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The Conference on Historic Site Archaeology Papers 1971 - Volume 6

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The Conference on Historic Site Archaeology Papers 1971

Volume 6
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PREFACE

THE CHAIRMAN'S REPORT

The Twelfth Annual Conference on Historic Site Archaeology was held at the Ambassador Motel in Macon, Georgia on November 11, 1971. This Volume 6 of The Conference on Historic Site Archaeology Papers contains some of the papers presented at that conference, plus those submitted to THE HISTORICAL ARCHAEOLOGY FORUM.

This year's forum centered around my paper on ceramic analysis, and the response was excellent, as can be seen in the size of this volume. As a result the price for additional copies of this volume has been set at $7.00 instead of the usual $6.00. Those having paid their annual dues of $5.00 for the conference membership will, of course, receive this volume for that price, along with the index.

A major contribution to the Conference on Historic Site Archaeology is seen this year in the index accompanying this volume under separate cover. This was prepared by Dr. John Idol, Director of Graduate Studies for the English Department at Clemson University, and includes all published papers of the Conference from its beginning in 1960. Extra copies of the index are available for $1.00. This index should be most useful, and I would like to thank John for this fine contribution to The Conference on Historic Site Archaeology Papers.

I would like to thank those who participated in this year's forum by contributing papers. Thanks are also due to those at the Institute of Archeology and Anthropology at the University of South Carolina who assisted with the preparation of this volume. I would like to thank the Director of the Institute, Bob Stephenson for his continued support of the Conference Papers. I would also like to thank Mary Jane Rhett, Myra Smith, Judy McClung, Dianne Maroney and Carleen Regal for their assistance, as well as Gordon Brown, photographer for the Institute. For her assistance with the preparation of the subject index I would like to thank my wife, Jewell.

Stanley South, Chairman
The Conference on Historic Site Archaeology
A STUDY OF CLAY SMOKING PIPES PRODUCED AT A NINETEENTH CENTURY KILN
AT POINT PLEASANT, OHIO

B. B. (Ted) Thomas, Jr. and Richard M. Burnett

Introduction

The Lakin-Kirkpatrick-Davis-Peterson Pottery was located in Point Pleasant, Clermont County, Ohio about 25 miles southeast of Cincinnati. The kiln was operated on the southern bank of Indian Creek at its juncture with the Ohio River (Map 1).

The most singular feature of the kiln and its operation are the number of differing mold-types or varieties of short stem pipes produced there. To date we have located, identified, and classified 67 varieties of pipes. Each of the varieties is pictured and numbered in Figure 8 which appears later in the article.

Pipes were made in many varieties of color and composition of clay and with differing glazing methods. Most are either unglazed or salt-glazed, but some specimens evidence glazing by fly ash, and still others, perhaps, were glazed by an alkaline process. The pipes were formed in two-piece molds, fired in saggers (Fig. 4), and then allowed to cool, with the total process taking about four days.

The operation time span of the pottery falls into two basic categories, i.e., those dates that have thus far been documented with some certainty, and those that are probable but lack documentary support. These are summarized as follows:

- Documented time span - 1848 to 1880 - 32 years
- Probable time span - 1838 to 1890 - 52 years

In 1838 the site was purchased by the Lakin family (Clermont County Deed Book) and the pottery was probably established during this period by William P. Lakin (Rocky and Bancroft 1880). In 1849 the property was sold to Cornwall E. Kirkpatrick (Clermont County Deed Book), who, along with his brother Wallace W., operated the kiln until 1856 (Perrin 1883). At that time it was purchased by Nathan S. Davis (Clermont County Deed Book). Davis continued a pottery operation at the location until his death in 1874 (Clermont County Deed Book). Shortly thereafter, during that year, the land and factory were conveyed to the Petersons (Clermont County Deed Book), who were still operating at the location in 1880 (Rocky and Bancroft 1880). The site, however, was no longer recorded as a pottery by 1891 and was no longer in the Petersons' possession by that time (Clermont County Map 1891).
TOP VIEW OF POTTERY SITE

POINT PLEASANT, OHIO

U.S. HIGHWAY 52

DIRT ROAD

BIG INDIAN CREEK

OHIO RIVER

MAP 1

CROSS SECTION OF WASTER DUMP

Scale 1" = Approx. 6'

MAP 2
The extent of the operation during the various time periods is open to speculation, however, it does appear to be rather extensive judging from the amount of material in evidence in the kiln and waster dump areas. Although we are concerned here with the making of clay smoking pipes, it is of interest to note that the pottery did produce other earthenware material. Contrary to a larger variation of products produced at certain other early potteries, the waster dump material indicates that pipes and pipe-making accessories such as saggers, spacers, stilts, trivets, etc., compose well over half of the dump contents (Fig. 4). The remaining materials include earthenware preserve jars (Fig. 5), bowls, saucers, pie plates, storage crocks, jugs (Fig. 7), firebricks, and flue pipes (Fig. 7).

Several factors relating to the pipes themselves seem significant in addition to the variety of styles produced. The face or anthropomorphic pipes show a high degree of ingenuity and creativity by the designer. Contrary to the detailed designs of these pipes, it is evident that at some point in the history of the operation, perhaps from the very beginning, a philosophy of mass-production gained prominence. Quantity, not quality, seemed to be the guideline. There were literally thousands of pipes produced at Point Pleasant, and a comparison of these pipes with those made at other kilns proves them to be quite inferior in workmanship. The design seems good, but the finished product very crude. Most are either overfired or misfired, or were apparently broken during the firing or glazing process. No single specimen observed thus far shows any attempt by the maker to remove the mold marks. Contrary to Stanley South's findings at Bethabara, North Carolina (South 1965, 1970), the makers at Point Pleasant did not use sagger pins, but merely stacked the pipes in large saggers for firing. Perhaps, the results often obtained by this are best illustrated by the sagger pictured in Figure 4.

At this point it is appropriate to comment on the deteriorated physical condition of the site itself. Although not professional historians or archaeologists, and not professing to be, we are sincerely attempting to fill a small segment of American History that has, heretofore, been afforded little concentrated research. Our efforts in Ohio have been greatly hampered because both the kiln and waster dump have been nearly desecrated over the years by an untold number of our "pothunter friends". With this background in mind, we seek your patience as we attempt to unravel the story of this kiln. Figure 6 shows a portion of the site which, if studied, will indicate the extent of upheaval in the area.

In 1970 we attempted a stratigraphic excavation of six square feet in the waster dump (Map 2). Materials uncovered were separated and classified for each level of six inches to a depth of three and one-half feet. No significant knowledge was gained from our efforts. We feel certain that this particular area had been disturbed at some time.
in the past. Also, the relatively short time span of the operation tends to negate any benefits which might be derived from an excavation of this nature.

Although the only probable remaining evidence of the kiln itself are a few heavily glazed bricks, and what appear to be foundation stones, it is our belief that the kiln was quite small and was designed and constructed to accommodate a relatively small volume at any one time.

Dates Pertinent to the Pottery

Following is a chronological listing of dates relating to the pottery and the property:

February 20, 1836  John Bushman obtained the property from David B. Bushman, et. al. (Clermont County Deed Book).

November 24, 1838  John S. Lakin from John Bushman (Clermont County Deed Book). [During the period 1838 to 1849, William P. Lakin probably established the first pottery operation here (Rocky and Bancroft 1880)].

October 9, 1847  William H. Richards from John S. Lakin and on the same date Sarah Lakin from William H. Richards (Clermont County Deed Book).

April 2, 1849  Cornwall E. Kirkpatrick from Sarah Lakin (Clermont County Deed Book).

November 30, 1851  Cornwall and brother Wallace W. Kirkpatrikcs' establishment "was burned by an incendiary fire" (Bonham 1883).

February 1, 1852  Kirkpatrikcs rebuilt and commenced operations in the new kiln (Bonham 1883).

Fall of 1853  Kirkpatrick "sold out" and moved to Cincinnati (Bonham 1883). (It is probable that Nathan S. Davis was employed by Kirkpatrick and operated the pottery beginning in 1853.)

July 26, 1856  Davis purchased the property (194 acres) from Kirkpatrick for $1,100.00 (Deed Book 64:537).
March 24, 1874  Davis deceased and John B. Turner became Executor under the Davis Will (Will of Nathan Davis 1871).

August 24, 1874  George, Henry, and James Peterson obtained at public auction from the Estate for $1,205.00 (Deed Book 98:497-498).

1874 to 1880  The Petersons operated the pottery (Rocky and Bancroft 1880).

By the year 1891  Property was owned by James W. McRibben. No longer listed as a pottery (Clermont County Map 1891).

**Owners and Operators**

**THE LAKINS** (Period of influence - 1838 to 1849). The property was owned by John S. and Sarah Lakin at various times during the period 1838 to 1849, although we have uncovered no documented proof that they actually operated a pottery during this period (Clermont County Deed Book). John Lakin, according to local sources, was a "brickmaker" by trade. When Cornwall Kirkpatrick purchased the property in 1849, he "bought a large pottery" (Bonham 1883). We can, therefore, safely assume that someone established the works prior to that time. It is of interest to note the following from Rocky and Bancroft's *History of Clermont County* published in 1880: "William P. Lakin came to this place (Point Pleasant) to establish a pottery, having previously had one near Laurel (3 miles N.E. of Point Pleasant). The works were burned but rebuilt by Nathan Davis" (Rocky and Bancroft 1880). If this is correct then apparently William Lakin, who we might assume to be related to John and Sarah, originally established the pottery. Nathan Davis must have worked for or with Lakin as he is given credit for "rebuilding the pottery". To date we have been unable to find further information relating to William Lakin's involvement in the pottery.

**THE KIRKPATRICKS** (Period of influence - 1849 to 1856). Cornwall and his wife Amy moved to Point Pleasant from Covington, Kentucky and purchased the pipe factory from Sarah Lakin in 1849. Born in 1814 in Fredericktown, Ohio, Cornwall was one of a family of thirteen brothers and sisters (Perrin 1883). His father Andrew began making earthenware in Pennsylvania in 1812 being one of the first such manufacturers (*History of LaSalle County, Illinois* 1886). At the age of twelve, Cornwall left home and school to become a store clerk and keeper of the books, but seven years later he returned to learn the potter's trade under his father. After a year he went to Cincinnati, then to New Orleans by flat-boat to "see the country". In 1837 he first engaged in the trade for himself in Urbana, Illinois, and from 1839 to 1848...
maintained a pottery at Covington (Perrin 1883). Selling his shop, he bought the Lakin property and for a residence purchased the historical house which had been the birthplace of General and later President Grant (Bonham 1883).

Further information appears in the History of Alexander, Union and Pulaski Counties by Perrin (1883): "In 1848 he (Kirkpatrick) sold out and removed to Point Pleasant where he bought the 'Lacon' Pottery and the house in which Gen. Grant was born, and two of his own children were born there" (Perrin 1883). The "Lacon" obviously refers to Lakin. County records differ in that they indicate the property to be transferred on April 2, 1849. Perhaps the business rights were first purchased and legal property conveyance occurred later.

The Kirkpatrick establishment burned in 1851, and he immediately began rebuilding, being back in full operation by February 1, 1852. He "had built most substantially of stone, immediately on the banks of the Ohio River" (Bonham 1883). Cornwall was one of seven sons to become potters (History of LaSalle County, Illinois 1886), and Wallace, his younger brother, joined him at the Ohio site at the age of twenty in 1849. His brother remained only a year and in 1850 moved to California to "try his luck at mining" (Perrin 1883).

"In the Fall of 1853 Cornwall sold out and removed to Cincinnati, engaged in manufacturing ware in the Fulton Pottery for the following four years--" (Bonham 1883). We have no further record of the selling of the Point Pleasant business in that year, but perhaps Nathan Davis purchased manufacturing rights at that time prior to acquiring a deed in 1856.

Cornwall later operated in partnership with Wallace in Mound City, Illinois and finally in Anna, Illinois from 1859 until his death (Perrin 1883) in 1890 (Madden 1971). Serving as mayor of Anna for three terms, he was apparently quite a creative and innovative businessman. Cornwall may logically be the original designer of the unique face pipes made at Point Pleasant. He is mentioned in the Journal of the Illinois Historical Society article entitled "Art in Southern Illinois". It reads in part: "The products made at the Anna Pottery (Kirkpatrick Brothers) were numerous and varied. The pottery produced utilitarian umbrella stands, canning (preserve) jars, churns, chamber pots, tiles, firebricks, and clay pipes" ("Art in Southern Illinois"). "The clay pipes manufactured are mostly shipped South, for use on the plantations. Simply the bowls are made, the planters and negroes inserting reed in them for stems" (Crawford 174).

NATHAN S. DAVIS (Period of ownership - 1856 to 1874). On July 26, 1856, Nathan Davis purchased the pottery from Kirkpatrick (Deed Book 64:537). It is probable that Davis was already involved in the production of pottery items at the kiln as far back as the 1840's when owned
by the Lakins. He may well have continued as a potter under the Kirk­
patricks and even operated the pottery between 1853 and 1856, when
he took legal title. A map dated 1870 shows the location with this
notation: "Pottery N. S. Davis" (Clermont County Map 1870). There is
little doubt that the initialed "N. D." and "D. N." pipes were made at
some time between the early 1840's and 1874, when Davis died. Shortly
after his death, the property was sold to pay the debts of the estate
at public auction to the Pettersons (Deed Book 98:497-498).

An additional note concerns Henry H. Bushman (1843-1917) (Clermont
County Cemetary) who is said to have married one of Nathan and Martha
Davis' daughters. The Encyclopedic Directory of Clermont County lists
"H. H. Bushman" of Point Pleasant as engaged as a "Pipe Manufacturer"
in 1896 (Encyclopedic Directory 1896). We feel certain that he was
employed at one of the two other known potteries across Indian Creek.
These are mentioned briefly later in the article.

THE PETERSONS (Period of influence - 1874 to 1880 or 1890). George,
Henry, and James Peterson obtained possession from the Davis Estate on
August 24, 1874 (Deed Book 98:497-498). "For most of the time (between
1874 and 1880) a pottery has been carried on here, the present owners
being Peterson & Co. Lately the product has been smoke-pipes, stove­
flues, and drain tiles, immense quantities of the former article (i.e.
smoke-pipes) being made. Employment is given to from six to ten hands"
(Rocky and Bancroft 1880). This was in 1880. Presumably the operation
continued at the site for a few more years, however, a County map dated
1891 shows a new owner, James W. McKibben, and the location is no long­
er listed as a pottery (Clermont County Map 1891). Thus, by 1890, the
pottery at this location apparently ceased to operate. We do not as yet
know the exact reason for this discontinuence. In connection with the
"Peterson Period" several broken firebricks have recently been found.
One in particular bears the letters "--erson", which leads one to believe
that the word "Peterson" must have appeared on the complete brick.

Other Point Pleasant Pipe Kilns

A map dated 1870 also shows a second pottery site, this one on the
northern bank of Indian Creek but very near the old Davis works. The
owner is listed as Tom Peterson (Point Pleasant Map 1870). A local res­
ident believes that this pottery, which primarily made pipes, eventually
sold out to the Akron Clay Pipe Company of Akron, Ohio around the turn
of the century. Efforts to verify this through contacts in Akron have
yielded nothing conclusive. C. Dean Blair in his book The Potters and
Potteries of Summit County (Akron) 1828-1915, lists the Akron Smoking
Pipe Company, Mogadore, Ohio as producing clay smoking pipes between
1885 and 1895. Incidentally, he lists five potteries as making clay
pipes in Summit County covering a period for the entire group of forty­
five years from 1850 to 1895 (Blair 1965, 1971).
A third pottery producing pipes operated on the present site of the Grant Memorial Church across Indian Creek in Point Pleasant. This operation is believed to have been discontinued around 1907. "The Clermont Sun" (Batavia, Ohio) recently published a picture of this plant. The picture, furnished by the Cincinnati Historical Society, was originally published in "The Illustrated Clermont" in November 1900. It reveals a rather large two-story brick structure with several individuals appearing, one holding a large crock or bowl. Older area residents recall that the pipe factory was operated for many years by John Bainum and Thomas Peterson ("The Clermont Sun" 1971).

Classification, Description, and Production of the Pipes

To date, sixty-seven different varieties of clay pipes have been recovered and classified. We have divided these into the following four groups:

<table>
<thead>
<tr>
<th>CLASSIFICATION</th>
<th>NUMBER DESIGNATION</th>
<th>TOTAL TYPES</th>
<th>PICTURE REFERENCES</th>
</tr>
</thead>
<tbody>
<tr>
<td>I. Anthropomorphic Design</td>
<td>AN-1 to 22</td>
<td>22</td>
<td>Figure 8 (a-c)</td>
</tr>
<tr>
<td>II. Geometric Design</td>
<td>GD-1 to 31</td>
<td>31</td>
<td>Figure 8 (d-g)</td>
</tr>
<tr>
<td>III. Plain</td>
<td>PL-1 to 11</td>
<td>11</td>
<td>Figure 8 (g-i)</td>
</tr>
<tr>
<td>IV. Special Design (initialled)</td>
<td>SP-1 to 3</td>
<td>3</td>
<td>Figure 8 (i)</td>
</tr>
<tr>
<td>Total</td>
<td></td>
<td>67</td>
<td></td>
</tr>
</tbody>
</table>

Of particular importance are those pipes designated "Special Design". There are three varieties: (1) those with an "N" on one side of the bowl and a "D" on the other; (2) those of a different mold type and with the "N" and "D" initials reversed; and (3) those of a third mold type with a very crude set of initials on the bowl which may be a "T" and "J". We have thus far been unsuccessful in connecting these initials ("T" and "J") with anyone known to have owned or been employed at the kiln.

The clay used in making the pipes was of local origin. It appears generally to be of fairly poor quality, however, there are certain pipes which were made with a rather fine clay paste. The composition of the clay varies almost with every pipe.

To illustrate the above, we are summarizing certain statements from a letter of June 2, 1971, from Iain C. Walker of the National Historic Sites Service, Ottawa, Canada (Walker and Cox 1971). We sent Mr. Walker eight typical pipes from the site. The comments which follow are by an
associate of Mr. Walker's, Richard Cox, who is not only an archaeologist but an experienced potter.

<table>
<thead>
<tr>
<th>CLASSIFICATION</th>
<th>NUMBER OF PIPE</th>
<th>COMMENTS</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>AN-2</td>
<td>Brown paste with blue-gray subsurface; brown thin glaze of salt or fly ash type; probable iron oxides present in paste (Fig. 8-a).</td>
</tr>
<tr>
<td></td>
<td>GD-2</td>
<td>Gray-blue paste; overfired; gray exterior of body and blue-gray interior, possibly indicates oxidizing kiln atmosphere; probable iron oxide content in paste; no glaze of any kind (Fig. 8-d for pipe type).</td>
</tr>
<tr>
<td></td>
<td>GD-20</td>
<td>Light blue-black paste; much overfired; heavily glazed (Fig. 8-f).</td>
</tr>
<tr>
<td></td>
<td>GD-28</td>
<td>Red-buff paste containing probable oxides of iron; oxides reduce firing point of clay, possibly causing collapse of bowl; no glaze (Fig. 8-g).</td>
</tr>
<tr>
<td></td>
<td>PL-1</td>
<td>Blue-gray interior paste coloration; oxidizing effect on normally buff iron including clay; slight glaze effect by salt or, more probably, fly ash in kiln atmosphere (Fig. 8-g).</td>
</tr>
<tr>
<td></td>
<td>PL-3</td>
<td>Blue-black paste, somewhat overfired; heavily glazed, volatilization type; stacking mark appears on lower part of stem (Fig. 8-h).</td>
</tr>
<tr>
<td></td>
<td>PL-4</td>
<td>Dark purple-brown paste, slightly overfired; iron oxides probably present in the paste; no glaze of any kind evident (Fig. 8-h).</td>
</tr>
<tr>
<td></td>
<td>PL-10</td>
<td>Light blue-gray paste, probably product of oxidizing atmosphere acting on iron products in naturally buff clay; salt or fly ash glaze present especially on right side; well fired (Fig. 8-i).</td>
</tr>
</tbody>
</table>

It is obvious from the above that there exists a great variation in the paste content, firing time, and glaze (or lack of it) with each pipe. For this reason we have not attempted to describe in detail the pictured pipes used in this article.

In answer to a question raised with Mr. Walker regarding fly ash or ash glaze versus salt-glazing of the pipes, he wrote on August 23, 1971. Following are certain comments by Richard Cox as contained in the letter:
"A Thought on the so-called 'salt-glazed' pipes which you showed me (and I described as salt-glazed) - yes, these pipes could easily be ash-glazed. But - this raises several points which I feel have been badly ignored by some ceramic researchers.

"First - the apparent firing temperature is an obvious indication of manufactory sophistication. The pipes we viewed were certainly over-fired but also they were thermochemically glazed (ash? salt?) which indicates high firing temperatures sufficient to permit the vapour glazing (and cause slumping and discolouration). We know that it is not desirable to high fire clay pipes, for several obvious reasons. -----It seems apparent that the pipes in question originated in a primitive factory that was unable to control heat effectively and did not employ saggar techniques well-known elsewhere in N. A. The likelihood of a kiln accident causing all the observed effects is quite unlikely. We have discussed the observable constituents of the paste earlier. They too indicate a primitive operation" (Walker and Cox 1971).

The pipes at Point Pleasant were formed in two-piece molds and perhaps a method of production similar to that used at the Pamplin, Virginia factory near Appomattox Courthouse was employed at Point Pleasant (Heite 1970). The Smithsonian Institution has obtained one of the Pamplin pipe making machines used in the late nineteenth and early twentieth centuries. A description of the machine is as follows: "The device is simple: a single metal block mold that opens to reveal the pipe shape and receive a ball of clay; when closed by a single action, plunger pins from the top and side enter the mold to form the bowl and stem openings" (Ahlborn 1970). In the letter describing the Pamplin mold, further comments regarding pipe molds are made, including this statement: "---the block molds were made, perhaps in an Atlantic Coast factory and then supplied to pipe making firms such as the one in Virginia and possibly your (Point Pleasant) site in Ohio; one type is known as 'Akron'. Local producers could have built their own apparatuses to hold and operate the block molds" (Ahlborn 1970).

We have not been able to locate any portion of a mold or mold apparatus used at the Ohio site. A long-time resident of the area (over 85 years old) remembers seeing one of the molds and describes it basically as follows: The mold was a two-piece iron mold with two wooden plungers used to create openings for the stem and bowl. In addition long wooden levers were used, but in exactly what matter or for what purpose he does not know.

Two molds from South Carolina in our possession are similar in some ways to the just mentioned description. They are two-piece, but are made of soapstone and lead or pewter in lieu of iron. Each mold is equipped with two wooden handmade plungers, one for the stem opening and the other for the bowl. In addition there is a wooden form used
by the maker to secure the two mold pieces in firm place around the clay or slip while it dried. There is no "lever" or other such apparatus, and we are quite sure that these particular molds were designed for "home use" rather than for mass production.

There is no need to go into detail or speculation as to the exact process used in making the pipes at this site. Apparently after the raw clay was procured and "seasoned" it was then inserted in the molds, and later the unfired pipes were allowed to dry for a period of time. Large saggers with many ventilation holes were used in firing (Fig. 4), and spacers, stilts, or trivets separated the saggers during the firing process. It is related that four fires, one in each corner of the kiln building were employed with wood used as the principal fuel. Naturally, we have no proof of this statement. In some cases where a glazed effect was desired, common salt was shoveled on the fires toward the end of the process. The gaseous sodium from the salt was combined with the silica content in the clay to form a rather hard and glossy surface glaze of sodium-silicate. The purported source was a salt quarry across the Ohio River near Maysville, Kentucky.

As an alternative method of glazing a "fly ash" process may have been used on many of the Point Pleasant pipes. This is described by Mr. Cox as follows (Walker and Cox 1971):

When clays rich in silica are fired to a temperature of 1,000 degrees centigrade or higher, the silica is easily converted to a glassy form. Consequently, if a flux is present either in the paste or, here, in the atmosphere, the silica will form a glaze similar or identical to that of 'salt glazing'. The fire that generally causes ash-glazing is wood-fuelled; and that fuel should be high in basic constituents such as sodium hydroxide (i.e. lye, such as early soapmakers utilized originating in wood ash). Moisture is necessary to complete the reaction in order to convert the superheated silica into a silicate or glass. In any case several phenomena are in effect: ash (fly) as a flux with silica, and also chlorates combining with H₂O to form HCl - hydrochloric acid, again working on the silica to create a glass.

As mentioned, all of the pipes are the so-called short stem type designed to be used with a reed inserted to complete the stem portion of the pipe, and there exists within a short distance of Point Pleasant a type of reed quite suitable for this purpose.
Distribution of the Pipes

We are not sure what marketing methods were employed from time to time by the pipe-makers, but we do have physical evidence that somehow they managed to distribute certain pipes to many parts of the country—some as far as 2,000 miles from Point Pleasant.

Several known pipes which illustrate the above are pictured in Figures 1, 2, and 3; and they were located in the following areas:

1. "Mad River" Sand Dunes, west of Arcata, California. This pipe, found in 1925, is in the collection of Dr. Herbert H. Stuart of Eureka, California, who so graciously lent it to us for photographing (Stuart 1970-71) (Fig. 1 left).

2. "Big Lagoon", 35 miles north of Eureka, California. Dr. Stuart excavated this pipe himself in 1929 (Stuart 1970-71) (Fig. 1 middle).

3. "The Dalles" area of the Columbia River in Oregon. This pipe was kindly given to us by Mr. Emory Strong of Skamania, Washington. It was found on an Indian village site dating about 1885 (Strong 1970-71) (Fig. 1 right). Although we have not classified this exact type, Mr. Strong's specimen is almost identical to our GD-3.

4. Louisville, Kentucky (River-front Urban Renewal Project area). Located by bottle hunters in ruins between Main Street and River Road (Bullitt and Fifth Streets). Several additional pipes have been found in the diggings in this area of Louisville (Fig. 2).

5. Maukport, Indiana near the Ohio River. Found in 1966 by Mr. Jim Matthews of Louisville in an open field which contained evidence of a house site (Fig. 3).

In addition to the pictured examples Mr. Strong owns another specimen (Type GD-21) which was found on the Columbia River area near "The Dalles" in Oregon also (Strong 1970-71). This particular pipe is pictured in the book Indian Trade Goods by Arthur Woodward and edited by Mr. Strong (Woodward 1967:22).

In an attempt to locate additional distribution patterns we forwarded several specimens and photographs to Rex L. Wilson of the United States Department of Interior, Southwest Archeological Center, Gila Pueblo, Arizona, with the request that he make comments as to similar pipes he might have studied in connection with his extensive work particularly in the West. Listed next in summary form are his comments relating to certain pipe types (Wilson 1971):
CLASSIFICATION
NUMBER OF PIPE

AN-3

Your figurehead (Arab type) appears an exact duplicate of the pipe I illustrated on page 39 of my paper on Fort Union (New Mexico) as Figure 6-E (Wilson 1966). Your pipe also seems to precisely duplicate a specimen from the U. S. S. Cairo, sunk during the Civil War by the Confederates near Vicksburg, Mississippi, December 12, 1862 (Fig. 8-a for pipe type).

AN-4

A specimen from Fort Laramie (Wyoming) is similar but does not duplicate this pipe. Fort Laramie was occupied as a fur trading post from 1834 to 1849, and as a military post until 1890 (Fig. 8-a).

AN-7

Same comment as for AN-4 (Fig. 8-a).

GD-2

A similar style found at Fort Laramie, however, it was of red clay (Fig. 8-d).

GD-21

Fragments of bowl rims from Fort Laramie and an almost complete specimen from Arkansas Post (Arkansas) seem to duplicate your pipe (Fig. 8-f).

GD-28

A pipe of this style was found by Jackson W. Moore, Jr. in his recent excavations at Bent's Old Fort, Colorado. This style, to my knowledge, has been reported from no other site (Fig. 8-g).

PL-1

This is made in the same style as the Pamplin pipes represented at both Fort Union (Wilson 1966) and Fort Laramie (Wilson 1961) except that your pipe is smaller. I suspect that many manufacturers were turning out pipes of this style which, obviously, were popular during the late nineteenth century (Fig. 8-g).

PL-10

Looks similar to a Pamplin pipe described in their literature as the "'Powow' Smooth Shaker" (Fig. 8-i).

PL-3

Like a specimen reported from Sage Creek Stage Station, Wyoming (Fig. 8-h).

PL-4

Appears to duplicate specimens from Fort Massachusetts (New Mexico) 1852 to 1858; Fort Sanders (Wyoming) 1866 to 1882; Arkansas Post 1804 to 1863; and from Fort Union 1863 to 1891 (Fig. 8-h).
In 1970 we forwarded eight different pipes to the Smithsonian Institution for comments. Following are excerpts from a letter received on October 14, 1970, from Richard E. Ahlborn, Curator, Division of Ethnic and Western Cultural History of the Smithsonian's Museum of History and Technology (Ahlborn 1970):

I have shown the specimens to Mr. Ewers (John C. Ewers, Senior Ethnologist in the Department of Anthropology at the National Museum of Natural History in Washington) who states that he had not seen exactly the same designs, but closely similar ones. There are comparisons with the Pamplin types, and one could speculate that the molds were carried between factories.

Comments and Conclusions

We have attempted to collect and collate as much information as possible in a relatively short period of time to uncover the history of a single pottery which produced clay pipes in quantity during the nineteenth century. The pottery is significant because of the variety of pipe designs produced, as well as the sheer volume of pipes turned out over the years.

The classification as to style of the sixty-seven varieties may serve some future use in comparing pipes from other locations. We continue to seek any information concerning short stem pipes and perhaps at some later date can present a paper giving comparisons of additional styles from other kilns with those made at Point Pleasant.

Study and research in this area is in its infancy and it seems a sad commentary that research on the subject has been so lacking over the past years. This short paper, we trust will generate some additional interest in this rather specialized and neglected segment of our country's history.

Acknowledgements

We wish at this time to acknowledge with sincere thanks those individuals who assisted in our research, shared their knowledge with us, and gave so generously of their time and efforts in the preparation of this paper. These include

Others who assisted in our research through correspondence include


In addition there are many individuals from Clermont County such as Mrs. Eloise Ellis of The Clermont Courier, who afforded us several hours researching newspapers of the late 1800's; W. C. Carter and other county officials and employees who assisted in our search through the records and maps of the county; and a number of citizens of the Point Pleasant area and nearby Cincinnati (including Ralph L. Behymer, Charles Davis, and Parker Melvin) who also assisted in our project. Our final and special "thank you" goes to our patient wives, Martha and Leigh, who endured along with us the hours which culminated in the completion of this paper.
Figure 1 Left pipe type GD-1 found Arcata, California. Middle pipe type AN-2 found Eureka, California. Right pipe similar to GD-3 found Columbia River, Oregon.

Figure 2 Three pipes found by bottle hunters while excavating downtown Louisville, Kentucky Urban Renewal site. Left--GD-19; Middle--AN-21; Right--similar to plain types.

Figure 3 Pipe found on old house site at Mauckport, Indiana. Type GD-12.
Figure 4 - Top and side views of broken sagger filled with pipes. Pipes are cemented together by a combination of pressure and glaze. Found in waster dump.
Figure 5 - Three of nine preserve jars found along inside of stone foundation.

Figure 6 - View of a small portion of waster dump facing Indian Creek.
Figure 7  (left) Flue pipe 11 inches high, 5 inches diameter at base. (right) Jug 10 3/8 inches high, 4 1/4 inches diameter.
Figure 8-a

Right: AN-1
Left: AN-2

Right: AN-3
Left: AN-4

Right: AN-6
Left: AN-5

Right: AN-8
Left: AN-7
Figure 8-e

Right: GD-10
Left: GD-9

Right: GD-11
Left: GD-4

Right: GD-13
Left: GD-14

Right: GD-15
Left: GD-16
Figure 8-h

Right: PL-3
Left: PL-2

Right: PL-5
Left: PL-4

Right: PL-6
Left: PL-7

Right: PL-8
Left: PL-11
BIBLIOGRAPHY

Ahlborn, Richard E.

1970 Personal correspondence.

Blair, C. Dean


1971 Personal correspondence.

Bonham, Jeriah


Crawford, Comp.


Heite, Edward F.


1970 Personal correspondence.

Madden, Betty

1971 Personal correspondence.

Perrin, William Henry, editor


Rocky and Bancroft

1880 *History of Clermont County.*

South, Stanley


1970 Personal correspondence and interview.

Strong, Emory
1970-71 Personal correspondence.

Stuart, Herbert H.
1970-71 Personal correspondence.

Walker, Iain C. and Richard Cox
1971 Personal correspondence.

Wilson, Rex L.


1971 Personal correspondence.

Woodward, Arthur

Research Sources:

Clermont County Deed Book.

Clermont County Deed Book, Vol. 64, pp. 537.


Clermont County Map 1870.

Clermont County Map 1891.

Encyclopedic Directory (Clermont County). 1896.
Grave marker of Henry H. Bushman located in Clermont County Cemetery.


Map entitled: _Point Pleasant - Monroe Township._ 1870.

_The Clermont Sun, picture entitled Down Memory Lane, published in Batavia, Ohio._ 1971.

_Will of Nathan Davis. Probate Courts Case #51 (signed March 27, 1871)._
Fort Desha is a local name of undetermined origin which refers to the location of the French Post of Arkansas from about 1735 until 1750.

The first location of the Arkansas Post was on the north bank of Lake Dumond in southern Arkansas County. This location was reasonably well established through work done there by Dr. James Ford (1961). This post was established in 1686 by men under the command of Henri di Tonti, and apparently remained in that approximate location until the middle 1730's when the commandant, Lt. Montchervaux, moved the garrison downstream to somewhat alleviate the problem of bringing supplies from the Mississippi River. The results were disastrous, and in 1750 the garrison once again moved back upriver to the relative safety of the Grand Prairie. The settlement known now as Fort Desha was the only known location of the post off the Grand Prairie, and away from the protection of the Quapaw Indians; as such the earliest exclusively European settlement in Arkansas. There is some question about a later location of the French Post at the mouth of the Arkansas River; the Spanish, much later, did have a garrison in the vicinity of the later town of Napoleon.

The Arkansas Post was described in some detail in 1765 by Captain Phillip Pittman (1770) of the Royal Engineers. It has been suggested that Pittman never actually visited Arkansas Post first hand, and what he recorded was a secondhand description of the previous installation which had been vacated in 1750. This idea is mentioned in Faye (1943) and was again brought to my attention by Mrs. Margaret Ross, historian for the Arkansas Gazette. Definite corroboration has not been forthcoming.

No further mention of this particular location is made until Dr. Edward Palmer of the Smithsonian Institution visited the area in 1882-83. In his field notes he refers to "remains of old fortification on Arkansas River," Desha County, Arkansas.

Using Palmer's notes, the legal description of the area in question was determined. On the ground research, however, failed to produce any results. Other interested writers had mentioned Fort Desha (Hodges 1943; Faye 1943, 1944*). Dr Cyrus Thomas (1894) utilized Palmer's original statement, and Thomas' published report indicates that quite a bit went on between the two. In Palmer's notes the only portion of the site which is described in any detail is a "covered walkway." In the Bulletin, however, dimensions oddly enough correspond to those cited by

*These are probably the most complete and comprehensive works on Arkansas Post.
Pittman in his 1770 volume. This would tend to suggest that perhaps both Thomas and Palmer knew of Pittman's work, they certainly should have, and further, also knew it to be erroneous as to time. Unfortunately there is no record that this was actually the case.

In May, 1971, I wrote a short article for Field Notes (McClurkan 1971) which stated that I had been looking for Fort Desha. This was followed very quickly by a phone call from a local amateur archeologist, Mr. Harvey McGehee, who thought he might have the site. This ultimately proved to be so.

In November, 1967, McGehee, along with J. T. Ross and Larry Adkison, was fishing on the Arkansas River chute and saw what appeared to be an old well casing eroding out of the bank of the river. They excavated into the casing and found ceramic and bottle fragments, pieces of badly rusted metal, and the bowl of a clay pipe with a maker's mark.

I looked at the material in the possession of Mr. McGehee. At that time none of the material could have been considered conclusive except the pipe bowl, which unfortunately had been stolen. I was shown an illustration in the June issue of Archaeology (Walker 1967) which showed that the maker's mark was that of the Gouda pipe makers, apparently one of a shipment from Gouda for release in French colonial outlets in the middle eighteenth century, and also that the pipe was manufactured prior to 1739.

On subsequent visits to the site with McGehee, and also with other members of the Arkansas Archeological Survey, a sizeable collection of material was gathered including bottles, ceramic sherds, a bale seal, musket balls, and numerous pieces of badly corroded metal.

On November 3, 1971, representatives of the Corps of Engineers, Vicksburg District, visited the site in an attempt to evaluate the problems involved in bank stabilization and preservation. The study was completed in mid-December, but before any work could be considered the river rose over twenty feet in a remarkably short time and completely destroyed the entire bank in which the site was located. It was never determined exactly how much of the site was left, but it is doubtful that very much of it remained when it was brought to the attention of the survey. Although excavation of the site was not possible, the location of the site was determined. Prior knowledge had consisted only of the existence of the site, and no one had been sure of its location.

The artifact descriptions below were prepared by Mr. Pat Martin, Department of Anthropology, the University of Arkansas, and include those items found up until the first of November, 1971. Some material has been found since then by McGehee, but has not been described.
The Artifacts

All of the ceramics recovered from the site thus far are earthenwares and all are of types commonly found on 18th century sites in the New World. For purposes of description, they have been grouped into types roughly corresponding to some of those described by Miller and Stone (1970), in their study of ceramics from Fort Michilimackinac.

I. Tin-glazed earthenware: 32 sherds (8 decorated, 24 undecorated; Fig. 1 a–j)

A. Blue and white (this category includes all plain white sherds as well). Body color ranges from salmon to buff and the tin-glaze color ranges from pale blue to white. The identifiable vessel forms are predominantly plates, though there is one basal sherd that appears to be from a drug jar. Three sherds (Fig. 1 a–c) bear decorative motifs seen on 18th century French faience, especially from the potteries of Rouen. One of these sherds (Fig. 1 a) also retains evidence of a hole drilled for the repair of a crack or break. One rim sherd (Fig. 1 d) has a decorative treatment common on Puebla Blue on White majolica from Mexico. Two others show parallel blue and white bands (Fig. 1 e,f) and the last two bear foliate decorations (Fig. 1 g,h).

B. Polychrome - only two sherds were found that were decorated in colors other than blue. One sherd (Fig. 1 i) bears a scrolled band in dark blue and red-orange, a design seen on the faience of Rouen and St. Cloud. The other sherd (Fig. 1 j) bears an unidentifiable design in the same red-orange color.

II. Coarse earthenwares: 11 sherds (Figs. 1 k–m, 2 a–c)

A. Brown-glazed redware: 6 sherds
Body color of these sherds is fairly uniform red-orange. The glaze on three examples is a caramel color, and on one other a very dark brown. The two remaining sherds (Fig. 1 k,l) are not uniformly glazed and their color ranges from dark brown to gray. These two sherds also differ in that they contain large flecks of tempering material, apparently quartz sand. These sherds appear to be of a general type Dr. Hale Smith has called El Morro, believed to have been manufactured in Puerto Rico and Cuba since the 16th century.

B. Green-glazed earthenware: 1 sherd (Fig. 2 a)
This is a large rim sherd from a bowl. It is unglazed on the exterior and has a light green lead glaze over a white slip on the interior surface. This type has been called French by Miller and Stone.
Figure 1.

a–h Tin-glazed earthenware, blue and white (Arkansas Archeological Survey negative no. 723145)

i,j Tin-glazed earthenware, polychrome (AAS negative no. 723145)

k,l Coarse earthenware, El Morro (AAS negative no. 723146)

m Coarse earthenware, brown-and-green glazed (AAS no. 723146)

n,o Lead bale seal, obverse and reverse views (AAS negative nos. 723148, 723149)
C. Brown and green-glazed earthenware: 3 sherds
   Body color is a light reddish-orange, and the lead glaze is
   mottled olive green and a caramel brown (Fig. 1 m).

D. Slip-decorated earthenware: 2 sherds (Fig. 2 b, c)
   These two sherds form a part of the base of a shallow, footed
   bowl, with the body red-orange in color and the decoration a
   swirled white slip covered overall with a brownish lead
   glaze. This type vessel and decorative motif has been found
   in probable 18th century French contexts at Louisbourg.

III. Glass: 142 fragments, including 3 bottle bases, 3 bottle necks,
      and 1 square bodied bottle (Fig. 2 d-k).

   The glass in this collection represents an indeterminate
   number of vessels, most of them apparently wine bottles.
   The bases and necks found intact indicate both French and
   English origins. One (Fig. 2 d) shows a form common in
   English bottles at various times, an outward sagging at the
   base and relatively straight sides above. Two bases (Fig.
   2 e) exhibit the "flower-pot" shape attributed generally to
   French bottles, tapering outward from the base. The empontilling
   techniques utilized in manufacturing these bottles strengthen
   the argument for French and English origins. The English
   example (Fig. 2 f) has the mark of the sand pontil, while
   the two French bottles have the round, rough scar formed by
   using the glass that remained on the blowpipe as the pontil
   after the bottle was removed (Fig. 2 g). This technique is
   thought to be characteristic of the French bottle tradition
   and is illustrated in Diderot's encyclopaedia. These origins
   are further substantiated by the forms of the intact bottle
   necks, two of them exhibiting the triangular cross-sectioned
   string rim seen on French bottles (Fig. 2 h, i) and the other
   with the English tooled rim (Fig. 2 j). The other bottle form
   encountered is a pale blue, square-bodied bottle that Noël
   Hume says is French origin (Fig. 2 k).

IV. Metal Artifacts: 76 specimens (Fig. 1 n, o)

   A. Lead shot: 5 specimens, 2 badly flattened
      Two intact samples measure 13.8 mm. in diameter, one measures
      3.3 mm.

   B. Lead bale seal: one specimen (Fig. 1 n, o)
      This lead bale seal is 27.8 mm. in diameter and consists of a
      flat disc with a smaller, circular post on the reverse side,
      and is stamped: UNES
      17½

      Roughly similar seals have been found at numerous 18th century
      sites.
Figure 2.

a Coarse earthenware, green-glazed (AAS negative no. 723150)

b, c Coarse earthenware, slip-decorated (AAS negative no. 723152, 723153)

d, f English bottle base (AAS negative nos. 723155, 723156)

e, g French bottle base (AAS negative nos. 723155, 723156)

h, i French bottle necks (AAS negative no. 723154)

j English bottle neck (AAS negative no. 723154)

k Pale blue French bottle (AAS negative no. 723157)
C. Other Metal
Approximately 70 objects, appearing as clods of rust colored earth, were metal objects were X-rayed with good results; virtually all of them were shown to be hand-wrought nails of various sizes. A small number of them appeared to be some form of strap iron, probably barrel hoop fragments.

