 10 to 20 years. If these units had been lumped into a single mega-assemblage, the distribution would have been as in Table 4.

From the Table 4 data, it can be clearly seen that the Clifts' site assemblages are skewed towards the last 10 years of occupation, which accounts for 57 percent of the vessels recovered. Given the level of skewing that can take place, one would be leery of the data from Pettus and Utopia. The Pettus site has some documentation related to a change in generational occupation of the site. Perhaps with further work on the collections, the vessels could be separated into generational components.

The objective of this discussion has been to suggest some simple questions that can be asked of archaeological assemblages which will help researchers sort out assemblages according to their potential for comparative research. Further, this discussion is a call for data to be included on what an archaeological assemblage represents in terms of an estimate of how much of the site was excavated, how it was excavated, and other factors which can lead towards a better understanding of our data. I

would appreciate comments on these thoughts and suggestions for other ways in which the archaeological data base can be improved.

Table 1 & 2 combined

Site	Dates	Years	Vessels	Vessels/Year
Franklin Glass Works House	1824-1832	8	141	17.6
The Maine	1618-1626	8	88	11.0
Pasbebay Tenement	1625-1650	25	18	0.7
Kingsmill Tenement	1625-1650	25	78	3.2
Pettus	1640-1700	60	335	5.6
Utopia	1640-1700	60	55	0.9
Clifts I	1670-1685	15	34	2.3
Van Sweringen I	1672-1700	18	64	3.6
Gov. Drummond II	1680-1710	30	102	3.4
Clifts II (tenant)	1685-1705	20	32	1.6
Van Sweringen II	1700-1720	20	58	2.9
Clifts III	1705-1720	15	70	4.7
Clifts IV	1720-1730	10	185	18.5
Van Sweringen III (tenant arm)	1720-1745	25	84	3.4
John Hicks	1721-1740	19	263	13.8
Gov. Calvert	1728-1735	7	148	21.1
Wellfleet (C-9)	1690-1740	50	236	4.8
John Howland	1710-1730	20	136	6.8

Table 3

Site	Vessels	Vessels/Year	Bone	Animals	Meat
Pettus	335	5.6	707	50	7,121
Utopia	55	0.9	994	71	7,973

Table 4

Site	Dates	Years	Vessels	Vessels / Years
Clifts I-IV	1670-1730	60	321	5.4
Clifts I	1670-1685	15	34	2.3
Clifts II (tenant)	1685-1705	20	32	1.6
Clifts III	1705-1720	15	70	4.7
Clifts IV	1720-1730	10	185	18.5

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Thoughts Towards a User's Guide to Ceramic Assemblages, Part III: Breaking Archaeological Assemblages into Functional Groups.

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Response to Part II

Part II of this series on "Thoughts Towards a User's Guide to Ceramic Assemblages" drew a long letter of response and just criticism from Bill Kelso. I had hoped that Bill would send his letter stating his views on the subject to the [Council for Northeast Historical Archaeology] *Newsletter* to add to the discussion of ways in which we present data and excavate sites. I shall take the liberty of presenting this summary of some of Bill's comments.

Kelso's comments on the way in which I presented my case for the importance of the plow zone were right on target. Clearly, in the real world, the excavation of a site represents a contest between limited funding and available time versus the compromises made in getting the most information from a site given those constraints. My bemoaning what is lost when the plow zone is "bladed away" is not a solution to the problem of the information loss and the compromises that have to be made, particularly in environmental archaeology. Bill also objected to my use of the terms "bulldozed" and "blown away." These terms are value laden and were not necessary to the discussion of the need to have

a description of what an excavated sample represents. I thank Bill for pointing out my use of these value-laden terms.

Another point that Kelso raised was that plow zone from the Pettis site was screened, but off site. The site was to be graded down 20 feet to make a marina and time was limited, so the plow zone, which was "loaded" with artifacts, was trucked back to the lab yard (16 truckloads), where it was screened over the next four years. Therefore, the recovered sample from that site was a very high proportion of the archaeological population.