It is of some interest to note that in the entire artifact inventory there are no recognizable gun parts or gun flints. This would seem to be unusual since the Post was considered a quasi-military outpost.
BIBLIOGRAPHY

FAYE, STANLEY

1944 The Arkansas Post of Louisiana: Spanish Domination. The Louisiana Historical Quarterly 27, No. 3:629-716.

FORD, JAMES A.

HODGES, T. L. AND MRS. T. L.
1943 Possibilities for the Archaeologist and Historian in Eastern Arkansas. The Arkansas Historical Quarterly 2, No. 2:141-163.

McCLURKAN, BURNEY

MILLER, J. JEFFERSON, II, AND LYLE M. STONE

PITTMAN, PHILLIP
1770 The Present State of the European Settlements on the Mississippi.

THOMAS, CYRUS

WALKER, IAIN C.
This paper was conceived as a progress report on the opening of a new and, thus far, very exciting area to archaeological researchers relevant to the historic period— that area being the City of New Orleans and its surroundings.

To give an idea of the potential of this area, we offer a brief summary of the applicable historic development of the city.

The city was founded in 1718 by the French-Canadien Jean Baptiste Le Moyne Sieur de Bienville and named in honor of Philippe, Duke of Orleans, Prince Regent governing the Kingdom of France during the minority of his cousin Louis XV. It was established as a trading and administrative center of the Company of the Indies which were promoting Louisiana as a commercial and economic venture.

The site chosen was on a bend of the Mississippi at the terminus at an Indian portage by which there was short and easy communication between the river and Bayou St. John which connects with Lake Pontchartrain.

Between 1718 and 1721 the settlement grew in a haphazard manner without plan. A few post houses were built paralleling the river. By 1722 a city plan had been surveyed and it conforms generally to the plan of the current Vieux Carre, or French Quarter, with a slight difference in the street names. As may be expected, the houses that were already built did not fit into the new plan. This caused a few problems in the tearing down of the misplaced houses. The owners were naturally upset. There may have been more problems if a hurricane had not happened along in September of 1722, which rendered all the structures in town uninhabitable so that they had to be torn down anyway.

From this time the city grew in an orderly fashion around a parade ground and cathedral in the center of town on the riverfront. By 1769 it filled a six by eleven block area and was surrounded by a flimsy palisade and shallow ditch. There were about 7,000 people exclusive of slaves.

As a result of the French and Indian War, New Orleans was turned over to the Spanish in 1763. The Spanish were slow in taking possession. In 1765 Don Antonio de Ulloa came to take inventory but not possession. He was run out by the locals in 1768 in the first American revolt against a European power. There was some talk at that point of declaring
independence, forming a republic, and proclaiming New Orleans a free port. Don Alexandro O'Reilly arrived with Spanish troops in 1769 and took formal possession of the city for the Spanish Crown.

Under Spanish control the city flourished with ever increasing amounts of trade coming through her port facilities. Occasional hurricanes marred the tranquil growth of the city, and in 1778 a major fire destroyed a large portion of it, with another major fire in 1794 destroying most of the remainder.

In response to persistent rumors about an American attempt to take over the city, five forts connected by a palisade were built around the inhabited area in 1792. In the late 1790's the city began to expand beyond the original plotted limits up river into an area called Fauberg St. Mary.

On November 30, 1803, the province of Louisiana was transferred back to France; and on December 20, 1803, Louisiana became part of the United States. From this point New Orleans blossomed. By 1810 two new subdivisions were opened and rapidly filled. Under the American flag New Orleans became the second largest port in the eastern United States carrying the bulk of the water born traffic from the entire mid-continent.

The study potential of the city is gargantuan and multi-problematic. To be initially considered are those overall problems concerning the gross changes and developments that took place with the successive colonial governments. For instance, some of the problems are the kinds of imported material during each period, changes in architectural fashion, growth patterns of the city, sources of materials used within the city, and so forth.

The city, as an archaeological resource, provides a wide range of anthropological problems which should eventually prove quite illuminating. Throughout the city's history there has been an influx of diverse cultural influences which have coalesced to give the city its unique character. There are, of course, strong remnants of the primary colonial characteristics—French, Spanish, and Anglo-American—but added to these are a variety of other influences—African, Afro-Caribbean, Irish, Italian, German, and Acadian (French Canadian)—which expand the scope of the cultural diversity present in the city today. To be considered eventually are the evolution of neighborhoods, comparison of land use and material use between neighborhoods, culture change as introduced behavior patterns are adopted and adapted by the resident populations, and behavioral conservatism and/or change given specific historic incidents—such as constructional events, response to the fires, or changing house plans or artifact use in the time of governmental change or a new immigration.
The exciting thing about the city is that the archaeology is not the only anthropological aspect under investigation. There is a small but growing element of urban anthropology concentrating on the living city, tracing its folklore, recording its celebrations and their histories, and investigating its populations within a comparative framework.

Actual historic site archaeology in New Orleans is very recent, the first being initiated in October of 1970. The lack of prior interest in historic site archaeology and its potential contributions to restoration and historic interpretation may be attributed to a lack of education of the New Orleans people. This situation is changing and interest has been steadily increasing as work on the few projects thus far attempted has gained local publicity. The interest is not yet, however, grown to the point where the state or city has contracted to undertake the finding of a systematic survey and research program.

The first systematic historic site work was begun by Jack Hudson on two sites in the Vieux Carre district that were being restored by the Ella West Freeman Foundation. The restoration architect Sam Wilson, an architectural historian, convinced the Freeman Foundation to fund an archaeological examination of the material recovered during the restoration construction activities.

The two sites are adjoining properties covering 1118-1132 Royal Street. Collectively the sites are known as the Gallier House complex and consist of the Gallier House itself built in 1856 by James Gallier, a prominent New Orleans architect, and the adjoining property that was used as a warehouse since 1833. Both sites were part of the original Ursuline Convent Grant laid out about 1722. The land had no discernable use until the entire square, of which the two properties are a part, was laid out into building lots in 1825 and sold.

At present only the Gallier House site is written up, the warehouse site still being under analysis. Prior to the construction of the house in 1857, the lot was vacant. As the adjoining properties were built up beginning in the late 1820's, the Gallier lot was subject to trash dumping. Presumably most of the material recovered as general detritus dates from that period and should be composed predominantly of the trash from the three adjacent properties. In 1857 the house was constructed and the lot closed off from further outside depositors by the house and garden walls. Deposition of material in the latter half of the nineteenth century would be restricted to the cistern and woodshed area and to a minor extent the non-flagstoned areas of the garden.

This investigation was primarily a salvage operation—that is, collecting material from contractors' excavations. Controlled excavations were undertaken in three areas, but stratigraphic information was not obtainable due to multiple disturbances that have
occurred within the site—such as, the prior construction of a swimming pool, excavation of wall trenches at various times, and the excavation of a new privy with a brick foundation around the turn of the century. The privys themselves were not stratified, apparently because they were periodically cleaned.

Ceramics from the Gallier House were predominately of British manufacture, many being made for specific New Orleans Importers as evidenced by marks on several sherds which give the firm's name and local address. Wares recovered include pearlware, shell edge, annular ware, banded ware, mocha, transfer ware, ironstone, porcelain, and heavy salt-glazed stoneware. Other artifacts include French wine bottles, an optical lens, window glass, French gun flints, porcelain doll fragments, table glass, ex-iron objects, a-perfume bottle, and some medicine bottles.

Jack Hudson and some archaeology students from LSUNO spent three weekends in Spring, 1971, salvaging material from several sites located in an Urban Renewal area in which the city razed a portion of one of the 1810 subdivisions, the Fauberg Treme, to build a new Cultural Center. Salvage was facilitated in that bottle hunters had located many of the privys, and all we did was sift their back dirt for artifacts that they left. Work was curtailed due to some legal problems with the destruction contractor but not before we had collected 80 shopping bags filled with material of all descriptions including possibly 30 different kinds of banded ware dating into the 1840's and 1850's. This material is being analyzed now and will be fully reported on later.

In August, 1971, the Louisiana State Museum contracted J. R. Shenkel for the archaeological examination of Madame John's Legacy. This residence, named for a character in a short story by George Washington Cable, was one of the first houses to be constructed after the fire of 1788. It is a raised cottage—a design developed during the French period. It consists of a brick foundation some eight feet high surmounted by a brick and beam construction house with a high peaked roof. The rooms formed by the high foundations were generally rented as warehouse space. Ingress to the house was via a narrow walkway from the street beside the house to the courtyard in the rear and thence up the entrance stairs.

The specific problems in the examination of this site were

1. Locate the original entrance stairway.
2. Determine the modification sequence in the house.
3. Provide an artifact sequence of material used in the house.
4. Locate any remains of the original structure on the site that was burned in 1788.
Of these the stairway was not found but cautious success may be claimed for the other three.

By examination of the subterranean foundations, several modifications in the original structure were noted. The final analysis is not yet completed as to exactly what the modification sequence is.

The artifacts recovered from Madame John's were not particularly plentiful, but sherds from several vessels of polychrome and blue on white majolica and Faience were found in several of the excavation units. A complete report on this site is due around February 1 and should be available sometime after that.

Excavations are on-going on the National Trust property "Shadows on the Teche" in New Iberia, Louisiana. This is a plantation which contributed to the nineteenth century development of Southern Louisiana. Current work is centered in the slave quarter area and a structure shown in a painting and identified as a schoolhouse. The plantation was occupied by Union forces in 1862. Artifacts thus far recovered include heavy wall banded ware, a wine seal, a Union cartridge belt buckle, a pelican militia button, cut nails, stoneware, and Chelsea ware matching the service in the house.

As in all archaeological work, the objectives relate to illudication of several levels of phenomena and the Shadows work is no exception. The initial objectives are, of course, to locate, describe, and interpret the structures in question in order to make a more effective exhibit to the public. It is to the fulfillment of this objective that the funds are allocated. The data recovered hopefully will provide threads by which we may eventually weave a fabric of understanding concerning the rural-urban relationships that were extant in the nineteenth century between New Orleans and its neighboring plantations, towns, and cities. This understanding may eventually contribute to overall generalization about the urban process.

As I indicated earlier historical archaeology is just beginning in the New Orleans area. The projects mentioned are still in varying stages of analysis, but plans are underway for new projects in and around the city. In the immediate future we expect to undertake exploratory excavations in the Old United States Branch Mint grounds where one of the original five peripheral forts was located. Another project scheduled is a cooper's house and shed.

In the coming year, we expect to provide a significant quantity of data on this heretofore ignored area of early American activity feeling that it will appreciably broaden our interpretive perspective in dealing with our multi-faceted heritage.
THINKING THE WHOLE SITE: 
SOME CONSIDERATIONS IN PLANNING AN EXCAVATION

Edward F. Heite

In August and September, 1732, William Byrd of Westover made his famous "Progress to the Mines" in search of information about the iron industry. In the course of his overland journey, Byrd visited two of the four furnaces that were then operating in the Virginia Colony: Fredericksville and Tubal. We are indeed fortunate that he left a detailed description of each operation, with cost figures, labor estimates, and a thousand other bits of useful information.

Fredericksville is the least-known but perhaps the most interesting of the pioneer Virginia furnaces. It was built in 1727 by a company that included Major William Gooch, the lieutenant governor; Richard Fitzwilliam, the surveyor-general of the customs; and Charles Chiswell, agent for the Royal African Company. These gentlemen and their partners built an ironworks on 15,000 acres of land that they had accumulated by the usual combination of hard work, diligence, and outright corruption.

Byrd described the various operations at Fredericksville, from mining to final shipment, in sufficient detail that we can reconstruct the works on paper, at least. At the iron mine, the ore was blasted loose with gunpowder, stacked in carts, and carried a mile to the furnace, where it was mixed and roasted. Charcoal was burned by a contractor. A warehouse at the furnace was capable of holding 40 loads of charcoal, each load containing 160 bushels; this must have been a considerable structure. A force of eighty slaves operated the furnace; their housing alone must have constituted a sizable village. The ironmaster lived in a house that probably was more substantial than the others. The company maintained 25 miles of roads and bridges. There was also a wharf on the Rappahannock below Fredericksburg, and a plantation devoted to the care of the company's oxen.

Besides the ironmakers and the colliers, Byrd mentions the carpenter, the wheelwright, and the cartwright. Since the furnace was built of brick, there must have been a brick kiln on the property. About 350 yards above the furnace stood a wooden dam, from which water was conveyed to the waterwheel through a wooden raceway. Of course, there was the furnace itself, with casting shed and bellows house.

So we have a word-picture of Fredericksville, a complex arrangement of buildings, roads, machines, and people, working in a wilderness 25 miles from the nearest village. This was a self-sufficient community that must have needed bakehouses, granaries, stables, and other storage buildings that Byrd did not describe.
The Fredericksville company failed after Chiswell's death in 1737. For the next 230 years, even the identity of the furnace was forgotten. Then, in 1969, the Virginia Electric and Power Company announced plans to flood the site. The power company gave the Virginia Historic Landmarks Commission enough money to salvage parts of the site that were to be destroyed. It was my good fortune to direct the excavation during the spring of 1970. When we arrived, the furnace stack lining was the only feature that showed above ground; the bricks had long since been carried away.

We concentrated our efforts on the features that would be destroyed, although I did survey the surrounding territory to find the mine, the brick kiln, and the road traces. A thick deposit of charcoal on the hill above the furnace may represent the remains of the charcoal warehouse. Our crew of six college students spent nine days digging around the base of the furnace. We found some Flemish-bond walls that had not been destroyed; we found the wheel-pit, and we found postmolds in the masonry where the builders had set their scaffolding. We were able to produce a groundplan and profile of the furnace; we identified the dam site, that is now covered by a modern stone dam. The millrace is visible in the hillside, as are the principal road traces.

One might say that we were successful; we had salvaged those parts of the site that were to be destroyed by the power company. Our grant was limited to work on the power company property itself. But what of the charcoal pits, the mine, the brick kiln, the ironmaster's house, the slave quarters, the roads, the warehouses, the shops, the offices, and the stables, that constituted the largest part of an iron plantation? They were left for some future archaeologist, someone who will not be pressured by the urgencies of a nuclear power plant or a highway relocation project.

Unfortunately, the same has been true on many other iron furnace sites in the United States.

Archaeologists have excavated hundreds -- literally hundreds -- of iron furnace stacks. We have lovingly excavated and restored them, and we have shown them to the public under the pretense that they represent a true picture of the early American iron industry. Such fragmentary presentations have considerable appeal. Furnace stacks are fun to dig up; they are big ruins, and they have a certain romantic attraction. In short, they draw a lot of attention, aside from their historical importance.

But we must stop such madness. Outside of the curiosity factor, I doubt that there is much to be learned from the excavation of another isolated cold-blast charcoal-fired iron furnace. We know what they looked like, how they operated, and how they were laid out.
Most iron-furnace archaeologists can go onto a site and quickly interpret the groundplan with little or no excavation, just as we did at Fredericksville. It is time to go looking for coal houses, blacksmith shops, company stores, bridges, mine shanties, roadways, slave cabins, and even brick kilns.

We have heretofore excavated this class of site in terms of what already is known. We have dug the same old thing, time and again, almost by a formula. The results, naturally, have been remarkably similar, whether they came from Saugus, Hopewell, or Fredericksville. Some interpretive programs have gone beyond the bare furnace; some have gotten all the way to the ironmaster’s house.

But an ironworks is not just a furnace and a few houses. Nor is a plantation merely a manor house and its dependencies. Nor is a fort merely an earthwork and a few bombproof shelters. A site is the sum of the human activity represented by the material remains.

Our profession gives lip service to this principle. We make noble remarks about putting sites in their cultural context, their historical perspective, and their places in the landscape. We come to conferences and we congratulate ourselves on our insight and our breadth, and on our scientific detachment. Yet we continue to grope like the blind men with the elephant. We continue to dig the familiar, in the familiar fashion, time and again.

The reasons for such expensive repetition may lie at the very heart of the planning process, in the reasons we dig in the first place. There are three sorts of justification for archaeology: salvage, restoration, and pure research. Of these three categories, restoration for publicertitent represents the vast majority of funding available for historic archaeology. The reverse is true of prehistoric archaeology, where most of the funds go to pure research or to salvage. Pure research is a rare motivation indeed in historical archaeology, where it is currently needed most.

Historical archaeology too often plays a subservient role in the preservation game, where it necessarily goes for much of its financial backing. The archaeologist is expected to find the foundations for the architect, and then to quietly fold his tent and steal off in the night. Under no circumstances should a well-behaved restoration archaeologist upset any apple carts, or ask embarrassing questions about social history.

These strange bedfellows -- the preservationist and the archaeologist -- can seldom be expected to enter a project with the same objectives, especially when it comes to the budget. The records of this conference are full of papers that chronicle the successes and the failures of archaeology in historic preservation. Indeed, the
preservationist and the archaeologist are at cross-purposes. They will not ask the same questions, and they will not seek the same sort of answers.

Historic preservation is a movement, an emotional phenomenon. In its commercial sense, it involves tourist attractions and tourist development. Historic preservationists cultivate funding that is based on economic, patriotic, and aesthetic themes. Words like "heritage" and "patriotism" frequently appear in even the most technical preservation documents.

Archaeology, on the other hand, is (or should be) a scientific, historical study, interested in both the visible and the invisible. Most of the money that we spend for archaeology never produces anything that the public will see, or even understand. Is it any wonder therefore that preservationists are reluctant to spend their money on archaeological research?

Even the research that is actually financed, however reluctantly, by the preservation movement, is usually channelled into projects that will be visible and useful for interpretation of existing structures.

This situation is eating away at the foundations of both archaeology and historic preservation. Neither party is thinking anything new; both the preservationists and the scholars are too frequently content to pursue the same tired old objectives, restoring one local shrine after another, copying and glorifying one another's mistakes. To put it bluntly, the history business in general is wasting its substance running down blind alleys. Substantial research projects continue to take a back seat, while the financing and personnel concentrate on site-oriented, restoration-inspired, locally-restricted research that can no longer answer the questions that we should be asking.

A very few large restoration projects are producing concrete contributions to our knowledge of material culture, but the majority of our research remains disjointed, parochial, and redundant. The big operators make substantial contributions because they are large, but more importantly because they are managed by a handful of imaginative archaeological innovators.

Perhaps there is some hope that creative thinking will some day filter down to the provincial level. If it does, we can expect that most of us will follow slavishly and repetitively in the footsteps of our betters.
Editor's Note: This series of colorful word pictures was used by Mr. Heite to illustrate the points he makes in his paper.
COLONO-INDIAN POTTERY FROM CAMBRIDGE, SOUTH CAROLINA
WITH COMMENTS ON THE HISTORIC CATAWBA POTTERY TRADE

Steven G. Baker

Abstract

An assemblage of Indian pottery made in imitation of European vessel forms has been recovered from a context of about 1800 during excavations at the extinct community of Cambridge (38GN2) at Ninety Six, South Carolina. The pottery is part of that general class of wares known as "Colono-Indian" and includes pots, pans, bowls, and lids. The wares are identical to documented Catawba ceramic types and include both mottled and smother fired vessels. These basic wares have been found at other sites in South Carolina and were produced by the Catawbas largely for the commercial pottery trade. This trade involved all classes in South Carolina's plural society of Whites, Blacks, and Indians and was often conducted on an itinerant basis. The pottery trade became important to the Catawbas during the latter eighteenth century and today is still a source of livelihood for a few potters. Colono-Indian forms seem to have replaced traditional Catawba pottery by a relatively early date. The pottery trade has long enabled the Catawbas to earn a livelihood and to retain something of their ethnic identity. Although simple imitation of European vessel forms can be ascribed to occasional innovative potters in areas of English domination during the early contact period, for the Catawbas at least, the sustaining and major motivations for the full-scale production of Colono-Indian pottery were economic.

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EXPLORATORY EXCAVATIONS AT FORT HAWKINS, MACON, GEORGIA

Richard F. Carrillo

Historical Background

Fort Hawkins was built by the United States Army in 1806 to serve as a trading center, and a place where negotiations could be conducted with the Creek Indians. It was named for Benjamin Hawkins, a former North Carolina senator, who was the then Indian Affairs Agent and the person most instrumental in conducting relations between the government and Creek Indians during the first quarter of the nineteenth century (Butler 1859:59-60). By 1821, the military and trading functions for which the fort had been constructed were ended and it was closed by the army. The fort continued to be occupied as a settlement which eventually led to the present day city of Macon (Butler 1879:77-78).

The fort was described, a half century after its abandonment by the army, as follows:

One hundred acres of ground were, for many years, reserved for the uses of the fort. The fortifications consisted of two large block houses, surrounded by a strong stockade. The stockade was built of posts of hewn timber, fourteen feet long, and fourteen inches thick; they were sunk in the ground four feet, with port holes for a musket in every alternate post. The area within the stockade was fourteen acres. There were four long houses, one in the centre of each side of the stockade, their front forming part of the stockade to the width of each house, about 20 feet. These houses were used for soldiers' quarters, provisions, and for the factory goods to be sold to the Indians, and peltries received in return. In the centre surrounded by oaks, were the officers' quarters (Butler 1879:60).

The above description by John C. Butler and a sketch drawn by E. D. Irvine (Figure 1; taken from a sketch in Butler's Historical Record of Macon and Central Georgia, page 61) indicate that Fort Hawkins consisted of a stockaded fort enclosing 14 acres with two blockhouses located at opposite ends. Inside there were four buildings built in the center of each of the four walls which served as integral portions of the stockade and were approximately 20 feet in width. Located in the central area of the stockade was a building used as the officer's quarters.

Description of the Fort Hawkins Site

Fort Hawkins elementary school is presently located on the site
Figure 1
Sketch of Fort Hawkins by E. I. Irvine (Butler 1879:61)
where Fort Hawkins once stood. This area consists of the northern half of a city block (437 feet square) which is owned almost entirely by the Bibb County Board of Education. The present streets bordering the block are; Woolfolk Street on the north, Fort Hill Street on the west, Maynard Street on the east, and on the south Emery Highway.

The northern half of the block constitutes the crest of a high hill which is flat probably as a result of grading for the playground. A brick retaining wall is situated along the north and east sides of the block encompassing the northeast quarter. Fill was apparently removed from the center of the playground and deposited around the edges of the hill to level the playground. It appears that along the crest of the hill, on the south side, there has been considerable filling over the years. Along the north and west sides, there appears to have been a natural dropoff, now broken by street cuts.

Situated at the crest of the hill at the southeast corner of the Fort Hawkins schoolyard is a large, imposing, reconstructed concrete blockhouse. Surrounded by a chain link fence 60 feet square the concrete blockhouse was built in 1936-37 by the Nathaniel Macon Chapter of the Daughters of the American Revolution through the Works Progress Administration and was supposedly reconstructed on the location where the original southeast blockhouse once stood (South MSa).

Archeology at the Fort Hawkins Site - 1936

In 1936, while work was progressing on the reconstruction of the southeast blockhouse, a series of archeological test were conducted to determine whether the blockhouse was being built on the original site and to locate the stockade.

A two-week investigation was carried out by Gordon R. Willey who was at that time working on the major archeological project being conducted at Ocmulgee National Monument.

The excavation consisted of a series of trenches dug perpendicular to the palisade ditch. The results of the excavation indicated that the reconstructed blockhouse was located on the original site and a row of partially decomposed palisade posts was found projecting from the north and west sides of the blockhouse.

It was found that the palisade extended 105 feet from the blockhouse along the east side before it suddenly ended. It was relocated ten feet farther north, where it continued for 20 more feet before it was interrupted once more by a 20 foot gap with the wall, once again, continuing on the other side (Willey MS).

Although the excavation conducted in 1936 did reveal that the reconstructed concrete blockhouse was located on the cellar foundations of the original corner blockhouse with evidence of the original stockade
radiating from the north and west walls, the data available did not allow for the positioning of the stockade line. This in itself was justification to require additional archeology.

**Interpretation of 1936 Excavations**

On the basis of Willey's data, South (MSa) made the following interpretation:

Willey indicated that the wall extended a distance of 90 feet from the north face of the blockhouse at which place a ten foot gap occurred. From this, Willey suggested that this could represent a gate. Past the ten foot gap Willey encountered a 20 foot row of posts followed by another gap of 20 feet. Willey postulated that this area had been scraped, thereby causing the 20 foot gap. It was found that with no profile or plans of this area, it was impossible to determine whether it had actually undergone a removal of posts as Willey suggests, or whether this was a gap where a structure had been located. In his description of Fort Hawkins, Butler (1879:60) states:

That there were four long houses, one in the centre of each side of the stockade, their fronts forming part of the stockade to the width of each house, about 20 feet.

South (personal communication) suggests that if logs were laid horizontally, as was usually the case when buildings of this period were constructed, this type of architectural design would not necessitate a palisade ditch. Therefore, one could postulate that if this gap represents the section of the structure which was incorporated into the palisade wall, an equal distance, to that between the blockhouse and the center of the gap (120 feet) north would probably locate the northeast corner of the stockade. The distance of 240 feet from the blockhouse would situate the northeast corner of the palisade wall in the area where Woolfolk Street is presently located.

Therefore, by the interpretation of Willey's data, and the topography of the land, South conjectured that the fort would have been 240 feet square, or slightly over an acre in size, provided that it was square. Although the hypothesis did not correlate with Butler's description that the stockade encompassed an area of 14 acres, nor with Bryan's interpretation that it had been four acres in size, it did conform with the terrain. Thus, South stated that the area where Fort Hawkins school now stands is located in the original stockaded area of Fort Hawkins.

South further indicates that if the interpretation of four acres is adhered to, the north wall of the fort would have extended north over the crest of the hill two hundred feet past Woolfolk Street. Defensively, this would have been unwise. Also an area of only 20 feet would have been utilized as a storage shed, soldiers' quarters, etc., in the center of a wall 430 feet in length. This would have caused the fort to be con-
siderably out of proportion in relation to the existing interior structures, and would not conform to the specifications of other forts that used such interior structures as part of the palisade wall. Another important factor that should be considered is that a musket would be far more efficient at a range of 240 feet than at 430 feet (South MSa).

Thus additional archeological excavation was required to answer questions raised by the 1936 excavations and to clearly delineate the outlines of the fort.

Archeology of Fort Hawkins - 1971

The present archeological investigation was conducted using methods similar to those used by Willey in 1936. The initial excavations were begun along the areas north and west of the reconstructed southeast blockhouse since these were the locations where Willey had found evidence of the palisade ditch and posts (Figure 2).

The East Palisade

Along the east side of the fortified area, posts and a palisade ditch eight-tenths of a foot wide, were detected at a depth of one foot from the surface in initial excavation Units 2 and 3 located 44 feet and 66 feet, respectively, from the north side of the reconstructed blockhouse. In Unit 7, 94 feet from the blockhouse, a four foot gap was encountered in the palisade ditch. When this measurement is compared to Willey's map, this gap lies at approximately the same place where Willey dug a trench in 1936. Aside from the gap, no other evidence of disturbance was able to be detected. Willey's map indicates a measurement from the south wall of the trench to be 91 feet from the blockhouse in contrast to the present distance of 94 feet.

Beyond the 1936 slot trench, at a distance of 109 feet from the blockhouse, the palisade ditch stopped. It was at this same place that Willey (1936 MS) described a ten foot gap which he interpreted as being a gate. This section is located in Units 7 and 17. This was one of the areas along the palisade ditch where it was possible to see where the posts had been set abutting the west (right) wall of the ditch. The ditch is one and one-tenths feet wide; the posts were eight-tenths of a foot wide with the exterior fill comprising three-tenths of a foot.

A backhoe was used to dig Unit 17A to reveal the portion of the palisade ditch north of the ten foot gap. In this unit the ditch was again encountered and extended 17 feet north before ending. Due to lack of time, detailed excavations only revealed the north and south ends of the 17 foot section. Midway between the palisade ditch, two uprooted bricks were uncovered by the backhoe immediately west of the ditch.

North of the 17 foot palisade ditch section another break in the ditch was encountered extending a distance of 22 feet, where once more
Figure 2

Archeological Plan of Fort Hawkins Excavations
the palisade ditch was located. Two bricks were found oriented in a north-south direction west of the ditch. The palisade ditch was only visible for a short distance since it was not found in Unit 11. Since Willey's map indicates that the ditch was located farther north, the fact that it was not located in the present excavation may be due to its destruction in 1936. There was no visible stratigraphic sequence in this area; the soil beneath the top humus layer resembling subsoil.

Units 23 and 24 were excavated parallel to the palisade ditch, Unit 23, to determine if the ditch had turned west since it was not found at the furthest point north near the retaining wall. Unit 24 was excavated parallel to Unit 17 in an effort to locate evidence of interior structures. No evidence of a palisade ditch nor posts was found in this unit.

Along the east side, the palisade ditch was located near the surface and supports Willey's observations that the palisade tended to rise as it progressed northward (MS).

There were few artifacts found along this side. The ceramics found correspond to the types described for Units 6 (Feature 8) and 13B along the south palisade.

**The South Palisade**

Along the south palisade side, Unit 4, located near the chain link enclosure of the reconstructed blockhouse, revealed the palisade ditch and the remains of a post at a depth of 2.06 feet from the surface. The post impressions were clear in the ditch, including the mold of a round post considerably larger than the ditch. The ditch was seven-tenths of a foot in width. Within this unit, extraneous postmolds occurred north of the ditch (interior portion of stockade). The postmolds located in this unit tended to be very soft and after a rainstorm, some collapsed.

In Unit 6, excavated a distance of 57 feet from the west wall of the blockhouse, a brick floor and foundation (Feature 8) was found at a depth of one and one-tenths feet below the surface protruding from the north and west walls of the unit. The bricks were not mortared and appeared to be bonded with clay. The bricks comprising this floor were set with the lengths oriented in a north-south direction. At the north half of the east edge of this fragment of the floor, the foundation two layers high, was located. The basic style of construction appears to be English bond.

A number of artifacts were recovered in the vicinity of the brick floor and wall consisting mainly of ceramics. These ceramics, mainly English, comprise types which have a general time span from ca. 1780 to ca. 1835 (South, personal communication). The types of ceramic sherd found in Unit 6 (Feature 8) consist of creamware and pearlware.
Creamware is a ceramic type that was produced by Josiah Wedgwood ca. 1759 and underwent varying evolutionary stages until its extinction in the early 1800's (Noel Hume 1970:126). The style of creamware found in the context of Feature 8 was probably produced after ca. 1785 based on its lighter coloration. A rim sherd can be dated prior to 1783 when only plain rims were produced and which are found in late eighteenth and early nineteenth century contexts (Noel Hume 1970:126).

The majority of the ceramics consist of pearlware. This type was also made by Josiah Wedgwood about 1779 and lasted through ca. 1820. Some forms continued to be made until ca. 1835. Pearlware can be recognized by a bluish coloring appearing in foot-ring crevices and around handles (Noel Hume 1970:128-29).

Evidence of later disturbance in Unit 6 is indicated by a modern .22 calibre short cartridge which was found south of, and below, the brick floor. According to Willey's map a trench was excavated in this location in 1936 which may help explain the cartridge case. As was the case along the east palisade, evidence of a recent disturbance was not detected.

In Unit 13, located 105 feet from the blockhouse, another brick floor (Feature 13B) was located at a depth of 2.80 feet from the surface (Figure 3). It was evidence that this feature was not directly associated with Feature 8 (Unit 6), located 48 feet to the east, since Unit 9 had been excavated between Unit 6 and 13 and no features were found. This floor was made up of a double layer of bricks laid in no apparent pattern. The bottom section was intact except for the south edge, but most of the upper portion had been disturbed, and some bricks removed. A considerable number of artifacts, consisting mainly of ceramics, were recovered from the top of the brick rubble and floor.

As with Unit 6 (Feature 8) the ceramic types comprise a time span from ca. 1779 to ca. 1835. The most abundant type is pearlware. The pearlware and other ceramic types found in Unit 13B represent a time span between ca. 1779 through ca. 1820. These dates complement other data in associating this brick floor with the period of 1806-1821.

One ceramic type recovered from this level is known as Parian ware. It is a porcelain which was not glazed when fired, or perhaps given a slight "smear" glaze which gave it the dull appearance of polished marble. It was originally produced for making figurines in the early 1840's (Ramsay 1961:81). Since this ceramic type constitutes a later date than the above, possibilities exist for later intrusion.

Within Unit 13, as with Unit 6, the palisade ditch was not found. The palisade trench was found in Unit 14 adjacent to Unit 13 at a depth of 3.5 feet below the surface (Figure 4). As illustrated, considerable filling has occurred in this area.

The palisade ditch was again located along the south side of the area in Unit 25 at a depth of 4.50 feet below the surface. This unit
FORT HAWKINS
(9BI21)
SOIL PROFILE
EAST FACE-TRENCH 13 (A & B)
I TOPSOIL (LIGHT BROWN)  IV DISTURBED CLAY FILL (ORANGE BROWN)
II HUMUS (GRAYISH BROWN)  V BRICK LAYER & RUBBLE (RED)
III CLAY FILL (REDDISH ORANGE)  VI SUBSOIL (RED)

FIGURE 3
Figure 3

Profile of Brick Floor and Fill in Unit 13
FORT HAWKINS
(9B211)
PROFILE OF UNIT 14 (A)-EAST FACE

FIGURE 4
was excavated with a backhoe and considerable amounts of charcoal were found in the ditch, suggesting that the posts had been burned. Unit 25 revealed that the palisade ditch ended at this location suggesting a gap for a gate or some other feature, conceivably a structure somewhat suggestive of another blockhouse.

Two other trenches were excavated west of Unit 25, one with the backhoe, but no trace of a ditch was revealed.

Machinery was used the last three days to accomplish the goals set out for the project. A backhoe was used since considerable time had been expended along the south side due to the depth of the palisade ditch. It was necessary to try and follow the south palisade wall toward the west until it turned north or otherwise ended before work could commence along the west side.

The West Palisade

Excavations proceeded along the hedge parallel to Fort Hill Street. As seen in Figure 1 most of the units along the west palisade side were excavated by use of the backhoe.

The initial excavation, Unit 27, revealed a large, charred postmold at 1.0 feet below the surface. Protruding from it toward the south and northwest, was a feature resembling a narrow palisade ditch. Further trenching north of this unit did not produce any further feature evidence, except in Unit 29 which revealed a postmold in the south profile.

As excavations proceeded toward the north, south of the school sidewalk, Unit 31 at 1.0 feet below the surface revealed considerable amounts of brick and pieces of partially rotted wood. Plaster fragments were recovered from this unit with a whitewash finish indicating that the walls in this structure had been plastered and whitewashed. The feature encompassed the entire length of the trench.

North of the school sidewalk, Unit 32 was excavated beginning near the west hedge and continuing east with the soil profile consisting of subsoil beneath the topsoil and humus horizon. Approximately 30 feet from the schoolhouse, a large, heavily burned, round postmold was found at 1.4 feet from the surface.

From this location, units were excavated north and south with some units producing evidence of a palisade ditch. Sections of a palisade ditch were found in Units 32, 34, 34A, and 37.

Along this side the ditch was not as well defined as along the south and east sides, with no evidence of post occurring within the ditch. All occurrences of the palisade ditch were at a depth of 1.0 feet or less with the exception of Unit 37 where the ditch was found at a depth of 4.0 feet from the surface. At this depth it was not well
defined, and the stratigraphy indicated considerable disturbance above. The palisade ditch seemed to occur sporadically along the west side. Its nearness to the surface may be the reason for it not occurring in all the units.

In Units 22 and 40 a ditch was located which was slightly wider than the palisade ditch in the other units. Also it appeared to be slightly offset to the west of the palisade ditch. This may represent the east wall of an exterior structure. This same ditch was found in Unit 42, a vertical cut in the bank south of Woolfolk Street. The soil profile revealed that the ditch extended in the direction of the street. The ditch was situated 1.40 feet from the surface and was 1.40 feet in height.

The artifacts recovered from the units along the west side constitute mainly ceramics such as were recovered along the south and east sides, including window and bottle glass fragments, iron nails, and bone. Since this area was excavated with a backhoe, no specific proveniences can be given the artifacts recovered except to assign them to their primary units.

Interpretation of Archeological Data

The main result of the 1971 archeological investigation is that the east-west dimensions of Fort Hawkins are now known. It has been definitely ascertained that the size of Fort Hawkins is 290 feet from the east wall to the west wall. However, the question still remains as to whether the fort was actually square in plan or not. This information was impossible to retrieve since the northern wall of the palisade had been destroyed. Due to this it is impossible to present the complete plan incorporating the actual dimensions of Fort Hawkins.

Through the combined use of archeology and historic documents it was possible, in this instance, to clearly identify some major dimensions of Fort Hawkins and, by inference, conjecture the other dimensions. We therefore have, with some assurance, the complete plan of the fort.

The distance between the east and west wall is 290 feet (Figure 2). The distance between the north wall of the southeast blockhouse and the center of the 22 foot gap is 148 feet. This figure when doubled results in a conjectured dimension of 296 feet for the east palisade. The fact that the assumed north-south dimension coincides closely with that of the known east-west dimension is evidence that the 22 foot gap represents one of the areas mentioned in the historic account:

There were four long houses, one in the centre of each stockade to the width of each house, about twenty feet (Butler 1879:60).

The west palisade was extended a distance of 296 feet north to correspond with the assumed length of the east palisade. The conjec-
tured north wall was extended from the northeast corner west for 290 feet. The northwest corner was thus formed by the intersection of the west wall, and the conjectured north wall. A line was then extended diagonally in alignment with the northwest and southeast corners of the southeast blockhouse toward the conjectured northwest fort corner. This line intersected the presumed northwest corner. A conjecture of the northwest blockhouse was possible by projecting equal measurements obtained from the center of the southeast blockhouse.

The data presented in the above discussion provides us with substantial evidence indicating that Fort Hawkins did comprise a square configuration.

Plans similar to that of Fort Hawkins were used in the construction of both military and civilian forts of the period and extending into later times. Some examples are Fort Okanogan, built in 1811, by the Pacific Fur Company in the state of Washington, utilizing a square palisade wall with two blockhouses at opposite corners (Grabert 1968); Fort Spokane, built in 1812, also operated by the Pacific Fur Company, using a similar plan (Combes 1964); and Fort William, predecessor to Fort Laramie in Wyoming, built in 1834 and constructed on a similar plan as the above forts (Heib 1854).

Information is scarce regarding military forts constructed approximately the same time as Fort Hawkins for the southeastern part of the United States.

The East Palisade

Aside from the 22 foot gap along the east side, the ten foot gap is another feature which must be interpreted. Willey (1936 MS) concluded that the gap was a gate. The sketch drawn by E. D. Irving (Figure 2) indicates a gate along a palisade to the right of a blockhouse. It cannot be ascertained which blockhouse is represented in the sketch, but if one can assume that it is the southeast blockhouse, then a gate in this area would explain the gap along the east palisade. A further assumption can be made that even if this is not the southeast blockhouse represented in the sketch, a gate would have existed along two, and possibly all four palisade walls of the fort.

The palisade ditch was evident along the east side for a distance of 150 feet from the southeast blockhouse. Remains of posts are still in evidence, although not complete. The posts, and postmolds were square, lending support to the historic account which states that:

The stockade was built of posts of hewn timber fourteen feet long and fourteen inches thick (Butler 1879:60).

Although the posts were square, they were not fourteen inches thick. Measurements taken of the posts indicate that they varied between six
and eight-tenths of a foot in width in most cases. Willey (1936 MS) states that the posts were eight inches wide. The ditch is 1.1 feet wide with post encompassing an area of eight tenths feet and the remainder taken up by fill. The posts abutted against the west side of the ditch and the ditch was filled along the exterior side.

Samples of wood comprising the palisade were not taken. Willey (1936 MS) states that the wood used was pine. It seems reasonable that a coniferous species would have been used in making wooden posts 18 feet in length.

The palisade tends to angle slightly toward a westerly direction as can be seen in Figure 1. The south and west walls angle in comparable directions giving the fort a slight parallelogram shape. Willey (1936 MS) indicates that the east wall angled west at a rate of 23 inches (approx. 1.9 feet) every 40 feet.

The bricks found along the palisade ditch may comprise portions of structures which were destroyed. The palisade ditch along this side is a foot or less in depth. The 1936 excavation showed that posts and the palisade ditch along this side were located close to the surface, and seemed to rise as the ditch progressed northward.

The South Palisade

The 1936 excavation did not extend west very far from the blockhouse along this side, however, Willey (1936 MS) states that he found posts and a palisade ditch at two and one half feet (2.5 feet) below the surface. The present excavation revealed the palisade ditch in three units, 4, 14, and 25. In Unit 4 the ditch was located below two feet of fill, in Unit 14 over three and a half feet, and in Unit 25 the ditch was below four feet of fill.

During the 1936 excavation a brick foundation was found which Willey attributed to the post-Civil War period, based on the fact that brick was present. The brick structural remains found in the present excavations are presumed to be the same as, or related to, the structure found by Willey. Evidence gathered during the present excavation by elevation comparisons and ceramic analysis found in the context of the brick floor in Unit 13B, and Unit 6 (Feature 8) indicates that these structures were associated with the fort during its period of occupation by the military, and afterward. The structures may represent a kitchen or a mess area. No evidence of the palisade ditch was found in Units 6 and 13 presumably because the palisade walls were incorporated into the stockade. The southern edges of the foundation of both Feature 8 and 13B were not intact. Willey's map shows that posts were located 57 to 67 feet from the blockhouse. This area encompasses the location where the present Unit 6 was excavated. This may explain the disturbed brick floor and also the cartridge case.

The palisade ditch found in Unit 25 corresponds closely in eleva-
tion to the ones obtained in the preceding units. The ditch ends abruptly which suggests that a structure may have been associated with the wall at this location. The wall did not turn north to form a corner, and is approximately 15 feet east of where the southwest corner was conjectured to be located. A gate would not seem feasible situated so near to a corner, therefore, a structure such as a blockhouse may be conjectured for this corner until an accurate assessment can be made.

The West Palisade

The excavations along the west side of the palisade were mainly constructed with the use of a backhoe. Results of these excavations indicate that considerable disturbance has occurred in this area. The palisade ditch was located near the surface. In only one unit, 37, was the ditch found to be about four feet from the surface, and at this depth it was not easily recognizable.

In all of the locations where the ditch was found along this side, considerable burning had occurred.

The rubble-fill in Unit 31 located 70 feet west of the palisade ditch possibly represents a structure which was located outside the confines of the stockade. This was not an uncommon practice.

The wide ditch located near the north portion of the schoolyard, extending northward into Woolfolk Street, can be interpreted as a possible builders' trench dug after the fort ceased to function as a military complex. This interpretation is based on the nearness of the structural remains to the west palisade. A structure located so close to the wall would have made musket or rifle fire ineffective for defending the west wall from the northwest blockhouse.

Comparisons Between the East, South, and West Palisades

The ditch, comprising the east and west palisade remains, occurred near the surface, while the south palisade ditch was found beneath two and a half and four feet of fill (Figure 4). The reason for this is that disturbance, grading or otherwise, has occurred over the past 35 years, and earlier, in the schoolyard which is probably the reason that no evidence of the ditch was found near the north retaining wall. Also, along the west palisade side, considerable disturbance has occurred which may explain why the palisade ditch was not consistently located. The fill removed from the east and west palisade areas has been used to fill the area along the location of the south palisade.

Present elevations taken from the top of the palisade ditch along the east and south sides reveal that the south palisade was three feet lower. It is possible that in historic times the south palisade was
lower than the east palisade due to differences in the topography. De-
fensively, this would not have caused any great concern since there are
no topographic features near the south section of the hill upon which
the fort was situated which are at a higher elevation.

The feature found along the west palisade side in Unit 31 com-
prising brick rubble and rotten wood may represent a structure asso-
ciated with the fort during 1806-1821. If this structure was present
during the period of military occupation, it would tend to verify
Irvine's sketch (Figure 2) which depicts a high three-story structure
in the background. Furthermore, this would then also identify the
blockhouse in the left forefront as the southeast blockhouse, and the
gate would represent the ten foot gap located along the east palisade
ditch.

Artifact Discussion

The artifacts recovered during the course of the excavation con-
sisted mainly of ceramic fragments, although quantities of glass, both
bottle and window were found, as well as iron nails and bone.

Of the above, ceramic sherds probably represent the most important
items in helping to date an archeological site. Ceramics, as every-
things else, undergo typological and stylistic changes through time,
brught about by social preferences, conditions, etc. Dating may be
accomplished by utilizing historical records which may indicate that
a certain potter or factory in England produced a certain type and
style of pottery at a particular period of time. The above factors
can be combined to arrive at dates which can be very close to the
actual dates for the beginning and abandoning of a site. This can
be extremely useful when dates are not known for a specific archeo-
logical site.

The dates for the military occupation of Fort Hawkins are doc-
umented as 1806-1821. Information is scarce relating to the post-1812
occupation, although historical accounts (Butler 1879:77-78) indicate
that it was occupied and from this settlement the city of Macon was
founded.

The ceramic dates for Fort Hawkins based on sherds found in con-
trolled archeological contexts range between ca. 1779-ca. 1835 (Noel
Hume 1970:126-32; and South, personal communication). The ceramic
time span overlaps the military occupation by several years in either
direction, but certain factors must be considered. The date ca. 1779
represents the date when a specialized type or style of ceramic was
initially manufactured, but may have an ending date of ca. 1810 which
would correspond with the military occupation of Fort Hawkins. All of
the ceramic types found at Fort Hawkins and taken from known associated
architectural features, regardless of their initial manufacturing date,
fall within the 1806 through 1821 range.
Summary

The exploratory archeological investigations conducted by use of slot trenching, located the length of the south wall and demonstrated it to be 290 feet in length. Portions of the palisade ditch were revealed by slots along the east and west walls. The east palisade was found to be 148 feet from the north wall of the southeast blockhouse to a presumed mid-point and was conjectured to extend to a length of 296 feet. The west palisade was conjectured to be a length of 296 feet based on the information derived from the east palisade. The north wall is clearly shown to be in Woolfolk Street but may be assumed to be as long as the south wall or 290 feet.

The above dimensions (Figure 2) clearly outline the fort, suggest blockhouse locations and indicate locations of other structures. The area which the fort encompasses is not 14 acres, or even four acres but appears instead to be approximately one and one-half acres.
BIBLIOGRAPHY

BRYAN, BENJAMIN L.
MS Fort Hawkins -- Its History and Partial Reconstruction. On file at the Southeast Archeological Research Center, Macon, 1939.

BUTLER, JOHN C.
1879 Historical Record of Macon and Central Georgia. Middle Georgia Historical Society, Inc., Macon, 1969.

COMBES, JOHN D.

DOLLAR, CLYDE D.

GRABERT, G. F.

HIEB, DAVID L

HUME, IVOR NOEL

RAMSEY, L. L. G.

SOUTH, STANLEY

MSa An Examination of the Site of Fort Hawkins in Macon, Bibb County, Georgia, with an Evaluation of the Potential for Historical Archeology, with a View Toward Historic Site Development. On file at the Institute of Archeology and Anthropology, University of South Carolina, Columbia, 1970.

MSb Evolution and Horizon as Revealed in Ceramic Analysis in Historical Archeology. On file at the Institute of Archeology and Anthropology, University of South Carolina, Columbia, 1971.

WILLEY, GORDON R.
MS Report on Fort Hawkins Excavations. On file at the Southeast Archeological Research Center, Macon, 1936.
HISTORICAL ARCHAEOLOGY FORUM

Section One

Centered Around a Paper on English Ceramic Analysis

by

Stanley South

and

A Second Forum on Pipe Stem Formulas
INTRODUCTION

HISTORICAL ARCHAEOLOGY FORUM

This fifth HISTORICAL ARCHAEOLOGY FORUM is in two parts for the first time. The first forum is centered around my paper on ceramic analysis in which a mean ceramic date formula is presented, and the results of its application discussed and interpreted. The paper was sent to several archeologists interested in ceramic analysis and the comments of those who responded are included in this forum. It is hoped that the tools presented in my paper will be utilized by archeologists and the results published so that refinement of the formula and the concepts relating to it can be effected. Those interested in application of the formula to their own data should read not only the original paper, but my second paper in which I comment on the use of the formula by those who contributed to the forum. In this second paper I suggest the elimination of the use of Chinese Porcelain from the ceramic types used with the formula as a more accurate application for arriving at a mean ceramic date for their archeological sample.

The second part of this forum centers around the concept of tobacco pipe stem formulas. A new formula was presented by Robert Heighton and Kathleen Deagan in their paper, which was delivered at the conference. Shortly afterward I received a statement from Lewis Binford on Hanson's formulas as published in "Kaolin Pipe Stems--Boring in on a Fallacy" in Volume 4 of The Conference on Historic Site Archaeology Papers. I sent this paper of Binford's on to Hanson for comment, which he did. Paul Cresthull submitted some statistical tables and graphs relating to the Hanson and Binford formulas, and these, along with the other papers revolving around the pipe stem formulas are published here as the second part of this forum.

I would like to thank those participants who have contributed toward the success of this fifth HISTORICAL ARCHAEOLOGY FORUM.

Stanley South, Chairman
HISTORICAL ARCHAEOLOGY FORUM
The Conference on Historic Site Archaeology
EVOLUTION AND HORIZON AS REVEALED IN CERAMIC ANALYSIS
IN HISTORICAL ARCHEOLOGY

Stanley South

INTRODUCTION

In this paper we will examine the relationship between the manufacture period of ceramic types found on British American sites and the occupation period for the sites on which type fragments are found. We will present data indicating that on eighteenth century sites there is a high correlation between the ceramic manufacture dates and the site occupation period. We will also look at the effectiveness of ceramic analysis based on presence and absence as compared to quantification of fragments of ceramic types. The process of evolution and horizon as reflected in analysis of ceramics from historic sites will also be examined.

Terms

Attributes are those observable criteria by which a ceramic type has been defined, including shape, paste, hardness, design, decoration, color, glaze, etc. A type is a term used to refer to pottery defined by one or more key attributes. With historic ceramics a type is often distinguished on the basis of a single attribute.* Shape is used to the physical form of an object, such as a teapot shape, or a teacup shape. Form is a generalized term which includes shape, as well as those other attributes from which types are defined. Thus the evolutionary process is seen in the change of form through time.

Quantification

In 1960 I urged historical archeologists to utilize quantification of historic pottery based on frequency distribution, and illustrated the validity of statistically dealing with ceramics from colonial American sites (South 1962:1; Appendix I, this paper). The point made at that time was that quantification of European ceramics from eighteenth century British American sites would allow the archeologist to date the occupation period of a ruin. An assumption was that a comparison of the percentage relationships from enough historically dated ruins would allow a prediction to be made as to the occupation of ruins of unknown dates based on the frequency distribution of ceramic types.

The percentage relationships of ceramic types from various ruins in the mid-eighteenth century colonial English town of Brunswick, North
Carolina were compared. The bar graphs of ceramic types frequencies were found to be similar when similar occupation periods for the ruins were involved (Appendix I). Ruins having a beginning historical date in the 1760's could be separated from those having a beginning date in the 1730's based on the frequency occurrence of creamware, a separation not possible when using presence-absence alone. Historical archeologists were urged to use frequency occurrence in ceramic studies to further test the possibilities of this approach with historic site data.

As can be seen from the historic site literature since that admonition there has been no general rush toward frequency analysis of historic ceramics. There even seems to be an attitude held by some that quantification of pottery fragments on the historic site level will not reveal information of any significance beyond that gained from presence or absence of the ceramic types. In this paper we will present quantification data that tend to demonstrate that there are advantages to be gained through use of type fragment frequency in conjunction with certain analysis tools.

Type Manufacture Date and Deposition Date

In historical archeology the period during which artifacts were manufactured can be arrived at through documents, paintings, patent records, etc. The beginning date for the manufacture of a type may depend on the innovative action of one individual acting to introduce an additional attribute which is subsequently used to establish a type. The green glaze of the Whieldon-Wedgwood partnership developed in 1759, for instance, (Noël Hume 1970:124-25) which quickly went out of production, provides us with a known beginning manufacture date, and an end manufacture date probably no later than 1775. In many cases the end manufacture date cannot be fixed with the degree of accuracy of that of the beginning date. The point midway between the beginning and end manufacture date would be the median manufacture date, an important date for the purpose of this study. As Noël Hume points out, "The trick is to be able to date the artifacts..." (1970:11). The knowledge of manufacture dates for artifacts is an invaluable aid in the determination of occupation dates for historic sites. This is not to say that the manufacture date and the occupation date are the same, but rather than there is a connection between the two in that the manufacture date provides a *terminus post quem*, "a date after which the object must have found its way into the ground." (Noël Hume 1970:11). This is, as Noël Hume points out, "the cornerstone of all archaeological reasoning." However, there are those who believe there is such a slight connection between the date of manufacture and the date of deposition of ceramic type specimens on historic sites that they view as error any attempts to fix the occupation of sites by association of ceramics with the known date of manufacture (Dollar 1968:41-45). A major concern of this paper is to present data revolving around the artifact-manufacture-date and the artifact-deposition-date.
Evolution

Another major consideration here is the evolutionary concept of changing ceramic form through time as a dating tool as seen in fragments recovered from historic sites. Sixteen years ago this writer emphasized the necessarily intimate relationship between the process of archeology and evolutionary theory as a basic framework of archeology (South 1955). This paper also is anchored in the assumption that evolution of form is basic to the culture process and is the foundation for the "cornerstone of all archaeological reasoning" of which Noël Hume speaks in his discussion of terminus post quem.

Horizon

Through the excavation of a variety of eighteenth century historic sites I have become increasingly convinced that groups of ceramic types from different ruins of the same time period are similar enough to allow them to be used as dating tools for determining site occupation periods. This seems to be so regardless of whether the site is a remote frontier fort, a Cherokee village, a congested port town house, or a mansion. This has resulted in the development of analytical tools for use in determining the occupation dates for eighteenth century British American sites. These tools are useful and reliable when used on sites of varying functions over a broad area (Maryland, North Carolina, South Carolina). The explanation of this can be suggested in terms of the horizon concept (Willey and Phillips 1958:31-34), where the horizon is defined as:

\[ \text{a primarily spatial continuity represented by cultural traits and assemblages whose nature and mode of occurrence permit the assumption of a broad and rapid spread.} \]

The archaeological units linked by a horizon are thus assumed to be approximately contemporaneous (Willey and Phillips 1958:31-34).

This concept of a broad and rapid spread of groups of contemporaneous ceramic types in the eighteenth century is examined through the tools described in this paper.

The Unimodal Curve

The ceramic types are seen to represent a unimodal curve that had an inception (beginning manufacture date), a rise to popularity, and a decrease in popularity to extinction (end manufacture date). This basic assumption is expressed by Dunnell based on concepts outlined by Rouse, Ford, Phillips, and Griffin:
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The distribution of any historical or temporal class exhibits the form of a unimodal curve through time. The rationale for this assumption is that any idea or manifestation of an idea has an inception, a rise in popularity to a peak, and then a decrease in popularity to extinction (Dunnell 1970:309).

An example of this concept is seen in Mayer-Oakes' study of illumination methods used in Pennsylvania between 1850 and 1950 as cited by James A. Ford in *A Quantitative Method for Deriving Cultural Chronology*. Washington 1962, Figure 6.
THE PROBLEM

In the seventeenth century, British American settlements were relatively few and far between compared with those of the eighteenth century, and population density was considerably less. As a result there are fewer seventeenth century sites for archaeologists to examine. This, plus fewer historical references to the manufacture dates of ceramics, combine to limit our knowledge of seventeenth century ceramics. We do know that the lower class seventeenth century household had a much greater dependence on pewter, leather and wooden trenchers, and other vessel forms and less daily use of ceramics than did the gentry. From the ruins of the mansions of the seventeenth century we would therefore expect to find ceramics more abundantly represented than from ruins of the lower class homes (Noël Hume 1970:24; personal communication on October 26, 1971). This status difference is not seen to be reflected in ceramics from archeological sites in the eighteenth century.

Also to be considered is the fact that the limits of our present knowledge of seventeenth century ceramic manufacture dates and the temporally significant attributes within certain wares, results in a broader manufacture time span being assigned in comparison with the eighteenth century where short manufacture periods can be assigned to a number of marker types. As a result of this lack of refinement of our knowledge of seventeenth century ceramic types a comparison of manufacture dates with site occupation may well reveal less correlation than such a comparison made with data from eighteenth century sites. We might at first be inclined to interpret this as a time lag phenomenon, and indeed some time lag may well be involved in that with less use of ceramics in the lower class seventeenth century homes less breakage would naturally be expected to occur, resulting perhaps in a greater percentage of older ceramic types finding their way into the midden deposits. In the upper class homes, however, we would expect more ceramics and a closer correlation between manufacture dates and site occupation dates due to more frequent use in the home. However, as far as the time it took barrels of ceramics to make the trip from Britain to America aboard a vessel, there would be no appreciable difference between the seventeenth and the eighteenth century, in either case it was a relatively rapid process.

An hypothesis can also be constructed regarding a ceramic chronology model. Ceramic types found on colonial sites are well enough known from documents and kiln site excavations that an approximate beginning and end manufacture date can be assigned to ceramic types within certain limits of variability. Each of these ceramic types is seen to represent a unimodal curve through time as the type was introduced, reached a peak of popularity and then was discontinued. The median date for the ceramic types is the point mid-way through the duration of its period of manufacture. When the median date for a group of ceramic types is known, the types can be arranged so as to represent...
a chronology based on the median dates. Since such a chronology is based on documented duration periods of manufacture it is seen as an historical chronology, not a relative one such as those derived from stratigraphy and seriation on prehistoric sites. In constructing such a chronology, ceramic types such as locally made wares of unknown manufacture duration periods, or coarse English earthenwares of unknown periods of manufacture are not included for the obvious reason that they will contribute nothing to the chronology. If coarse earthenware and local wares of known periods of manufacture are present, they are most certainly to be used as valuable additions to the chronology model. From these postulates we can state that British ceramic types can be arranged in an historical chronology on the basis of the median known manufacture date, and this chronology reflects the evolutionary development of the ceramic forms through time. Colonial French and Spanish ceramics could also be arranged in a similar historical chronology provided the manufacture dates are known for the ceramic types. Once the approximate beginning and end manufacture dates of groups of historic artifact types such as wine bottles, wine glass, tobacco pipes, buttons, etc., are established, these too can be used to construct historical chronologies representing the evolution of form through time that in turn can be used to arrive at the duration of occupation of historic sites.

We can also state an hypothesis involving the horizon concept as defined by Willey and Phillips (1958:31-34). Eighteenth century English ceramics were manufactured in groups of several types at any one point in time, with some types having a shorter manufacture span than others. They were available in several types at the factories and groups of types were exported to British American ports. A limited number of these ceramic types were available on order through agents in Britain or through American outlets. Among those types available to the colonist was Chinese porcelain which took its place along with British ceramic types in the colonial American home. The purchasers of these ceramic types were no farther than a few days or weeks at the most from the remote frontier of the colonies, thus the possibility was present for the rapid distribution of ceramic types over a broad area (Noël Hume 1970:25). This broad and rapid spread of a limited number of ceramic types at any one point in time can be described as a horizon in which the cultural traits are approximately contemporaneous (Willey and Phillips 1958:31-34). Thus eighteenth century historic sites ceramics can be seen to represent a series of horizons in sequence.

Ceramic types of short manufacture duration are excellent temporal markers for determining the approximate brackets for the accumulation of the sample, allowing an interpretation to be made regarding the occupation period of the historic site. Such short-manufacture period types can be used effectively on a presence and absence basis as clues to sample accumulation. An important consideration here is that a ceramic type specimen cannot appear on a site prior to the beginning
manufacture date for the type, thus creating a temporal relationship between the manufacture date and the occupation of the site by those who used and broke the ceramic objects.

Regarding broken ceramics we can state a final hypothesis based on several postulates. The cultural use-patterns of the eighteenth century were such that not long after ceramic types arrived in the home in a town or frontier fort, breakage began to occur. The broken ceramic types were discarded and older types broken along with the most recent acquisitions resulted in a number of types becoming associated in the midden deposits. Although a few heirloom pieces would be broken along with a few of the most recent acquisitions, the majority of the fragments would represent those most in use during the occupation of the site. Those few most recent acquisitions would provide the clue for placing the end date on the deposit using presence-absence. From these postulates we can state that an approximate mean date for the ceramic sample representing occupation of an eighteenth century British American site can be determined through the median manufacture dates for the ceramic types and the frequency of the types in the sample. With these problems in mind we will construct tools for use in ceramic analysis to examine the data.
THE TOOLS

The Chronological Model for Constructing the Analysis Tools

The first step in constructing ceramic analysis tools is to build a chronological model upon which the tools can be based. An excellent example of the potential of historic site data in this regard is the use of hole measurement of tobacco pipestems by Harrington (1954) for arriving at an approximate date of the accumulation of the sample, and the expression of this by Binford (1961) in terms of a regression line formula. The pipestem analysis tool as well as our ceramic analysis tools and other constructions built on a chronological framework are based on the evolution of form through time.

Any unique combination of attributes, constituting a type that becomes extinct, represents a time capsule having a median date that can be fixed as an approximate point in time, provided the beginning and ending dates can be reasonably determined. If a series of overlapping ceramic types with known median dates can be determined historically and refined archeologically, we have a temporal scale by which we can fix a collection of ceramic types in time. If this scale is established through occurrence or frequency seriation, as is the case with prehistoric artifact types and classes, the seriation can be viewed as a gross chronology, verifiable only through carefully controlled stratigraphic studies designed to accompany the seriation, or through radiocarbon dating (Dunnell 1970:315). However, if previously dated groups of attributes representing historical stylistic types are used, such as Deetz and Dethlefsen (1966) have done with dated New England gravestones, there is a positive historical chronology involved that provides a more direct rather than a gross framework with which to work. In their study Deetz and Dethlefsen demonstrated variation in time and space because they were dealing with an artifact form that was a locally manufactured folk object. With the present ceramic study, however, a standardized factory product with a known manufacture period is involved, thus eliminating local variation. Therefore, with known historically based typologies such as those found in historical archeology, a specific chronology can be constructed in a manner not possible on the prehistoric level. Historical archeologists are only beginning to explore the possibilities offered by this unique quality of their historic site data toward the examination of cultural problems.

Historic site archeologists have constructed typologies of ceramics based on the references available to them and on their own observation, and these have been dealt with in temporal terms with varying degrees of success. Some have seen the numerous historic types and the accompanying documents as a confusing situation, and one not to be improved by attempts at typology and seriation of historic artifacts (Dollar 1968:14). Meanwhile, others have continued to define the diagnostic
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criteria for recognition of ceramic types in time and space with em­phasis on those attributes of color, surface finish, design, decoration, form, etc., by means of which delineation of types can be accomplished. One of the leaders in the field of English ceramics has been Ivor Noël Hume, Chief Archeologist at Colonial Williamsburg. Before the publica­tion of his book A Guide to Artifacts in Colonial America (1970), he and others were exposed to some criticism for what was seen as a lack of concern for artifact description based on specific criteria (Cleland and Fitting 1968). With the publication of this book, however, it is clear that Noël Hume is concerned with the determination of specific ceramic attributes that have significance in time and space. A book incorporating a definitive typology for English ceramics is still to be written. Meanwhile this book along with basic ceramic references can be used by the archeologist to acquire an acquaintance with the ceramic types found on British American sites. Noël Hume does not use quantification based on ceramic fragments from archeological sites, but prefers to use vessel shape along with presence and absence in his analysis. Some of us, on the other hand, have utilized specific attributes of ceramic types as Noël Hume has done, but have added the ingredient of frequency occurrence of the fragments as well as pre­sence and absence.

With the present availability of information regarding ceramic types, both descriptive and temporal, the historical archeologist should be able to explore the next step. For years to come we will continue to be concerned with description in historical archeology, as we should be, but we should not lose sight of the fact that this is not the goal, only the means toward attaining the goal. Lewis Bin­ford has quoted Sherwood L. Washburn, a physical anthropologist, in regard to this point:

The assumption seems to have been that description (whether morphological or metrical), if accurate enough and in sufficient quantity, could solve problems of process, pattern, and interpretation....But all that can be done with the initial descriptive information is to gain a first understanding, a sense of problem, and a prelim­inary classification. To get further requires an elabor­ation of theory and method along different lines (Binford and Binford 1968:26; after Washburn 1953:714-15).

It is time we began to construct hypotheses and tools with which to deal with historic site data. Descriptive typology, temporally an­chored in history is available for a number of classes of historic site artifacts. This descriptive base will be refined as more information becomes available. However, for illustrating the analytical tools in this paper we have confined ourselves to Noël Hume's criteria as seen in A Guide to Artifacts in Colonial America, and through personal com­munication with him and Audrey Noël Hume.
The procedure used to construct the model was to select seventy-eight ceramic types based on attributes of form, decoration, surface finish, hardness, etc., with the temporal dates assigned by Noël Hume for each type. These were given type numbers and classified according to the type of ware (Figure 1A), with page numbers following the types discussed in Noël Hume's book. Since Noël Hume has spent a lifetime attempting to define and delimit the attributes and temporal brackets for the manufacture of English ceramic types, his manufacture dates can be assumed to be based on the historical and archeological documents available to him at the time the book was written. These dates were recently updated in a conference with him. It should be emphasized that in arriving at the median manufacture date Noël Hume's generalized "1770's", was expressed as 1775 for the model, and that he frequently uses "about" and "around" and "c." to indicate that he is generalizing. The variation introduced by our conversion of these qualifying statements as definite dates is seen to be a relatively minor one when we consider the scale of the model we are building. In this study we are dealing with the ceramic types often seen on colonial sites in the English tradition, and comparable chronological models need to be constructed for sites reflecting French or Spanish tradition. This is illustrated by debased Rouen faience (Type 21) which is found on French sites to date around 1755, whereas on English sites it dates some twenty years later (Noël Hume 1970:141), clearly demonstrating the need for separate models for different cultural traditions.

Type 49, decorated delftware, is seen to have a time span of two hundred years (Figure 1A). Because of this a median manufacture date of 1650 was assigned for use when the site is obviously of the seventeenth century, and a date of 1750 for use when associated types are from the eighteenth century. This is the only deviation from the true median manufacture date that was used in this study, however, if other types having manufacture duration periods of from 140 to 160 years could be separated into more than one type having shorter temporal brackets the chronology would be considerably refined from that presently known for those types as presently defined. These types are "catch-all" in nature, such as types 26, 39, 49, and 65, and therefore reflect less sensitive temporal data.

The chronology might be extended through the nineteenth century by anyone interested in testing it during those decades, but our study only includes a few nineteenth century types. It should also be kept in mind that additional types can be added by the archeologist who knows the manufacture dates for such types, and it may well be found that some of the longer time span types can be eliminated from consideration until such time that diagnostic temporal attributes can be determined. Thus the degree of refinement of the model is dependent upon the degree of sophistication of the archeologist's ceramic knowledge. Because of this it might be argued that the more knowledgable archeologist may find he has little use for the analysis tools outlined
in this paper. The extent of usefulness of the tools presented here is yet to be determined, but we have found them useful. The archaeologist may well be able to distinguish white salt-glazed stoneware from creamware, pearlware, and "clouded" ware, but not be well acquainted with the manufacture brackets for the types. For such an archaeologist the tools presented in this paper may well assist him in interpreting the occupation period of his historic sites.

The Tools - Visually Interpreting the Occupation Period of the Site From a Sample Using Manufacture Duration Dates and Presence and Absence

Once the unimodal curve representing the duration of manufacture for each ceramic type in a sample from a site is plotted on a time line as a bar, and the type bars are arranged one above the other in a graphic manner, it is possible to see at a glance the limits for the duration of manufacture for all ceramic types. For instance, on the chart (Figure 1D) we see that most of the bars for the Charles Towne Site (38CH1) include a time span from 1580 to 1725. Immediately we can see that this surely indicates a relationship between the manufacture date and the occupation of the site. To demonstrate otherwise would take some doing. However, we are interested in narrowing the temporal bracket, and a method used by us for a number of years involves placing a vertical bracket to the left and right on the ceramic bar graph, with the resulting time span between being the interpreted period inside of which the occupation of the site took place. The placing of the left bracket is determined by choosing the point at which at least half of the ceramic type bars are touching or intersecting the bracket. The right bracket is placed generally using the same rule, however, it must be placed far enough to the right to at least touch the beginning of the latest type present. An exception to this are surface collections from sites revealing multiple occupation periods as revealed in a gap or discontinuity between the ceramic bars of the first occupation period and those of the later period. In such cases brackets for both occupations must be placed (see Goudy's, GN3, and Fort Prince George, PN1, in Figure 1D). Using this method we can place the brackets for site 38CH1 at 1650 and 1700, which happens to include the known historic date of the site of from 1670 to 1680. This is a tool that has proved most useful through the years in arriving at an interpreted occupation date for a site from ceramics from historic sites. It should be pointed out that this is entirely a presence-absence approach.

The time period can be further narrowed in some instances by consideration of the ceramic types conspicuously absent from the sample. For instance, the Goudy's Trading Post cellar hole from site 38GN1-5 (Figure 1D) has a bracket date range from 1740 to 1775, which can be narrowed when we realize that absent from the sample are types manufactured during the 1750's and 1760's usually present on sites of the
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1760's (Types 27, 33-36, 41, 42). If creamware (Type 22) was present, we would have to leave the bracketed date at 1775. In the absence of it as well as other types of the 1760's, we can assign an occupation date from approximately 1740 to the early 1760's for the cellar hole. This matches well the historical information that the site was occupied in 1751 and was attacked by Cherokee Indians and most of the buildings burned in 1760. This bracketing from ceramics alone is seen to work well in arriving at an occupation period for historic sites with known dates of occupation, and since this is the case we have it in the same manner on sites of unknown historic dates, such as Cherokee Indian village sites. This is basically a terminus post quem approach also using marker type absence to interpret an end occupation date.

A point we should make clear here is that in a sealed archeological deposit the beginning manufacture date for the latest type present gives us a date after which the deposit was made. This is the traditional terminus post quem. The interpretive tools we are discussing here are designed to assist us in going beyond merely determining the date of the fill, and allowing us to make an interpretation as to the occupation period reflected by the ceramics in the deposit. This information is not based solely on the latest ceramic type present, but is interpreted through the frequency of other ceramic types. We should keep in mind the nature of the deposit, which may have an important bearing on our interpreted occupation brackets. For instance, if the fill is an accumulation of midden thrown from a house over a long period of time we would expect a different result than if the cellar hole was filled at one moment in time using soil and refuse collected from other areas of the site. In the latter case the fill would have no bearing on the structure represented by that particular cellar hole. However, our interpreted occupation period in either case would be based on the ceramic fragments in hand, and whether they are from a single feature, a combination of features, a cellar hole, or are the sum of every sherd recovered from the ruin site (such as is the case with the Brunswick Town and Fort Prince George samples), an interpreted occupation period represented by the sample will emerge. The judgment of the archeologist is important here as to the significance of the interpreted occupation period. The validity of the interpreted occupation period would still depend on the nature of the archeological data on which it is based.

On sites such as Brunswick Town, Fort Prince George, Goudy's Trading Post, Fort Moore and Charles Towne there has been little occupation since the eighteenth century period use of these sites. In high density urban occupation areas there may well be continuous occupation to the present. Because of this it would be necessary to isolate features from high density sites and deal with these so as to reduce the effect of later ceramic types, whereas on sites such as Brunswick, Fort Moore, Fort Prince George, etc., every sherd from the site can be included in our sample and still allow an interpreted occupation period relative to the eighteenth century. We should keep in mind the fact that in
discussing **occupation periods** represented by ceramics we are dealing with cultural generalities and not historical specifics. For instance our occupation periods interpreted from ceramics as revealed on the chart in Figure 1 vary from fifteen years in duration to eighty years, but we should also notice that these brackets most often do include the known historic occupation period for the sites.

Similar versions of this interpretive tool have long been used by some historic site archeologists for arriving at an approximate occupation period for their sites. However, a drawback is that it does not take frequency into consideration, and a single sherd of creamware (Type 22), for instance, has the same weight as five hundred sherds of white salt-glazed stoneware in determining the approximate temporal range for the sample. Consideration of frequency of occurrence would certainly place the relationship between the types in a more valid perspective than presence-absence alone. In order to consider both presence-absence and frequency in the determination of our approximate occupation period, we have devised a formula useful in arriving at a mean ceramic date for a group of ceramic types from an historic site. This date can then be used with the historical data, or with **terminus post quem** dates to arrive at an interpreted occupation period represented by the sample. This date can also be compared with mean pipestem dates, as well as with other artifact data to arrive at an interpretation of the site occupation period.

The **Tools** - The Mean Ceramic Date Formula Using Presence-Absence and Frequency

The mean manufacture date for the group of British ceramic types from an eighteenth century historic site taking into consideration the frequency of occurrence of fragments of the types, can be determined by a mean ceramic date-frequency formula as follows:

The mean ceramic date, \( Y \), is expressed:

\[
Y = \frac{\sum_{i=1}^{n} X_i \cdot f_i}{\sum_{i=1}^{n} f_i}
\]

Where \( X_i \) = the median date for the manufacture of each ceramic type  
\( f_i \) = the frequency of each ceramic type  
\( n \) = the number of ceramic types in the sample

The median manufacture date for each ceramic type in the sample is determined from the documents, and in this study we have derived this from the book by Noël Hume (1970), and through personal communication.

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with him. This information is seen in the list of ceramic types in Figure 1A. In order to use the formula the sherd count for each type is placed in a column beside the median date and these are multiplied, producing a third column, which is a product of the median date times the frequency of occurrence. The sum of the frequency column is divided into the sum of the product column, producing the mean ceramic date for the sample. Although this frequency-adjusted manufacture date might be assumed not to have anything to do with the occupation date for an historic site, we will see that there is a remarkable degree of similarity between the mean ceramic date derived from use of the formula and the historically known median occupation date of the eighteenth century historic sites on which it has been used.

Note: Before proceeding to use the formula the reader should also read my later paper in this forum in which it is recommended that Types 26 and 39, Chinese Porcelain, not be included. By eliminating these types from use in the formula a more accurate mean ceramic date is obtained.
FIGURE 1

CERAMIC ANALYSIS TOOLS FOR EIGHTEENTH CENTURY COLONIAL ENGLISH SITES
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APPLICATION OF THE TOOLS

Applicability

The beauty of the Binford (1961) and the Hanson (1971) formulas for dating tobacco pipestems is the fact that anyone can pick up a set of drills and proceed to measure a sample and arrive at a mean pipestem bore size from which a mean date for the accumulation of the sample can be determined. The mean ceramic date formula is not as easily applied since the user must know something about British ceramic types before he can determine a mean ceramic date from a group of types. If he has little understanding of the attributes for separating the seventy-eight types used in the model he will not get far in arriving at a meaningful mean ceramic date from the formula. For the formula to be used, therefore, a knowledge of ceramic types is necessary, which can be learned from the many references available. This reference work must be combined with a familiarity with the archeological specimens. A knowledge of the ceramic type attributes cannot be overemphasized for there are far too many meaningless descriptions appearing in the historic site literature now in spite of the availability of numerous excellent sources to act as guides for learning. It is totally meaningless to describe a ceramic type as being "Whieldonware or Rockingham ware" (Harris 1971:67), types with a source of origin separated by the Atlantic Ocean and one hundred years in time. Historical archeology is plagued by reports revealing no interpretation of any kind, historical, anthropological, cultural, or archeological to justify a catalog type publication of objects. To use the mean ceramic date formula, therefore, there is no easy way out. The archeologist should have more than a passing knowledge of the ceramic types with which he deals. Some archeologists may prefer to deal primarily with a terminus post quem date for a deposit, and feel they have no need for a median date such as the formula provides. Others may find it useful in the interpretation of site occupation periods.

The Sample

The size of the sample cannot always be controlled by the archeologist due to the fact that only seven sherds may be recovered from a feature from which he wishes to apply his ceramic analysis tools. He should remember, however, that a sample of that size would be somewhat less reliable than one of a much larger size. The nature of the sample would most certainly also have a bearing on the date that results from any interpretive analysis of the ceramics. For instance, a sloppily excavated cellar hole where poor contextual control was maintained by the archeologist might contain fragments of creamware or ironstone that fell into the hole during excavation from layers out-
side the actual contents of the cellar fill, or were carelessly thrown into the bag by an irresponsible worker. These fragments would require a much later date to be assigned to the feature than would have been the case had these one or two fragments been allowed to intrude upon the sample from the context of the cellar. The importance of tight provenience control in the field cannot be overemphasized (unless the reasons for the control are not understood by the practitioner and an unnecessarily expensive and fruitless nit-picking approach is used to no effectual end, as is too frequently witnessed on historic sites). A large, tightly controlled sample is desirable, regardless of the length of time a site was occupied. In the absence of a large sample, however, the tools described here can still be used but the reliability might naturally be expected to be less.

Instead of the frequency occurrence based on individual sherds by ceramic type as we have done in this study, quantification by type and shape could as well be used, and in some instances where shape is a sensitive attribute, a more refined temporal bracket may result. It is through an analysis of shape (teacups, saucers, plates, platters, mugs, etc.) that this writer feels that certain sensitive cultural differences may be reflected. Our present study is concerned, however, with ceramic type analysis as a reflector of the occupation period of historic sites.

Noël Hume has provided us with a frequency tabulation for the ceramic types from the Trebell Site Cellar (TS 807C) by object and by sherd count. With a cellar fill date of c.1810, and a construction date of c.1769, based on creamware, the median date should be around 1790. The documents indicate a probable occupation period from 1768 to 1826, with a median date of 1797. Using both sets of data with the formula we obtain a mean ceramic date of 1780.5 using the object count and 1788.9 using the sherd count. This would tend to point to a more accurate formula date using sherd count than when an object count is used.

The Technique of Application of the Visual Bracketing Tool to Historic Site Ceramic Samples

In Figure 10 eleven sites have been plotted with the following information graphically shown. The duration of manufacture of each ceramic type has been plotted as a bar against a time line. The known historic occupation period is plotted as a heavy horizontal bar with arrows indicating the approximate beginning and end dates as determined from the documents. The visual bracket for the interpretive occupation period of the site is plotted as two vertical lines that touch at least half of the ceramic type bars on both ends. The mean ceramic date for the site sample derived from the use of the ceramic date formula is plotted as a vertical line of large dots, with the pipestem date represented as a vertical line of small dots. The influence of absent ceramic types within a zone where they are usually found on historic sites is plotted as a
The Technique of Application of the Mean Ceramic Date Formula to Historic Site Ceramic Samples

An example of this process is illustrated by unit S7 in the ruined town of Brunswick, North Carolina. This ruin was a stone-lined cellar located on lot 71 in Brunswick (South 1959). The records reveal that the structure was probably standing by 1734, and was burned in 1776. The collection of ceramic material from the entire ruin was used as the sample. The historic date would bracket the period from 1734 to 1776, with a median historic date of 1755.

(Coded by subtracting 1700 from the Type Median)

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<th>Ceramic Type</th>
<th>Type Median (X_i)</th>
<th>Sherd Count (f_i)</th>
<th>Product</th>
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<td>91</td>
<td>43953</td>
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<td>67</td>
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<td>53,54</td>
<td>1733</td>
<td>33</td>
<td>1716</td>
</tr>
<tr>
<td>56</td>
<td>1733</td>
<td>33</td>
<td>9438</td>
</tr>
<tr>
<td>29</td>
<td>1760</td>
<td>60</td>
<td>2263</td>
</tr>
</tbody>
</table>

The mean ceramic date formula

\[ Y = \frac{\sum_{i=1}^{n} X_i \cdot f_i}{\sum_{i=1}^{n} f_i} \]

\[ Y = \frac{123657}{2263} + 1700 = 1754.6 \]

It is interesting to note that the mean ceramic date derived from the formula is the same as the known median historic date for the ruin. As we will see, this appears to be more than a coincidence. The pipe-stem date for this ruin using the Binford formula (1961) is 1756, revealing an interesting correlation between historic, ceramic, and pipe-stem dates.
HISTORICAL ARCHAEOLOGY FORUM - South

Ceramic Analysis of Samples from Historic Sites

Charles Towne (38CH1) The First English Fortification in South Carolina

Each of the eleven sites on the chart (Figure 1D) can be discussed to reveal various aspects seen in refining a temporal bracket for the occupation of a site through ceramics using the methods outlined here. Our discussion will follow the chronological chart from bottom to top (Figure 1D), beginning with the fortification ditch dug by the first Charles Towne settlers in South Carolina in 1670, and abandoned by 1680, provided a median historic date of 1675. The bracketing tool reveals a date from 1650 to 1700, which includes the historic occupation period. Attempting to narrow this date by means of the mean ceramic date formula produces a date of 1654.4 some twenty-one years prior to the known historic median date. This difference may well reflect our present knowledge of the ceramic types from which the mean date was derived. It may also reflect a time lag by the latest items not being present in the households at Charles Towne when the first settlers arrived in 1670. This gap may also relate to the fact that far more references are available to leather and wooden trenchers being in the town than ceramics revealing, perhaps, less daily use of ceramic items and thus less breakage (South 1971 MS). In this case the breakage that did occur would reveal a greater time lag than is seen on eighteenth century sites where ceramics came into more daily use and breakage. This hypothesis needs to be checked by the use of the mean ceramic date formula on more seventeenth century sites of known occupation dates. This time lag may well be found to be a factor present on any seventeenth century site, in which case the formula can be altered to take this into consideration once enough data is at hand from seventeenth century sites. The pipe-stem date from this feature is also too early, being 1667 (Hanson 1971: 2), again possibly reflecting a true time-lag situation with artifacts in the seventeenth century. From this site we see an exception to the high reliability seen in the use of the mean ceramic date formula on sites of the eighteenth century. Noël Hume has pointed out that on seventeenth century sites of the wealthy class he has found many ceramic types represented, with little time lag being evident, whereas on the ruins of the less affluent there are definitely fewer ceramic types present, thus revealing a socioeconomic distinction not seen to exist on sites of the eighteenth century (Noël Hume personal communication).

The First Fort Moore? (38AK4-15) An Eighteenth Century Frontier Fort and Trading Post

The second site is a cellar hole of a timber and clay structure with a clay chimney, located on the bank of the Savannah River at the historic site of Fort Moore, South Carolina. The first Fort Moore was built in 1716, and a second one was ordered built in 1747 with the site going into private hands in 1766. This site was excavated during the summer of 1971 by Richard Polhemus, Assistant Archaeologist of the Institute of Archeology and Anthropology at the University of South Carolina. Using the
bracketing method we can see that the site was likely occupied between 1700 and 1775. The mean ceramic date formula produces a date of 1726.1, not far from the historic median date for the first Fort Moore of 1732. The presence of creamware (Type 22) (two sherds in the top layer of the cellar), but the absence of pearlware (Type 17), does not allow us to narrow the date bracket using absence (shaded area of the graph). The Hanson pipestem formula produces a date of 1730.9. These early dates within the known historic range for the occupation of the first Fort Moore allow us to interpret this cellar and this area of the site as likely that for the first Fort Moore. Even though creamware is present in the top layer of fill, providing us with a terminus post quem date for the final filling of the cellar, the frequency of types of the earlier period is such that a first Fort Moore period of occupation is interpreted as being represented by the ceramic sample.

Fort Moore (38AK5-A) An Eighteenth Century Frontier Fort and Trading Post

One hundred yards away from the cellar just discussed another cellar of the same type of construction was excavated some years ago, and the material from this cellar is stored at the Institute of Archaeology and Anthropology at the University of South Carolina. The bottom two feet of this cellar fill was used in the ceramic analysis, which contained the large majority of the ceramics present. The bracketing bars reveal a likely date of 1700 to 1775 for the occupation of the site. However, the fact that there is an absence of types 22, 28, 33, 35, and 36, usually seen on sites of the 1760's and 70's, this range can be narrowed to include the period from 1700 to the early 1760's. The mean ceramic date formula produced a date of 1741.7 and the pipestem date was 1744.16. The mean ceramic date is virtually the same as the known median historic date of 1741 for the occupation of Fort Moore from 1716 to 1766.

From the use of the bracketing and mean ceramic date tools on the Fort Moore site it was possible to separate a ceramic sample from a cellar likely representing the entire occupation of Fort Moore, from a cellar with a ceramic sample interpreted as representing the occupation period of the first Fort Moore. An interesting point here is that the cellar having the earliest mean ceramic date has creamware present in the fill, whereas the cellar without creamware has a later mean ceramic date, the reverse of what one might interpret from presence-absence alone. This illustrates the potential value of the mean ceramic date in such instances, particularly when supported by the same relationship between the pipestem dates as seen here. This does not mean we ignore the terminus post quem date indicated by creamware for the final fill of the cellar. It does mean that we are giving consideration to the mass of the ceramics rather than to the latest type on the sample (perhaps represented by a single sherd), when it comes to interpreting the major
HISTORICAL ARCHAEOLOGY FORUM - South

occupation period represented by the collection.

Brunswick Town, North Carolina (S7) A Colonial English Port Town

We have discussed this ruin previously and found the historic median to be 1755, the mean ceramic date to be 1754.6, and the mean pipestem date to be 1756. Other Brunswick Town ruins demonstrate the following comparison between the historic median and the ceramic formula mean:

- S15 historic median date 1751.0
  - ceramic formula date 1746.4
  - pipestem date 1748.0

- S2 historic median date 1754.0
  - ceramic formula date 1749.0
  - pipestem date 1748.0

- N1 historic median date 1754.0
  - ceramic formula date 1750.1

- S18 historic median date 1769.5
  - ceramic formula date 1776.2
  - pipestem date 1756

Large samples, such as those from Brunswick Town are particularly desirable for use with the mean ceramic date formula (see tables in Appendix).