My use of Pettis and Utopia collections for a comparison of two assemblages was not aimed at making a comment on the quality of the archaeology. I merely wanted to suggest how other artifact categories such as food bone can sometimes shed some light on the ceramic samples in terms of how well they represent the population from which they were recovered. I hope that Bill Kelso and others will respond in letters to the CNEHA [and this] *Newsletter* because we need open discussions of the issues involving archaeological research. Before leaving this topic, I would like to restate that the main thrust of Part II of the "Thoughts Towards a User's Guide to Ceramic Assemblages" was to encourage archaeologists and others using their data to provide much more information on just what their excavated samples represent and to provide their readers with some suggestive estimate of what proportion of the archaeological population was recovered.

Before going on with the discussion of use of ceramic assemblages, I would like to comment on Edward F. Heite's letter in the April issue of the [CNEHA] Newsletter. Mr. Heite's letter was in response to Julia King's review of the COVA Symposium on Historical Archaeology in which she stated that "Chesapeake historical archaeologists are adopting more mainstream methodologies, including the standard use of screens and the recovery of data from plow zone contexts."

Mr. Heite took a strong stand against screening which he supported with a quote from Ivor Noel Hume published in 1969 on the loss of artifacts and information by relying on the screen. It should be pointed out that Colonial Williamsburg's Department of Archaeological Research has made the screen a standard tool of their excavations since Marley Brown came to Williamsburg in 1982. I have had eight years of looking at assemblages in the Colonial Williamsburg collections from both screened and unscreened contexts, and it is my impression that the screened contexts have higher counts of small finds such as straight pins, small bone fragments, and mini-sherds. In the field, I have seen the finely troweled soil taken to the screen, which resulted in the recovery of additional artifacts. I would add that this impression has not been put to any tests because few of us working in Williamsburg questioned the value of screening. Given that Julia King's above statement also mentions the recovery of data from the plow zone, one wonders if Mr. Heite trowels the plow zone of the sites he excavates. The screen is a tool, just as is the gradall, and all have their place in the excavation of sites. We may have different opinions on when and how to use them, but to suggest that screens are not an appropriate tool for historical archaeology is going a bit far.

Breaking Archaeological Assemblages into Functional Groups

The analysis of ceramic assemblages has come a long way in the last couple of decades. Early site reports often included doll parts, marbles, and floor tiles in their listing of ceramic counts, which were duly calculated into the percentage tables of recovered ceramics. We are still looking for meaningful functional breakdowns for the analysis of archaeological assemblages. Anne Yentsch, in an article (*Historical Archaeology* 1990) titled "Minimal Vessel Lists as Evidence of Change in Folk and Courtly Traditions of Food Use," provided a useful breakdown for food-related vessels into the following classes: 1) food preparation and storage, 2) kitchen, and food consumption, 3) food distribution, 4) beverage distribution, and 5) beverage consumption. Organizing assemblages along these lines will begin to bring order to the data that will greatly facilitate comparison among collections. Yentsch's breakdown covers most of the food-related types of vessels typically found in archaeological assemblages and appears to work very well for assemblages from the 17th and 18th centuries.

I have been using a simpler functional breakdown for ceramic assemblages in an attempt to establish the "market basket" of ceramics typically available in country stores for the period 1780 to 1880. The market basket research grew out of a project titled "English Ceramics in America, 1760 to 1860: Marketing, Prices and Availability" that was funded by the National Endowment for the Humanities (RO-21158-86) [published in 1994 as "Changing Consumption Patterns: English Ceramics and the American Market from 1770 to 1840" by George L. Miller, Ann Smart Martin, and Nancy S. Dickinson, pp. 219-248, in *Everyday Life in the Early Republic*, edited by Catherine E. Hutchins, Henry Francis du Pont Winterthur Museum, Winterthur, Delaware]. Additional funding for this research was provided by two fellowships to Winterthur Museum, which has an incredible collection of invoices, account books, and price lists. The "market basket" is being generated from invoices for ceramics imported to the American market and invoices for ceramics purchased by country merchants. Economists use the "market basket" concept as a tool in establishing the consumer price index. The "market basket" represents the best estimate of what the American consumers were purchasing at a given point in time. For the ceramic "market basket," I have used invoices to establish the makeup of wares. These wares have been broken down into four basic functional groups, which are 1) tableware, 2) tea ware, 3) kitchen ware, and 4) toilet ware. Most of the available invoices were limited to refined English or American wares and did not include the coarse wares such as would have been used in storage and dairying. The classification system being used is the one used by the Staffordshire potters in their price-fixing lists, and the categories hold together very well when compared over a long period of time. There are some choices which some would question. For example, the potters had a grouping titled "Mugs and Jugs ware," which included bowls. In this classification, jugs are pitchers. The English still call pitchers "jugs." These wares all came in dipt decorations such as mocha, common cable, annular bands, and other types of decoration which are rarely found on table or tea wares. Therefore, bowls in this classification,