Goudy's Trading Post at Fort Ninety Six, South Carolina (38GN1-3 and 38GN1-5)

Goudy's Trading Post at Ninety Six, South Carolina, was begun in 1751 and was attacked and burned in 1760. Preliminary excavation revealed a small cellar hole with some eighteenth century objects in the top surface of the fill. The cellar is yet to be excavated. Only four ceramic types and a total of seven sherds were recovered, but these were used to attempt to date the deposit using the tools under discussion here. The median historic date is 1756, with a mean ceramic date of 1754.6, an impressive match using only seven sherds. However, without the known historic date we can establish a duration using our bracketing tool of from around 1740 to 1775. In the absence of types 27, 33-36, 41, 42 (representing the types likely to be present if the sample dated from the 1760's), and also using the mean ceramic formula date of 1754.6, we could say that the deposit represents an approximate date range of from around 1744 to the early 1760's, impressively close to our 1751 to 1760 historic data. We have arrived at this date using the ceramic analysis tools here under discussion, and not our historic data.

The surface layer and plowed soil zone of Goudy's Trading Post site revealed creamware, which was absent from the cellar hole sample. This sample was designated 38GN1-3, and has an historic occupation date of unknown length after the first occupation of 1751 and the fire of 1760. From the mean ceramic date formula we determine a date of 1769.3, and
with this and our known beginning date of 1751 as half of our date range, we can conjecture a date from 1751 to around 1787 for the period represented by the sample, since if we know the mean date and one end we can interpret the approximate position of the opposite bracket. It should be noted that one sherd of whiteware was found on the site in the plowed soil (Type 2), and because of the absence of pearlwares, this clearly reveals a disconformity between it and the other ceramic types, reflecting a post 1820 occupation and not a continuous one.

Fort Prince George, South Carolina (38PN1) A British Military Post on the Cherokee Frontier

Fort Prince George was built by Governor Glen of South Carolina in 1753, and the last reference to it is in 1768 when it was abandoned. The median historic date is 1761. The site was dug by John Combes, Assistant Director of the Institute of Archeology and Anthropology, University of South Carolina. The ceramic sample includes all sherds recovered from the entire site. From the bracketing technique of the ceramic type bars we arrive at a date of around 1745 to 1775 for the site. The mean ceramic date formula reveals a mean date of 1763.0, and the pipestem date is 1750.14 (Hanson 1971:2). In this case the mean ceramic formula date is much closer to the median date for the site than is the pipestem date. Without the known historic date we might take our interpreted end date of 1775 and the mean ceramic formula date of 1763, and conjecture a date bracket of from 1751 to 1775, again not far removed from the known occupation of 1753 to 1768.

The Paca House, Annapolis, Maryland (19J,27B) A Town House Mansion

The Paca House was built in 1763 by William Paca, signer of the Declaration of Independence, and is still standing and in the process of being restored. Archeological work was carried out there in 1967 by this writer (through a contract with Contract Archeology, Inc.) and two eighteenth century midden deposits were discovered still relatively undisturbed (South 1967 MS). These were combined for this analysis. The median historic date for the sample is not known, but the context in which the midden was found indicates that it was among the earliest midden thrown from the house after it was constructed in 1763. The presence of creamware and one piece of pearlware, however, indicate that the midden received material at least as late as the 1780's. The mean ceramic formula date for the deposit is 1763.1. The left and right bracketing lines fall at 1720 and 1780, and using the mean ceramic date of 1763, we can narrow our interpreted date range to 1748 to 1780.
The Dump at Brunswick Town (S10)

Nath Moore's Front in Brunswick Town (ruin S10) was burned in 1776 (South 1958) and the interior of the stone foundation wall for the cellar was used as a garbage dump for some years afterward, in fact, judging from the whiteware present it was used into the 1830's. The last reference to anyone living in Brunswick was in the early 1830's. The median historic date for the dump would be 1803. Using the vertical brackets we arrive at a date of from 1740 to 1820. The mean ceramic date is found to be 1794.0, not too far from the historic median date of 1803. An interesting feature of this ceramic profile is the continuation of the overlapping ceramic type-bars throughout the period of the Revolution into the early decades of the nineteenth century.

The Nipper Creek Site (38RD18)

No historical information is available on this pit, which was located in a bulldozed area of an Archaic Indian site. The brackets point to a short time span from 1795 to 1815, with a mean ceramic formula date of 1801.3. The absence of types of the 1815-35 period indicate that this ceramic sample can be interpreted as representing an occupation period from around 1795 to about 1810.

Tallassee A Nineteenth Century Cherokee Indian? House Site in Tennessee

The historic information available on this site indicates that it was transferred from Indian to White hands in the early nineteenth century. Other than this no information is available, except that a quantity of Cherokee ceramic types were found associated with the house ruin, suggesting possible Indian occupants.

The mean ceramic formula date was found to be 1818.1. In the absence of type 2 we would interpret a date bracket of from 1800 to 1820 as the likely range for the occupation represented by the sample.

Additional Cherokee Indian Village Sites Not Shown in Figure 1
The Rock Turtle Site (38PN4) An Eighteenth Century Indian Village Site

One hundred yards from the site of Fort Prince George a Cherokee Indian village site (38PN4) was tested, and revealed ceramic types producing a mean ceramic formula date of 1749.7, and a Hanson pipestem date of 1756.36. There is no historic data associated with the site other than its close association with Fort Prince George and the eighteenth century Cherokee Town site of Keowee.
## COMPARATIVE TABLE OF CERAMIC ANALYSIS DATA

**FIGURE 2**

<table>
<thead>
<tr>
<th>Site</th>
<th>Historical Date Range</th>
<th>Bar Graph Date Range</th>
<th>Historical Median Date</th>
<th>Mean Ceramic Formula Date</th>
<th>Years Away From Historical Median With Quantification</th>
<th>Pipestem Date</th>
<th>Formula Date Without Quantification</th>
<th>Years Away From Historical Median Without Quantification</th>
<th>Site Name</th>
</tr>
</thead>
<tbody>
<tr>
<td>38CH1</td>
<td>1670-1680</td>
<td>1650-1685</td>
<td>1675</td>
<td>1654 (21)</td>
<td>1667</td>
<td></td>
<td>1661 (14)</td>
<td></td>
<td>Charles Towne</td>
</tr>
<tr>
<td>38AK4-15</td>
<td>1716-1747</td>
<td>1725-1775</td>
<td>1732</td>
<td>1726 (6)</td>
<td>1731</td>
<td>1736 (4)</td>
<td>1738 (3)</td>
<td></td>
<td>1st Ft. Moore</td>
</tr>
<tr>
<td>38AK5-A</td>
<td>1716-1766</td>
<td>1725-1775</td>
<td>1741</td>
<td>1742 (1)</td>
<td>1744</td>
<td>1738 (3)</td>
<td>1749 (6)</td>
<td></td>
<td>Ft. Moore</td>
</tr>
<tr>
<td>S7</td>
<td>1734-1776</td>
<td>1740-1775</td>
<td>1755</td>
<td>1755 (0)</td>
<td>1756</td>
<td>1752 (4)</td>
<td>1759 (6)</td>
<td></td>
<td>Brunswick</td>
</tr>
<tr>
<td>38GN1-5</td>
<td>1751-1760</td>
<td>1745-1775</td>
<td>1756</td>
<td>1755 (1)</td>
<td>-</td>
<td>1755 (6)</td>
<td>1752 (4)</td>
<td></td>
<td>Goudy's Post</td>
</tr>
<tr>
<td>38GN1-3</td>
<td>1751-?</td>
<td>1740-1775</td>
<td>-</td>
<td>1769 (2)</td>
<td>-</td>
<td>-</td>
<td>1773 (30)</td>
<td></td>
<td>Goudy's Post</td>
</tr>
<tr>
<td>38PN1</td>
<td>1753-1768</td>
<td>1740-1775</td>
<td>1761</td>
<td>1763</td>
<td>1753</td>
<td></td>
<td>1773 (30)</td>
<td></td>
<td>Ft. Prince</td>
</tr>
<tr>
<td>Paca</td>
<td>1763-?</td>
<td>1750-1780</td>
<td>-</td>
<td>1763</td>
<td>1753</td>
<td></td>
<td>1773 (30)</td>
<td></td>
<td>(George)</td>
</tr>
<tr>
<td>(19J,28B)</td>
<td>c.1800-?</td>
<td>1800-1815</td>
<td>?</td>
<td>1801</td>
<td>-</td>
<td>-</td>
<td>1818 (17)</td>
<td></td>
<td>Paca House</td>
</tr>
<tr>
<td>S10</td>
<td>1776-1830</td>
<td>1740-1820</td>
<td>1803</td>
<td>1794 (9)</td>
<td>-</td>
<td>1773 (30)</td>
<td>1818 (17)</td>
<td></td>
<td>Bruns. Dump</td>
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<tr>
<td>38RD18</td>
<td>?</td>
<td>1795-1805</td>
<td>?</td>
<td>1801</td>
<td>-</td>
<td>-</td>
<td>1818 (17)</td>
<td></td>
<td>Nipper Creek</td>
</tr>
<tr>
<td>Tallassee</td>
<td>c.1800-?</td>
<td>1800-1815</td>
<td>?</td>
<td>1818</td>
<td>-</td>
<td>-</td>
<td>1818 (17)</td>
<td></td>
<td>Tallassee</td>
</tr>
<tr>
<td>S18</td>
<td>1763-1776</td>
<td>1740-1775</td>
<td>1770</td>
<td>1776 (6)</td>
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<td>1753 (17)</td>
<td>1753 (17)</td>
<td></td>
<td>Brunswick</td>
</tr>
<tr>
<td>S15</td>
<td>1726?-1776</td>
<td>1740-1775</td>
<td>1751?</td>
<td>1746 (5)</td>
<td>1748</td>
<td>1755 (4)</td>
<td>1755 (4)</td>
<td></td>
<td>Brunswick</td>
</tr>
<tr>
<td>N1</td>
<td>1731-1776</td>
<td>1740-1775</td>
<td>1754</td>
<td>1750 (4)</td>
<td>-</td>
<td>1746 (8)</td>
<td>1746 (8)</td>
<td></td>
<td>Brunswick</td>
</tr>
<tr>
<td>S2</td>
<td>1731-1776</td>
<td>1740-1775</td>
<td>1754</td>
<td>1749 (5)</td>
<td>1748</td>
<td>1757 (3)</td>
<td>1757 (3)</td>
<td></td>
<td>Brunswick</td>
</tr>
<tr>
<td>38PN4</td>
<td>18th Cent.</td>
<td>1750-1775</td>
<td>?</td>
<td>1750</td>
<td>1756</td>
<td></td>
<td>1756 (30)</td>
<td></td>
<td>Rock Turtle</td>
</tr>
<tr>
<td>38OC3</td>
<td>18th Cent.</td>
<td>1725-1780</td>
<td>?</td>
<td>1736</td>
<td>-</td>
<td>-</td>
<td>1736 (30)</td>
<td></td>
<td>Toxaway</td>
</tr>
</tbody>
</table>

Average Years From Historical Median With Quantification (4) Average Years From Historical Median Without Quantification (8)
This Indian village site was excavated by John Combes some years ago. The absence of creamware, and the presence of pearlware and nineteenth century stoneware clearly reveal a nineteenth century occupation distinct from the eighteenth century occupation represented by white salt-glazed stoneware and combed yellow slipware. For this reason two dates were determined for this site. This is an excellent example of two occupation periods clearly revealed through the absence of a major ceramic type, in this case, creamware. If creamware were present there would be no archeological justification for separating the ceramic groups for obtaining separate mean ceramic dates since there would be a continuous sequence of types represented.

A Discussion of the Reliability of the Ceramic Analysis Tools

The measure of the reliability of the temporal bracketing and mean ceramic formula analysis tools is the degree of correlation between the interpreted dates and the known historic dates for the particular site. Prehistorians do not have such a readily available check on their chronologies and seriations. As we have seen with the individual samples from various historic sites the bracketing and mean ceramic tools, along with presence-absence consideration, allows a relatively high percentage of correlation between the interpreted and the historically known dates. The table (Figure 2) illustrates the comparison between the historical bracket and median date, and the visual bracketing tool and the mean ceramic formula date for those sites in this study, with a detailed tabulation in the Appendix. The correlation between the historical median date for a site and the mean ceramic formula date is seen to be quite high in most instances. What is needed now is more application of the tools to determine the limits of reliability on a broader time and space frame of reference.

To judge the role of quantification in the mean ceramic date formula between the known historical median date and the formula date, we substituted the frequency of one for each of the ceramic types and thereby nullified the effect of quantification on the date derived from the formula. This reduced the formula to a presence-absence tool, and by comparing the date thus determined with the ceramic formula date, we can see which is closer to the historical median. This comparison can be seen on the chart in Figure 2. This reveals a slight advantage in reliability when using quantification as opposed to presence-absence along. This advantage can be seen by comparing the number of years away from the historical median are the formula dates with frequency and without frequency being considered. Using frequency only one date is as much as nine years from the known historical median for the occupation of the site, whereas without considering frequency two of the ten sites are seventeen and thirty years distant from the known historical median. The average deviation from the historical median date using frequency is only four years, whereas the average deviation without consideration of frequency is eight years, or twice that when frequency is considered. Our conclusion from this is that frequency consideration appears to have
a refinement advantage over presence-absence when used with the mean ceramic date formula.

From this average four years variation from the known historic median occupation for the ten eighteenth century sites in this study we can make an additional refinement of our mean ceramic date. We can now state that when frequency is considered, the mean ceramic date derived can be followed by an average deviation of plus or minus four years on sites of the eighteenth century. As the ceramic collection from a larger number of sites are examined with this formula, this plus or minus factor can be refined as the data indicates. Without using frequency by type, thus utilizing the formula strictly on a presence-absence basis, a plus or minus eight years should be added to the mean ceramic date thus derived. The number of plus or minus years may well be found to vary by area as groups of sites are tested using this formula. Such variation may be found to reflect areal cultural variation within the broader cultural horizon.

We will now look at the one seventeenth century site represented in this study, the Charles Towne fortification ditch (38CH1). The deviation here between the known median date of 1675 and the ceramic formula dates with and without frequency considered is 21 and 14 years respectively. This is a dramatic contrast to the ten eighteenth century sites for which the median historical manufacture dates are known. At present this gap seems to be a result of possibly two factors, lack of knowledge of seventeenth century ceramic types and manufacture dates, and a possible status factor. Noël Hume has found seventeenth century upper class mansions have more ceramics represented than do the lower class homes of that period, but has not found this to be so in the eighteenth century (Noël Hume 1970:25; and personal communication). This writer thinks that the lower class seventeenth century homes may well have had a greater time lag represented in ceramics than was in the mansions. This is not seen, however, as a lag resulting from less "broad and rapid spread" of ceramic types but from the greater non-functional status role played by ceramics within the lower class seventeenth century household. The rapid distribution of ceramics from factory to British American ports, and the subsequent journey to the frontier is seen to result in the horizon phenomenon in both the seventeenth and eighteenth century periods. This will probably best be demonstrated through analysis of ceramics from the more affluent seventeenth century homes, but such a status difference is yet to be demonstrated through ceramics from eighteenth century British American sites. On the eighteenth century sites included in this study the high percentage of correlation between the mean ceramic manufacture date derived from the formula and the historic median date for the occupation of the site is seen as a clear demonstration of the horizon phenomenon.

In instances where we might have wanted more precision in our tool
we can sometimes see a possible explanation in terms of a small sample. The Paca House midden for example had only 46 sherds, and a probable historical range for the deposit of from 1763 to around 1780 when the house was sold to a new owner, producing a median date of around 1771, some eight years later than the formula date of 1763 plus or minus four years. However, if no historical data were available our slightly "too early" mean ceramic date would still be only eight years away from the actual date.

It is hoped that more such formulas will be forthcoming with which to deal with historic site data, with buttons, beads, wine bottles and glasses all contributing their individual chronologies and mean artifact dates suitable for comparison with the mean ceramic date and brackets, pipestems, and coins, but this only as introduction to the examination of questions of broader scope.

The apparent success of the tools discussed here is thought to be due to the fact that with colonial artifacts we are dealing with a historical chronology reflecting cultural process, just as we would be doing with a study of motifs from a collection of dated coins from the same cultural tradition. The coins are indicators of the historical as well as the cultural process as well as reflecting the temporal occupation span for a site just as we have seen ceramics to be. For instance, at Brunswick Town the documented duration of the site was from 1725 until it was burned in 1776. The coins from the ruins of houses burned at that time date from 1696 to 1775. The coins from all ruins including those occupied after the Revolution into the 1830's date to 1820. Thus coins are used along with ceramics to help fix dates for historic site occupation. However, they are not often found in quantity sufficient for them to be a major tool. They can provide auxilIary data as historically fixed documents, just as we have seen ceramics utilized in this study.

In order to help understand what the use of the mean ceramic date formula does we might visualize each sherd as having imprinted on it the median manufacture date, equivalent to finding a dated coin for each sherd. Thus Type 61, North Devon Gravel Tempered Ware sherds found in the amount of 45 sherds would equate with 45 coins having the date 1713 clearly revealed on each. The formula allows us to deal with this wealth of dates represented by each sherd found on the site, and arrive at an interpreted date representing the mean of all the median dates represented by the sherds.
INTERPRETIVE SUMMARY

In this study we have concentrated on the similarity between groups of eighteenth century ceramic types as found on colonial English historic sites over a wide area and of varying functions. We have suggested that this can be done due to the horizon nature of the ceramic groups in the eighteenth century, and the fact that the ceramic types reflect culture change through time. We have not dealt with the important differences between ceramic forms as reflectors of functional or socioeconomic factors at work within the culture. The potential of such a study has been pointed out by Stone (1970) and others regarding porcelain as an index of status. Miller and Stone (1970) have also indicated that ceramic analysis offers great potential in studies of sociocultural change, status and social level, and functional interpretations. The study of ceramic types as we have done in this paper as indicators of site occupation periods reflecting the cultural horizon concept does not negate the study of ceramic shapes as more sensitive indicators of status and function within the culture. Although ceramic analysis by type can be demonstrated to vary but relatively little from a port town such as Brunswick and the frontier forts of the same period, thus providing us with a valuable temporal tool for use on eighteenth century sites, an analysis of the same ceramic fragments using shape might well reflect status or cultural pattern of a different sort. Garry Stone at the 1970 meeting of the Society for Historical Archaeology presented a paper illustrating the use of a number of ceramic shapes dealing with the tea ceremony at the frontier outpost of Fort Dobbs, North Carolina. In the present study of the nineteen ceramic types present at the frontier site of Fort Prince George, ten were represented by the presence of teapots, teacups or saucers, tending to support the observations made by Stone in North Carolina regarding the extension of the tea ceremony to the far corners of the colonial frontier (see Roth 1961). The emphasis on shape as opposed to type, reflecting perhaps an emphasis on function as compared to time can be seen in the manner in which archeologists approach their data. Noël Hume, for instance, classifies and catalogs his ceramics by quantification of the shape of various types present, whereas this archeologist has always used quantification by fragments of ceramic types present. Analysis by shape would seem to be a more sensitive indicator of function and possible socioeconomic level, whereas that by type is useful for discovering the kind of cultural information dealt with in this present study. Thus the manner in which we classify our data has a bearing on our interpretations.

Other points dealing with this subject should be mentioned. Ceramic analysis should consider such factors as absence, which may well correlate with documents, such as the period from about 1640 to 1680 when the English were barred from Chinese ports, thus having a definite effect on the import of Chinese porcelain during this period (Noël Hume
1970:257). The absence of porcelain in the collection from the Charles Towne deposits of 1670-1680 is therefore no surprise. Another point is that from the first Fort Moore of the early eighteenth century fewer ceramic shapes were present dealing with the tea ceremony than were found on the later frontier forts in the area. This difference in ceramic shape between these eighteenth century forts may reflect the greater popularity of the tea ceremony from the mid-eighteenth century on as opposed to its popularity in the early part of the century (Roth 1961).

Although Stone (1970) found an association between porcelain and the more affluent in the inventories he studied, we surely need more data before we can say that this is reflected in archeological collections. Miller and Stone (1970:100) have also suggested that archeologists "should be able to establish the relative socioeconomic level of a population and define any major status differences which existed at a site by means of the distributional analysis of ceramics." Archeologists often give lip service to this view, but we have yet to see the demonstration of this milking process archeologically demonstrated. Comparison of French with English ceramics at Michilimackinac was done by Miller and Stone with interesting differences observed, but whether status or socioeconomic differences can be witnessed within the context of an eighteenth century British American site is yet to be demonstrated. Cleland (1970:122) has mentioned differences in ceramics from two row houses being interpreted as reflecting social status of the occupants, and suggests that this interpretation can validly be made in the absence of specific historical data for the row houses themselves. I suggest that this is only one of the possibilities, but one yet to be validly demonstrated. I do not think interpretations based on a single comparison can be considered to be valid. We need several such ceramic differences in comparisons made between a number of archaeologically examined historic ruins. I would suggest that we need a pattern of such differences before we could archeologically demonstrate that a status situation is indeed responsible. Another approach to this problem could come through the excavation of ruins of homes of historically known affluent people at one particular point in time and comparison of the ceramics recovered from ruins of historically known non-affluent individuals at the same period in time. This would provide a control against which the archeological data could be examined. At such a time we might begin to be able to make statements regarding status as reflected by the ceramics from the sites we excavate. Meanwhile status appears to be a goal we all think we should somehow milk from our ceramics, but as yet we have not discovered the proper grip for producing this stream of cultural knowledge from our archeological data from eighteenth century British American sites.

Functional interpretations from historic site ruins are also often frustratingly unproductive. With kiln sites, furnaces, and other specialized structures the interpretation becomes obvious as the data is revealed. However, with the town ruins of Bethabara, North Carolina,
for instance, maps of 1760 and 1766 revealed the functional use for each structure at that time, the tailor shop, kitchen, pottery shop, business manager's house, the doctor's laboratory, the apothecary shop, the blacksmith shop, the millwright's house, the gunsmith shop, and the tavern, but when excavation was complete not a single structure could be interpreted from the archeological data as to its correct function except the pottery shop of Gottfried Aust, identifiable from the clay wedging floor and the kiln waster dump. We should be cautious, therefore, and anchor our research goals in something more productive than a consideration of the function of the structure we are examining. Fortunately, there are other questions that can be asked about historic site data, such as those examined in this paper.

In this study we have seen that eighteenth century British American sites of varied functions, from port town ruins, to town house mansions to frontier forts and Indian villages have similar groups of ceramic types present at similar periods of time. This has been interpreted in terms of the horizon concept (Willey and Phillips 1958:31-34). The time required for the spread of the cultural material representing the horizon is a factor to be considered, as Willey and Phillips point out. Therefore, an approximate contemporaneity is involved. With our historic ceramics used in this study we are dealing with a class of objects that originated, for the most part, in England and were brought into American aboard vessels to ports such as Charleston, Savannah, Boston, New York, and Philadelphia, and from these centers were distributed to inland sites. This distribution was often quite rapid, being only as long as it took a man on horseback to ride the distance from the port town where the limited collection of ceramic types was available, to his frontier destination at Fort Prince George, Goudy's Trading Post, or Fort Moore. A few months at the most might have been involved, so that within a few weeks after a ship arrived in a port town, teacups, teapots, and saucers of white salt-glazed stoneware or "clouded" polychrome painted cream-colored ware could easily have been used by an Indian to pour a cup of the "black drink" at the Cherokee town of Keowee opposite Fort Prince George. Such ceramic types and forms are found in Cherokee middens deposits, and whether they reached the Cherokee nation by way of Philadelphia or Charleston is immaterial when we consider that in either case the journey would take but a few weeks at the most. Thus the argument that considerable time lag must have been involved for English ceramic types to reach the various remote corners of the colonial frontier is a more difficult position to support than that dispersal of goods was a relatively rapid process. If this was so then we can understand why a great deal of uniformity would exist among ceramic types from sites of the same time period, regardless of the fort, port, or Indian village function of the site on which the ceramics were used.

Documents from port records may well reveal that certain colonial ports received ceramic goods from different English ports, thus theoretically introducing another variable into the picture. However, as
Cleland has said (1970:122), "These are historic facts that are really irrelevant to the interpretation of the archaeological data." For example, if the historical documents were to reveal that Charleston did not receive any Oriental porcelain in the eighteenth century this would not alter the percentage relationships of this type from the sites in this study, or the applicability of the mean ceramic date formula, or the interpretation of the data in terms of the horizon concept. It would point to questions centering around transportation and supply routes relative to the sites in this study merely as additional historical information.

From this examination of our hypotheses we can see that the bracketing and mean ceramic date formula tools have proved of value in producing a time bracket for eighteenth century sites that correlates well with the historically known occupation periods. From this correlation the validity of our hypotheses has tended to be demonstrated to the limits of our present data. More use of these and similar tools on a broader scope should now be undertaken by historic site archeologists in similar studies if we are to interpret the most from our historic site data.

The construction of tools such as pipestem and ceramic analysis formulas, however, is only a first step toward discovering answers to the larger questions of culture process. This paper has attempted to address itself to some of these questions. Historical archeology data particularly lends itself to analysis in a controlled and specific manner not possible on the prehistoric level. For this reason it offers an ideal arena for the examination of cultural concepts long explored on prehistoric sites. Historical archeology has now matured to the point where we should begin to explore this potential rather than continuing to crowd our bookshelves with descriptive catalogs of our systematized relic collecting devoid of any redeeming analytical or interpretive value. Historical archeologists have a challenge and a responsibility to abstract order through analysis and meaning through interpretation of their data. "From the pages of the earth, the historical archeologist gathers bits and pieces representing past human activity and relates these to the shreds and patches surviving as the worn documents and faded words of history. From this collection of essentially meaningless, unique fragments of the past, he abstracts the order, and strives to press a meaning" (South 1969). Too often we stop with description of the bits and pieces and the relation of these to the documentary shreds and patches without attempting to abstract the order and discover the meaning. We historical archeologists should more frequently take that next step from data to theory, a step so clearly stated by Hempel (1966:15):

The transition from data to theory requires creative imagination. Scientific hypotheses and theories are not derived from observed facts, but invented in order to account for them. They constitute guesses at the connections that might obtain between the phenomena under study, at uniformities and patterns that might underlie their occurrence.
HISTORICAL ARCHAEOLOGY FORUM - South

In this paper we have made guesses at some of the connections and uniformities we have observed from historic site ceramics. If our guesses prove valid we have sharpened our theoretical tools (Deetz 1968:130), and revealed the cultural "treasure from earthen vessels", a goal of archeology.

I would like to thank John and Joan Combes, George Teague, Robert L. Stephenson, and Audrey and Ivor Noël Hume for discussing this paper with me and helping to clarify some of the concepts.
HISTORICAL ARCHAEOLOGY FORUM - South

BIBLIOGRAPHY

BINFORD, LEWIS H. AND MOREAU S. MAXWELL

BINFORD, SALLY R. AND LEWIS R. BINFORD

CLARK, DAVID L.

CLELAND, CHARLES E.

CLELAND, CHARLES E. AND JAMES E. FITTING

DEETZ, JAMES

DEETZ, JAMES AND EDWIN DETHLEFSEN

DOLLAR, CLYDE D.

DUNNELL, ROBERT C.

FORD, JAMES A.

HANSON, LEE H., JR.
HARRINGTON, J. C.

HEMPEL, CARL G.

MAYER-OAKES, WILLIAM J.

MILLER, J. JEFFERSON AND LYLE M. STONE

NOEL HUME, IVOR

ROTH, RODRIS

SOUTH, STANLEY
1958 Nath Moore's Front, Unit S10, 1728-1776. MS on file at State Department of Archives and History, N. C. and at the Institute of Archeology and Anthropology, University of South Carolina, Columbia.
1959a The McCorkall-Fergus House, Unit S18, c.1760-1775. MS on file at State Department of Archives and History, N. C. and at the Institute of Archeology and Anthropology, University of South Carolina, Columbia.
1959b The Hepburn-Reonalds House, Unit S7, 1734-1776. MS on file at State Department of Archives and History, N. C. and at the Institute of Archeology and Anthropology, University of South Carolina, Columbia.
1967 The Paca House, Annapolis, Maryland. MS on file at the Institute of Archeology and Anthropology, University of South Carolina, Columbia.
HISTORICAL ARCHAEOLOGY FORUM - South

SOUTH, STANLEY
1971 Archeology at the Charles Towne Site (38CH1) on Albemarle Point in South Carolina. MS on file at the Institute of Archeology and Anthropology, University of South Carolina, Columbia.

STONE, GARRY WHEELER

WASHBURN, S. L.

WILLEY, GORDON R. AND PHILIP PHILLIPS
Percentage Relationship of Certain Ceramic Types
from Several Structures at Brunswick Town, N.C.

| PROPERTY OWNER | ONSING | HAMMETT | HAMMETT'S HRN | WILLIAM HORTON | WILLIAM HORTON AND LUCY DEAN HORTON | CHARLES HORTON AND HARRIETT BARKER | JOHN FENIG | FRANCISCO VIEITES BARTON | NATIONAL HOUSE | "BUSH, ROGERS HOUSE" | "BUSH, ROGERS HOUSE" |
|----------------|--------|---------|---------------|----------------|--------------------------------------|-------------------------------------|------------|------------------------|-----------------|------------------|
| DEED DATE+     | 1731   | 1730    | 1730          | 1730           | 1732                                 | 1734                                 | 1733       | 1730                   | 1730            | 1730             |
| ORIG. LOT NO.   | 019    | 97      | 97            | 97             | 97                                   | 97                                   | 97         | 97                     | 97              | 97               |
| ARCHAEOLOGICAL UNIT NO. | MI 325* | 325 | 325           | 325           | 325*                                 | 325                                 | 325        | 325                    | 325             | 325              |
| SHARD PROVENIENCE |      |         |               |               |                                      |                                      |            |                        |                 |                  |
| TYPE NO. | WHN. |        |               |               |                                      |                                      |            |                        |                 |                  |
| 17   | UNDERGLAZE BLUE HAND-PAINTED PEARLWARE | | | | | | | | | | |
| 18   | MOCHA AND ANGULAR PEARLWARE | | | | | | | | | | |
| 19   | WHITEWARE | | | | | | | | | | |
| 21   | BLUE GREEN EDGED PEARLWARE | | | | | | | | | | |
| 22   | TRANSFER-PRINTED PEARLWARE & WHITEWARE | | | | | | | | | | |
| 23   | "CLOISONNE" MOTTLED GLAZED | | | | | | | | | | |
| 24   | GREEWARE BURNISHED | | | | | | | | | | |
| 25   | BURNWICK BURNISHED COLONIO-INDIAN WARE | | | | | | | | | | |
| 26   | BURNWICK PLAIN COLONIO-INDIAN WARE | | | | | | | | | | |
| 38   | CHINESE PORCELAIN | | | | | | | | | | |
| 43,45 | WHITE SALT-GLAZED STONEWARE | | | | | | | | | | |
| 45,49 | DELFTWARE | | | | | | | | | | |
| 56   | COMBED YELLOW LEAD-GLAZED SLIPWARE | 100% | 100% | 100% | 100% | 100% | 100% | 100% | 100% | 100% | 100% |
| SHARD COUNT | 200 | 1276 | 1270 | 1453 | 829 | 741 | 835 | 204 | 84 | 1027 | 757 |

% SCALE

A house was to be built within one year
after purchase of lot, or forfeit the lot.
APPENDIX I

Percentage Relationship of Certain Ceramic Types from Several Structures at Brunswick Town, N.C.

from a paper delivered at the first Conference on Historic Site Archaeology in 1960

by Stanley South

entitled

"The Ceramic Types at Brunswick Town, North Carolina"

Published in

Southeastern Archaeological Conference Newsletter Vol. 9, No. 1 (1962)

This chart demonstrated the similarity of percentage relationships between several ruins of similar documented time periods, providing data of value in determining the occupation period of ruins of unknown time periods from a percentage relationship comparison of the ceramic types.

The ceramic type numbers used in this study have been added to the original chart.
APPENDIX II

THE MEAN CERAMIC DATE FORMULA USING PRESENCE-ABSENCE AND FREQUENCY

The mean manufacture date for the group of Colonial British ceramic types from an historic site taking into consideration the frequency of occurrence of fragments of the types, can be determined by a mean ceramic date-frequency formula as follows:

Where the mean ceramic date, $Y$, is expressed:

$$Y = \frac{\sum_{i=1}^{n} X_i \cdot f_i}{\sum_{i=1}^{n} f_i}$$

Where $X_i = \text{the median date for the manufacture of each ceramic type}$

$f_i = \text{the frequency of each ceramic type}$

$n = \text{the number of ceramic types in the sample}$
APPENDIX III

Application of the Mean Ceramic Date Formula to Samples from Historic Sites

Presented here are the types and frequency data for the sites discussed in this paper as used with the mean ceramic date formula from which the mean ceramic dates used in Figure 1 were taken.

[In some cases the data has been coded by subtracting 1700 from the Type Median dates for ease of computation. This was later added to produce the mean ceramic date.] (This note was inadvertently omitted from the draft of this paper distributed for Forum comment.)
APPENDIX III

APPLICATION OF THE MEAN CERAMIC DATE FORMULA TO SAMPLES FROM HISTORIC SITES

Charles Towne (38CH1) The First English Fortification in South Carolina

<table>
<thead>
<tr>
<th>Ceramic Type</th>
<th>Type Median</th>
<th>Sherd Count</th>
<th>Product</th>
</tr>
</thead>
<tbody>
<tr>
<td>64</td>
<td>1665</td>
<td>4</td>
<td>6660</td>
</tr>
<tr>
<td>62</td>
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<td>13</td>
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</tr>
<tr>
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<td>1720</td>
<td>10</td>
<td>17200</td>
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<td>1660</td>
<td>60</td>
<td>99600</td>
</tr>
<tr>
<td>70</td>
<td>1635</td>
<td>62</td>
<td>101370</td>
</tr>
<tr>
<td>58</td>
<td>1668</td>
<td>1</td>
<td>1668</td>
</tr>
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<tr>
<td></td>
<td></td>
<td>151</td>
<td>249818</td>
</tr>
</tbody>
</table>

Historic dates 1670-1680

Historic median date 1675

Mean ceramic date 1654.4

Pipestem date 1667

The First Fort Moore? (38AK4-15) An Eighteenth Century Frontier Fort and Trading Post

<table>
<thead>
<tr>
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<th>Type Median</th>
<th>Sherd Count</th>
<th>Product</th>
</tr>
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<td>1760</td>
</tr>
<tr>
<td>43</td>
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<td></td>
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</table>

Historic dates 1716-1747

Historic median date 1732

Mean ceramic date 1726.1

Pipestem date 1730.9
## Fort Moore (38AK5-A)

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<th>Product</th>
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### Historic dates 1716-1766

Mean ceramic date 1741.7

Pipestem date 1744.16

### Historic median date 1741

### Goudy's Trading Post at Fort Ninety Six, South Carolina (38GN1-3) (Plowed Zone)

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</tr>
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<td>88</td>
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<td>43</td>
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<td>2</td>
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<td>1750</td>
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<td>1</td>
<td>38</td>
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</table>

Historic dates 1751-1760?

Mean ceramic date 1769.3

### Goudy's Trading Post at Fort Ninety Six, South Carolina (38GN1-5) (Cellar)

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Historic dates 1751-1760?

Historic median date 1756

Mean ceramic date 1754.6
### Fort Prince George, South Carolina 38PN1
A British Military Post on the Cherokee Frontier

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Historic dates 1753-1768

Historic median date 1761

Mean ceramic date 1763.0

Pipestem date 1750.14

### The Rock Turtle Site (38PN4)
An Eighteenth Century Indian Village Site

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Mean ceramic date 1749.7

Pipestem date 1756.36
**Brunswick Town, North Carolina (Ruin S15)**

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\[ \text{Mean ceramic date } 1746.4 \]

Historic dates 1726-1759-1776

Historic median date 1751

Mean ceramic date 1746.4

Pipestem date 1748

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**Brunswick Town, North Carolina (Ruin N1)**

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\[ \text{Mean ceramic date } 1750.1 \]

Historic dates 1731-1776

Historic median date 1754

Mean ceramic date 1750.1

113
## Brunswick Town, North Carolina (S2)

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Historic dates 1731-1776
Mean ceramic date 1749.0
Historic median date 1754
Pipestem date 1748

## Brunswick Town, North Carolina (S18)

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Historic dates 1763-1776
Mean ceramic date 1776.2
Historic median date 1770
Pipestem date 1756

114
The Paca House, Annapolis, Maryland 19J,27B  A Town House Mansion

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Historic dates 1763-80?
Mean ceramic date 1763.1

The S10 Dump at Brunswick Town  A Post Revolutionary War Dump

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Historic dates 1776-1830
Historic median date 1803
Mean ceramic date 1794.0
### Tallassee, A Nineteenth Century Cherokee Indian? House Site in Tennessee

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Mean ceramic date 1818.1

### Toxaway (380C3) An Eighteenth Century Cherokee Indian Village Site

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Toxaway A Nineteenth Century Occupation

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Mean ceramic date (18th century = 1735.8
Mean ceramic date (19th century = 1855.4

### The Nipper Creek Site (38RD18)

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Mean ceramic date 1801.3
"EVOLUTION AND HORIZON AS REVEALED IN CERAMIC ANALYSIS IN HISTORICAL ARCHAEOLOGY"

-A STEP TOWARD THE DEVELOPMENT OF ARCHAEOLOGICAL SCIENCE

Lewis R. Binford

Stanley South's paper is excellent. It argues a closely reasoned justification for the development of a research tool which when properly applied should be of great value in historical archaeology. I will not try to summarize this paper since the readers will have it available for their own enjoyment. Instead, I have chosen to direct my comments to the role of such research in archaeology as I view the needs from the perspective of recent debate in historic sites literature.

In a recent series of exchanges, numbers of historical sites archaeologists have expressed their views regarding the field and its current development. A wide variety of opinion has been expressed and clearly some tempers have been aroused.

Since this debate has been largely stimulated by Clyde Dollar's (1968) paper, I will attempt to make a few points germane to his discussion, the implications of which I feel have not been fully explored by his critics.

I will try to summarize these points as I see them:

1. Researchers in the field of historical archaeology are encountering problems, the solutions to which seriously strain the ability of traditional anthropological methods to solve (Dollar 1968:4).

2. The historian uses two main research methods or "tools." The first of these is the logical process of deductive reasoning, or going from the general to the particular, and the second is the application of tests for validity or the research processes of verification (Dollar 1968:11).

I feel that Dollar is correct in these assertions and in general his sketch parallels the analysis frequently made by philosophers of science regarding historical methods.

It is a rather widely held opinion that history, in contradistinction to the so-called physical sciences, is concerned with the description of particular events of the past rather than with the search for general laws which might govern those events (Hempel 1965:231).
One might reasonably ask: Where does the historian obtain the general propositions which, as Dollar points out, permit him to proceed from the general to the particular?

...in history no less than in any other branch of empirical inquiry, scientific explanation can be achieved only by means of suitable general hypotheses, or by theories, which are bodies of systematically related hypotheses. This thesis is clearly in contrast with the familiar view that genuine explanation in history is obtained by the method which characteristically distinguishes the social from the natural sciences, namely the method of empathic understanding: The historian, we are told, imagines himself in the place of the persons involved in the events which he wants to explain; he tries to realize as completely as possible the circumstances under which they acted and the motives which influenced their actions; and by this imaginary self-identification with his heroes, he arrives at an understanding and thus at an adequate explanation of the events with which he is concerned (Hempel 1965:239-240).

Dollar himself is silent on this issue but Walker, with whom Dollar clearly identifies (Dollar 1968a:139), provides us with a classic statement of the method "of empathic understanding."

...We cannot understand the history of prehistoric man unless we become in our own mind a prehistoric man - so also with historic man. Each site excavated is individual to some degree, and the product of individuals (Walker 1968:119).

This procedure seems to be justified by the advocates of historical methodology as an appeal to the "uniqueness thesis" which is widely held by historians. It has been stated this way:

History is different in that it seeks to describe and explain what actually happened in all its concrete detail. It therefore follows a priori that since laws govern classes or types of things, and historical events are unique, it is not possible for the historian to explain his subject-matter by means of covering laws. If he is to understand at all, it will have to be by some kind of special insight into particular connections (Dray 1957:45).

That this seems to be an adequate summary of Dollar's and Walker's views is best demonstrated by their words:
HISTORICAL ARCHAEOLOGY FORUM - Binford

It would seem to me, therefore, that the study of anthropology in general, is incapable of producing techniques for the recognition of either specific actions or single cultural contributions of any given individual within any given culture (Dollar 1968:10-11).

...anthropological studies tend to formalize generalizations which are difficult to prove...to make conclusions which may be valid as generalizations; but being generalizations, are often inapplicable to single instances (Walker 1968:108).

While Dollar characterizes himself as a "differing dragon" (Dollar 1968b:156), the general stance of both Dollar and Walker was championed in history during the last half of the nineteenth century by what is frequently referred to as the "German School." Proponents differentiated between those subjects which were considered amenable to the idiographic method, e.g., exploring particular connections. Nomothetic approaches were used where establishing generalizations was a goal. This idiographic approach in history has recently come under scrutiny by historians themselves. In an interesting study, Joynt and Rescher (1961) conclude that the uniqueness thesis cannot be sustained either through logic or an appeal to the actual activities of historians. Since all events may from one perspective be considered unique there is, therefore, no essential difference between the historical and natural sciences deriving from the character of their data. Distinction only arises by choosing to treat events as unique. One may choose to use events or facts as members of a type or class with the aim of establishing generalizations and propositions of law-like validity. Such a choice is not dictated by the character of the data.

Inspection of the works of historians demonstrates their dependence upon categories, classes, and generalizations:

It is clear that the historian in effect reverses the means-end relationship between fact and theory that we find in science. For the historian is interested in generalizations and does concern himself with them. But he does so not because generalizations constitute the aim and objective of his discipline, but because they help him illuminate the particular facts with which he deals (Joynt and Rescher 1961:153).

The essentially dependent character of historians for the propositions which serve as links in their interpretative arguments is well stated by Ernest Nagel.
There is an important asymmetry between theoretical (or generalizing) science and history. A theoretical discipline like physics seeks to establish both general and singular statements, and in order to do so physicists employ previously assumed statements of both types. Historians, on the other hand, aim to assert warranted singular statements about the occurrence and the inter-relations of specific actions and other particular occurrences. However, although this task can be achieved only by assuming and using general laws, historians do not regard it as part of their aim to establish such laws (Nagel 1961:550).

That this is the general stance of those who recommend to us "the stiffening discipline of historical philosophy" (Walker 1968:108) is well demonstrated in the following statement by Walker (1970:67):

"...I suggest that the end result is of more importance than the abstract theory woven in an attempt to find that end result; If Cleland and Fitting...can recreate the past from an excavated site in a way which enables us to see, however dimly, how people lived and worked, then provided their conclusions are legitimate deductions from the evidence available their philosophical beliefs are not of the first importance."

This comment presents us with some interesting notions. For instance, how does one deduce conclusions from available evidence? Deductions are made from propositions which specify relationships between things and events. Conclusions regarding observations are warranted to the degree that their relevance to such propositions can be established and the conclusions regarding specific cases are justified logically. Surely if one is to work deductively then one has some interest in the validity of the propositions from which deductions are drawn. One wonders, in the absence of concern for philosophical "beliefs," what criteria Walker might use to determine whether an archaeologist's conclusions "are legitimate deductions from the evidence." I fear that Walker's criteria would be intuitive tests of plausibility, internal consistency, or critical evaluation of the accuracy of the facts cited. For the scientist there are other major concerns: the validity of the assumptions regarding man, of the character of the archaeological record, of the past, of contingencies believed to affect man's behavior, etc. These are assumptions which we must all use in order to interpret the data of archaeology. Facts do not speak for themselves.

Dollar points out that many general propositions current in anthropology appear vastly inadequate when adopted for use by
historians. In this position I would agree with Dollar. Anthropologists have not addressed themselves to the job of making explicit and testing the validity of the law-like propositions which they have expounded. For instance, Dollar points out the failure and lack of retrodictive accuracy when using seriation techniques as they have frequently been employed by anthropologists. He equates seriation with a dating technique. This has clearly been done by archaeologists working with prehistoric materials, but the utility of the technique, qua technique, need not be equated with some individual's excesses. Seriation is simply an arranging of samples or items in a series with regard to some specified criterion. As generally used, samples or populations are arranged in series with regard to some measure of similarity between the samples. As outlined this is purely descriptive procedure. Dollar seems to object to the proposition that all observed variability is referable to cultural change or differentiation, an assumption which must be made if a scale of differences is equated with time. I think that Dollar would find very few anthropologists anxious to defend that proposition, and in fact the heyday of seriation used in this manner has been over in American archaeology for at least ten years.

This does not mean that seriation as a technique is not still being widely employed: it is. As archaeologists, our job is the explanation of observed similarities and differences in the archaeological record, and seriation techniques are very useful in evaluating such differences and similarities. This is not to say that in many cases similarities and differences as measured by seriation techniques do not demonstrate significant temporal patterning: they do. By discouraging the use of "extended anthropological techniques" Dollar is not just complaining about the utility of some of the anthropologists' general propositions, he is advocating a historical approach and asserting anew the "uniqueness thesis," i.e., that historic sites are unique, and are therefore not appropriately investigated as cases in the testing of propositions of potential general validity. Strangely, he inveighs against formal descriptions of artifacts, statistical techniques, etc., though these approaches are completely consistent with traditional historical approaches.

The debate largely boils down to an attempt on the part of Dollar to set forth the "uniqueness thesis" as the justification for adopting a set of goals commensurate with traditional historical perspectives. His concern is with specific events, dates, and actions of individuals summarized in the pursuit of reconstruction. Dollar offers further justification through a criticism of the accomplishments of "generalizing anthropology," which are set forth as a contrastive set of "failures," which he sees as further support for "uniqueness" claims and for the dismissal of generalizing propositions in historic sites archaeology.
The "uniqueness" position is invalid as a justification for pursuing idio­graphic goals. All events or facts may be viewed as unique, or one may choose to treat them as cases in the context of generalizing propositions. The character of the data is never a justi­fication for the limited pursuit of limited goals. Similarly the failure of attempts to treat events and specific observations in a generalizing framework is not sufficient justification for abandoning generalizing goals.

Reliable historical interpretation and hence reconstruction of life ways, etc., is dependent upon valid general propositions which can serve as the pivotal points for interpretive arguments treating the specific facts of a specific case. In order to increase our powers of historical interpretation, making use of archaeological facts, progress is needed on two fronts: (1) An active attempt to explicitly state and test the validity of high level generalizations regarding (a) the processes responsible for the formation of the archaeological record, and (b) the processes responsible for bringing about changes and maintaining diversity in the life ways of peoples. (2) An active attempt to increase the accuracy with which we observe and describe the facts of the archaeological record. What is needed to increase our abilities in historical reconstruction is the development of a science of archaeology. Anthropologists smugly displaying their scorn of historians must stop working as historians and start working as scientists to meet the need for valid general propositions.

Another note, however, seems to dominate the discussion and this is the note I find disturbing. I am not disturbed that historians find anthropological generalizations inadequate: they generally are. I am not disturbed that in the absence of a sound scientific basis for the interpretation of specific historical events that some should be pragmatic and follow an eclectic procedure. I am disturbed with what appears to be a commitment to the maintenance of this state of affairs. Dollar seemingly wants to become more restrictive in the degree that we investigate the archaeological record itself and decries the use of statistical procedures when they do not yield "accurate" results. Walker, although less radical, appears to be saying that an explicit concern with science is not necessary since the expedient positions forced on historians in the absence of sound scientific understanding are in fact desirable:

...the suggestion that anthropology must become history or become nothing is not just a smart remark to Willey and Phillips: it is the statement of what I conceive to be the goal of all those who study man and his past, a conclusion to which Boas came in 1932 (Walker 1970:65).

I would amend this position in the following way: anthrop­ology must become a science before it can adequately serve to enhance our historical understanding of man and his past. I suggest to historians,
anthropologists, and interested bystanders alike, that insofar as we agree that our goals in historical sites archaeology are historical understanding of the events and the people which were responsible for the production of the archaeological record, such understanding will not be forthcoming until a science of archaeology is developed. Rejection of the pursuit of scientific or nomothetic understanding because of failures in this direction within the field of anthropology, or a commitment to particularistic approaches in the absence of such understanding is counterproductive. Historic sites archaeologists should actively engage in nomothetic studies aimed at the specification of general propositions amenable to testing regarding (a) the processes responsible for the formation of the archaeological record and (b) the processes responsible for change and diversification in human lifeways. Success along these lines will lead to a greater success in the understanding and reconstruction of specific events and specific historical facts.

Historic sites work seems to me to be particularly suited to this type of development since many conditions of the past are known through written documentation. We are, therefore, in a more informed position to test the validity of generalizing propositions than many prehistoric archaeologists working in a much less informed domain.

How does all this discussion relate to the paper presented by Stanley South? I think it is germane in a number of ways. South's paper is a fine example of the development of a research tool making claims of general utility. South has summarized a series of median dates for the periods of production of a selected control group of ceramic types. He proposes that the arithmetic mean of these dates, for examples of the types found in archaeological contexts, when weighted individually by the numbers of each type found will yield the best estimate of the median date of the elapsed time during which the archaeological sample was accumulated. This suggestion assumes that the maximum production of any given ceramic form will be in essential identity with the median date of that form's production history.

In the context of the current debate I anticipate a number of potential responses to these suggestions. I fear that many will view South's proposal in a limited perspective seeing it only as a dating technique. Some will almost certainly offer numerous arguments as to why, as a dating technique, it cannot be valid. In such arguments the most common citation will be to conditions in the past which when operative would tend to result in inaccurate dates. Others will take the position that the inaccuracy is not tolerable in the context of their specific historic needs and therefore dismiss the research tool as irrelevant to their interests. Still others may well conclude that the procedure is too time-consuming when they can date their sites more accurately with other means.
I will try to treat each of these anticipated responses in the context of my analysis of the current debate in the field.

The first response, that of offering many reasons why the proposal cannot work, is not a sufficient justification for an archaeological scientist to reject the proposal. South offers a general proposition and a demonstration that at least in the context of the materials used it does work. The scientist could only take the enumeration of reasons why it shouldn't work as a challenge. The exploration of the organizational relationships between differential production, the logistics of differential distribution and the differential utilization of products, and their final loss to the system as potential contributors to variability in the archaeological record are essential kinds of knowledge for the accurate interpretation of archaeological variability. South's methods when used in conjunction with others provide us with an interesting tool for the initiation of such studies aimed at the understanding of processes which were certainly operative with regard to the above mentioned factors. The fact that South's proposal seems to yield consistently reliable results needs to be explained. Similarly, cases of demonstrated inaccuracy demand explanation.

The second response, that of dismissing the methods because the levels of inaccuracy as a dating technique are unacceptable, betray a naive notion regarding the character of the archaeological record. The only accurate dating techniques which the archaeologist might develop for treating unknown materials are those which are dependent upon the operation of processes independent of the operation of cultural processes. Leaving aside for a moment the levels of accuracy problem it should be clear that Carbon 14, dendrochronology, and all other such techniques are dependent upon the operation of regular processes in the past, independent of the operation of cultural processes. We may make use of knowledge of such processes for dating by the demonstration of correlations or associations with materials relevant to cultural processes. Any technique of dating which is based on cultural materials may exhibit regular trends temporally, but it is always dependent for its accuracy on the stability of certain relationships in the organization of cultural dynamics. When we can demonstrate the accuracy of such a temporal trend in cultural materials we have isolated a phenomenon which once again demands that we seek an understanding. How general is its reliability, how variable is its accuracy, what were the determinants operative to produce variable accuracy, etc., are the questions we want to answer. If we understand these things, we would be both learning more about the past and increasing the utility of our original observations as a dating technique which could be utilized with confidence within stated ranges of accuracy. The persons who dismiss South's technique
and view it simply as a dating technique of questionable utility are relinquishing their scientific role in favor of ignorance regarding the character of cultural processes operative in the past.

The response that South's procedure is too time-consuming and that other more reliable means of dating materials may be available is taking a very short sighted view of the field. More reliable dating means places such a researcher in the position of contributing some of the most important information to investigations into the character of the organizational relationships obtained in the past. Such persons are in a position to evaluate the accuracy of the procedure and hence the degree that the relationships in the past which resulted in the regular trend were truly stable, fluctuating, or affected periodically by other unknown sets of variables.

Thus I suggest that in the context of scientific development there are no foreseeable justifications for not using South's procedure, for dismissing it, or for giving precedence to other procedures. Scientific understanding is necessarily a cumulative process, particularly insofar as it is dependent upon a broad-scale comparative strategy. The scientist must frequently engage in work which is not directly productive of results in the context of his limited immediate goals. We must all be aware of the need to accumulate documented case material and well-described situations as a prerequisite to rewarding comparative studies. I sincerely hope that the suggestions of Dollar do not influence the field of historic sites archaeology. Since it is a relatively new field, the documented basis for productive comparative studies is not yet available in spite of the number of sites which have been excavated. To dismiss the application of "extended anthropological techniques" insures that such a basic corpus of data usable in the context of a controlled procedure will never accumulate. I urge historic sites archaeologists to use the technique, whether they agree with it or not, since the only way of gaining sufficient knowledge for evaluating their skepticism is to have available a broad comparative body of data for evaluation in the context of scientific methods.
BIBLIOGRAPHY

DOLLAR, CLYDE D.


DRAY, W. H.

HEMPEL, CARL G.

JOYNT, C. G. AND N. RESCHER

NAGEL, ERNEST
1961  The Structure of Science, Harcourt, Brace and World, New York

WALKER, IAIN C.
1968  Comment on Clyde Dollar's 'Some Thoughts on Theory and Method in Historical Archaeology.'  The Conference on Historic Site Archaeology Papers, 1967 2, Part 2:105-123. Stanley South (ed.).

Having once been told by a former colleague that just because I knew everything [sic] about clay pipes I was not entitled to comment on reports on ceramics, I approach the task of commenting on this paper with some trepidation. The feeling is not lessened by the knowledge that the paper in question has been written by one of the two most eminent historical archaeologists in North America utilizing to a considerable extent data collected by the other of the two.

Essentially, South sets out to prove his contention put forward at the first Conference on Historic Site Archaeology in 1960 and published two years later (South 1962:1–5) that quantification in the form of frequency-distribution of ceramic types could give a precision to the dating of the archaeological contexts of the ceramics beyond that available by a simple presence-absence analysis of the ceramic content of the deposit [pp. 71, 81–83].* In 1960 South was able to show the usefulness of the approach at Brunswick Town, North Carolina, where ceramic type frequencies expressed as bar graphs allowed separation of ruins historically dating to the 1760's from those dating from the 1730's by reference to the frequency of creamware, a separation which would not have been possible using a presence-absence approach alone.

As South notes, there has been no general rush towards frequency-analysis since its use was proposed, a fact which strikes this writer as strange when one considers the enthusiasm of so many North American archaeologists for a mathematical approach to everything in sight. Now, however, with the mathematics expressed as a sophisticated formula and its successful application to a number of varied sites in the Southeast to show the original applications were not successful merely by happenstance, the concept is put forward again to a possibly more receptive audience.

Housman once claimed that the only reason it took a mere twenty-three years to sell the 400 first-edition copies of Volume 1 of his *Marci Manilii Astronomicon* was that "it found purchasers among the unlearned, who had heard that it contained a scurrilous preface and hoped to extract from it a low enjoyment" (Housman 1937). Whether the rapid exhaustion of Volume 1 of *Historical Archaeology* and South's recent announcement that the first three volumes of his conference series are out of print are due to this writer's scurrilous articles in these publications is uncertain, but it is undeniable that he has gained a reputation for acerbic reviews and adversaria which perhaps overshadows both his favourable reviews and his much more important archaeological

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*Page references to South's paper are given in brackets throughout.*
studies. To save readers turning to the back to see what abusive conclusions on South's ideas I come to, therefore, I should state here that my conclusions on his paper are generally favourable, and those disappointed readers who wish to go on to the following review of South's paper can do so at this point.

However, it is reasonable to examine critically the philosophical concepts whatever one feels about the pragmatic applications, and while one hates to argue with success, particularly when one is neither a philosopher nor a mathematician, it does seem to this writer that one of South's basic assumptions simply will not hold water. This is the concept of the unimodal curve. To say as does Dunnell [quoted by South 74] that

The distribution of any historical or temporal class exhibits the form of a unimodal curve through time. The rationale for this assumption is that any idea or manifestation of an idea has an inception, a rise in popularity to a peak, and then a decrease in popularity to extinction

may satisfy those anthropologists who look for the immutable generalizations which are alleged to underlie cultural variations, but to me it seems one of these sterile and artificial speculations which loom too large in too much of North American anthropological archaeology, to its detriment. The fact that such a statement is based on concepts evolved by such well-known anthropologists as Rouse, Ford, Willey, and Griffin [73] merely emphasizes the artificiality of so much North American anthropological thought.

Of course in a great many cases artefacts will tend to grow in popularity after introduction and subsequently decline to extinction, but give me one good reason why any item should not have a multimodal life, such as the yo-yo which revives every five or ten years. The clay pipe in England grew in popularity from the end of the sixteenth century to the early eighteenth then declined throughout the rest of that century to revive in the early nineteenth century and essentially die during the second half of that century. Tin-glazed earthenwares (hereinafter called delft to save my typewriter ribbon) in the eighteenth century may well have had no peak at all despite the fact they were being produced throughout the century: it is quite possible there was a continuous decline in delft production as first white salt-glazed stonewares, then creamwares, pearlwares, willow-patterned transfer-printed earthenwares, and increasingly available porcelains all combined to progressively cut away its market. The peak in popularity of delft may well have been prior to 1700, and certainly to admit two median dates for delft because it covers two centuries [p.80] can give a totally false representation of production of this ware.

South notes [99] that during the period ca.1640-80 English traders were banned from China ports and that this had a definite
effect on the importation of Chinese porcelain (South's Types 26 and 39) during this period: surely this would result in a bimodal curve, not admittedly representing production but representing use (and therefore breakage) in the seventeenth century? Further, terminal dates for European ceramics other than stonewares (e.g., North Italian marbled slipware, South's Type 70) as given by South are more likely to be terminal dates of supply—because of the Navigation Acts and other legislation in the 1650's to 1680's (Noël Hume 1970:138-41, 276)—than terminal dates of manufacture; while other terminal dates cited by South (e.g., for Buckley earthenware, South's Type 47) are also terminal dates of supply, in this case the disruptive event being the American Revolution (Noël Hume 1970:133).

Again, South notes [80 ] the need for different chronological models when different cultural traditions are involved, citing different dating for Rouen faience on French and English North American sites, but the primary difference here must surely be that after the Peace of Paris in 1763 France had no mainland North American colonies so that Rouen faience on French sites must be pre-ca.1763 at least in terms of manufacture whereas its appearance on English or Anglo-American sites in North America during the period ca.1775-1800 is occasioned partly by the rescinding of the British law forbidding trade in non-stoneware ceramics but mainly by the American Revolution altering the supply markets. A similar example is the appearance of Dutch clay pipes at Williamsburg only in Revolutionary War contexts (Noël Hume 1968; 1970:307)—their absence prior to that time is surely attributable to Britain's mercantilist policy and their presence during this period to French sympathy with the United States (the French largely relying on Dutch pipes at this time) when the Revolutionary War cut off British trade.

True, this would not effect South's calculations, for there is little effective difference between a terminal date occasioned by cessation of supply and one caused by production ending, but from the point of view of methodology it seems as well to clarify what is really meant by the terminal date. In certain cases, indeed, there could be an appreciable difference between terminal date of manufacture and terminal date of supply. The Glasgow clay pipe industry, which dominated the North American market for a good deal of last century, appears to have started ca. 1800, began to expand in the 1840's, reached its zenith in the years on either side of 1880, and thereafter had an extremely prolonged decline, there still being three firms in 1950 and the last firm not closing until 1967; but the period of domination of the North American market had already started by the 1840's and was over before 1891: the mid-range date of manufacture for Glasgow pipes is thus ca.1885, whereas the mid-range date for their major trade to North America is ca.1860.

In fact, historical events causing major cessations of supply such as those indicated above, or other events such as the introduction in the United States in 1891 of legislation requiring imported items to be
marked with their country of origin, are themselves horizons as such changes are likely to be more sudden than the decline and eventual cessation in production of an item, though like other horizons with a "negative" aspect they may tend to be less clear in the archaeologi-cal record because of the heirloom factor. However, the assumption above that Glasgow pipe exports to North America had largely ceased by 1891 is based on the almost total absence of pipes with the maker's name and SCOTLAND on them as opposed to the continent-wide distribution of those with the maker's name and GLASGOW on them (Walker n. d.). The horizon concept, incidentally, as stated by South [p. 76] is certainly correct, Noël Hume giving a clear exposition of it and its ramifications on some prevailing popular thoughts of historical archaeology (Noël Hume 1970:25-6).

The more I work in historical archaeology the more I become convinced its roots lie in economic history: let us have evidence—trade figures, shipping lists, factory production figures, whatever exists in this line—to substantiate as far as possible our arguments, not philosophical conjectures such as Dunnell's. And once we have this, let us have the evidence of social and manufacturing history so that we can discuss the rise and decline in social and technical acceptance of classes of material.

However, as South accepts this concept of a unimodal curve as axiomatic let us examine its use on the temporary assumption it is valid. It seems to me to be implicit in South's use of the date midway between the initial date of manufacture and the terminal date of manufacture that he accepts the idea of the unimodal curve being symmetrical. I do not find South specifically saying this, but he does say [75]:

> Each of these ceramic types is seen to represent a unimodal curve through time as the type was introduced, reached a peak of popularity and then was discontinued. The median date for the ceramic type is the point midway through its period of manufacture

and though this statement is strictly speaking a non sequitur (to which I shall return later) it is difficult to avoid the conclusion that South's median date of manufacture, which is properly speaking the mid-range date, is also assumed to be that of maximum popularity. In other words, the mode of the curve coincides with both the median and the mid-range. If it does not, then why is the mid-range date (which is the term I shall use in this paper for South's median date of manufacture) preferred to the modal or median dates? It seems to me, therefore, that by taking, say 1767 as the mid-range date for production of green-glazed cream-bodied ware (South's Type 33) because the dates for this ware are ca.1759-75, one is also accepting that the ware was increasing steadily in popularity from the time of its initial manufacture until 1767 and that from that date until it was no longer made the ware was steadily declining in popularity.
I see no reason to accept this as an axiom anymore that than all artefacts have a unimodal curve in the first place. Theoretically, indeed, it seems to me more likely there would be an initial sharp rise as the ware was placed on the market, that it would stay at or about a high level initially, and that it would decline relatively slowly from some point thereafter—yet another proof of the primordial pattern, if you wish. Thus a curve resembling that in Figure 1 might be obtained rather than the curve in Figure 2 which I take to be that accepted by South. If the significant date is that at which the ware is at maximum popularity—that is, the point at which there is more of it produced than at any time before or after—then the date is still that at which the curve peaks, the modal date, but taking Figure 1 as the hypothetical curve for green-glazed cream-bodied ware this date is now 1762.50, not 1767. If, however, the date is to signify the point at which as much of the ware has been produced before the date as after it, the median date (and this is the date for which the term median should be reserved) then using Figure 1 the significant date becomes 1764.04. In both these cases the mid-range date, of course, remains as 1767.

Such differences from the date used by South may not be significant, but the variability possible by critical modifications of the basic premises have to be discussed before they can be, probably with adequate reason, disregarded. The terminology used should also be clarified. Even if a difference of four or five years were to be transferred to the final date obtained by South for a site from all his ceramic types the resultant date would still be reasonably accurate when, as South specifically notes [80], the individual mid-range dates for production are themselves only approximations and the mid-range dates for the sites examined are similarly approximations in many cases [pp.90–96 passim]. As South rightly notes, this sort of inaccuracy is relatively minor to the scale of the model being constructed.

One must assume, however, if only on grounds of chance, the presence of an entirely anomalous production pattern for some ceramics, of which quite possibly delft is an example: the mid-range date for its production in the eighteenth century is undeniably 1750, but surely, for reasons noted earlier, there must have been much less production after that date than before—a graph such as that in Figure 3 might approximately represent delft production and use in the eighteenth century, in which case the median date would be 1731.24, eighteen years earlier than the mid-range date, and the modal date 1700.00, fifty years earlier. The former date at least must have some relevance, for theoretically fewer pots being produced should mean fewer sherds will be found as production lessens: as South makes a point [pp. 83, 84, 88] of showing that incorporating sherd counts into his formula adds accuracy to the results vis-à-vis the historical mid-range...
date, one feels the use of alternative dates obtained as indicated above by this writer might produce equal refinements if, of course, there were a practical method of estimating these dates.

Further à propos of sherd counts, will a delft plate and a white salt-glazed stoneware plate (or a delft chamber pot and a Westerwald chamber pot) of comparable size and shape break in approximately the same number of fragments or will one type consistently produce more fragments than the other? Will large utilitarian wares produce significantly more sherds than small decorative items? In other words, would a vessel count be better for calculations than a sherd count?

I also find it difficult to accept South's feeling [101] that factors such as certain ports receiving ceramics from different English ports are irrelevant here, and I am very surprised to see him suggesting, via a quotation from Cleland (1970:122), that these factors are "historical facts...irrelevant to the interpretation of the archaeological data." No fact germane to the subject is "irrelevant" to its interpretation, though such are the differing strands and types of evidence we have to work with that much evidence may be impossible to use. The factors may well be of negligible importance to the horizon concept because distribution of material in colonial North America was by the eighteenth century well enough organized that if Charleston did not import one particular ware Philadelphia would and distribution would not differ [101]; but if Virginia were getting increased amounts of delft because Robert Dinwoodie was owner of the Delftsfield pottery in Glasgow founded in 1748 and lieutenant-governor of the colony 1751-58, one might expect a mode here in the amount of delft appearing on Virginia sites (Walker 1970a:101), a mode which might well not appear outside the colony. A similar situation might obtain in the early eighteenth century, when James Blundell of Liverpool was trading tobacco there from Virginia in exchange for local cargo, for delft was being made in Liverpool by 1716 (Honey 1952:370). None of these occurrences would effect the mid-range date for delft production, but again they might affect the amount of material—that is, the number of sherds appearing on an excavation, which figure forms part of the calculation of South's formula—and thus introduce an at-least-theoretical local variability.

None of this affects the horizon concept—the latter is a generalized statement covering an area where similarities are greater and more important than local divergencies, whereas items such as local supplies from sources such as Blundell and Dinwoodie are the local divergencies. When dealing with the Southeast as a whole one uses the horizon concept, when working with local material within that area one recognizes the need to give more weight to local factors.

Some other questions also arise about the key date used in the formula being that of the period of manufacture. South does admit [72] that the terminal manufacture date cannot usually be fixed with the same
Figures 1-4

To ease calculations the figures opposite appear as bar graphs; the originals were drawn to the same scale as these and on ten-squares-to-the-inch graph paper. In Figures 1, 2, and 4 the scale of the x-axis is a tenth of an inch to a year and in Figure 3 the scale of the x-axis is a tenth of an inch to two years; in all cases the y-axis represents entirely arbitrary amounts.

Figures 1 and 2 both cover an "area" of 144 tenths of an inch; Figure 4 covers an "area" of 154 tenths of an inch. Figure 3 has an "area" of 542 tenths of an inch. The two peaks depicted in Figure 3 occurring in the 1710's and 1750's are a purely artificial indication of possible temporary rises because of, respectively, the beginning of the Liverpool and the beginning of the Glasgow delft industries.

I am very grateful to S. Epps of the National Historic Sites Service for preparing these drawings for publication.
degree of accuracy as the commencing date of manufacture, and later [77 ]

he recognizes the heirloom factor as contributing sherds from an earlier
period into a later context; but he still accepts [ 73-75 ] the mid-
range date of only the period of manufacture as his working date. It

seems to me that while the ware can certainly appear on a site the same
year as it was first manufactured and obviously cannot appear before
that date, it is certainly not likely to disappear from use the year
it ceases to be made. Even if most owners had ceased to buy the ware
before the terminal manufacture date—which they would tend to do if
the ware were going out of favour, though not necessarily if the manu-
facturer arbitrarily decided to end a line and force another on the
market like clothing designers with minis and maxis—the ware would
continue in use some years after that date simply because it had a cer-
tain life expectancy.

How much longer would depend not only on how long a ware remained
in favour but on such things as toughness and use. Bellarmines have
lasted in extreme cases for over 200 years (NoH1 Hume 1958:439; Moore
1965:75-6) and one would expect good mileage out of tough Westerwald
chamber pots and beer mugs; but one would also anticipate a long life
for prestige wares such as the pseudo-classical pieces in rosso antico
and black Basaltes. One also has to remember, when talking of the
lifespan of ceramics, that they could be mended after breaking as
South himself pointed out elsewhere (South 1968:62-71), and that mend-
ing is known for ceramics as varied as coarse earthenware and porcelain.
One might, however, estimate two to ten years at a rough guess for the
life expectancy of good quality household ceramics in the eighteenth
century, though one might also expect that period to be reduced if a
family conscious of technological and social change deliberately re-
placed its tableware as white salt-glazed stonewares, creamwares, and
pearlwares were in their turn introduced.

It seems to me that the heirloom factor is too lightly disregarded
by South, his only recognition of it in his Figure 1a is a reference to
a bellarmine dated to the 1760's.* At a practical level it may not
significantly affect calculations using the mid-range date for the
period of manufacture only, but again it deserves to be considered: as
noted earlier, what is really meant by terminal date? Adding say five
years' continuation (obviously at an attenuated level) to the curve for
green-glazed cream-bodied earthenware as depicted in Figure 1, we then
have a curve such as that in Figure 4 for the theoretical production and
lifespan of the ware in terms of the period over which its use (and
breakage) will remain for the archaeological record. The median date,
the point at which equal amounts of material have been produced before
and after, is now 1764.60; the midpoint date for the combined period of
manufacture and use, on the other hand, is now 1769.50.

*I am not sure whether South means here a bellarmine with an in-
scribed date of the 1760's or one found in a 1760's context; NoH1 Hume
(1963:289) notes bellarmine fragments at Williamsburg in 1760's con-
texts. Fleming (1923:254-6), who is admittedly not a reliable source,
says bellarmines were being made at Prestonpans near Edinburgh in the
eighteenth century.

[Editor's Note]: A bellarmine with an inscribed date in the 1760's
was recently brought to NoH1 Hume's attention.
If these objections have any validity then why are South's dates as derived from his formula so accurate for apparently the whole of the eighteenth century? The answer is very likely that the potential variables noted above are either too small to alter things substantially, or that they cancel each other out, or that both contribute. If we do not accept the symmetrical unimodal curve as axiomatic, it will be replaced for many of the ceramic types by an infinite set of variations running through asymmetrical unimodal curves to multimodal and amodal. Even a multimodal curve can have a "best fit" unimodal curve superimposed on it, and while the result would be meaningless for giving an accurate idea of annual trade or consumption it might still be grossly accurate enough to be acceptable to South's formula.

Individual curves for certain production centres may make little difference to a generalized overall curve. A curve for the Bristol pipe industry would closely follow the generalized curve representing English pipe production as described earlier; with the industry starting in the early seventeenth century, reaching its height in the first three decades of the eighteenth century, almost dying following the loss of colonial trade with the Stamp Act troubles and the American Revolution, reviving in the earlier nineteenth century to come to a peak in mid-century and to rapidly and finally collapse in the 1860's. A curve for the Liverpool pipe industry, on the other hand, would indicate little before 1760 but a spectacular rise thereafter with a peak or peaks in the earlier nineteenth century followed by the final decline with a possible temporary rise ca.1850; but this seeming eccentricity does not offset the general decline of the English pipe industry over the greater part of the eighteenth century.

The rise of the Glasgow and Edinburgh pipe industries in the first half of the nineteenth century after very minor existences for two centuries produce asymmetric curves of the opposite shape to that shown in Figure 1, but adding their production to that for England to produce a curve for British pipes in the nineteenth century would make little difference beyond extending the final decline in use of the clay pipe rather more into the present century. Even the addition of curves for the Montreal pipe industry (ca.1850-1910) and that in Detroit (ca.1880-1910) would make little appreciable difference to a generalized curve showing the popularity and decline of the clay pipe in North America—the most it would do would be to cushion somewhat the final decline of the clay pipe in the second half of the last century. The addition of a curve for Liverpool delft for the period ca.1716-ca.1784—with a high-point at least so far as number of makers is concerned ca.1760 when 12 makers are known (Charleston 1955:101)—and a curve of some sort for the Delftsfield production over the period 1748-ca.1810 (Archer 1966: 16-18) would not materially affect the decline of British delft production in the face of successively superior ceramic types being produced throughout the eighteenth century as noted earlier.
Essentially, therefore, median (sensu stricto) dates for a diversified collection of ceramics are likely to be scattered on either side of South's mid-range dates, though still within the period of manufacture (South's "date range" in his Figure 1a) unless some type has a colossal and disproportionate heirloom factor or some other cause of vast timelag. Further, as South points out [80], "it may well be found that some of the longer span [artefact] types can be eliminated from consideration until such time that diagnostic temporal attributes can be determined" and this would remove objections to such long-lived and amorphous categories as delft noted above. Certainly if, say Bristol, Liverpool, or Glasgow delft can be identified with reasonable certainty considerable refinement of dating would be possible, for Bristol was producing delft ca.1647-ca.1777 (Honey 1952:92) and Liverpool and Glasgow during the periods noted above. As it is, the delft problem is eased by South's being able to give dates within the 200-year odd span of this class of ceramics for different vessel-types such as pedestal-footed ointment pots (South's Type 32), plain washbasins (South's Type 65), or polychrome apothecary jars and pots (South's Type 72).

The question must now be asked as to whether all the preceding pages of argument are not merely tendentious minutiae. Certainly one can point to the attempts by Chalkley (1955a:9 pages; 1955b:3-10) to discredit Harrington's method of dating pipes as vivid warnings of how not to appraise a new method of examining archaeological evidence. However, it seems to this reviewer that the validity of some of the methodological concepts could have been more rigorously and critically examined before being accepted. Hanson (1971:2-15), for example, has shown that Binford used both poor methodology and faulty mathematics when evolving his straight-line regression formula for dating clay pipes from Harrington's bar graphs: in fact dates derived from Binford's formula will fall within the standard deviations of the date from the same material when the relevant Hanson formulae are applied, but the inmost cases' minor differences between Binford and Hanson dates for the same material* are not valid reasons for dismissing Hanson's criticisms.

The fact that the methodology is probably as accurate as is practical should not be allowed to obscure the essential artificiality and

*To cite a single example, the dates for the stems found at the Schurz site, Bronx County, New York City (Omwake 1958:3-13), are 1716.44 by the Binford formula; 1716.08±18.00 for the Hanson formula covering the same period as the Binford formula (Number 7, 1620-1800); and 1709.20±17.50 for the Hanson formula most appropriate to the period of time covered by the Schurz site pipes on other evidence (Number 6, 1680-1750). The date estimated from the Harrington bar graphs is ca. 1700-40.

[Editor's Note]: See papers by Binford and Hanson on pipe stem formulas in the following section of this forum.
shakiness of some of the basic concepts. In particular, I do not accept
the concept of unimodal rise and decline just because Dunnell puts it
forward as a sort of anthropological Newton's law. Further, and this
goes back several pages to my passing comment that one of South's state-
ments is really a non sequitur, there is really no connection between
Dunnell's premise even if it were true, and the method of calculating
a date for a site as outlined here by South. South is working with the
mid-range date (or at least a mid-range of manufacture date) for classes
of ceramics, and any connection between this date and Dunnell's concept
is chance and in any case irrelevant. The method could be improved (in
theory) by taking into consideration this writer's suggestions as to
which dates to use but the only relevance I see to Dunnell's idea is
that the latter happens to have set off a chain of thought in South's
mind leading to the evolving of the formula described in this paper.
South is taking a relatively arbitrary but logical enough date, applying
it consistently to all his ceramic assemblages, and then modifying it
to allow for quantity within each assemblage: this is simply a piece of
logical mathematics unconnected to Dunnell's philosophical pose.

At a practical level, however, South repeatedly emphasizes the
need to apply his formula intelligently and in the light of archaeologi-
cal and historical evidence relating to the particular site under
investigation; and it is at this level that South in fact includes some
of the modifying circumstances touched on earlier by this writer when
querying the accuracy of the principles applied to South's formula, such
as the likely absence of porcelain from English sites during the period
c.1640-80 [99], and other points such as the greater popularity of tea
drinking from the mid-eighteenth century being reflected in the archae-
ological record from that time [99]. As South notes [82]:

We should keep in mind the nature of the deposit, which
may have an important bearing on our interpreted occupa-
tion brackets....The validity of the interpreted occupa-
tion period would still depend on the nature of the
archaeological data on which it is based.

Indeed, most of the sites noted by South [90-96] illustrate the need
to use intelligence and experience in interpreting the data—the cream-
ware in the top layer of fill in the cellar at First Fort Moore giving
the terminus post quem for the fill of the cellar but not being related
to the date of the occupation material beneath the fill [90-91] for
example, or the single whiteware sherd in the plough zone at Goudy's
Post being a late stray and unconnected with the 1751-60 occupation of
the post [92-93]. As an adjunct to this, of course, is the necessity
for adequate excavation and recording techniques [88], though it must
be stressed a sloppy excavation will negate any form of interpretation.

Equally important, however, for the meaningful application of South's
formula is a knowledge of ceramic types and a familiarity with archae-
ological specimens [86]. South's formula is no shortcut to some
objective mathematical date devoid of the contamination of Cleland and Fitting's "mystique of expertise" (Cleland and Fitting 1968:130)—on the contrary, its "degree of refinement...is dependent upon the degree of sophistication of the archaeologist's ceramic knowledge" [80]. The accuracy and usefulness of South's formula will depend entirely on the intelligence and interpretive ability of the archaeologist using it, which is exactly as it should be. My only worry here is that some archaeologists may feel they have done an adequate report when they have successfully applied the formula, thus hindering rather than stimulating further research; hopefully no one will try to distort the historical evidence for the date of a site to make the formula work (Walker 1970b:160-61).

Regarding its usefulness South merely notes that while more knowledgeable archaeologists may find little need for the method, and some may prefer to deal primarily with a terminus post quem date for a deposit, he personally has found the formula useful [80, 86]. One might query, however, whether the division between those using quantitative evidence of the sort used by South in evolving this formula and those using only presence-absence of material is as sharp as South appears to suggest. Surely most archaeologists if faced with (to take an oversimplified example) a large amount of creamware but little pearlware from a context which they were satisfied was a unity would assume that occupation had ceased soon after ca.1780 when pearlwares would have been coming into use. They would then extend this farther and deduce from, say, the total lack of willow-patterned transfer-printed pearlware that the site was no longer in use by ca.1795 and had therefore been deserted shortly after 1780 and definitely by ca.1795. The fact that there had not actually been a sherd count would not prevent a purely visual estimation based on amount of material in artefact boxes on a tray.

South also notes [81] the extent of usefulness has still to be determined, but he has certainly presented convincing evidence that in the eighteenth century and in the southeast United States it is remarkably accurate. Further, to whatever degree one wishes to pursue the methods outlined here, the presentation of 82 ceramic types of the seventeenth, eighteenth, and earlier nineteenth centuries in a clear set of categories with their at present known date-ranges and page references to Noel Hume's A Guide to Artifacts of Colonial America in his Figure 1a puts all historical archaeologists in a considerable debt to South for providing such a handy reference. Perhaps the presentation of a scientific-looking formula to mathematics-prone anthropologists who feel their lack of knowledge of historical artefacts helps them create objective descriptions of the latter will make them learn something about the field— that way we may avoid such gems of objective description as "blue-on-white crockery" and "crockery plain, decorated" which still appear (Mayer-Oakes 1970:247, 250, 255, 270 Fig. 116) from those archaeologists who are unequipped to deal with historical material.
Over the past few years this writer has tried to express his feeling about the poverty of so much North American historical archaeology (Walker 1968a:23-34; 1968b:105-23; 1970c:62-9). In trenchant comments in his paper South says succinctly much of this reviewer's feelings:

"Historical archaeology is plagued by reports revealing no interpretation of any kind, historical, anthropological, cultural or archaeological to justify a catalog type publication of objects... Historical archaeology has now reached the point where we should begin to explore [cultural concepts] rather than continuing to crowd our bookshelves with descriptive catalogs of our systematized relic collecting devoid of any redeeming analytical or interpretive value [86, 102]."

Quoting Sherwood Washburn, South suggests [79] there has been an assumption among archaeologists that if description is accurate enough and plentiful enough problems of "process, pattern, and interpretation" will be solved, a view which is patently wrong, as Washburn indicates; and five years ago this writer quoted W. I. B. Beveridge as saying exactly the same thing (Walker 1968a:27). Citing Hempel's Philosophy of Natural Science, South emphasizes [102] that hypotheses and theories are invented to account for observed facts, not derived from them; and again five years ago this writer said much the same thing when asserting scientific investigation was an art, not a science, and that artefact typologies were hypotheses which might or might not prove to be true and were occasioned by a lack of knowledge in a subject, being the beginning of a study, not the culmination (Walker 1968a:30).

Perhaps what is lacking among historical archaeologists is the "creative imagination" Hempel indicates is necessary to move from data to theory. What does one do with a report such as one which crossed my desk, which took months to compile and consists of descriptions of largely amorphous sherds whose thickness was measured to three decimal places of a centimetre, whose Mohs hardness was religiously measured, and whose decorative colours were all given in Munsell code but which failed to interpret this accumulated data or (perhaps significantly) even suggest to what use the data might be put? These reams of undigested and undigestible figures and attributes are for the catalogue card; a report is supposed to be the abstraction from the catalogue card of order and discovery of meaning, to paraphrase South [102], not the pagination of catalogue cards.

The trouble is, however, that in order to reach the stage to which South urges us we have to have the basic data, and in far too many cases we do not have this either at all or in acceptable quality. Where are the well-illustrated corpora of ceramics (or any other artefacts, for that matter) from sites major and minor throughout North America? Very few have ever appeared (G. Stone 1970:78). The nine photographic plates
illustrating European ceramics in South's paper on Brunswick Town ceramics of ten years ago (South 1962:1-5) are still among the best I have seen, and they and those accompanying Fairbanks' article on European ceramics from New Echota in the same volume as South's paper (Fairbanks 1962:10-16) should be imitated by every historical archaeologist. South's paper on the products of Gottfried Aust at Bethabara (South 1967:33-52), rightly described by Noel Hume as "splendidly illustrated" (NoéHume 1969:310), is a model of how drawings in a ceramic report should be presented—so far as I am aware few in North America have attempted to emulate this. Other excellent ceramic reports which come to mind are those by NoéHume and Watkins on the products of the "Poor Potter" of Yorktown (Watkins and NoéHume 1967) and by Watkins on North Devon wares (Watkins 1960); while NoéHume's Rosewell (1962), Clay Bank (1966a), and Tutter's Neck (1966b) reports present a quality of presentation which few people—least of all those most vociferous in accusing NoéHume of "low level" work, such as Cleland and Fitting (1968:passim)—seem able to emulate. Much more solid research and presentation of archaeological data appears in the four issues so far (1967-70) of Post-Medieval Archaeology than in the first four issues for the same period of Historical Archaeology, which latter seems in danger of becoming a repository for maudlin articles on philosophy, poor photographs, and worse section-drawings.*

In partially defending restoration projects against Cleland and Fitting’s accusations that they are some sort of latter-day Sodom and Gomorrah’s indulging in professional perversion, this writer has pointed out (Walker 1970c:63; 1970a:105-6) that for all their undoubted faults and limitations they still offer us the greatest potential, for they have both the money and the desire to undertake total, or relatively total, excavation of properties of socially different people living within the same community [cf. South, 100]. However, it must be admitted that this potential has been singularly wasted so far. Not one of the major historical reconstructions in North America involving archaeology—Jamestown, Plimouth, Williamsburg, Michilimackinac, Louisbourg, Bethlehem—has provided historical archaeologists with adequate publication of their vast stores of dated and datable artefact material; some, indeed, notably Plimouth and Bethlehem, appear to have published no formal reports at all that I know of.

Considering Cleland’s association with the Fort Michilimackinac programme one would have expected a series of "high level" monographs on the interpretive archaeology of that site from his pen, but this is hardly the case; and he appears to be unfamiliar with some of the practical divisions of ceramics when he protests (Cleland 1970:119) G. W. Stone’s separation of delft from other earthenware in his study of ceramics entries in his study of Massachusetts inventories (G. Stone 1970a:78). It would have been obvious to someone less concerned with artificial classification that delft differed from other earthenwares for several reasons, not the least obvious being its social position as the

*Two days after sending this manuscript to South I received the 1971 issue of Historical Archaeology; it is only fair to add here that its contents are somewhat better than those of its four predecessors.
poorer man's porcelain by virtue of its imitative style, as indeed Stone (1970b:124) points out in his reply. Cleland and Fitting also espouse the cause of "well-defined classificatory systems" (Cleland and Fitting 1968:134) such as Marwitt's punch card system for ceramics (Marwitt 1967:19-26) but I know of no study of theirs which applies such a system and considers the accuracy of the results—indeed, Fitting has indicated he has never written a report on an historical excavation (Walker 1970c: 67)—which makes it difficult to avoid concluding they simply have a penchant towards any system that looks neat, tidy, and scientific, which is not necessarily the most intelligent approach.

The trouble with all those "well-defined classificatory systems" is that they are never well-defined enough to prevent someone else altering it without our ever seeming to get nearer a viable system. Thus B. B. Powell criticizes Caywood, Cotter, Maxwell and Binford, and South (Powell 1962:34-45) and is in turn criticized by Miller and L. M. Stone (1970: 3-4). I do not propose to review Miller and Stone's work; but any classification system which supposes that "Blue and White' and "Polychrome" are key attributes to meaningful delft types or the classes "Plain," "Relief Borders," "Polychrome," "Handles, Finials, Spouts"(!), and "Transfer Printed" (Miller and L. Stone 1970:28-37, 44-50) to be well-defined and non-overlapping subdivisions of English creamware does not get my plaudits for representing "the objective methods of anthropology" (Jelks 1970:vii)—if I ever thought that anthropology or any other field of research was objective.