unless they are clearly identified as tea wares such as slop bowls, have been classified as kitchen ware. Bakers and nappies, on the other hand, have been classified as tableware because they have been classified as such by the potters. Clearly, some bakers would have been used to prepare food in the kitchen, just as some bowls would have been used to serve food at the table. No classification system is perfect, and one is reminded of recorded incidents where chamber pots have been used as soup tureens. Table 1 presents the percentage of these functional types for invoices dating from 1783 to 1858.

Table 1
Percentages of Vessels by Major Functional Groups

Year	Vessels	Tea	Table	Kitchen	Toilet
1783	5,058	68.0	22.1	8.5	1.4
1806	39,018	52.8	27.0	18.6	1.6
1824	1,475	60.2	30.0	9.8	--
1825	893	57.4	35.5	5.7	1.3
1826	2,244	59.1	25.7	13.5	1.7
1827	2,715	53.9	34.0	10.7	1.3
1828	1,917	55.8	26.7	16.9	0.6
1829	1,371	58.6	31.1	9.4	0.9
1830	564	62.2	34.0	1.6	2.1
1856	2,463	63.5	26.7	6.0	3.8
1857	4,344	54.8	33.5	7.7	4.0
1858	6,725	53.1	38.3	6.1	2.4

From the distributions shown in Table 1, it can be seen that there was a high level of consistency among these groups from 1783 to 1858. Over half of the vessels in all of the above assemblages was tea ware. Tableware seems to gradually increase through this period, which probably is a reflection of the declining use of pewter. The surprising category is toilet ware, which seems rather low. This may be because a large number of chamber pots would have been made in red wares and salt-glazed stonewares. Toilet wares were on the increase by the 1850s, which seems to be related to the introduction of white granite wares.

Most Common Types of Vessels

Some archaeologists feel intimidated in their identification of vessels because of the great variety that were produced. For example, the 1789 Whitehead pattern book lists 57 different types of vessels, while the 1814 Leeds pattern book lists 86 different vessel forms. The number of available forms was expanding as ceramic prices dropped during the first half of the 19th century. For example, the 1796 Staffordshire potters' price-fixing agreement only lists 35 vessel types. The potters had expanded the types of vessels available to 93 types by the 1846 price-fixing list. This seems like a lot of forms

to attempt to identify; however, most of these vessel types are rarely found listed in invoices or in archaeological assemblages.

Fortunately, over 90% of the vessels in our assemblages can be accounted for by just 16 types of vessels. Table 2 lists these forms and the percentages of the vessels that they account for in five assemblages from 1783 to 1858. These are simple forms that most archaeologists can identify from small sherds. If you are willing to break down your assemblages into these basic simple vessels, then you will have greater interplay between documentary and archaeological assemblages.

Table 2
Forms and Percentages of Vessels for Six Assemblages, 1783-1858

		1783	1806	1826	1828	1856	1858
TEA WARE	**Cups & Saucers**	42.7	41.7	52.9	52.6	57.5	48.2
	Coffees	17.1	--	--	--	2.9	0.7
	Bowls & Saucers	--	6.2	--	--	--	--
	Teapots	4.3	3.2	3.5	0.8	3.2	0.6
TABLE WARE	**Platters**	1.4	0.8	2.7	1.0	1.0	2.4
	Plates & Twifflers	14.2	16.9	13.9	7.5	14.1	14.6
	Muffins	--	7.4	7.1	16.3	10.2	17.8
	Bakers & Nappies	--	0.5	1.6	0.6	1.0	1.8
KITCHEN WARE	**Bowls**	5.7	14.2	11.1	13.8	3.9	3.4
	Mugs	2.8	1.8	--	1.3	0.7	0.4
	Pitchers	--	2.0	2.3	1.9	1.3	2.2
TOILET WARE	**Chambers**	1.4	1.0	1.1	0.6	0.6	0.7
	Ewers & Basins	--	0.7	0.6	--	3.1	1.3
	TOTALS	89.6	96.4	96.8	96.4	99.5	94.1
	Other types of vessels	10.4	3.65	3.2	3.6	0.5	5.9

The information presented here is from the project on establishing the "market basket" of ceramics available in country stores from the 1780s on to 1900. If you have any invoices for ceramics that you would care to share, I would appreciate having a xerox copy. I will be more than willing to pay the cost of reproduction. Once again, responses to this column are encouraged and welcomed.