The fact that all these systems break down does not, of course, mean that some adequate system may not eventually be found; but North American archaeologists seem to be wasting decades trying to find it when perhaps they might better consider the possibility the system will never be found and thus turn to more profitable fields of research. Dunton, who has spent many years with artefacts first at Williamsburg and then at Louisbourg, has indeed said:

Nine times out of ten, for artifacts of the North American colonial period, the subject will not conform to the strict approach and still retain a working usefulness. Historic-period artifacts seem to have a built-in resistance to regimentation. Systems which attempt this tend to be either so general as to be pointless, or so convoluted in their detail that no one cares to bother (Dunton 1970:401).

What is needed is a pragmatic description of key attributes, not some universal Linnean classification. A pottery type can have one key attribute or several [cf. South 71]: in pearlware the key attribute is a faint blue tinge to the glaze, in stoneware the decisive attribute is body hardness while colour becomes important only with the next stage of stoneware identification when one is considering whether it is Rhenish or Westerwald or English or whatever. Mocha and related
wares have banded decoration as their key attribute and can be creamware or pearlware or various nineteenth century white earthenwares.

The trouble with classification systems is they become an easy out for people unable to do basic research—they represent, in fact, a displacement activity. As Noël Hume has pointed out, anthropologically trained prehistoric archaeologists ignorant of historical artefacts waste time and money "laboriously compiling useless pottery typologies in quest for dating and nomenclatures that should be sought amid the vast corpus of material already published on the subject" (Noël Hume 1967:104-5). The root of the problem is simply, as Noël Hume goes on to say, that not being acquainted with the field, the unqualified archaeologist assumes he must start from scratch in a totally unknown subject. Some Forum contributor will doubtless protest South's classification in his Figure 1a because mixing together technological categories (wares), functional categories (basins, jars), and decorative categories (Littler's blue on three different wares) isn't "logical"; and shouldn't green-glazed cream-bodied ware be under creamware not refined earthenware (or maybe creamware should be under refined earthenwares along with all the pearlwares) and not all mocha is pearlware or even creamware—frankly I see no point in this sort of intellectual dead end. Ford, who is among the American anthropologists cited by South as contributing concepts towards Dunnell's unimodal curve philosophy, also says (Ford 1969:5)

in regard to the establishment of pottery types or any other useful historical device, the classificatory units must be selected on the basis of a reasoned guess as to the actual sequence of events....That there is an empirical methodology for the selection of "traits", "types", or cultural phases that will reveal the historical facts when properly manipulated is a fallacy that at the moment is wasting thousands of dollars spent on computer time.

Neustupný (1971:37) has said the same thing:

One chooses the properties worth studying on the basis of one's previous knowledge of the subject....Once the archaeologist knows that what he has before him is an axe, or at least suspects it, he can describe it effectively....If he does not recognize the original function of an archaeological object, one can rarely describe it so that the description is useful to others.

This is why it is possible to describe clay pipestem fragments as tubular beads, as a prehistoric anthropologist did in western Canada when called upon to excavate an historical site (Walker 1968a:31). It is all very well Miller and L. M. Stone quoting approvingly
L. Stone 1970:5) my remark that an artefact typology is the beginning, not the end, of a study and that it is occasioned by a lack of knowledge of the field; but enough is known about ceramics to obviate the need for classification systems of the type they themselves present. True, some historical archaeologists may not have this knowledge, but as South says [80, 86] they should have this knowledge.

I happen to know that Jew's harps are musical instruments, but if I did not, I do not think I could find this definition in L. M. Stone's paper involving this artefact (L. Stone 1971:90-102). The history of the instrument is not touched and its distribution in colonial North America and chronology in terms of the typology put forward are barely noted. Where were those used in North America made? To sample documentary sources and list five "typical references" to contextless shapements seems to me a pretty thin attempt to search for literary evidence, unless one is really to believe with Cleland that documentary sources here are like knowledge of who lived on a site, what they did for a living, and what their net worth was:"historic facts that are really irrelevant to the interpretation of the archaeological data" (Cleland 1970:122).

It is perhaps worthwhile here to consider the terms "history" and "evolution." South believes strongly in the "evolutionary concept of changing ceramic form through time as a dating tool as seen in fragments recovered from historic sites" [73], and indeed one of the raisons d'être of his paper is to show that this method of dating is valid and that Dollar is wrong in feeling it to be so inaccurate as to be useless (Dollar 1968:passim esp. 13-19). That ceramics (and most other artefacts) do tend to evolve and change through time, and also for that matter regionally, is presumably not seriously questioned by anyone. The trouble is that typologies appearing logical to the person constructing them are (even if they are indeed logical, which is usually not the case) likely to be irrelevant because the manufacturers or the consumers were either not logical in what they wanted or liked or else were logical in a way not considered by the typology-compiler. Therefore unless we have some extra-archaeological assistance in seeing how items evolved we are generally wasting our time. Thus L. M. Stone's Jew's harp typology may indeed be an adequate shorthand for describing "types" of Jew's harps but we need evidence of how relevant this is to how successive manufacturers, possibly in different countries, changed their styles over a period of time before we can use such a "typology" for dating purposes. Specifically, we compare and contrast material from chronologically-known sites to find the evolutionary pattern of an artefact; we do not, with any hope of accuracy, date sites relative to each other by observing that Site B Jew's harps "logically" are of later style than those on Site A, and therefore Site B must succeed Site A unless we are totally bereft of all other dating techniques.

Typologically, English clay pipes pass through, or at least appear in, a number of types in the seventeenth century mostly of fairly short
time-span and sometimes in regional styles; many of these styles overlap chronologically and regional varieties can exist together in the same production centre. By the eighteenth century types are changing more slowly and with more minor variations, though regional varieties still exist. By and large typology for dating plays a less useful part in dating eighteenth century pipes than in the preceding century. In the nineteenth century regional styles have pretty well died out with the exception of some Broseley features made famous by the top-quality products of the Southern family there; shapes and sizes abound, Continental forms and features are copied as are eighteenth century shapes, and generally mass-production and an imitative market make identification of material extremely difficult unless the maker's name and place of work or some other definitely distinctive mark appears. Typologically speaking it usually isn't possible to discern evolution of one form to another. In 1900, when the heyday of the Glasgow pipe industry was over, the firm of William White still included 600 pipe-styles in a price-list, all of them available; and by the time they closed in 1955 the firm had accumulated a stock of 2,000 moulds. The London firm of J. G. Reynolds and their successors working in the later nineteenth and earlier twentieth centuries claimed to have 1,000 moulds available; and McDougall's of Glasgow were still using between 30 and 40 of their hundreds of moulds when they closed in 1967, in order to cater to regional and other tastes. To identify and illustrate these types (virtually no illustrated catalogues have survived) would be of the greatest use, but how they could be "typologized" I cannot see. Admittedly, other dating criteria can be found, and a nineteenth century imitation of an eighteenth century shape can usually be identified by rougher clay and poorer finish (the first a technological "improvement," the latter a socioeconomic effect), but one really does not have a "typology." On the other hand, "evolution" certainly continues to take place.

I see no necessary contradiction between South's belief—and convincing proof by use of his formula—that deposits of eighteenth century ceramics in the Southeast can be dated through a knowledge of their date range, and Dollar's belief that in the West in the nineteenth century such a dating method does not seem to work (Dollar 1968:passim esp. 13-19)—both may be correct. It will be up to those of us in various areas to use South's formula to see what does happen over a protracted period of time in their own area. Certainly the nineteenth century is going to cause problems: Noel Hume (1970:130-1) talks of the nineteenth century hard white wares and semi-porcelains as being "extremely difficult to date with accuracy (unless bearing factory marks)"; and Collard in her scholarly work on nineteenth century Canadian ceramics believes that any attempt to date willow-pattern decorated earthenware other than by maker's marks is very uncertain (Collard 1967:122).

My major reservation about the extolling of typology is the danger of its becoming an excessively generalizing technique. As I noted five
years ago (Walker 1968a:30), it tends to suppose linear successions. Indeed, as T. G. E. Powell has gone so far as to say, à propos of European Neolithic chambered tombs, a much typologized and re-typologized subject, "the greatest abuse of typology lies in its implication for evolutionary development"; though Daniel correctly points out the abuse is the assumption a system is right before it is securely anchored at the beginning, end, and preferably middle, by independent chronological evidence (Daniel 1970:266). Nothing is wrong with typologies as hypotheses, it is the mindless following and aping of them that is disastrous.

Elsewhere, however, South means much more by "evolution" than merely typology, using it to mean the ultimate goal we should be after in our work (South 1970:57). This end is what I prefer to call history (Walker 1970c:63-5), though I realize this latter term appears to have a much more restricted meaning to many historical archaeologists, South (1970:57) and Cleland and Fitting (1968:132) taking it to mean nothing beyond "unique" or "specific" events. I do not know what school of thought fosters this definition, but it is certainly doing the field of historical archaeology considerable harm by suggesting history is nothing beyond a grade-ten outlook on Forts, Battles, and Dates. Admittedly "historic site" markers tend to foster the idea that what is important are certain specific (not unique) events devoid of cause and effect, the "here was born/died/was killed X, the first/last president/general/king/inventor to sign/do/find/massacre/be massacred" outlook; but history is the interpretation of whole series of interrelated events, their causes, and their effects—little about it is ever unique. The editorial of the first volume of Medieval Archaeology (1956) noted "The day is past when the historian, the philologist, the art historian and the archaeologist could ignore each other...without attracting adverse criticism for such action" (Wilson 1957:2): if we continue to think of history as laundry-lists of dates, etc., we are never going to get anything out of it for the interpretation of our sites, and without this our ideas are going to be seriously deprived of needed stimulation. It is all very well to say we have to keep strands of evidence separate and data separate from interpretation, but in real life these strands and aspects are never separate and insistence on separation into neat classified chests of drawers simply leads to artificial beliefs and situations.

Some philosophical trivia downgrade the study of artefacts per se. Take for example Schuyler's "The Use of Historic Analogs in Archaeology" (Schuyler 1968:390-2). If Schuyler is indeed the first American Archaeologist to realize literary sources are useful for identifying archaeological material then he must be congratulated; but if so he cannot object, as he does elsewhere (Schuyler 1970:230-1), to my once having suggested American archaeology seems about two generations behind British archaeology; if on the other hand most American archaeologists are aware of the usefulness of literary sources, where does this leave Schuyler?
Clearly, stating the obvious and doing so in an unnecessarily scientific-sounding manner: the use of analogue (or analog) in the title either betrays unfamiliarity with the English language—which may be the case, as elsewhere Schuyler appears to think Johnson's dictionary is the standard reference for British English (Schuyler 1971:83)—or more likely indicates a desire to impress the reader by using a word which the layman thinks of as something to do with a computer and therefore frightfully scientific. The field is not improved by such misuse of terminology, as Osborne has noted (Osborne 1968:382-3). However, nothing is so bad it could not have been worse—we might have found a connection between literary sources and artefacts by throwing dice (Rowlett 1970:491).

The point is, to return to the original issue, we are too often not getting the basic data in the way suited to its use for the interpretive archaeology South so correctly urges. Think of the potential in tracing the social and cultural trends of two successive colonial capitals within the same geographical area possible from a study of Jamestown and Williamsburg material: the chances of this seem remote because there appears no evidence that the National Park Service is going to produce well-illustrated monographs on the Jamestown material and because Colonial Williamsburg appears to prevent its Director of Archaeology from publishing reports of excavations done there (Cotter 1970:432-4; 1971: 109). (This is not to decry the fine booklet series covering both artefacts and excavations written by Noël Hume (1969, etc.)—would that any other of the restorations noted earlier had similar series—but they do not remove the need for a monograph series if detailed work of the sort suggested above is to be undertaken.) To take another sort of comparative study possible, that of two settlements of similar date and origin but geographically separate, are we ever going to be able to compare the two seventeenth century colonial settlements of Jamestown and Plymouth (Stone 1970a:75-7, 79)?—it does not appear likely. Equally valid would be comparisons between, say, two chronologically contemporaneous or geographically adjacent areas whose settlers were markedly different ethnically—the Pennsylvania Deutch or the French Canadians would be obvious groups with which to start. What about studies comparing and contrasting different cultural occupations on the same site—Cleland has shown the potential of this in his comparative study of faunal remains and hence meat sources of the French and English at Fort Michilimackinac, and has added for good measure a study of faunal remains from a slightly earlier Indian site to represent the native adaption to the local environment (Cleland 1971:7-23); and ten years ago Binford briefly touched upon other possible topics in this field with reference to Michilimackinac (Binford 1962:50-2), but otherwise this rich topic has been little touched.

Foley can tell us what industrial archaeology isn't (and mustn't and shouldn't) (Foley 1969:66-8; 1970:93-4) but we still await excavation reports and artefacts studies from Bethlehem. Michilimackinac has for over ten years been producing popular booklets and leaflets and
generally doing what Cleland and Fitting appear to object to most, the haute vulgarisation of restoration archaeology; though at least Miller and L. M. Stone's publication provides us with excellent illustrations of ceramics from the site as well as a bonus in the form of some other material, including unpublished material from Louisbourg.* However to take one example, I do not believe the late H. G. Omwake's study of some of the pipes from Michilimackinac (Omwake 1962) written ten years ago has ever appeared, though two popular papers including information from it have appeared (Peterson 1963a; 1963b:1-6). How about studies within the one site comparing and contrasting socially-different occupancies of houses? There is some evidence that archaeology at least in some instances can discern differences here [South 90 ] but there are major problems as both South [100-01] and this writer (Walker 1970a: 104-6) have noted. Still, it remains a major challenge which will have to be examined.

While in entire agreement with South, therefore, as to the desirability of moving away from systematized relic-collecting to explore cultural concepts, I fear the field as a whole is not yet producing enough adequate basic data on which to build more ambitious studies. Not all of us can hope to excavate the perfect site which will enable us to create the perfect report, for too frequently sites simply do not have surviving evidence to provide reasonable socioeconomic suggestions or even proof of use of a structure excavated [see South but we can all publish our material in an acceptable standard even if we were excavating for something as peculiar as the base of the flagpole from which flew the star-spangled banner at Fort McHenry (Larrabee 1970:69).

South notes at one point that "A book incorporating a definite typology for English ceramics is still to be written" [79 ]. I have some doubt as to whether such a work will ever be written, but until it does appear, what I should like to see is a well-illustrated handbook with brief paragraphs on the history of each ware or class, description of key attributes, listing of various terms used for the ware or class of object or varieties of these (e.g., bellarmine, d'alva bottle, Cologne ware, tiger ware, and Rhenish brown salt-glazed stoneware; or mocha ware, annular wares, banded wares, and dipped wares; or creamware, cream-colour(ed) earthenware, queen's ware, Leeds ware, and

*Since this was written a survey of glass from Fort Michilimackinac—Margaret Kimball Brown's "Glass from Fort Michilimackinac: A Classification for Eighteenth Century Glass," The Michigan Archaeologist Volume 17 Number 3-4 (September-December 1971)—has also appeared.
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Whieldon ware*), and most importantly a list of false or misleading attributes. Under the latter would be such things as "blue (or green) edging is not part of the definition of pearlware"; "not all transfer-printed ceramics have to have a willow-pattern design"; "not all mocha ware has to have a brown background to the decoration"; "feather-edging is not restricted to creamware"; and the like.

No such handbook is going to be perfect, but if it enables the average historical archaeologist to correctly identify 75 per cent of his ceramic finds this will be 74.9 per cent better than talking about blue and white glazed crockery. The future of the field lies with the ability of those at a local level to produce usable compendia of data, and local work depends greatly on minor professionals and nonprofessionals. Many of these people work in difficult circumstances with little encouragement, and in situations where they cannot easily have access to the vast and complex literature in the field or to good comparative collections. These are the people for whom good illustrated catalogues would be particularly valuable.

I should like to see a publication taking South's 82 listed ceramic classes and types plus any other relevant material (e.g., the green-glazed coarse earthenware found on French sites from Canada to the Gulf States) and being produced as such a handbook. Its need, indeed is indicated, both consciously and unconsciously, in a statement by South, who criticizes Harris (1971:67) for talking of Whieldon ware and Rockingham ware as though they were the same when in fact their "source of origin [is] separated by the Atlantic Ocean and one hundred years in time"[86]. South is certainly right in protesting this misuse of terms, but he himself is guilty of inexactness here, for Rockingham ware (assuming we mean by this the Rockingham glaze) was certainly not a wholly American product as South's statement here implies; and while it was certainly being produced in the mid-nineteenth century as South indicates, it originates in the first or second decade of the century at the Rockingham factory at Swinton in England (Mankowitz and Haggar 1957:193). The problem with Harris' description appears to be that the term "Rockingham glaze," which correctly means a mottled brown glaze, has been used as though it means any mottled effect, as with Whieldon ware which is a creamware, or at least a ware with a cream-coloured body of the creamware family, with mottled grey, green, yellow, and brown glazes. Here is clearly a case of an incomplete understanding of what the attributes of Rockingham glaze are, and one which could be clarified by a handbook of the type envisaged.

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*The appearance since this paper was drafted of I. Noël Hume's "The What, Who, and When of English Creamware Plate Design" (Antiques Volume CI, Number 2 (February 1972:350-5) goes much of the way to the sort of discussion envisaged on the creamware family.
In summing up this examination of South's article, the following points can be made. Regardless of the artificiality of Dunnell's premise on which South has based his formula for obtaining a median date for a site or context through its ceramic contents South has presented us with a formula which works with considerable accuracy on a wide range of types of site in the southeast United States in the eighteenth century. From this research can proceed in several ways: it can examine by the same method sites within the same area but of an earlier period to confirm or modify what South has found when applying his formula to the seventeenth century site of Charles Towne. The archaeologically observed socioeconomic difference between the wealthy and poorer classes noted by Noël Hume to be present in the seventeenth century but absent in the eighteenth [South 90] seems a probable cause of the discrepancy in the system observed at Charles Towne; G. W. Stone found hints of this in his survey of Suffolk County, Massachusetts, wills (G. Stone 1970a:83-4). The research can also go forward into the nineteenth century: it may be that both South and Dollar are correct when the former produced evidence from the eighteenth century Southeast to uphold the usefulness of manufacturing dates for dating sites and the latter finds dating sites in the West by this method to be so inaccurate as to be virtually useless.

This brings one to the next piece of research, the comparison between different geographical areas at the same period of time. This writer has already suggested an example in the comparison of Plimoth and Jamestown; again G. W. Stone's work in the Boston area suggests different ceramics were used there from those found at Jamestown, but whether economic, religious, or some other factor or a combination of various factors is responsible cannot yet be said (G. Stone 1970a:77). However, before such a study it would be preferable to do a study similar to South's to see what differences there are in the Northeast compared to the Southeast: does the horizon concept work here, and if so is it the same as that found by South or has it differences, and when does it first appear? G. W. Stone (1970a:75-6) notes delft appears to have been much more common at colonial Providence and at Brunswick Town than at Boston: is this chance, or caused by the incomplete evidence we generally have to use, or are there social and/or economic factors to be found?

Regarding South's suggestion that his dating method could be extended to other classes of artefact, there seems no reason to doubt this provided reasonably accurate date ranges can first be obtained for the material to be used. Ceramics at present do seem to be the most comprehensively studied class of historical artefact, but Noël Hume's glass bottle typology (Noël Hume 1961:91-117) appears to have stood the test of time and should be adaptable provided fragments can be adequately typed. Essentially, provided accurate enough date-ranges for the production (and use) of material can be had, the method of dating should be applicable to any class of artefact. Again, however, I still come
back to the feeling that mass production in the nineteenth century is likely to force us to seek some better dating criteria—and may even provide it in some instances, such as providing dated horizons of technological and other innovations or providing datable identifying marks. For example, as yet unpublished research on buttons by the National Historic Sites Service suggests that with nineteenth century buttons, particularly military types, historical and technological evidence can date buttons as close as a few years (Herst n. d.).

It may also be that while there is a horizon in one class of artefacts there may not be in another class contemporary to the first. As Rick points out (Rick 1968:123), one is on shaky ground when one presumes without caution that the nail chronology set forth by Fontana (et al. 1962) in "Johnny Ward's Ranch" in Arizona can necessarily be applied to a site 1,700 miles away in Canada. It seems possible nails would be more likely to have been relatively locally produced, or at least produced in a number of centres, with the likelihood of varying styles and varying technical forms appearing at different times over these areas; while nineteenth century ceramics even in the United States do appear to have been predominately of English, specifically Staffordshire, manufacture. One might therefore guess at fewer meaningful widespread horizons from a study of nails than from one of ceramics, though this is only a suggestion and not based on a study of nails in various areas.

That some artefacts give quite different dating evidence depending on the social, political, and economic factors bearing on the occupation of the site is suggested by coin evidence. South notes [98] that occupation periods at Brunswick Town were quite accurately reflected by dated coins—for example, the period 1725–76 was represented by coins dated from 1696 to 1775—but this sort of accuracy would not have held for Louisbourg, where as this writer has noted (Walker 1970a:102-3), French coins, some dating as far back as the 1650's, evidently remained in circulation so long as to sometimes become entirely smooth on both sides, though English coins representing the English and New England occupation of Louisbourg, 1745–49, did indeed represent reasonably accurately this period, dating generally to the 1730's and 1740's and being relatively little worn. The difference here is attributable to France's lesser interest in its colonies and its inferior maritime connections compared to the active colonial policy of the British and the strength of the Royal Navy and British mercantile trade generally. It would certainly be worthwhile comparing French and British coins from, say, Michilimackinac to see if a pattern similar to that at Louisbourg is discernible; Cleland's study of the faunal remains from Michilimackinac suggests differences attributable to British dominance of trans-Atlantic trade routes (Cleland 1971:16-9).

Review articles are not the easiest to write, but few pieces of work enable one to sit down and examine ideas as well as this does.
As always, the South Fora present not only original ideas but stimulating challenges which force one to think about things in the widest contexts, and South's own paper is fully up to the now-expected standard. For this, then, as much as for the research they contain, the Forum series deserves recognition as the leading contribution to thought in historical archaeology.
ARCHE, M.

BINFORD, L. R.

CHALKLEY, J. F.


CHARLESTON, R. J.

CLELAND, C. E.


CLELAND, C. E. AND JAMES E. FITTING

COLLARD, ELIZABETH

COTTER, J. L.
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DANIEL, G.

DOLLAR, CLYDE D.

DUNTON, J. V. N.

FAIRBANKS, C. H.

FLEMING, J. A.
1923 Scottish Pottery, pp. 254-256.

FOLEY, V. P.


FONTANA, B. L. et al.

FORD, J. A.
1969 A Comparison of Formative Culture in the Americas: Diffusion or the Psychic Unity of Man. Smithsonian Contributions to Anthropology, No. 11, p. 5.

HANSON, L. H., JR.

HARRIS, D. A.
1971 A French Colonial Well: Its Construction, Excavation and Contents. The Conference on Historic Site Archaeology Papers 1970 5, caption a to Fig. 5, facing p. 67.
HERST, D. L., MISS
n. d. Personal Correspondence.

HONEY, W. B.
1952 European Ceramic Art From the End of the Middle Ages to About 1815 2, pp. 92, 370.

HOUSMAN, A. E.
1937 M. Manili Astronomicon 1930.

JELKS, E. B.

LARRABEE, E. McM.
1970 Historic Site Archaeology in Relation to Other Archaeology. Historical Archaeology 1969 3, p. 69.

MANKOWITZ, W. AND R. G. HAGGAR

MARWITT, RENEE H.

MAYER-OAKES, W. J.

MILLER, J. J., II AND L. M. STONE

MOORE, E.
1965 A Bartman Jug from Mukah. The Sarawak Museum Journal 12, No. 25, 26, pp. 75-76.

NEUSTUPNÝ, E.

NOËL HUME, IVOR


1963 *Here Lies Virginia*, p. 289.


1969 *Historical Archaeology*, p. 310.

1969, Colonial Williamsburg Archaeological Series. et. seq.


OMWAKE, H. G.


OSBORNE, D.


PETersen, E. T.


POWELL, B. B.

RICK, J. H.

ROWLETT, R. M.

SCHUYLER, R. L.

SOUTH, S.

STONE, G. W.


STONE, L. M.
WALKER, I. C.


n. d. Ph. D. Thesis, which is still in the process of being written.

WATKINS, C. M.

WATKINS, C. M. AND IVOR NOËL HUME

WILSON, D. M.
For some time Stanley South has been a quiet hero among many archaeologists; particularly younger archaeologists who still believe that field and laboratory work are as important as the self defeating mouthtalk that has come to dominate many of our major journals. He has made many detailed local studies which, unfortunately, are known primarily through papers presented at meetings. He has been the workhorse of numerous organizations as a perpetual editor and committeeman; posts with lots of work and little direct reward. His intellectual efforts should be better known, and after years of telling students about his work, I welcome the opportunity to refer them to a paper such as this. It presents an intellectual synthesis which in many ways has more significance than the data with which he is dealing.

I write as one who feels that historic sites archaeology is but a part of the study of archaeology and that archaeology has much to offer to the entire study of cultural stability and change. For the past several years I have been involved in prehistoric projects but have followed the literature of historic sites archaeology with much interest. The four years which have passed since the appearance of Dollar's "Some Thoughts on Theory and Method in Historical Archaeology" have been particularly important. Since I am writing in the field, I do not have access to exact sources, but papers by Robert Schulyer, Jeff Miller and Lyle Stone, and now Stanley South, have demonstrated that the historian and archaeologist can work together and that the same person may be both.

Four years ago Charles Cleland and I singled out Noël Hume as following an analytical approach which we felt to be unproductive. South has demonstrated beyond question the value of Noël Hume’s work as a starting point for other types of analysis and, by doing so, has effectively answered our criticisms as to its usefulness. A classificatory system which is an end in itself is sterile, but one which can be used to elucidate cultural problems is to be applauded.

There are some specific comments, however, which must be made about South’s paper. While he has dealt with the limitations of the entire concept of horizon and reconciled it with the nature of his historic data, he has not done as well with the concept of evolution. It is a philosophical question, and one which may never be settled, as to relationship of biological and cultural evolution. I read South’s paper as implicitly accepting cultural evolution as homologous with biological evolution. His statement, "thus the evolutionary process is seen in the change of form through time" is parallel
with the biologist's definition of evolution as a change in gene frequency. The latter statement is clearly a post-Darwinian concept, but South fails to develop this point. There is nothing explicit in his paper to contradict the impression that he views ceramic change, hence cultural change, as a random process.

A contemporary biological approach to evolution would consider the variables of mutation, selection, drift and flow. All of these have cultural analogies (homologies?). Cultural mutation is the manifestation of individual creation. In the study of historic ceramics such innovations are real and apparent and to the art historian are the main object of study. These "mutations" or innovations may be accepted and spread to wide areas or they may die out. The pattern of acceptance or rejection may be a result of either selection or drift. Both selection and drift have been given short-shrift in archaeology where flow, or diffusion and blending, has been used as a major explanatory device. South has used Ford's concepts of cultural change, the concept of the cultural continuum and the unimodal curve, which imply a type of drift.

Selection is difficult to deal with in the study of single cultural traits since it is tied in with total cultural change. Garry Stone's paper in the 1970 Historical Archaeology Forum might be considered as an attempt at dealing with such change in form (in the sense that South uses this term) as selection resulting from social change. The milieu in which this social change occurred is complex and one that involved the breakdown of traditional society and the reestablishment of the social order on a global, time oriented scale. It may take a Lewis Mumford to abstract the social processes, but an archaeologist is clearly in a position to deal with the material expressions of such selection.

Evolution is a significant concept for the study of social change, a fact which South has been aware of for many years. Noël Hume, Garry Stone and South have all consciously or unconsciously dealt with various aspects of evolution, but we still need a study of cultural change incorporating all of the above variables.

Another point which needs to be emphasized is the heuristic nature of South's assumption of a unimodal curve of cultural change. I find it significant that it has been used most successfully with prehistoric materials which lack the counterchecks of historic sites archaeology. The unimodal curve works as long as there is no basis for contesting it, and I shall look forward to the comments of those more familiar with the ceramics involved as to its applicability.

In fact, I would question whether it ever works except under institutionalized conditions (army posts, etc.). If it is true probabilistic drift, there should be random fluctuations based on chance alone rather than unimodal development.
What is worse, human beings have a perverse way of seeking what is "camp" and reviving older styles. This may be clearly seen on college campuses today, but there are many concrete archaeological examples as well. A good revival would be most difficult to separate from an original, and this too is a potential weakness in the unimodal curve.

Even though the assumption of the unimodal curve can be challenged on both theoretical and practical grounds, it is far superior to the assumption that it is impossible to find order in artifacts. If these assumptions can be used to order apparently random data (and in prehistoric archaeology, I think, this has sometimes happened) they are better than nothing.

There are several points about the mean ceramic date statistic that need clarification. When I tried to duplicate the Brunswick S7 results, I initially had some problems until I realized that the data had been coded. The number 1700 had been subtracted from $x_i$ before it was multiplied by $f_i$. After the mean date had been obtained on the coded data (54.6), 1700 was again added to obtain 1754.6.

Coding is a most efficient way of saving time when using a desk calculator, but it should have been explained in the paper. Confusion could result in Appendix III where 38AK5-A, 38GN1-3, 38GN1-5, 38PN1, 38PN4 and N1 have been coded with a (+1700) symbol whereas others have not.

When I recoded the data I subtracted the lowest date in the series from all dates reducing the lowest date to zero. This made the data even easier to deal with without affecting the results. This was necessary since all I had in the field was a Bohn Context-10 manual calculator. As a result, the sum of my $f_i \cdot x_i$ column for S7, as an example, was 55,767 as opposed to South's 123,657. While this degree of coding may have been unnecessary for South's original formula, it became most important in following this formula to its next step, a study of dispersal and variance.

A major complaint which I have had about the regression pipestem dating formula is that the single answer implies an accuracy that is just not there. A group of pipestems which yield a 1700 date and a group which yield a 1800 date, when mixed equally, give a nice 1750 date which is clearly a distortion. There is nothing to indicate the degree of distortion, and there is no way of taking sample size, nature of distribution and sampling error into consideration.

South is aware of this problem, particularly as it involves estimating $\mu$ (the true mean) by $\bar{X}$ (the sample mean). He has attempted to deal with it by taking the average deviation of plus or minus four years for eighteenth century sites as an indication of variability.
While this is esthetically pleasing, and very easy to do, it takes neither the variable of sample size nor that of range and dispersal into consideration. I can see no statistical validity in his ±4 and I would suspect only spurious cultural validation.

A more efficient statistic would be the standard deviation which takes the above variables into account. This is derived by the formula:

$$S = \sqrt{\frac{\sum f_i \cdot X_i^2 - \left(\frac{\sum f_i \cdot X_i}{n}\right)^2}{n-1}}$$

The only additional column which is needed for South's tables to derive this statistic is $$f_i \cdot X_i^2$$ which can be obtained by multiplying $$f_i \cdot X_i$$ by $$X_i$$ and summing the column. It is at this point that coding becomes very important.

When this is applied to Brunswick S7 the result is 1755 ± 21 years which correlates perfectly with the known range of historic occupation. As a rule, the standard deviation will cover somewhere around two-thirds of the range and it was around 20 years for the several samples to which I applied it.

South's four year deviation, like a pipestem date, looks more accurate, but I am reminded of the archaeologist who preferred Yale radiocarbon dates to Michigan dates because they were published with only one standard deviation instead of two and therefore looked more accurate. I believe that philosophers call this the fallacy of misplaced certainty.

Actually, dealing with a mean and standard deviation you are back to South's original assumption of a unimodal curve, in this case a normal curve. A visual inspection of the data in Appendix III indicates that only 38CH1 approaches such a distribution although 38PN4 may be viewed as a hyper-normal curve.

Very little archaeological data ever seems to fit a normal curve. Some types of data, particularly measurements, follow a regular left skewed hyper-normal pattern. I noted this in a paper on Virginia fluted points several years ago, and I understand Henry Wright at the University of Michigan is now involved in a computer analysis of the significance of skew and curve height for the interpretation of cultural materials.

In summary, I am impressed with South's paper as an attempt at ordering and understanding material culture and, by extension, quantifying human behavior. We are far from a perfect understanding of human behavior and archaeologists will continue to count and measure things which they do not understand. It is through this counting and measuring that we will be able to isolate the important independent
variables. This paper is significant because you can argue with it, with specific definitions and approaches. It is far superior to the simple statement that order does not exist or that, if it does, it is too complex to deal with. It is a bold, imaginative paper which is wrong in places but wrong for the right reasons. As J. O. Brew wrote many years ago, "we need have no fear of changing established systems or designing new ones, for it is only by such means that we can progress" (1946:65).
BIBLIOGRAPHY

BREW, JOHN O.

CLELAND, CHARLES E. AND JAMES E. FITTING

DOLLAR, CLYDE D.

FITTING, JAMES E.

MILLER, J. JEFFERSON AND LYLE M. STONE
1970 Eighteenth-Century Ceramics from Fort Michilimackinac, Smithsonian Institution Press, Washington, D. C.

STONE, GARRY WHEELER
MEAN CERAMIC DATES, MEDIAN OCCUPATION DATES, RED ANT HILLS
AND BUMBLE BEES: STATISTICAL CONFIDENCE AND CORRELATION

David South

Stanley South in his paper states:

Although this frequency-adjusted manufacture date might
be assumed not to have anything to do with the occupation
date for an historic site, we will see that there is a
remarkable degree of similarity between the mean ceramic
date derived from use of the formula and the historically
known median occupation date of the eighteenth century
historic sites on which it has been used (South 1972).

One might also find a degree of similarity between the mean ceramic date
derived from the use of the formula and the number of red ant hills found
on the historic site. Of these sets of data, which are as of yet
unrelated (mean ceramic date—median historic occupation date and mean
ceramic date—number of red ant hills), I will examine the degree of
correlation between S. South's mean ceramic date and the median
historic occupation date. More data on red ant hills is needed before
a ceramic date—ant hill correlation can be demonstrated.

One might very well infer that S. South based his formula on the
assumption that the popularity of ceramic types follows a normal curve,
with the beginning and end dates of manufacture falling on the beginning
and end dates of popularity. Therefore, the median date of manufacture
would also be the peak date of popularity. I doubt that this is the
case. I believe it is the percentage of confidence one can have in a
formula that is important rather than whether the formula is based
on solid assumptions. A formula based on strong facts but giving the
wrong answer is not helpful. (Engineers have demonstrated that aero-
dynamically a bumble bee can't fly.)

Before we examine the degree of confidence we can place in S. South's
formula, let us first find what the formula actually means. South takes
each type median date and multiplies it by the number of sherds of that
type. The sum of these products he divides by the number of observations
(or sherds). This answer is the mean of the observations, with each
observation being a median date of production of the ceramic type re-
presented by a specific sherd. Therefore, each sherd represents a median
date of production as determined by data in Figure 1 of S. South's paper,
and what the formula actually tells us is the mean date of these median
dates. From this mean date of several median dates how can we statis-
tically predict the median historic occupation date?
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I used ten data sets of ceramic mean dates and historic median dates from S. South's paper, and two more sets which he furnished me.* The procedure involved selecting sites of the eighteenth century for which historic median dates were available. I used the mean ceramic date and historic median date to the nearest tenth of a year to reduce any error made in rounding off, (S. South rounded up every .5 or greater year). A chart was constructed illustrating the historic median date (HD), and the mean ceramic date (CD), and the number of years difference between them (CD-HD) (Fig. 1). I also plotted the mean ceramic date against the historic median date and fitted a straight line to the 12 points by the least squares method.

By visual inspection of the graph (Fig. 2), it can be seen that from 1725 to 1800 most of the fitted straight line lies below the ideal comparison 45° line. This indicated to me that perhaps S. South's formula tends to underestimate the median historic occupation date. To check on this, I found the mean of the differences between the mean ceramic date and the historic occupation median (-2.225 years) (Fig. 1). This statistic says that for these 12 observations, on the average, the mean ceramic date underestimates the known historic median date by 2.225 years.

What we would really like to know, however, is the average number of years that the mean ceramic date formula may be off in all cases in the eighteenth century. Although we cannot obtain data from all of the possible applications of the formula in the eighteenth century, we are able to infer from our small sample of 12 what the actual mean for all cases might be (Mendenhall 1969:193-195).

The following formula is a tool used by statisticians when dealing with small samples to infer the range in which the total population mean might fall:

$$\bar{Y} \pm t_{\alpha/2} \frac{S}{\sqrt{n}}$$

Where $\bar{Y}$ = sample mean

$t_{\alpha/2}$ = number used when confidence coefficient desired is $x$ percent and $n$ observations are used (see table below)

$S$ = standard deviation of the sample

$n$ = number of observations in the sample

*Ivor Noel Hume: Trebell Site Cellar, Virginia (TS 807C). Historic Median = 1797.5; Ceramic Mean = 1788.9.

Roger Grange: Castle Hill, Placentia, Newfoundland. Historic Median = 1762.5; Ceramic Mean = 1762.7.

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### COMPARISON OF HISTORIC MEDIAN AND MEAN CERAMIC DATES SHOWING DIFFERENCE, MEAN, AND STANDARD DEVIATION

<table>
<thead>
<tr>
<th>Site</th>
<th>Median Historic Date</th>
<th>Mean Ceramic Date</th>
<th>CD-HD</th>
<th>(CD-HD)^2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Brunswick S7</td>
<td>1755.0</td>
<td>1754.6</td>
<td>- .4</td>
<td>.16</td>
</tr>
<tr>
<td>38AK4-15</td>
<td>1731.5</td>
<td>1726.1</td>
<td>- 5.4</td>
<td>29.16</td>
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<td>38AK5-A</td>
<td>1741.0</td>
<td>1741.7</td>
<td>+ .7</td>
<td>.49</td>
</tr>
<tr>
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<td>1754.6</td>
<td>- .9</td>
<td>.81</td>
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<tr>
<td>38PN1</td>
<td>1760.5</td>
<td>1763.0</td>
<td>+ 2.5</td>
<td>6.25</td>
</tr>
<tr>
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<td>1751.0</td>
<td>1746.4</td>
<td>- 4.6</td>
<td>21.16</td>
</tr>
<tr>
<td>Brunswick N1</td>
<td>1753.5</td>
<td>1750.1</td>
<td>- 3.4</td>
<td>11.56</td>
</tr>
<tr>
<td>Brunswick S2</td>
<td>1753.5</td>
<td>1749.0</td>
<td>- 4.5</td>
<td>20.25</td>
</tr>
<tr>
<td>Brunswick S18</td>
<td>1769.5</td>
<td>1776.2</td>
<td>+ 6.7</td>
<td>44.89</td>
</tr>
<tr>
<td>Brunswick S10</td>
<td>1803.0</td>
<td>1794.0</td>
<td>- 9.0</td>
<td>81.00</td>
</tr>
<tr>
<td>Castle Hill</td>
<td>1762.5</td>
<td>1762.7</td>
<td>+ .2</td>
<td>.04</td>
</tr>
<tr>
<td>Trebell</td>
<td>1797.5</td>
<td>1788.9</td>
<td>- 8.6</td>
<td>73.96</td>
</tr>
<tr>
<td>SUM</td>
<td></td>
<td></td>
<td>-26.7</td>
<td>289.73</td>
</tr>
</tbody>
</table>

\[
\bar{Y} = \frac{\sum_{i=1}^{n} Y_i}{n}
\]

\[
S = \sqrt{\frac{\sum_{i=1}^{n} Y_i^2 - \left(\frac{\sum_{i=1}^{n} Y_i}{n}\right)^2}{n-1}}
\]

\[
\bar{Y} = \frac{-2.225}{12/-26.7}
\]

\[
S = \sqrt{\frac{289.73 - (-26.7)^2}{12}}
\]

\[
S = 4.58 \ (4.575850641)
\]

\[
S^2 = 20.94 \ (20.93840909)
\]

**FIGURE 1**

166
Comparison of the Mean Ceramic Date to the Historic Median Date

Solid Line = Ideal 45° Comparison Line
Dotted Line = Least Squares Straight Line
HISTORICAL ARCHAEOLOGY FORUM - D. South

CRITICAL VALUE OF t

<table>
<thead>
<tr>
<th>Confidence factor =</th>
<th>80%</th>
<th>90%</th>
<th>95%</th>
</tr>
</thead>
<tbody>
<tr>
<td>ts/2</td>
<td>t.100</td>
<td>t.050</td>
<td>t.025</td>
</tr>
<tr>
<td>n = 11</td>
<td>1.372</td>
<td>1.812</td>
<td>2.228</td>
</tr>
<tr>
<td>n = 12</td>
<td>1.363</td>
<td>1.796</td>
<td>2.201</td>
</tr>
<tr>
<td>n = 13</td>
<td>1.356</td>
<td>1.782</td>
<td>2.179</td>
</tr>
</tbody>
</table>

(Mendenhall 1969:345)

\[ -2.225 \pm (1.363) \frac{4.5758}{\sqrt{12}} = -2.225 \pm 1.8004 \]

This means that one can have 80 percent confidence that the actual total population mean (\( \mu \)) of the differences (CD-HD) exists between -0.425 and -4.025. Stated another way, there is an 80 percent chance that on the average the mean ceramic formula date will be off from the median historic date an amount which falls between -0.425 and -4.025. Therefore I deduce that there is a 20 percent chance that anything outside of this bracket would actually be the population mean (\( \mu \)). Zero lies outside of this mean and therefore has less than 20 percent chance of being the true population mean. If a 95 percent confidence interval is involved, then:

\[ -2.225 \pm (2.201) \frac{4.5758}{\sqrt{12}} = -2.23 \pm 2.9074 \]

Therefore, one can have 95 percent confidence that the total population mean (\( \mu \)) would fall between +0.682 and -5.132. Zero falls within this range.

Again, what does the mean of the total population of differences tell us? This tells us the average number of years that the ceramic mean date formula misses the historic median occupation date, and whether there is an overestimation or underestimation. The mean of the total population of differences using a formula that neither overestimates or underestimates would equal zero.

From the above data, we can have 80 percent confidence in the fact that we are underestimating the true population mean and that we should add somewhere from 0.425 to 4.025 to the formula to bring the total population mean (\( \mu \)) of the differences (CD-HD) to equal zero. Therefore I contend that a constant number of years should be added to the mean ceramic formula date. This should be done in order that the confidence that the total population mean (\( \mu \)) of the differences (CD-HD) being equal to zero be greater, and that the constant be no lower than 0.425 years and no higher than 4.025 years. The number of years that I suggest be added to S. South's formula is approximately 2.2. This number I believe to be the best (from the data collected as yet) in order to increase the chances that the mean of the total population
of the difference is zero (CD-HD). (This number, of course, is equal to minus one times the mean of the differences (CD-HD) of the sample of 12.) When more data is collected, this value of 2.2 will likely change.

When 2.2 is added to each of the 12 mean ceramic dates, the new mean of the differences (CD-HD) will be -0.025 years (the mean would have been .00000 if rounding of 2.225 to 2.2 had not been done, but this accuracy is not required) (Fig. 3).

Again inferring from the new data, the range in which the true mean of the total population of the differences (CD + 2.2 - HD) lies can be predicted with a confidence factor.

\[
80\% \quad -0.025 \pm (1.363) \frac{4.5758}{\sqrt{12}} = -0.025 \pm 1.8004 = -1.8254 \text{ to } +1.7754
\]

\[
95\% \quad -0.025 \pm (2.201) \frac{4.5758}{\sqrt{12}} = -0.025 \pm 2.9074 = -2.9324 \text{ to } +2.8824
\]

So far all I have done is to adjust the ceramic mean formula so that the chances of \( \mu \) being equal to zero is greater, and I have placed confidence factors on several ranges over which \( \mu \) may occur. Now I will show the role of standard deviation in inferring confidence in the formula.

In working with standard deviation, a normal curve is assumed. I am assuming that the distribution of differences (CD + 2.2 - HD) forms a normal curve. I can see no reason why a normal curve is not involved. Standard deviation is calculated by:

\[
S = \sqrt{\frac{\sum_{i=1}^{n} (Y_i - \bar{Y})^2}{n-1}} \quad \text{(or)} \quad S = \sqrt{\frac{\sum_{i=1}^{n} Y_i^2 - \left(\frac{\sum_{i=1}^{n} Y_i}{n}\right)^2}{n-1}}
\]

When a standard deviation is applied to a normal curve, then approximately 68 percent of the measurements will lie within \( \pm \) one standard deviation of the mean. Ninety-five percent of the measurements are included in \( \pm \) two standard deviations of the mean (Fig. 4). This is known as the Empirical Rule (Mendenhall 1969:37). In finding what the standard deviation (\( s \)) might be for the total population of differences (CD + 2.2 - HD) a chi square variable can be used (Mendenhall 1969: 205-209).
### COMPARISON OF HISTORIC MEDIAN AND MEAN CERAMIC DATE PLUS 2.2 YEARS

**SHOWING DIFFERENCE, MEAN, AND STANDARD DEVIATION**

<table>
<thead>
<tr>
<th>Site</th>
<th>Median Historic Date</th>
<th>Mean Ceramic Date Plus 2.2 Years</th>
<th>CD+2.2-HD</th>
<th>(CD+2.2-HD)^2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Brunswick S7</td>
<td>1755.0</td>
<td>1756.8</td>
<td>+1.8</td>
<td>3.24</td>
</tr>
<tr>
<td>38AK4-15</td>
<td>1731.5</td>
<td>1728.3</td>
<td>-3.2</td>
<td>10.24</td>
</tr>
<tr>
<td>38AK5-A</td>
<td>1741.0</td>
<td>1743.9</td>
<td>+2.9</td>
<td>8.41</td>
</tr>
<tr>
<td>38GN1-5</td>
<td>1755.5</td>
<td>1756.8</td>
<td>+1.3</td>
<td>1.69</td>
</tr>
<tr>
<td>38PN1</td>
<td>1760.5</td>
<td>1765.2</td>
<td>+4.7</td>
<td>22.09</td>
</tr>
<tr>
<td>Brunswick S15</td>
<td>1751.0</td>
<td>1748.6</td>
<td>-2.4</td>
<td>5.76</td>
</tr>
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<td>Brunswick N1</td>
<td>1753.5</td>
<td>1752.3</td>
<td>-1.2</td>
<td>1.44</td>
</tr>
<tr>
<td>Brunswick S2</td>
<td>1753.5</td>
<td>1751.2</td>
<td>-2.3</td>
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<td>Brunswick S18</td>
<td>1769.5</td>
<td>1778.4</td>
<td>+8.9</td>
<td>79.21</td>
</tr>
<tr>
<td>Brunswick S10</td>
<td>1803.0</td>
<td>1796.2</td>
<td>-6.8</td>
<td>46.24</td>
</tr>
<tr>
<td>Castle Hill</td>
<td>1762.5</td>
<td>1764.9</td>
<td>+2.4</td>
<td>5.76</td>
</tr>
<tr>
<td>Trebell</td>
<td>1797.5</td>
<td>1791.1</td>
<td>-6.4</td>
<td>40.96</td>
</tr>
<tr>
<td><strong>SUM</strong></td>
<td></td>
<td>-0.3</td>
<td></td>
<td>230.33</td>
</tr>
</tbody>
</table>

#### Estimation of Mean and Standard Deviation

\[
\bar{Y} = \frac{\sum_{i=1}^{n} Y_i}{n}
\]

\[
S = \sqrt{\frac{\sum_{i=1}^{n} Y_i^2 - \left(\frac{\sum_{i=1}^{n} Y_i}{n}\right)^2}{n-1}}
\]

- \[\bar{Y} = \frac{-0.025}{12/-0.30}\]

- \[S = \sqrt{\frac{230.33 - (-0.30)^2}{12}}\]

- \[S = 4.58 \quad (4.575850641)\]

- \[S^2 = 20.94 \quad (20.93840909)\]

**FIGURE 3**

170
\[ X^2 = \frac{(n-1)s^2}{\sigma_0^2} \]

- \( n \) = number of observations in the sample
- \( S \) = standard deviation of the sample
- \( \sigma_0 \) = standard deviation of the total population one believes to be involved

In this case I believe 4.5 to be a good approximation of the standard deviation (\( \sigma_0 \)) of the total population of differences (CD + 2.2 - HD).

So: \[ X^2 = \frac{(12-1) \times 20.89}{20.25} = 11.37 \approx 11.37 \]

This value for \( X^2 \), (11.37) will fall between the critical values of chi square with a confidence interval of 80 percent, 5.57779 to 17.2750.

<table>
<thead>
<tr>
<th>Upper limit of curve</th>
<th>Lower limit of curve</th>
</tr>
</thead>
<tbody>
<tr>
<td>95%</td>
<td>90%</td>
</tr>
<tr>
<td>( X^2 )</td>
<td>( X^2 )</td>
</tr>
<tr>
<td>90%</td>
<td>80%</td>
</tr>
<tr>
<td>( X^2 )</td>
<td>( X^2 )</td>
</tr>
<tr>
<td>80%</td>
<td>90%</td>
</tr>
<tr>
<td>( X^2 )</td>
<td>( X^2 )</td>
</tr>
<tr>
<td>80%</td>
<td>95%</td>
</tr>
<tr>
<td>( X^2 )</td>
<td>( X^2 )</td>
</tr>
</tbody>
</table>

\( n=12 \)

\begin{align*}
\text{X}^2_{95\%} & = 3.81575 \\
\text{X}^2_{90\%} & = 4.57481 \\
\text{X}^2_{80\%} & = 5.57779 \\
\text{X}^2_{80\%} & = 17.2750 \\
\text{X}^2_{90\%} & = 19.6751 \\
\text{X}^2_{95\%} & = 21.9200 \\
\end{align*}

(Mendenhall 1969:347)

Now we take the \( X^2 \) value of the desired percent confidence and find the limits of the range in which the total population standard deviation (\( \sigma \)) may be.

\[ \frac{(11)(20.89)}{21.9200} < \sigma^2 < \frac{(11)(20.89)}{3.81575} \]
\[ 10.50 < \sigma^2 < 60.36 \]
\[ 3.24 < \sigma < 7.77 \]

This provides us with a confidence factor when predicting any eighteenth century historic median occupation date using the mean ceramic date formula plus 2.2 years.

**Statement of Confidence in the Adjusted Mean Ceramic Date Formula**

The following confidence factors can be applied to the dates derived when employing the S. South mean ceramic date formula using the D. South adjustment of plus 2.2 years.
Normal Probability Density Function

$\mu = \text{Total Population Mean}$

$\bar{Y} = \text{Sample Mean}$

$\sigma = \text{Standard Deviation for Total Population}$

$S = \text{Standard Deviation for Sample of Population}$

*To be used when the curve represents a sample of total population.

Figure 4
There is a 95 percent chance that approximately 68 percent of the mean ceramic formula dates will not be more than ±7.77 years off from the historic median date.

There is a 95 percent chance that approximately 95 percent of the mean ceramic formula dates will not be more than ±15.54 years off from the historic median date.

There is a 90 percent chance that approximately 68 percent of the mean ceramic formula dates will not be more than ±7.09 years off from the historic median date.

There is a 90 percent chance that approximately 95 percent of the mean ceramic formula dates will not be more than ±14.18 years off from the historic median date.

There is an 80 percent chance that approximately 68 percent of the mean ceramic formula dates will not be more than ±6.42 years off from the historic median date.

There is an 80 percent chance that approximately 95 percent of the mean ceramic formula dates will not be more than ±12.84 years off from the historic median date.

The above data can be condensed into the following table:

TABLE OF CONFIDENCE FACTORS WHEN USING SOUTH'S MEAN CERAMIC DATE FORMULA PLUS 2.2 YEARS

<table>
<thead>
<tr>
<th>Confidence Level</th>
<th>Margin of Error (±)</th>
</tr>
</thead>
<tbody>
<tr>
<td>90%</td>
<td>±15.52</td>
</tr>
<tr>
<td>85%</td>
<td>±14.18</td>
</tr>
<tr>
<td>76%</td>
<td>±12.84</td>
</tr>
<tr>
<td>64%</td>
<td>±7.77</td>
</tr>
<tr>
<td>61%</td>
<td>±7.09</td>
</tr>
<tr>
<td>54%</td>
<td>±6.42</td>
</tr>
</tbody>
</table>

*This is a very conservative statement since this is the upper limit of the range in which the total population standard deviation may be for a given confidence factor.
These confidence factors apply when using South's formula (plus 2.2 years) regardless of the theoretical base upon which the formula was built. The similarity between the mean ceramic formula dates and the number of red ant hills on an historic site is yet to be demonstrated. We have, however, demonstrated the correlation between the mean ceramic formula dates and the median occupation dates of the historic sites of the eighteenth century. The theoretical assumptions upon which S. South's formula was based might lead one to believe that he has constructed an aerodynamically unsound bumble bee; however, the confidence intervals I have derived relating to the use of his formula reveal that his bumble bee does indeed fly.

BIBLIOGRAPHY

MENDENHALL, WILLIAM

SOUTH, STANLEY
I concur completely with South that when ceramics typology is being used for dating, quantification of the typological sample is highly desirable because, if done properly, quantification tends to make the resultant estimated dates more precise. Furthermore, in my judgment, South's method for computing a mean ceramics date of a sample (actually, the mean of the median dates assigned to the respective sherds in the sample) is statistically sound. However, I do have several suggestions that I think would refine and generally improve the method. They are enumerated below in Items 1 through 4 in the hope that South and others interested in this kind of dating problem may find them of interest, or even of some utility.

1. Exclusion of Inappropriate Types

**PRINCIPLE:** Any type which is present in a sample being dated but which has no power to narrow the sample's estimated date within the sample's temporal parameters should be excluded entirely from the data put into the statistic.

South recognizes this principle when he excludes most coarse earthenware. But the principle should be applied consistently and other types should be excluded also, especially decorated delftware. There is no way that a type with no power to restrict can help make the estimated date for a sample more precise. It will, in fact, always make the estimated date less precise except when the median date for the type is exactly the same as the mean date for the sample exclusive of the type in question. And the farther the type's median is deviant from the sample mean, the greater will be the error caused by its inclusion in the statistic. In short, inclusion of such a type can never help, will almost always introduce an error of greater or lesser magnitude, and can at best be only neutral—and that only under very rare circumstances. The latter statement applies to South's assignment of two different median dates (1660/1750) to decorated delftware.

2. Establishing a Sample's Modality

South's formula assumes a unimodal distribution for the sample being dated—the equivalent of assuming that the sample represents a continuous occupation at the component of the
archaeological record from which it was drawn. In actual practice, any knowledgeable and experienced archeologist ordinarily would recognize a major hiatus, if there happened to be one, in the chronology of types present in a particular sample; and he would interpret that to mean that the sample represented two different periods of occupancy with a period of non-occupancy between. But rather than just eyeballing a sample to decide whether it is unimodal, bimodal, or multimodal, one could proceed much more scientifically by employing an appropriate statistical test for determining the sample's modality. A simple way to do this would be to divide time arbitrarily into equal intervals of, say, 10 years each, then to add up the number of sherds in the sample being dated whose median dates fall within each of the respective 10-year intervals. Comparison of the total number of sherds falling into the different intervals should establish whether or not the distribution of a particular sample is unimodal or not. A histogram could be used, if desired, to demonstrate visually the sample's modality.

3. Measuring Dispersion about the Mean

The use of standard statistical techniques for measuring dispersion might be more appropriate than the technique for estimating an "interpreted period" of occupation which South employs. At the least, standard techniques for computing the mean deviation and the standard deviation should be tried to see how well they work. Either would give some indication of the time span represented by a sample, and possibly one or the other might give a close approximation of the sample's beginning and ending dates.

4. Weighting Factors

One of the variables which patently affect the accuracy of a sample's mean date is the difference in the length of time that the respective types are estimated to have been in use. For example, a type that was in common use for only 10 years obviously is going to give closer dates than a type that was in common use for 50 years. South's formula could be improved, therefore, by the use of appropriate weighting factors based on this principle:

**Types used over relatively short spans of time should be weighted more heavily than types used over relatively long spans.**

This principle is intimately involved in the matters discussed in Item 1 above. Careful consideration and rigorous empirical testing should precede the selection of a weighting scale.
Miscellaneous Observations

The following observations relate to statements in South's paper which have nothing directly to do with his basic formula, but which I feel deserve comment.

Pages 71, 86-88, 99, etc. I find some of the terms, as defined on Page 71 and used throughout the paper, ambiguous and confusing. For example, it is stated on Page 71 that a type is "...defined by one or more attributes." and that "...a type is often distinguished on the basis of a single attribute." Shape is an attribute (p. 71): Therefore, any group of ceramics distinguished on the basis of shape is by definition a type; specifically, one of those types which is distinguished on the basis of a single attribute, namely, shape. This ambiguity renders the discussions on Pages 86-88 and 99, which contrast studies based on type with studies based on shape, confusing if not meaningless. Other ambiguities in the use of terms could be cited.

The source of this confusion probably lies in certain inadequacies in the definitions on Page 71. Elucidation of the definitions undoubtedly would resolve the confusion.

Page 72. Noel Hume's quoted statement that terminus post quem is "...the cornerstone of all archaeological reasoning" (italics mine) is a catchy metaphor but is hardly a statement of fact. I subscribe fully to the thesis that TPQ's are very useful for certain kinds of archeological reasoning. They are handy things to have around, and I urge anyone who has a chance to capture one to grasp it by the forelock and use it to full advantage. But TPQ has only to do with temporal reasoning (and with only one aspect of that), and archeologists—or most of them anyhow—do reason about things other than dates and chronologies. They often make inferences about the function of particular artifacts, for example, and about methods used in manufacturing certain artifacts, and about many other things that have nothing at all to do with time or with TPQ's. Some archeologists even make inferences about such abstract things as social organization, resource utilization, and population densities without invoking even the shadow of a TPQ. Moreover, such conceptual tools as the principles of association and typology manage quite nicely without TPQ underpinning.

Pages 81 and 87-88. It is not clear from the instructions on these pages how the vertical line placements were chosen in Figure 1D. For example, the lower-limit vertical line on the Charles Towne graph could be placed at 1630 and the upper line at 1660—or the lower line at 1640 and the upper line at either 1660 or 1700—and the requirements, as stated, would still be satisfied. Or on the Fort Moore (38AK-5) graph, why is the lower line not positioned at 1690 instead of 1700, as it would in that case touch seven of the nine type bars? It appears that a
large measure of subjectivity is involved in the placement of these lines, a procedure that is counter to statisticians' insistence that bias be eliminated from statistical procedures. Use of an unbiased measure of dispersion about the mean would eliminate this subjectivity if a suitable technique for achieving such a measure could be found.

General Observation

The occurrence of particular types at well-documented archeological components of known date is the only scientifically acceptable means of verifying archeological dates for the respective ceramic types. Tentative dates based on documented dates of manufacture are very useful as heuristic models, but it should be clearly recognized that that is all they are. A type's archeological date (not its date of manufacture) clearly can be verified only through direct observance of that type's occurrence in the archeological record itself. That South is well aware of this is evident from what he says in the paper. However, I feel that the point is so important that it should be stated explicitly and emphatically.

Valid dates for types which cannot be dated even tentatively by date of manufacture because documentation is inadequate can still be dated accurately by observing their occurrence in archeological contexts of known date. The well-known and frequently cited pipestem chronology is an excellent example of a firm chronology that was established in exactly that way.
COMMENTS ON STANLEY SOUTH'S "EVOLUTION AND HORIZON AS REVEALED IN CERAMIC ANALYSIS IN HISTORICAL ARCHEOLOGY"

Lyle M. Stone

Five major observations were made during my first reading of Stan South's manuscript; these were, in order, as follows:

1. The present paper is an outgrowth of an earlier paper by South, entitled "The Ceramic Types at Brunswick Town, North Carolina" and published in the Southeast Conference Newsletter in 1962.

2. Stan has unnecessarily constructed a "straw man" antithesis, against which his present thesis can be favorably compared.

3. South's ceramic chronology model, as defined and exemplified in this paper, works very well—provided that one accepts the ceramic date range assigned to each of 78 ceramic types, as well as the basic assumptions which permit this form of ceramic interpretation.

4. If a person is sophisticated enough in ceramics analysis to be able to efficiently use this dating model, he probably has little need for it.

5. The use of this model could have both positive and negative implications for research in historical archaeology.

Upon rereading the report, I have decided to present my comments in terms of these basic observations.

1. The present paper is a logical outgrowth of an earlier paper by South (1962), in which he outlined a technique of comparing ceramic type percentage relationships between artifact assemblages as a means of dating archaeological units. This was perhaps the first systematic application of such a technique, commonly used by prehistorians, in the study of historic site remains. This early paper, as well as the present report, are based on the premise that the quantification and comparison of ceramic type percentage relationships between archaeological units can be a valuable tool in dating these units. The dates derived will be more accurate than similar information produced by a simple presence-absence technique of analysis. This premise, adequately supported by South in 1962, has been applied since by other archaeologists with similarly valid results.

2. On this basis, then, I would continue to my second observation and ask Stan why he feels it necessary to construct his
argument in obvious opposition to an outdated concept of ceramic analysis, most recently espoused by Dollar (1968), which maintains that there is little relationship between the date of a site and the date of ceramics from that site. This position has so little credibility that it need not be mentioned at all, unless one is reviewing the history of theory in historical archaeology. Clearly, few readers of Stan's present paper will judge its merit on the basis that it has invalidated a previously outmoded concept.

3. The chronology model prepared by South is valid—it works. The model has to work given its straightforward statistical basis; and, assuming Noël Hume's dates to be valid; and, that there is a direct relationship between the date of an archaeological unit and the date of a ceramic assemblage from that unit. Given these assumptions, which have been adequately supported by South and others in the past, then, the present model must work within an acceptable and predictable degree of reliability. The high degree of correlation expressed between computed dates and documentary dates clearly affirms that there is a direct relationship between the date of an archaeological unit and the manufacture date of an artifact assemblage from that unit.

As a test for the South model, I have analyzed several ceramic assemblages from archaeological units at Fort Michilimackinac, Michigan.

The first unit, Feature 296, is a soil stratum which has been interpreted as a late British period (ca. 1770-1780) garden bed or refuse zone. This feature's date is based on its stratigraphic position and artifact content, as well as on its horizontal relationship with adjoining British structures. Table 1 on the following page summarizes the ceramic data from this feature in terms of South's chronology model.

This computed date, although somewhat earlier than anticipated, is acceptable when related factors are considered. The Feature 296 stratum, although apparently deposited after 1770, does contain a large proportion of artifacts which date from an earlier period. The presence of these materials would thus have a lowering effect on the computed ceramic date. This computed date is also closely approximated by the feature's pipestem date of 1755.8, based on 19 specimens.*

The second unit, which includes four basements (F. 262, 265, 267, and 297) within a multi-room rowhouse (F. 266), has been assigned a date of construction between 1754 and 1760. This unit was in use until 1780. Feature 266 was first used by the French and then by the British from 1761 until 1780.

*[Editor's Note]: By removing Type 39 as is now recommended (South summary paper later in this forum) the ceramic date becomes 1775.4, not far from the expected 1775 median.
Table 1

<table>
<thead>
<tr>
<th>Type Number</th>
<th>Median Date (xi)</th>
<th>Sherd Count (fi)</th>
<th>Product (xi·fi)</th>
</tr>
</thead>
<tbody>
<tr>
<td>40</td>
<td>1763</td>
<td>12</td>
<td>756</td>
</tr>
<tr>
<td>43</td>
<td>58</td>
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<tr>
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<tr>
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<td>91</td>
<td>59</td>
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</tr>
<tr>
<td>23</td>
<td>90</td>
<td>2</td>
<td>180</td>
</tr>
<tr>
<td>21</td>
<td>55</td>
<td>1</td>
<td>55</td>
</tr>
<tr>
<td>49</td>
<td>50</td>
<td>25</td>
<td>1,250</td>
</tr>
<tr>
<td>65</td>
<td>20</td>
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</tr>
<tr>
<td>34</td>
<td>60</td>
<td>4</td>
<td>240</td>
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</tbody>
</table>

\[\sum_{i=1}^{150} (x_i·f_i) = 150 \times 9,450 + 1700 = 1763.0\]

Table 2

<table>
<thead>
<tr>
<th>Type Number</th>
<th>Median Date (xi)</th>
<th>Sherd Count (fi)</th>
<th>Sum</th>
<th>Product</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(F. 262 F. 265 F. 267 F. 297)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>40</td>
<td>1763</td>
<td>3 5 11</td>
<td>19</td>
<td>1,197</td>
</tr>
<tr>
<td>43</td>
<td>1758</td>
<td>1 2 13</td>
<td>16</td>
<td>928</td>
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<tr>
<td>39</td>
<td>1730</td>
<td>7 2 14</td>
<td>3 26</td>
<td>780</td>
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<tr>
<td>22</td>
<td>1791</td>
<td>6 6 15</td>
<td>4 31</td>
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</tr>
<tr>
<td>21</td>
<td>1755</td>
<td>1 1</td>
<td>2</td>
<td>110</td>
</tr>
<tr>
<td>49</td>
<td>1750</td>
<td>5 2 11</td>
<td>18</td>
<td>900</td>
</tr>
<tr>
<td>31</td>
<td>1770</td>
<td>2</td>
<td>2</td>
<td>140</td>
</tr>
</tbody>
</table>

\[6,876 \div 114 (+1700) = 1760.3\]
The majority of artifacts recovered from this structure and from its basements are of British manufacture. The ceramics from all four basements were combined for the purpose of analysis as shown on Table 2 of the previous page.

This date, although somewhat earlier than the interpreted date median for Feature 266, is also acceptable. It is probable that both sample size and contamination with earlier French artifacts acted to hold the date down.* Pipestem dates for the four basements are: F. 262, 1755.8 (25 specimens); F. 265, 1759.6 (16 specimens); F. 267, 1752.0 (41 specimens); and F. 297, 1752 (5 specimens).

4. In reviewing South's ceramic typology, it occurred to me that a person who could readily identify all 78 ceramic types would probably have little use for the ceramic chronology model or for the objective treatment which it provides, since his command of the data would allow him to generate acceptable dates (and, perhaps with a greater degree of accuracy) in the absence of such a formula. Moreover, it is probable that a person with this level of sophistication in ceramics identification would probably utilize many other types of information in dating a site. Although this point would seem to have only passing significance, I would predict that persons with the greatest experience in artifact analysis will make less use of the model than persons with only a cursory knowledge of historic artifacts.

5. The chronology model appears to have both positive and negative research implications. These implications should be fully understood if the potential of this model is to be maximized.

The following are positive implications:

a. The model, in necessitating a ceramic key, or taxonomy, provides an excellent division of ceramic types and attributes in terms of their temporal significance. The use of this model will force us to recognize specific attributes which have interpretive significance.

b. The South model will work to systematize our analytic procedures, make them more objective and rigorous, and provide more accurate results. It should be obvious at this time that important chronological data is inherent in historic artifacts and that this data may be derived through a procedure such as South has proposed. South's analytic model should also have an application in dating other common artifact categories from historic sites.

* [Editor's Note]: Removing Type 39 results in a ceramic date of 1769.3, also not far from the expected median of 1768.5. This adjustment is now recommended when using the formula (see South paper later in this forum
c. The use and understanding of this model should promote a recognition of the many factors which effect the dating, distribution, and usage of historic artifacts, i.e., status differences, functional differences, time lag, and so on.

d. If the South chronology model is found to produce an unacceptable date, judging from other dating sources, we will be forced to explain the discrepancy. I have found that this type of inquiry, attempting to explain an interpretive discrepancy, may produce some of the most useful observations that one can make in the study of an archaeological site. In this sense, the use of South’s model may indicate new areas for study and should generate new questions to be answered.

In terms of negative implications, it is possible that the chronology model could be misused, or that its results could be misinterpreted. I would therefore urge that the model not be applied too inflexibly; it cannot serve as a substitute for analysis. This system is best viewed as an aid to analysis—it must be compared with independent data which bear on the same phenomenon.

It is also obvious that the model could stand to be improved through the delineation of dates for ceramic types which were not used and through the refinement of dates which have been established. I would therefore hope that this model not be applied as an end in itself—a view which would preclude additional inquiry into the dating and interpretation of historic ceramics. This model must be viewed as an important step in the right direction, not as an ultimate tool to meet our dating needs.

In conclusion, a number of additional minor observations were made.

1. Would it not be more profitable to use vessel count (if available) rather than sherd count?

2. Stan’s paper clearly points out the need for a systematic, technical description of all ceramic types identified. I doubt that few persons in our field could readily identify all 78 ceramic types.

3. The accuracy of ceramic dates produced by the use of South’s model, when judged against documentary sources, lends additional credence to the high caliber of archaeological research being conducted at Williamsburg by Ivor Noël Hume.

4. The model will have its greatest practical utility in evaluating archaeological units from which supplementary dating information is not available.
BIBLIOGRAPHY

DOLLAR, CLYDE D.

SOUTH, STANLEY
SOME NOTES ON SOUTH'S CERAMIC DATING TECHNIQUE
Charles E. Cleland

In his paper "Evolution and Horizon as Revealed in Ceramic Analysis and Historical Archaeology," South illustrates the utility of quantification in ceramic analysis and demonstrates that the ceramic content of eighteenth century sites on the mid-Atlantic seaboard reflects with some accuracy the temporal sequence of ceramic manufacture. In so doing, he reverifies several principles which form the theoretical basis of macrostylistic evolution in known segments of space and time.

South also presents a method for estimating the mean date of occupancy of historic sites. In discussing the practicality of the method presented, I am inclined to first question the application of a mean site date. I can think of few cultural or historical problems where even a documented mean date would be as valuable as a less accurate estimate of the span of occupancy. A mean site date seems to have the same basic problems associated with it as a calculated mean of several radiocarbon dates. Secondly, and regrettably, I wonder who among us can employ South's method. Certainly, I would be very skeptical of my own abilities in this regard. Successful application of the method requires enormous control over the ceramic data. Perhaps I underrate the ceramic expertise of my colleagues, but I can think of but a half dozen individuals who know eighteenth century ceramics well enough to use the South method. Perhaps half of these have little interest in the kinds of problems the method is designed to investigate. Be that as it may, these practical matters do not reflect on the technique of ceramic dating under consideration.

Let us turn our attention to more substantial criticism. First, South tells us that a test case of object count versus sherd count of a ceramic sample from the Trebell Site cellar demonstrates that sherd count is a more superior weighting device in calculating relative frequency of type occurrence than object count. Lacking details of sample size and condition, this judgment is difficult to evaluate. How can one make a more accurate comparison of proportion of types than by counting the actual number of objects of each type represented? Although vessel count is the best method of comparison, it is often impractical when a sample is large and breakage extensive; therefore alternate methods may prove more effective. Sherd count has an obvious disadvantage—that is, differential breakage because of a variety of natural and physical causes may result in biased factoring. Perhaps some simple experimentation with both breakage differential and average whole vessel weight by type would produce a factoring index which would permit a more accurate assessment of the proportion of types represented in an archaeological sample. My second criticism is theoretical and deals with South's implied and undefined assumptions.
about unimodal curves. While it is true that general evolution of a type follows a unimodal curve through the phases of inception, rise to popularity, and decrease in popularity to extinction, it is not true that such a unimodal curve always conforms to a normal curve. Indeed, as a perusal of extant curves drawn from the temporal distribution of many types of artifacts and many localities shows, such curves are rarely normal ones. Most frequently, types show their greatest popularity soon after inception followed by a long period of decline. Sometimes the popularity of a type may be abruptly truncated by the appearance of another type. The rapid decline in the popularity of white salt-glazed ceramics with the introduction of creamware is an example. Still other types may exhibit a sudden burst of popularity and an equally spectacular extinction. In any case, a mean manufacturing date is not necessarily an accurate indicator of the date of mean frequency of the type. I also believe that the chances of this assumption being correct are reduced in proportion to the degree of commercialization in the manufacturing process—that is, when the quantity of a ceramic produced depends upon both willful judgments and popularity. Could it be that the frequency of various ceramic types in tightly dated historic sites is a more accurate reflection of the popularity of these types than mean historic dates of manufacture?

Theoretically, South's seemingly invalid assumption about the temporal sequence of type popularity should invalidate his conclusions. Moreover, it would appear that some of South's samples were drawn from nonrandom populations and that sample size does not seem to seriously affect the ultimate accuracy of the estimated mean date. Since his method obviously does work, the question of why it does becomes an interesting one. Perhaps South's method works precisely because he has balanced out numerous sources of error by compounding means. Thus, by assuming the mean manufacturing date and by assuming that all types from the given site within the range of such means conform to the same kind of normal curve, and then by proceeding to factor by frequency of type, the sources of possible error are averaged.

Having been perhaps overcritical of the utility of the method of obtaining a mean site date, especially in the face of success, a comment is due concerning South's suggested technique for visual bracketing. I find this method of inspection an important contribution of South's ceramic study and of great potential for dealing with any class of artifact.

In concluding, South notes that historic site archaeology has reached the stage where we should begin to place less emphasis on descriptive reporting and more emphasis on the formulation and testing of hypotheses about cultural behavior. I am in complete agreement with this conclusion. Certainly, South's paper deals with cultural problems and, just as certainly, it makes a fine contribution to the archaeological literature.
The past five years have seen substantial gains in the application of analytical procedures dealing with ceramics found at historical sites. Two people who have contributed much to these gains are Ivor Noël Hume and Stanley South. Now, in "Evolution and Horizon as Revealed in Ceramic Analysis in Historical Archaeology", Stanley South, using eleven sites, has introduced a "mean ceramic date formula"—an equation for use in assigning approximate inclusive dates for assemblages from carefully excavated sites. As tested against reasonably reliable dating from other sources, the "mean ceramic date formula" came through with flying colors. There seems little room for controversy as to whether or not the formula works. It does work, and Mr. South (with the consultation of Mr. Noël Hume) has produced an excellent piece of scholarship. More important this article presents a valuable working tool to historical archaeologists. The question now is not one of praise or condemnation. Rather the next step would seem to be one of exploring possible adjustments and additions in order to refine this quantitative approach.

Scholarly obscurantism is as annoying as it is unnecessary. After Mr. South has succinctly and clearly explained that he is convinced in eighteenth century American sites that "groups of ceramic types from different ruins of the same time period are similar enough to allow them to be used as dating tools for determining site occupation period" and that "These tools are useful and reliable when used on sites of varying functions over a broad area...", he goes on to belabor this clearly defined position with "the horizon concept", as propounded by Willey and Phillips (1958:31-34), as "a primarily spatial continuity represented by cultural traits and assemblages whose nature and mode of occurrence permit the assumption of a broad and rapid spread". My God!

But the questionable need to refer to this exercise in pedantry does not detract from Mr. South's purpose, and he presents the various parts of his thesis in a convincing manner. The use of the unimodal curve makes sense and his delineation of "The Problem" is excellent. Proceeding to "The Chronological Model for Constructing the Analysis Tools", Mr. South neatly sums up the problem and the present state of the art. Here one substantive objection must be registered. In setting forth his typology (78 ceramic types as compiled by Ivor Noël Hume), the author states that each type was "based on attributes of form, decoration, surface finish, hardness, etc..." Here the word "form" evidently is roughly synonymous with the word "shape". Yet, in his introduction Mr. South defines "form" as "a generalized term which includes shape, as well as those other attributes from which types are defined". The convolution of a rather standard (and frequently
used) noun into such an artificial term often leads to confusion. Here the author himself (as I comprehend the statement) has abandoned (or forgotten) his own definition. Such a lapse, of course, is not earth-shaking. On the other hand it is not nitpicking to observe that artificial terminology such as this can lead to confusion and misunderstanding. As the practical intent of the author is to create a workable tool for historical archaeologists; a simple straightforward language would appear desirable from any point of view. Accepting the other terms it would seem less misleading to state that "the evolutionary process is seen in the change of ceramic attributes through time".

The "mean ceramic date formula" appears satisfactorily established. Graphs (Fig. 1D) for the eleven sites consistently depict a tightly knit chronology. The inclusion of pipestem data supplies comparative and/or corroborative data. The inclusion of relevant absent types represents a major corollary to the practical use of this tool. Of course, as pointed out by Mr. South, there is a quite reasonable requirement that the sample be adequate. A very small sample or one obtained from careless or untrained excavation might yield untenable dating.

One caveat of Mr. South's seems so important that it bears repetition. "To use the mean ceramic formula, therefore, there is no easy way out. The archaeologist should have more than a passing knowledge of the ceramic types with which he deals." Such knowledge is not easy to come by—especially when this knowledge is to be tested by typing sherds rather than complete, highly characteristic museum pieces. One only has to study Noël Hume's list of 78 eighteenth century ceramic types (Fig. 1A) to realize the extent of the problem presented to working historic site archaeologists. This leads to a train of thought directed toward the future. At this time when comparatively few historical archaeologists have the necessary experience and background to use the "mean ceramic date formula", what further steps might be indicated? Two assumptions seem reasonable. First, the training and usual activity of the working historical archaeologist gives him some knowledge of ceramics (often of specific types involving his past and present work). Second, with few exceptions the working historical archaeologist, in the normal course of events, will not become highly familiar with the overall range of non-aboriginal ceramics found in North American sites. Then how should we proceed?

Listed below, not necessarily in order of priority, are some possible courses of action designed to further familiarize historical archaeologists with the multiple ceramic types with which they might be involved.

1. One course could be an extension and refinement of Ivor Noël Hume's list of 78 eighteenth century types. This would be a major project for Noël Hume. He is a prolific worker, but quite independent, and he might not agree that such a project
has merit or he might be disinclined, for example, to work with 1850-1900 material or Southwestern material. Should this be the case, perhaps a small team of archaeologists and ceramic historians (with a grant for photographic work and travel) could accomplish this. Such a project might contemplate:

a. Inclusion of seventeenth and nineteenth century ceramics.

b. Concise descriptions of each type and updated bibliographical references.

c. Good black and white photographs of each type.

d. Good 2" x 2" color slides of each type made available on order.

2. Another course could possibly be continuing seminars at archaeological meetings on ceramic identification. Special seminars at museums (Colonial Williamsburg, the Metropolitan Museum of Art, the Fortress of Louisburg, Winterthur, and the National Museum of History and Technology) where study collections are available.

3. Lastly, the establishment of a ceramic artifact library where sherds (and more complete pieces) from as many sites as possible will be kept for study. Also available would be complete pieces of the various ceramic types for comparative purposes, a slide collection, and necessary books and periodicals. (The National Museum of History and Technology, Smithsonian Institution, has long hoped to establish such a center, but funds have not been available.)

These three ideas, if they have relevance, have most probably occurred to others. Possibly one or more of them are already being implemented. If so, then good. If not they might be considered as representing at least a kind of approach that could be pertinent if historical archaeology hopes to produce more scholarly analysis and applicable research. Within this context Mr. South's most recent publication stands as something of a landmark.
Concerning the horizon aspects of historic pottery type distribution, it is clear that such ceramics spread widely and rapidly. As an additional illustration of this, I recall observing the pottery from Fort Prince George as it was being processed in the field laboratory and noting at the time the high degree of similarity with material I had recovered at Castle Hill. The two sites are far removed from one another in space but fall within the same time range.

A test of South's mean ceramic date formula was made using ceramics from the British period of occupation at Castle Hill, Placentia, Newfoundland. This occupation of the site has a maximum time range of 1714 to 1811 on the basis of historical documentation. The mean pipe-stem date for the British period at the site is 1744 using Binford's formula (Grange 1971).

The ceramic sample used in computing the date omits a small number of intrusive French sherds from the early period of occupation at the site. It includes a number of olive jar sherds which probably were used by the English. The date range for the olive jars used was the combined range of Goggin's middle and late styles, 1580 to 1850 (Goggin 1964). A median date of 1715 was used for this type in the date computation. The mean ceramic date for the British period at Castle Hill was computed to be 1762.69, precisely the median date for the maximum occupation as noted above.*

The sample used in the calculation included sherds from all excavation lots probably attributable to the British period. Another calculation based on only the most reliably identified excavation units of the English period (those in which the probability of mixture from the early occupation was very low or absent) produced a ceramic formula date of 1777, well within the maximum range. In fact the redoubt was not occupied continuously during the English period at Placentia, and the most intensive British occupation of Castle Hill was probably between 1749 and 1806. The median date for this period is 1777.

Using the visual bracketing tool in South's system would lead to the conclusion that the British period of occupation was from 1700 to 1795, which compares favorably with the 1714 to 1811 maximum time range based on historical evidence.

It is clear in the case of Castle Hill that the ceramic date formula would provide a reasonably accurate estimate in the absence of historical records. This method should prove very useful in historical

* [Editor's Note]: Eliminating Type 39 results in a date of 1763 (see South's summary paper later in this forum where the elimination of Chinese Porcelain is recommended).
archaeology, particularly when similar ceramic date data are readily available for systematic application of the method using French and Spanish types as well as English ones.

South's method is essentially a numerical seriation technique in which the mean formula date indicates the temporal position of the site in a cultural time continuum. As such the method might also have value in the analysis of prehistoric sites. The date range for the approximate period of manufacture of prehistoric pottery types determined on the basis of C14 or tree-ring dates could provide data for the determination of type median dates. South's formula could then be used to "date" sites, placing them in a seriational sequence for the appropriate culture or region.

A crude test of such an application on some protohistoric and historic Pawnee pottery types (Grange 1968) using a combination of C14 and historic dates to determine type ranges was made. A date of 1771 for an historic Pawnee site occupied between c. 1775 and 1815 was obtained. This looks promising, but a real test of the method on this ceramic tradition will require a great deal of work. In any event this is a good example of an important use of historical archaeology where "known" cases can be used to develop and test techniques of analysis which can then be applied to prehistoric material with greater confidence.

Note: The research at Castle Hill was sponsored by the National Historic Sites Service, National and Historic Parks Branch, Department of Indian Affairs and Northern Development, Ottawa, Canada.
HISTORICAL ARCHAEOLOGY FURUM - Grange

BIBLIOGRAPHY

GOGGIN, JOHN
1964 The Spanish Olive Jar. *Indian and Spanish Selected Writings*, University of Miami Press, Coral Gables, Florida.

GRANGE, ROGER T., JR.

THE APPLICATION OF THE SOUTH MEAN CERAMIC DATE FORMULA TO A NINETEENTH CENTURY SITE

George L. Miller

I tried South's Mean Ceramic Date Formula using a sample from the Walker Tavern, Cambridge Junction, Michigan, which is located about seventy-five miles west of Detroit on the old Chicago to Detroit road (US 12). The earliest documented evidence for the tavern's existence is 1836, however, local tradition says that it was built in 1832. Unfortunately the Lenawee County Court House burned down in 1852 which makes it difficult to establish the dates for the tavern. During the summer of 1968, a crew under the direction of Dr. Gordon L. Grosscup of the Wayne State University Department of Anthropology excavated two five by five foot units into the filled-in basement located under the first addition to the tavern. These pits produced a sample of 287 sherds. Several of the sherds from different levels fit together which, with the nature of the fill, suggests that the basement was filled deliberately at one point in time. What the relationship of this basement is to the first addition above it is not clear at this time. It may be to an earlier structure, or it may just be a basement that was filled in due to weak walls or some other reason. I should add at this point that the Walker Tavern is still standing. The question of the relationship of the basement could be answered by excavating the rest of the basement, but as usual there isn't funding for that. Anyway, the sherds give us dates which indicate that it could not have been filled-in before 1834, and that it most likely was filled-in in the mid 1840's, say ca.1845-46.

To simplify the problem of mean dates, I decided to use just the 81 sherds for which the manufacture is known. These dates were further adjusted by using 1829 as the beginning date for red and brown transfer printed wares. This is the date which is suggested by Simein Shaw in his History of the Staffordshire Potteries. The following table shows how I arrived at the date of 1836 as the formula mean date. From the historical records and the estimated filling date for the basement, I get a date of either 1836 or 1841. I was amazed at the closeness of the results. However, it should be pointed out that in a site of ten years occupation an error of two to five years is quite significant.
Sherds from the Basement Fill of the Walker Tavern, Cambridge Junction, Michigan

<table>
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<tr>
<th>Potter</th>
<th>Type and Color</th>
<th>Dates</th>
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</tr>
<tr>
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<td>1826.5</td>
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<td>1835</td>
<td>27</td>
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<tr>
<td>T. Mayer</td>
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<td>1826 to 1838</td>
<td>1832</td>
<td>3</td>
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<td>1833.5</td>
<td>6</td>
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| 81 Sherds                     | 148731.5 = 1836.1       |

Figure 1
ANSWER TO SOUTH, et al

Barbara Liggett

A good bit of stuff and nonsense is coming out lately purporting to generate new information on American culture without due concern for the nature of the evidence.

Given the limits of field discovery, field retrieval and precise laboratory identification and analysis, given the limits of documentary survival and expected shifts in the meaning of words, it is no wonder that a backlash of conservatism is rising against the current vogue in so-called statistical analysis.

The resistance met by such efforts is one based not on seat-of-the-pants common sense, but on strict adherence to traditional techniques: simply stated, observed relationships between levels, their assemblages of artifacts, their relation in time to levels above and below them, and comparison of such evidence against known structures and known records. This complex task cannot be replaced by calculation.

There is an important difference between quantification and statistical analysis, between counting, that is, and testing to find out how right or wrong a quantified proposition may be. There is nothing especially sophisticated or mysterious about the difference, but it is creating a division among us as though we must take sides for or against the trowel. The division is superfluous; the argument a waste of time.

Quantification leads to useful inferences about proportional presence or absence of things known to be available in a given place at a given time; it presupposes knowledge as to what available alternatives existed. A discovery is something that extends the range of what was thought to be available, either in itself, or in that place.

So far, classic [quantification]* and the skills required are merely the ability to calculate percentages and to record them on bar graphs. But when the investigator moves to statistics, he must remember that all he is doing is testing a proposition.