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Thoughts Towards a User's Guide to Ceramic Assemblages, Part IV: Some Thoughts on Classification of White Earthenwares

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Classification of English white-bodied refined wares has presented a problem for archaeologists. Most classification systems for these wares have been based on visible traits, such as cream color or blue tint to the glaze, or vitrification of the body. Classic definitions for cream, pearl, white, and white granite wares are static and oversimplified. All of these wares evolved during their periods of production, and there was a great deal of variety within the types due to the fact that there were well over a hundred potters producing them. William Evans (1970) published a collection of formulas for bodies and glazes from a number of potters in 1846 which illustrates the great diversity of formulas used by the various potters for these wares and their glazes. When attempting to define the refined white earthenwares by one or two simple traits, one limits our understanding of the evolution of those wares. In the case of the distinction between whiteware and white granite, the resulting identification will be inconsistent with the way the potters classified and priced their products. Unless the changes in these wares are taken into consideration, the resulting classification will be inconsistent with the classification system used by the potters who made them and the merchants who sold them. In short, if our classification does not match that of the potters and merchants selling the wares, it will not be possible to scale them for the study of purchase patterns.

Static definitions of cream, pearl, white, and white granite wares are useful in establishing chronological control because the types can be associated with limited time periods. Unfortunately, the wares themselves were not static. They evolved as tastes changed, and as a result of competition between the potters to produce cheaper wares. For example, creamware became lighter through time. These changes mean the creamware of the 1760s is different from the CC ware of the 1780s and different still from the CC ware of the 1820s and 1860s. White granite, which evolved from the stone chinas and ironstone in the early 1840s, was almost always vitrified in the early period of production. By the 1870s, however, much of it was fired below the temperature necessary to produce a vitrified ware. If one defines white granite on the basis of vitrification, then much of what the potters and merchants would have called white granite in the post-1870 period would be classified as a white ware. This becomes a problem when one is trying to establish the expenditure patterns represented by an archaeological assemblage. For the study of the wares in terms of their cost, it is essential that the classification of the vessels be as consistent as possible with that used by the potters and merchants dealing in the wares. This paper is an attempt to provide some insights on the classification of these wares as they evolved at different points in time.

Thoughts on the Relationship Among the White Earthenwares, Chinese Porcelain, and Bone China

Creamware represented a major change in direction for the Staffordshire pottery industry away from white salt-glazed stoneware and the quest for the secret of porcelain. When Wedgwood perfected his version of creamware, he and his partner Thomas

Bentley went on to market it by selling sets to Queen Charlotte of England and Catherine the Great of Russia. Wedgwood and Bentley were able to promote creamware to the point where it could compete with porcelain in status. This was a major breakthrough for the Staffordshire ceramic industry, and the production of creamware expanded tremendously in the 1760s and 1770s. Popularity, however, does not last forever. As the public grew tired of creamware, the other potters began to experiment to find a product to catch the consumers' attention. The setback that the English porcelain industry had suffered due to the demand created for creamware began to ease, and the porcelain industry again began to advance.

In 1768 William Cookworthy took out a patent for producing a Chinese-style hard-paste porcelain using kaolin and china stone from Cornwall, England. Richard Champion was able to renew Cookworthy's patent in 1774; however, the use of the kaolin and china stone was available to others so long as they did not produce porcelain (Hughes 1960:110). Having the materials to produce porcelain, but not being able to produce it because of Champion's patent, some of the Staffordshire potters began to produce a ware that they called "China glaze" by at least 1775. This is the origin of pearlware (Miller 1987). China glaze can be distinguished from pearlware and provide archaeologists with a meaningful chronological indicator. As a ware type, it would have the following characteristics: 1) A blue-tinted glaze that gave the whole vessel a bluish tint in imitation of Chinese porcelain; 2) Blue painted and printed patterns in a Chinese-style pattern; and 3) Some of the vessel forms would be in a Chinese style, such as the handleless Chinese tea bowl shape for cups and undercut footrings on plates.

As a group, these traits cluster between ca. 1775 and ca. 1812. They seem to be pretty much out of style by the end of the War of 1812. The important thing to keep in mind here is that the bluing was added to copy Chinese porcelain in a product that was called China glaze.

Josiah Wedgwood was under pressure from his partner Thomas Bentley to produce something along the lines of China glaze in the late 1770s (Miller 1987). He referred to his new product as "Pearl White" and referred to it as a whiteware. Wedgwood used the cobalt to make his ware white in appearance, not to make it look like Chinese porcelain. His term never really caught on, and later scholars changed it to pearlware. If we consider the floral painted patterns on vessels in which the bluing can only be seen in the footring as pearlware, then we have a ware that would date from ca. 1780 to ca. 1830. Pearlware starts showing up in underglaze colors such as mustard yellow, olive green, brown, and blue around 1795, according to Ivor Noel Hume. These polychrome painted wares are rarely in Chinese-style patterns (Reimer 1991).

For the most part, these wares will show up on American sites following the Revolutionary War. The exception would be in areas that were occupied by the British. Pearl white went through its own evolution at the Wedgwood factory, where there were at least six formulas for the pearlware body from 1815 to 1846 (Delhome 1977). Pearlware production at the Wedgwood plant continued up into the 20th century. The

later pearlware, however, does not have any blue tint so would be classified as a whiteware by historical archaeologists. Llewellynn Jewitt described Wedgwood's pearlware of 1865 as being "not a pearl of great price, but one for ordinary use and of moderate cost" (Godden 1969:396). Some post-1860 Wedgwood pieces are impressed with the "Pearl" as part of the maker's mark. Those that I have seen would be classified as whiteware by archaeologists, which points out the problems in our classification system. Archaeologists have a definition for ware types that is related to a period of time, which is fine. However, there is another classification used by the potters and merchants, which evolves. That needs to be kept in mind when working with prices and consumer behavior.

Whiteware has been a major problem for historical archaeologists. Its origins are poorly understood, and until recently very little documentation had been published on its development. We have been using 1820 as the introduction date for whiteware, which is the date that Ivor Noel Hume (1970:130) estimates that pearlware was being replaced by whiteware. In a recent article John des Fontaines (1990:4) documented the production of whiteware by the Wedgwood factory by early 1805. Like China glaze, whiteware appears to have been developed as a copy of porcelain. Josiah Spode's success with his bone china appears to have been the impetus for change. Spode's bone china fires very white, and its popularity led the earthenware potters to move towards a whiter-looking ware (des Fontaines 1990:7; Miller 1980:17).

There were different ways of producing a whiteware. Given that there were over 100 potters in Staffordshire, it is not surprising that there were different approaches taken to the problem. The simplest solution was to cut back on the amount of the cobalt used so that it just countered any yellow tint in the glaze, but did not create a blue tint to the ware. We have all seen wares that are white except for a very light blue tint in the glaze gathered around the footring. Many people have classified these as pearlware. We need to keep the intent of the potter in mind. If it was to produce a whiteware, then the vessel should not be classified as pearlware because of a small amount of cobalt used to achieve a white appearance. White wares with a small amount of blue in the footring area probably show up on American sites as early as the end of the War of 1812 and seem to last into the 1840s.

White wares without any indication of the presence of cobalt were also probably showing up on American sites after the War of 1812, and they are still in production. Definition of these wares becomes very complicated because pearlware and CC wares appear to merge together. For example, Wedgwood's wares of the 1840 to 1860 period bearing the impressed mark "PEARL" do not have any indication of the use of cobalt (des Fontaines 1990:6).

Use of the terms China glaze, pearlware, and whiteware are very rare in Staffordshire potters' price-fixing lists and invoices from 1780 through the 19th century. The only undecorated wares that I have seen listed in invoices for wares sent to the American market prior to the early 1840s are CC ware. Clearly, all of the China glaze,

pearlware, and whitewares for this period were decorated and would have been classified by their type of decoration, e.g., edged, dipt, painted, or printed. In invoices for the period between 1824 and 1858, CC ware ranged from between 5% and 13% of wares sold to country stores (Miller 1990). CC ware is the potters' shorthand for cream color or creamware. Again, our definition of what is creamware falls short of what creamware became following the 1820s. It is very white and clearly is being classified as a whiteware by most historical archaeologists. Given that undecorated China glaze, pearlware, and whiteware are not in the potters' price lists or invoices, the plain undecorated vessels that we recover from contexts dating before the early 1840s are most certainly CC ware. Here again, we have two typologies. These vessels are whiteware by our chronological typology, but CC ware in the potters' terms, and that is how one needs to classify them to work with economic scaling of assemblages.

Early in the 1840s another plain undecorated ware begins to be imported in quantity to the American market. That ware is what most of the potters called white granite ware. It has been called ironstone by most archaeologists. Ironstone is the name that Charles Mason gave to his stone china in his 1813 patent. In a sense, ironstone is a brand name that became generic. The use of the term ironstone and its dates of production have led to some confusion in the dating of late 19th century assemblages. In his article on mean ceramic dating Stanley South lists "Ironstone and Granite China" and gives the dates 1813 to 1900 with a mean date of 1857. Mason's ironstone was but one of several stone chinas that began production around 1800. These wares, like China glaze, were most commonly copies of Chinese porcelain and also had blue-tinted glazes. The stone chinas were rarely undecorated, and they are rather rare on American sites (Miller 1991:9-10). The shift comes in the early 1840s when the potters began producing what they called white granite.

White granite, like the other ware types, went through its own evolution. In the beginning period, the term pearl came back into use in names such as pearl stone china, pearl white ironstone, and pearl white granite. Sometimes the blue is a tint in the glaze, and sometimes it is a tint added to the body (Miller 1980:18-19, 1991:9-10). The wares from the 1840s through the 1860s are generally vitrified. Molded marlys such as the Ceres pattern are common as are geometric shapes with eight, ten, and twelve sides. These give way to plain round shapes without molding. By the late 1870s it is not uncommon to find white granite wares that are not vitrified. Its price had been dropping, and the potters were cutting their production costs. When we define white granite wares as vitrified, we again are locking into a definition that might be helpful for chronological purposes, but it presents problems in scaling collections for the study of consumption patterns.

These wares began to change after the Civil War. During the war, the American tariff on imported ceramics was raised to over 50%. A large greenback currency was issued to finance the war, resulting in an inflated currency. Under these conditions, the cost of English ceramics almost doubled, which encouraged a number of English potters to move to Trenton, New Jersey, and begin production of American-made white granite.

It took these potters a period of adjustment to the new clays and other materials they had to work with, so many of the early American white granite wares were heavily crazed. Thus, white granite with heavy crazing probably is American made and dates from ca. 1865 to ca. 1890. This problem was worked on, and the wares improved as the American industry mastered their raw materials.

As white granite began losing its appeal, the potters cut its price and found ways to make it cheaper. One way was to fire it at lower temperature, and thus, the later white granite ware is often not vitrified. The later wares are usually unmolded, whereas the earlier ones commonly had embossed molding on the marly. In summary, the following traits would be helpful to keep in mind when separating white wares from white granite wares.

1) For pre-1845 assemblages, there will not be any white granite ware.

2) After ca. 1820, plain undecorated vessels are CC ware.

3) After the War of 1812, CC ware was mostly confined to toilet and kitchen wares such as bowls and chamber pots. Some plates are still being sold in CC ware, but teaware is very rare.

4) Importation of white granite ware began in the early 1840s. In post-1840s contexts the undecorated teaware is most likely white granite. The same is true to a lesser extent for tableware.

5) If it is vitrified, it most likely is white granite.

6) If it has embossed molding around the marly, it most likely is white granite.

7) If the body, rather than the glaze, has been tinted with cobalt to make it look light blue or gray, it is most likely white granite. Some white granite also has blue-tinted glaze. The term pearl came back into use in the post-1840 period in names such as “Pearl China,” “Pearl Stone Ware,” “Pearl White Ironstone,” “Pearl White,” and “Opaque Pearl.” Despite incorporating the name pearl, these marks occur on white granite wares.

There will be body sherds where one cannot distinguish white granite from whiteware. For those cases, one could use a category of whiteware/white granite. One should keep the use of this category to a minimum.

It should be kept in mind that while undecorated or molded white granite wares were the dominant type for the period ca. 1850 to ca. 1890, these wares also came with standard types of decoration such as shell edging, painted, and printed patterns. When you find white granite wares with color decoration, the decoration will be more important

for classification than the ware type. Therefore, one does not need to be quite as concerned with identification of the type of ware as in the case for undecorated vessels.

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