Statistically speaking, vast amusement results from juxtaposed uses of averages, e.g., two and a half children, and similar amusement arises from attempts to cluster and graph pipe stem diameters. Amusement shifts to scorn when the untrained take drill bits to the field to "date" stems, and thus the levels in which they were found.

As incredible as it is, it may as well be said and done with: unless the worker is prepared to duplicate the methodology of Binford, Harrington, Walker, et al, and be satisfied with a reliable range of 150 years, he may as well forget it. The significant word is reliable.

*Editor's Note: I have inserted this word in an effort to bring some coherence to this sentence.
and that is what statistics is all about. It takes into account the variables, such as how many bits of what lengths make up one pipe, the variation in the same pipe of its given diameter range from bowl to tip at the time it is made. Ask Adrian Oswald and William Henry. By allowing for the variables, we end up with proof of uselessness in an historic context. The time range is too wide for it to tell us any more than we already know.

This example is less that of misapplied statistical standards and controls, as it is of a misunderstanding of the nature of the beast being studied. Length of the whole stem itself and variations in diameter over a single pipe were not originally taken into statistical account. The same principle must now be laid against efforts to draw inferences about status from the appearance of porcelains in inventories. The variables are, again, several, including the linguistic problem of shifts in the meaning of words. Accident, bias, survival, choice, all the events that can lead to the disposal of a pot in a trash pit, can also effect inventories. Thus, suggesting an analogy that a trash pit and an inventory are the same thing in different places and should be treated similarly and with the same caution. They are different sides of the same coin.

I hope Noël Hume can appreciate the quantum jump in the level of concern in our fields when he reflects that two years ago, generations of students were being sent out into the world not knowing the difference between feather edge and shell edge, and now, a mixed bag of generations is being cleaved along the lines of those who have read Clarke, Redman, et al, those who can't get through them, those who own the books unopened, and those who dismiss the whole thing out of hand. It couldn't have happened without a spurt of the capacity to identify English ceramics thanks to excavations at potteries, and to several important publications in the past four or five years. Certain experiments, however, in the application of analytical theory to collections retrieved from filled pits are deceptive and misleading, as shown in South's attempt at cross-site correlations. If we are going to use analytical tools, let us use them correctly, thus dealing with the criticism we'll deserve if we don't.

If a sherd can be dated within five years, it should be, as Hume has so often said. Therefore, assigning its range and mean date of manufacture as South does as its interpretive date is pointless. Such a date has nothing to do with anything, much less occupation, and manufacture date-ranges are, themselves under serious and constant review and revision. Even then, what does a median date tell you? Quantity of

*Editor's Note: Sentence fragments and grammatical construction are the responsibility of the contributors to the Conference Papers and the Forum, as has always been the case. This editor does, however, try to catch obvious typographical errors and misspellings.
manufacture and export vary considerably over the period in which a given type was made. Without using a comparative English model which takes these variables into account, no useful statistical data can be generated from a handful of frontier pits.

The proof of the pudding can also be demonstrated in the current analysis of 21 pits excavated on five adjacent house sites located in the middle of early Philadelphia on the Franklin Court Market Street frontage. A date-range for each pit as a whole would be irrelevant since one of the historical facts of life in mid-city digging is that later cellar excavations invariably cut off the top eight to ten feet of any and all wells, privy wells previously in use. Zap. What is left for the archaeologist is what was left by the acts of 19th century developers. There is no way, statistically or inductively, to reconstruct the context and read the history of events through objects and their matrix once disassociated from their original context. It is tantamount to tearing out the last 10 pages of a murder mystery. The results are always skewed toward the lower levels of these features, and in such a way that potential error can be neither calculated nor controlled. Thus any statistical construct based on the contents of these pits must build in an allowance for a one-third to one-half absence factor — far too much for historic reliability. Thus, no more can be done than simple quantification, construction of date range by levels, computation of modal dates and then, inter-level correlations based on vessel occurrence. To attempt more with the known absence of information and skewing of content would be improper.

Therefore, when South proceeds on the unclarified assumption that his pits were equal in terms of type of stratification, and distribution of artifacts (concentrated, random, made fill, etc.) credibility of his results is already seriously in doubt.

I would quibble over the significance of a median historic date correlated with mean ceramic and pipe stem dates. I am struck, also, more by the appearance of statistically significant modes correlated with the given historic continuum: what happened on site between 1730-3/50 to produce the gap in the data? That is no statistical accident, it is proof of absence, and it is the kind of clue one uses to go back to the chronological model to interpret gaps in it. If the gaps correspond, you've defined a problem and can bring all the interpretive guns to bear. But to quantify and evaluate to this point, then pronounce the correlation "interesting" is a cop-out. Either there are valid historical conclusions, or the method is insufficiently developed. And that is what Clarke is all about: model building and comparisons, and valid conclusions, constructs to be used as one of the several tools available to the archaeologist in interpreting what he has found and not found on the sites he chooses to work. What to do about the bias of site selection is another subject.
South's formula itself fails internally, also. Betty Cosans takes it apart:

The purpose of statistical operations is twofold:

The first is the numerical description of the quantitative aspect of things or events—the presentation of groups of measurements, counts, and relationships between or within these measurements and counts. The second objective is the making of the best possible decisions in the face of uncertainty about numerical matters, and the process used is called statistical inference.¹

South's mean ceramic dating formula fulfills the first of these requirements but fails to deal with any aspect of statistical inference. Although the reliability of the formula was tested by comparison to externally defined criteria and found acceptable, the validity of the model from which the formula was derived has not been tested. Not only is the model itself untested but there is no way of determining at what point to accept or reject the mean ceramic date for any real sample of artifacts.

The formula was generated from a model based on two assumptions:

1. The distribution of any ceramic type over time describes a unimodal curve, the limits of which are defined by the date range of the type (South: 1972, p. 74). This distribution is a hypothetical curve with no numerical values assigned to the vertical coordinate (quantity)—only along the horizontal coordinate (time). Curve shape has been inferred from such nonstatistical variables as technology, fashion, and popularity. The mode itself can be assigned no location within the limits of the range in terms of a real numerical value.

2. The median or mid-point of the date range for any ceramic type can be assigned as an acceptable date for all examples of type (South: 1972, p. 75). This assumption presumes that the median is the best estimate of probable production. It is also

the only parameter of the curve which can be expressed as a real number.

In short, the mean ceramic dating formula was derived from the model by treating the hypothetical distribution as if it were a real distribution. The validity of this procedure hinges on whether or not the median is the best estimate of a production date. Theoretically speaking, the best estimate of production date in this distribution is the mode while the best for calculations is the mean. Because the mean is unknown and cannot be determined unless real quantities are assigned along the vertical coordinate all standard tests of statistical inference cannot be applied. In order to accept the median as the best estimate of manufacture date, it is necessary to determine the relationship between the mode and the median. Statistically speaking, the mode and the median fall at the same point in a normal distribution but a normal distribution for the ceramic types has not been established.

To the contrary, our experience in constructing and testing hypothetical curves for various ceramic types indicates that the distribution of a type over time probably describes a unimodal curve skewed to the right—a Chi Square rather than a normal distribution.

Where distribution A prevails, the mode is the median; but where distribution B prevails, the median actually falls somewhere to the right of the mode in the period of declining production. In the latter case, the median is not the best estimate of date for any ceramic type and introduces a potential error factor into the formula.

Because the curves from which the formula was derived are hypothetical, there is no way to actually determine how good an estimated date of production is provided by the median date. Goodness of fit in this case is a function of the range—the smaller the range, the closer the median to the mode hence the smaller the potential for error. Unfor-
Unfortunately, the range is not reflected in the mean ceramic dating formula. A type which is defined by the range 1740-1760 is assigned the same median date as a type dated 1700-1800. Because the error factor is not a standard error—that is to say, an error which is defined equally over the entire model, it cannot be ignored. On the other hand, because of the nature of the model itself, it cannot be measured and controlled.

By definition, the purpose of the mean ceramic dating formula derived from the ceramic type model is to assign a date to the mass of artifacts rather than defining the limits of the total context under consideration. But, at no point is the term "mass" defined either verbally or empirically.

Working on the assumption that the mean ceramic dating formula cannot possibly reflect that portion of artifact content which was not in production at the calculated date, a rough test was run on the contexts utilized for demonstration in this paper, comparing the mean ceramic date for each context with the proportion of artifacts assigned a date range, the lower limit of which exceeded the mean ceramic date. In 4 of 17 examples cited, the earliest possible production date for all ceramic types present fell at or below the mean ceramic date. In these cases, the mean ceramic date can be said to define the entire assemblage. In the remaining 13 cases, one or more ceramic types were not in production until after the date assigned by the formula. The proportion of objects having a terminus post quem exceeding the mean ceramic date varied from less than 1% to 48.9%. In other words, the best definition that can be given to "the mass of artifacts" is a set of limits ranging from more than 50% to 100%. Moreover, the percentages vary from assemblage to assemblage in no predictable manner. In the absence of any criterion for accept or reject, it must be assumed that a mean ceramic date which includes a little more than half the total content in a given field context is equally acceptable to the author as one which includes total content as long as it lies close enough to such external criteria as the historic median or dates derived from pipe stem analysis. In reality, mass itself is a variable treated for purposes of analysis and interpretation as a constant. In order to accept the whole concept of assigning mean ceramic dates as a useful analytical tool, methods must be found to hold the mass constant or utilize the variability of mass in establishing internal criteria for accept-reject tests.
A COMMENT ON THE CERAMIC ANALYSIS FORUM CONTRIBUTIONS

Stanley South

This forum reflects the contributor's views and thoughts resulting from reading my paper, and are all designed to explore the concepts around which that paper was built. My comments here are in the form of additional reflections since the paper was first written and presented, and after having read the various forum contributions.

Fitting's comments on my failure to mention that I was coding by subtracting 1700 from some of the Type Median Dates in Appendix III are well taken, and I have added a footnote to correct this omission.

In my paper I suggested that the mean ceramic date derived from the formula would likely average ±4 years from the documented occupation median. Fitting suggests that this is not statistically sound, and in view of the absence of a statistically derived confidence factor, this is true. He suggests using a standard deviation formula as a more efficient statistic, with which I agree, however, he implies that this range of the sample can be equated with the known range of historic occupation, which may or may not eventually prove to be the case. As Fitting points out, with the Brunswick S7 ruin this happened to be the case, but equating the standard deviation with the historic occupation is no more mathematically sound than equating the ceramic mean date ±4 years with the known historical median date. David South in his paper has dealt with this matter of statistical confidence and correlation in some detail regarding the use of the mean ceramic date formula. My ±4 years is better expressed statistically as a standard deviation of 4.58 years.

As Fitting correctly points out, very little archeological data ever seems to fit a normal curve. However, if a normal curve is involved we can say that approximately 68% of the ceramic type observations (sherds) will fall within ± one standard deviation from the mean ceramic date, and approximately 95% of the ceramic type observations (sherds) will fall within ± two standard deviations from the mean ceramic date. The first standard deviation bracket might be found to correlate closely with the known historic occupation date, but there is no statistical inference involved. We can illustrate this process by using the Goudy's data (38N1-5) from my paper as an example.

Coded by using (X-1700)

<table>
<thead>
<tr>
<th>Ceramic Type</th>
<th>Type Median ($X_i$)</th>
<th>Sherd Count ($f_i$)</th>
<th>Product</th>
</tr>
</thead>
<tbody>
<tr>
<td>29</td>
<td>1760 -1700 = 60 X 2</td>
<td></td>
<td>120</td>
</tr>
<tr>
<td>43</td>
<td>1758 -1700 = 58 X 3</td>
<td></td>
<td>174</td>
</tr>
<tr>
<td>49</td>
<td>1750 -1700 = 50 X 1</td>
<td></td>
<td>50</td>
</tr>
<tr>
<td>44</td>
<td>1738 -1700 = 38 X 1</td>
<td></td>
<td>38</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\frac{382}{7} + 1700 = 1754.6$</td>
</tr>
<tr>
<td></td>
<td>Historic Median Date is 1756</td>
<td></td>
<td>Mean Ceramic date is 1754.6</td>
</tr>
</tbody>
</table>
For determining the standard deviation we use the formula:

\[ S = \sqrt{\frac{\sum (f_i \cdot (X_i - \overline{X})^2) - (\sum f_i \cdot X_i)^2}{n-1}} \]

Coding for ease of manipulation can also be done by using \((X - 1738)\)

<table>
<thead>
<tr>
<th>Ceramic Type</th>
<th>((X_i - 1738))</th>
<th>((X_i - 1738)^2)</th>
<th>Sherd Count</th>
<th>(f_i)</th>
<th>Product (f_i \cdot (X_i - 1738)^2)</th>
<th>(f_i \cdot (X_i - 1738))</th>
</tr>
</thead>
<tbody>
<tr>
<td>29</td>
<td>22</td>
<td>484</td>
<td>X</td>
<td>2</td>
<td>= 968</td>
<td>44</td>
</tr>
<tr>
<td>43</td>
<td>20</td>
<td>400</td>
<td>X</td>
<td>3</td>
<td>= 1200</td>
<td>60</td>
</tr>
<tr>
<td>49</td>
<td>12</td>
<td>144</td>
<td>X</td>
<td>1</td>
<td>= 144</td>
<td>12</td>
</tr>
<tr>
<td>44</td>
<td>0</td>
<td>0</td>
<td>X</td>
<td>1</td>
<td>= 0</td>
<td>0</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>7</td>
<td></td>
<td>2312</td>
</tr>
</tbody>
</table>

\[ \sum (f_i \cdot (X_i - 1738)^2) = 2312 \]

\[ \sum (f_i \cdot (X_i - 1738))^2 = (116)^2 = 13456 \]

Applying these figures to the standard deviation formula we have:

\[ S = \sqrt{2312 - \frac{13456}{7}} \approx 8.06 \]

If a normal curve is involved then approximately 68% of the type median dates (sherds) will fall within ±16.12 years from the mean ceramic date of 1754.6.

In their discussion of the assumptions upon which my paper was based, Fitting, Walker and others mention the normal curve, unimodal curve, bimodal or multimodal curve possibilities reflected by the data. If the normal curve is bothersome we can derive our statements of confidence in our mean ceramic date by means of Tchebysheff's Theorem (Mendenhall 1969:35-36).
HISTORICAL ARCHAEOLOGY FORUM - South

Tchebyseff's Theorem

Given a number \( k \) greater than or equal to 1 and a set of \( n \) measurements \( y_1, y_2, \ldots, y_n \), at least \( 1 - 1/k^2 \) of the measurements will lie within \( k \) standard deviations of their mean.

Tchebyseff's Theorem is illustrated by Mendenhall as follows:

![Diagram of Tchebyseff's Theorem]

Tchebyseff's Theorem is a conservative statement that applies to any distribution of measurements, and does not involve a normal curve. Tchebyseff's Theorem reveals that 75% of the sample will lie within \( \pm 2 \) standard deviations, and 89% of the sample will lie within \( \pm 3 \) standard deviations from the mean. Using this concept and applying it to our mean ceramic date we can say that at least 75% of the total number of sherds lie within \( \pm 2 \) standard deviations of the mean ceramic date. This is twenty percent less confidence than if a normal curve is assumed to be represented by the sample. Thus the argument against a normal curve being involved in archaeological samples only relates to a twenty percent differential for the confidence intervals expressed for two standard deviations from the mean.

Turning now to another point, Fitting has correctly pointed out that I failed to develop adequately the concept of evolution as it relates to the data examined in my paper. He has interpreted my statement regarding changing form as reflecting the evolutionary process, as an acceptance of cultural evolution as homologous with biological evolution. I certainly accept the fact that there is an analogy between biological and cultural evolution, but I chose not to go into this philosophical question in my paper. The use of the word "homologous" and the discussion of detailed comparison is done by Fitting, not by me.

He has stated that there is nothing explicit in my paper to contradict the impression that I view ceramic change, hence cultural change, as a random process. It would seem to me that the fact that my paper
dealt with patterned cultural processes to the extent that Iconstructed a formula to express this regularity is sufficient evidence to indicate that I do not view culture as a random process. Formulas imply predictability based on pattern.

What I could have said to elaborate on the evolution concept is that ceramic change, hence cultural change, is not synonymous with evolution, which I view as rooted in a slower, broader base of cultural dynamics. Pearlware, for instance, is empirically identified by a single glaze attribute which was the result of Wedgwood's addition of cobalt to the glaze in 1779 (Hume 1970:128). This ceramic change has definite temporal significance for the archeologist, and reflects the goal of eighteenth century potters for an ever increasing whiteness in their ware, but this change is not seen to reflect the process of evolution. It is seen as a unique historical event, which, when combined with similar events that gave us white salt-glazed stoneware, creamware, and ironstone, allows us to view the cultural dynamics involved in the evolutionary change from white salt-glazed stoneware to modern hotel porcelain. The development of formulas from the examination of specific ceramic type relationships such as was done in my paper is the groundwork for questions of broader import, testing hypotheses relating to the process of evolutionary change. I agree with Binford that:

Archaeologists should be among the best qualified to study and directly test hypotheses concerning the process of evolutionary change, particularly processes of change that are relatively slow, or hypotheses that postulate temporal-processual priorities as regards total cultural systems (Binford 1962:224).

We turn now from the broader considerations to the practical application of the mean ceramic date formula as reported by various archeologists. George L. Miller successfully used the formula on ceramics from a tavern site in Michigan, utilizing his own median manufacture dates for transfer-printed ceramics as determined from his research. This adaptation of refined data to the formula concept is utilizing the formula as the flexible tool it was designed to be. George plans to apply the formula to specific inventories of ceramic types he has found in his research to determine its usefulness in this manner. His paper reporting the use of the formula on the Walker Tavern ceramics is included in this forum.

Roger Grange applied the formula to ceramics from Castle Hill, Newfoundland and found that for that site with a median historic date of 1762.5, he arrived at a mean ceramic formula date of 1762.69, which, Roger remarked to me in a letter "as a crude approximation, is not bad". Eliminating Chinese Porcelain resulted in a date of 1763, still "not bad".* His report on this application of the formula is included in this forum. Roger also used the formula with a variation, including some Spanish

*See the explanation for this on the following page.
olive jar fragments. Provided manufacture dates are known with reasonable accuracy this procedure is entirely acceptable. As was pointed out in my paper, locally made ceramics with well documented manufacture periods could also be used to advantage with the formula. However, if the olive jar dates assigned by John Goggin have been revised from new data, the revised dates should be used rather than those of Goggin.

A significant development in the use of the formula came as a result of a letter from Stephen Israel, who sent Table 4 (Appendix I of this paper) which listed the ceramic types recovered from Fort Dobbs, North Carolina. This site was discovered by me, and some preliminary work was done, with more extensive excavation being carried out by Garry Stone at a later time. The fort had a short time of occupation, from 1756 to 1763, with a median historic occupation date of 1759.5. The mean ceramic date using the formula was 1747.44, almost a decade prior to the occupation of the site, and twelve years earlier than the known median occupation date. An explanation of this lack of conformity of the ceramic date with the historic date was sought, and it was discovered that by removing Underglaze Blue Chinese Porcelain (Type 39), from consideration in the formula, the date became 1755.8, much closer to the known occupation period. The question then arose as to what the elimination of Type 39, as well as Overglaze Enamelled Chinese Porcelain (Type 26) would do to the formula results on the sites for which data was available. This was done for thirteen sites for which the historic occupation dates were known, and the results are seen in the statistical treatment in Appendices IV and V of this paper.

By statistically examining the data presented in my paper David South (this forum) was able to demonstrate that the ceramic formula tended, on the average, to underestimate the median historic occupation date by 2.2 years. By removal of Type 26 and Type 39, and using the statistical approach of David South, we have found that the formula now tends to overestimate the median historic occupation date by only 1.1 years (See Appendices IV and V for statistical data). It is now suggested that these types (Type 26 and Type 39) not be used in determining the mean ceramic date of an historic site. The reason for this is the fact that both these types as used in my paper are long time-span types with median dates tending to skew the formula results (as any long span type will tend to do). I would like to emphasize the fact that this does not mean that Chinese Porcelain needs to be dismissed from our ceramic studies! Certain design motifs are known to be valuable temporally in separating sub-types within Chinese Porcelain, and Overglaze Enamelled Chinese Export Porcelain is usually seen on sites dating after the first quarter of the eighteenth century. When considered in the broad manner as defined in my ceramic formula paper, however, these types tend to distort the mid eighteenth century dates, and should, as suggested, be left out of the formula analysis. If the archeologist knows Chinese Porcelain to the extent that he can typologically separate temporally significant sub-types, he should by all means use these in the formula to derive his mean ceramic date.
Ivor Noël Hume has supplied ceramic analysis data for the Trebell Site in Virginia. The first documentation presently available on the property is 1768, with documents indicating that a house on the site burned in 1826. The median historic date from this information is 1797. The mean ceramic date is 1797.6 (See Appendix II for data). When Chinese Porcelain types 26 and 39 are included the ceramic date was 1788.9. From this we see that eliminating the Chinese Porcelain produced a date nearer to the expected documented median date for the site.

Lyle Stone (this forum) used the formula on two samples from Fort Michilimackinac, Michigan and found the formula dates too early. However, when Type 39 was removed the formula produced a date of 1775.4 for an expected 1775 occupation median, and a ceramic date of 1769.3 for an expected median of 1768.5. Such examples clearly reveal the wisdom of removing Chinese Porcelain from the types used with the ceramic formula.

An important consideration that should not be overlooked is the relating of an archeological ruin to the documentation for a piece of property. Documentary data for a large tract of land can not be used indiscriminately to apply to a ruin found on that property since many ruins of various periods could be present on such a tract. Another difficult situation is seen with sites that may have been located in heavily populated areas, such as the James Geddy House property in Williamsburg, with a documented occupation from 1716 to the present. Perhaps the best sites for controlled dating are those isolated ones such as fort sites that were occupied for short periods of time, or documented house sites where occupation was limited due to burning of the house, and where there was no subsequent occupation. It is from such ruins that our best control is to come for refining the mean ceramic date formula.

Noël Hume also supplied us with an object count based on the fragments as well as an individual sherd count. As was mentioned above, the sherd count ceramic date was 1797.6. The object count using the same ceramic fragments produced a mean ceramic date of 1790.1 (Appendix II).

Richard Polhemus provided us with ceramic data from Unit One at the Tellico Blockhouse in Tennessee, a test excavation conducted some years ago. This site has a documented date range from 1794 to 1807, with a median historic date of 1800.5. The mean ceramic date using sherd count was found to be 1802.8, with a vessel count date for the same collection producing a mean ceramic date of 1802.9. The date for this study is included in Appendix III of this paper.

As was mentioned in my ceramic formula paper Ivor and Audrey Noël Hume were very helpful in refining the temporal manufacture periods for the ceramic types used in the analysis, and the result of this refinement session is seen in the 78 ceramic types listed on the analysis chart accompanying my ceramic formula paper. In addition to this a considerable time was spent by the Williamsburg archeological staff in sherd counting and preparing analysis data from the nineteen layers of a well excavated on the James Geddy House property (Well E.R. 1340). This was done to provide a check on the mean ceramic date formula in relation to the sequence of terminus post quem dates derived by Noël Hume from these nineteen layers of the well.
DATA FROM WELL E.R. 1340 EXCAVATED BY IVOR NOEL HUME
COLONIAL WILLIAMSBURG, VIRGINIA
ON THE JAMES GEDDY HOUSE PROPERTY

<table>
<thead>
<tr>
<th>Archeological Provenience Layer</th>
<th>South Mean Ceramic Date Using the Formula</th>
<th>Noel Hume terminus post quem Date</th>
<th>Average Mean Ceramic Date for Interpretive Levels (Rounded)</th>
<th>Years Between the Average Mean Ceramic Date and terminus post quem Date</th>
<th>South’s Interpreted Occupation Dates Represented by the Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>1340</td>
<td>B 1787.7</td>
<td>1842</td>
<td>1842</td>
<td>54</td>
<td>c. 1734 to 1842</td>
</tr>
<tr>
<td></td>
<td>C 1759.5</td>
<td>1780</td>
<td>1780</td>
<td>20</td>
<td>c. 1740 to 1780</td>
</tr>
<tr>
<td></td>
<td>D 1751.8</td>
<td>1770</td>
<td>1770</td>
<td>21</td>
<td>c. 1728 to 1770</td>
</tr>
<tr>
<td></td>
<td>E 1750.8</td>
<td>1770</td>
<td>1749</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>F 1745.2</td>
<td>1760</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>G 1749.0</td>
<td>1760</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>J 1746.5</td>
<td>1755</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>L 1748.7</td>
<td>1755</td>
<td></td>
<td></td>
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</tr>
<tr>
<td></td>
<td>M 1743.3</td>
<td>1755</td>
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<td></td>
<td>N 1749.7</td>
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<td>P 1750.3</td>
<td>1755</td>
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<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Q 1754.6</td>
<td>1755</td>
<td>1762</td>
<td>13</td>
<td>c. 1736 to 1762</td>
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<td></td>
<td>R 1751.5</td>
<td>1762</td>
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<td></td>
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<tr>
<td></td>
<td>S 1747.7</td>
<td>1762</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>T 1746.7</td>
<td>1762</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>V 1739.6</td>
<td>1750</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>W 1746.5</td>
<td>1750</td>
<td>1750</td>
<td>5</td>
<td>c. 1740 to 1750</td>
</tr>
<tr>
<td></td>
<td>X 1745.3</td>
<td>1750</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Y 1749.0</td>
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</tr>
<tr>
<td></td>
<td>1341A</td>
<td>1750</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: The traditional terminus post quem dates are derived from the most recent object. Any other objects, no matter how early they be, have no bearing on the actual date of the filling.

With the use of the mean ceramic date and terminus post quem, approximate occupation dates represented by the ceramic sample can be interpreted.

FIGURE 1
Figure 1 illustrates the result of this analysis of the ceramics from this well and is self explanatory when studied carefully, but several points should be noticed. If the formula is to work the mean ceramic dates should parallel the terminus post quem sequence of the various layers. The nineteen layers were grouped into five interpretive levels based on the terminus post quem date assigned by Noël Hume. These extend from 1750 for the layers at the bottom of the well to 1872 for the top layer of the well. Using these interpretive levels the mean ceramic dates were averaged for each level, resulting in five ceramic dates ranging from 1745 for the bottom layers to 1788 for the top layer. The years between the average mean ceramic date and the terminus post quem dates were derived by subtracting the mean ceramic date from the terminus post quem date. This remainder is then subtracted from the mean ceramic date in order to arrive at the interpreted beginning occupation date represented by the ceramic sample. This date in conjunction with the terminus post quem date is the interpreted occupation period represented by the ceramic sample.

It is interesting to note that the interpreted beginning date for the bottom five layers is 1740, with the top layer having an interpreted beginning date of 1734. Notice that the beginning date represented by the ceramic interpretation from each of the five levels varies relatively little, while the terminus post quem date reflects the temporal change in the deposit. One is tempted to observe that using this method the top layer has given us an interpreted beginning date for the occupation period represented by the entire fill of the well that changed relatively little as deeper layers were excavated, and that if the interpreted occupation date was all we were after we could have derived this from excavation of the top layer only! Each level is seen to reveal an interpreted occupation period represented by the entire deposit below it rather than for that level alone. This phenomenon is the same one often seen in excavation of cellar holes that have been intentionally filled, wherein the top layers reveal a ceramic date earlier than the floor level of the cellar since the floor level represents primarily the period of the destruction of the structure, whereas the fill was often taken from soil in the surrounding area, and represents the entire period of occupation of the site. The fact that the beginning interpreted date remains relatively unchanged in Well E.R. 1340 for each layer would point to this well having received a mixed deposit throughout the history of its fill period.

A well or privy that had been used as a deposit for midden directly from the kitchen over a long period of time, with no addition of soil from the surrounding area might well reveal a ceramic-interpreted beginning date also reflective of changing time as would the terminus post quem date for each layer. The mean ceramic date formula needs to be applied to many similar stratigraphic examples before general conclusions can be drawn. However, its use with the data from Well E.R. 1340 at the James Geddy House property in Williamsburg presents us with clues.
to its potential value as an interpretive tool for the occupation period represented by the ceramics from stratigraphic deposits.

In evaluating the mean ceramic date formula as an interpretive tool based on its use thus far by contributors to this forum, it appears that it has thus far worked well. I would suggest that the Chinese Porcelain types 26 and 39 be eliminated when using the formula until such time that we have available more refined temporal manufacture brackets based on specific sub-types. In so doing the mean ceramic date thus arrived at will on the average (based on the present data), be closer to the known historic occupation median than when the Chinese Porcelain types are included.

For those archeologists who have taken the time to apply the formula to their own data by scientifically testing the hypothesis, the results are highly encouraging. It is interesting to note that those who have been most critical are those who have not bothered to apply the tool to test its validity. Barbara Liggett's reaction is an example of the intuitive rejection of the concept rather than a weighing of empirical archeological data relating to the pragmatic application of the formula. Some of her objections are the same as those raised by others, while other points miss their mark completely, and reveal an apparent "misunderstanding of the nature of the beast being studied" (Liggett, this volume). Her straw men have little analogy to the problems faced in my paper. I would like to join Lewis Binford in urging historic site archeologists to use the mean ceramic formula date technique whether they agree with it or not:

...since the only way of gaining sufficient knowledge for evaluating their skepticism is to have available a broad comparative body of data for evaluation in the context of scientific methods (Binford, this volume).
HISTORICAL ARCHAEOLOGY FORUM - South

BIBLIOGRAPHY

BINFORD, LEWIS R.


FITTING, JAMES E.

GRANGE, ROGER T.
1972 A Mean Ceramic Date for Castle Hill, Newfoundland. Conference on Historic Site Archaeology Papers 1971 6, Part 2, Section 1.

HUME, IVOR NOËL

MENDEHALL, WILLIAM

MILLER, GEORGE L.
1972 The Application of the South Mean Ceramic Date Formula To a Nineteenth Century Site. Conference on Historic Site Archaeology Papers 1971 6, Part 2, Section 1.

SOUTH, DAVID

STONE, LYLE M.
1972 Comments on Stanley South's "Evolution and Horizon as Revealed in Ceramic Analysis in Historical Archeology". Conference on Historic Site Archaeology Papers 1971 6, Part 2, Section 1.

WALKER, IAIN C.
1972 Comments on Stanley South's "Evolution and Horizon as Revealed in Ceramic Analysis in Historical Archeology". Conference on Historic Site Archaeology Papers 1971 6, Part 2, Section 1.
APPENDIX I - Fort Dobbs

Data submitted by Stephen Israel

Site: Fort Dobbs, North Carolina "Table 4"

Documented Date: 1756–1763

Median Historic Date: 1759.5

<table>
<thead>
<tr>
<th>Ceramic Type Number</th>
<th>Median Date</th>
<th>Sherd Total</th>
<th>Product</th>
</tr>
</thead>
<tbody>
<tr>
<td>31</td>
<td>1770</td>
<td>4</td>
<td>280</td>
</tr>
<tr>
<td>49</td>
<td>1750</td>
<td>85</td>
<td>4250</td>
</tr>
<tr>
<td>40</td>
<td>1763</td>
<td>24</td>
<td>1512</td>
</tr>
<tr>
<td>34</td>
<td>1760</td>
<td>45</td>
<td>2700</td>
</tr>
<tr>
<td>36</td>
<td>1755</td>
<td>6</td>
<td>330</td>
</tr>
<tr>
<td>29</td>
<td>1760</td>
<td>18</td>
<td>1080</td>
</tr>
</tbody>
</table>

Mean Ceramic Date is: 1755.8

Note: 87 sherds of Type 39 not used.
Data submitted by Ivor Noél Hume.

Site: Trebell Site Cellar (TS 807C), Virginia.

Documented Date: 1768-1826.

Median Historic Date: 1797.

<table>
<thead>
<tr>
<th>Ceramic Type Number</th>
<th>Median Date</th>
<th>Object Total</th>
<th>Product</th>
</tr>
</thead>
<tbody>
<tr>
<td>15</td>
<td>1798</td>
<td>60</td>
<td>107880</td>
</tr>
<tr>
<td>19</td>
<td>1805</td>
<td>27</td>
<td>48735</td>
</tr>
<tr>
<td>11</td>
<td>1818</td>
<td>7</td>
<td>12726</td>
</tr>
<tr>
<td>12</td>
<td>1805</td>
<td>7</td>
<td>12635</td>
</tr>
<tr>
<td>14</td>
<td>1798</td>
<td>1</td>
<td>1798</td>
</tr>
<tr>
<td>6</td>
<td>1843</td>
<td>1</td>
<td>1843</td>
</tr>
<tr>
<td>17</td>
<td>1800</td>
<td>1</td>
<td>1800</td>
</tr>
<tr>
<td>40</td>
<td>1763</td>
<td>3</td>
<td>5289</td>
</tr>
<tr>
<td>24</td>
<td>1780</td>
<td>3</td>
<td>5340</td>
</tr>
<tr>
<td>49</td>
<td>1750</td>
<td>4</td>
<td>7000</td>
</tr>
<tr>
<td>65</td>
<td>1720</td>
<td>1</td>
<td>1720</td>
</tr>
<tr>
<td>56</td>
<td>1733</td>
<td>1</td>
<td>1733</td>
</tr>
<tr>
<td>47</td>
<td>1748</td>
<td>3</td>
<td>5244</td>
</tr>
<tr>
<td>37</td>
<td>1733</td>
<td>1</td>
<td>1733</td>
</tr>
<tr>
<td>46</td>
<td>1755</td>
<td>1</td>
<td>1755</td>
</tr>
<tr>
<td>54</td>
<td>1733</td>
<td>8</td>
<td>13864</td>
</tr>
<tr>
<td>44</td>
<td>1738</td>
<td>1</td>
<td>1738</td>
</tr>
<tr>
<td>58</td>
<td>1668</td>
<td>1</td>
<td>1668</td>
</tr>
</tbody>
</table>

\[
\text{Mean Ceramic Date is: } \frac{234501}{131} = 1790.1
\]

Note: 12 Type 26 and 13 Type 39 not used.
APPENDIX II (cont.)

USING COUNT BY SHERDS

<table>
<thead>
<tr>
<th>Ceramic Type Number</th>
<th>Median Date</th>
<th>Sherd Total</th>
<th>Product</th>
</tr>
</thead>
<tbody>
<tr>
<td>6</td>
<td>1843</td>
<td>5</td>
<td>9215</td>
</tr>
<tr>
<td>11</td>
<td>1818</td>
<td>88</td>
<td>159984</td>
</tr>
<tr>
<td>12</td>
<td>1805</td>
<td>62</td>
<td>111910</td>
</tr>
<tr>
<td>13</td>
<td>1805</td>
<td>25</td>
<td>45125</td>
</tr>
<tr>
<td>15</td>
<td>1798</td>
<td>434</td>
<td>780332</td>
</tr>
<tr>
<td>17</td>
<td>1800</td>
<td>19</td>
<td>34200</td>
</tr>
<tr>
<td>19</td>
<td>1805</td>
<td>309</td>
<td>557745</td>
</tr>
<tr>
<td>24</td>
<td>1780</td>
<td>3</td>
<td>5340</td>
</tr>
<tr>
<td>28</td>
<td>1769</td>
<td>6</td>
<td>10614</td>
</tr>
<tr>
<td>44</td>
<td>1738</td>
<td>1</td>
<td>1738</td>
</tr>
<tr>
<td>46</td>
<td>1755</td>
<td>2</td>
<td>3510</td>
</tr>
<tr>
<td>54</td>
<td>1733</td>
<td>71</td>
<td>123043</td>
</tr>
<tr>
<td>56</td>
<td>1733</td>
<td>1</td>
<td>1733</td>
</tr>
<tr>
<td>58</td>
<td>1668</td>
<td>1</td>
<td>1668</td>
</tr>
<tr>
<td></td>
<td></td>
<td>1027</td>
<td>1846157</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$1846157 \div 1027 = 1797.6$</td>
</tr>
</tbody>
</table>

Mean Ceramic Date is: 1797.6  
Historic Median Date is 1797.0

Note: 101 sherds of Type 26 and 50 sherds of Type 39 were not used.

[Editor's Note]: 4 sherd types are missing from the count by sherds.
HISTORICAL ARCHAEOLOGY FORUM - South

APPENDIX III - Tellico Blockhouse (40MR50)

Data submitted by Richard Polhemus.

Site: Tellico Blockhouse (40MR50), Tennessee. (Unit 1).

Documented Date: 1794-1807 (American State Papers, Vol. II)

Median Historic Date: 1800.5

<table>
<thead>
<tr>
<th>Ceramic Type Number</th>
<th>Median Date</th>
<th>Sherd Total</th>
<th>Product</th>
<th>Vessel Total</th>
<th>Product</th>
</tr>
</thead>
<tbody>
<tr>
<td>31</td>
<td>1770</td>
<td>7</td>
<td>12390</td>
<td>4</td>
<td>7080</td>
</tr>
<tr>
<td>6</td>
<td>1843</td>
<td>1</td>
<td>1843</td>
<td>1</td>
<td>1843</td>
</tr>
<tr>
<td>13</td>
<td>1805</td>
<td>1</td>
<td>1805</td>
<td>1</td>
<td>1805</td>
</tr>
<tr>
<td>10</td>
<td>1818</td>
<td>6</td>
<td>10908</td>
<td>2</td>
<td>3636</td>
</tr>
<tr>
<td>11</td>
<td>1818</td>
<td>23</td>
<td>41814</td>
<td>6</td>
<td>10908</td>
</tr>
<tr>
<td>12</td>
<td>1805</td>
<td>111</td>
<td>200355</td>
<td>29</td>
<td>52345</td>
</tr>
<tr>
<td>17</td>
<td>1800</td>
<td>71</td>
<td>127800</td>
<td>10</td>
<td>18000</td>
</tr>
<tr>
<td>19</td>
<td>1805</td>
<td>101</td>
<td>182305</td>
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<td>30685</td>
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<tr>
<td>15</td>
<td>1798</td>
<td>116</td>
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<td>26970</td>
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<tr>
<td>23</td>
<td>1790</td>
<td>7</td>
<td>12320</td>
<td>2</td>
<td>3580</td>
</tr>
<tr>
<td>20</td>
<td>1805</td>
<td>154</td>
<td>277970</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

Mean Ceramic Date from Sherd Count = $1078078 \div 598 = 1802.8$

Mean Ceramic Date from Vessel Count = $156852 \div 87 = 1802.9$

Note: 1 sherd of Type 39 not included.
HISTORICAL ARCHAEOLOGY FORUM - South

APPENDIX IV - Statistical Data-Minus Chinese Porcelain

COMPARISON OF HISTORIC MEDIAN AND MEAN CERAMIC DATES SHOWING DIFFERENCE, MEAN, AND STANDARD DEVIATION WHEN CHINESE PORCELAIN TYPES 26 AND 39 HAVE BEEN REMOVED

<table>
<thead>
<tr>
<th>Site</th>
<th>Median Historic Date</th>
<th>Mean Ceramic Date</th>
<th>CD-HD</th>
<th>(CD-HD)^2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Brunswick S7, N.C.</td>
<td>1755.0</td>
<td>1758.5</td>
<td>+3.5</td>
<td>12.25</td>
</tr>
<tr>
<td>38AK4-15, S.C.</td>
<td>1731.5</td>
<td>1730.0</td>
<td>-1.5</td>
<td>8.25</td>
</tr>
<tr>
<td>38AK5-A, S.C.</td>
<td>1741.0</td>
<td>1746.5</td>
<td>+5.5</td>
<td>30.25</td>
</tr>
<tr>
<td>38GN1-5, S.C.</td>
<td>1755.5</td>
<td>1754.6</td>
<td>-.9</td>
<td>.81</td>
</tr>
<tr>
<td>38PN1, S.C.</td>
<td>1760.5</td>
<td>1767.1</td>
<td>+6.6</td>
<td>43.56</td>
</tr>
<tr>
<td>Brunswick S15, N.C.</td>
<td>1751.0</td>
<td>1750.7</td>
<td>-.3</td>
<td>.09</td>
</tr>
<tr>
<td>Brunswick N1, N.C.</td>
<td>1753.5</td>
<td>1752.0</td>
<td>-.5</td>
<td>2.25</td>
</tr>
<tr>
<td>Brunswick S2, N.C.</td>
<td>1753.5</td>
<td>1750.4</td>
<td>-3.1</td>
<td>9.61</td>
</tr>
<tr>
<td>Brunswick S18, N.C.</td>
<td>1769.5</td>
<td>1777.9</td>
<td>+8.4</td>
<td>70.56</td>
</tr>
<tr>
<td>Brunswick S10, N.C.</td>
<td>1803.0</td>
<td>1801.8</td>
<td>-1.2</td>
<td>1.44</td>
</tr>
<tr>
<td>Trebe11, Va.</td>
<td>1797</td>
<td>1797.6</td>
<td>+.6</td>
<td>.36</td>
</tr>
<tr>
<td>Tellico Blockhouse, Tenn.</td>
<td>1800.5</td>
<td>1802.8</td>
<td>+2.3</td>
<td>5.29</td>
</tr>
<tr>
<td>Ft. Dobbs, N.C.</td>
<td>1759.5</td>
<td>1755.8</td>
<td>-3.7</td>
<td>13.67</td>
</tr>
<tr>
<td>SUM</td>
<td></td>
<td></td>
<td>14.7</td>
<td>198.39</td>
</tr>
</tbody>
</table>

\[ \bar{y} = \frac{\sum_{i=1}^{n} y_i}{n} \]

\[ S = \sqrt{\frac{\sum_{i=1}^{n} y_i^2 - \left( \frac{\sum_{i=1}^{n} y_i}{n} \right)^2}{n-1}} \]

\[ \bar{y} = \frac{1.131}{14.7} \]

*Note: Since these 13 sites were dealt with as shown here, dates for Castle Hill, Newfoundland and two Fort Michilimackinac samples were added, making 16 sites, the results of which are presented in a note on page 217.*
HISTORICAL ARCHAEOLOGY FORUM - South

APPENDIX IV - (cont.)

\[ S = \sqrt{\frac{198.39 - (14.7)^2}{13}} \]

\[ S = \sqrt{15.1475} \]

\[ S = 3.89198 \]

\[ S^2 = 15.1476 \]

The following formula is used when dealing with small samples to infer the range in which the total population mean might fall:

\[ \bar{Y} \pm \frac{S}{\sqrt{n}} t_{a/2} \]

Where \( \bar{Y} \) = sample mean

\( t_{a/2} \) = number used when confidence coefficient desired is \( x \) percent and \( n \) observations are used (see table below)

\( S \) = standard deviation of the sample

\( n \) = number of observations in the sample

<table>
<thead>
<tr>
<th>CRITICAL VALUE OF ( t )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Confidence factor = 80%</td>
</tr>
<tr>
<td>( t_{a/2} ) =</td>
</tr>
<tr>
<td>( n = 11 )</td>
</tr>
<tr>
<td>( n = 12 )</td>
</tr>
<tr>
<td>( n = 13 )</td>
</tr>
</tbody>
</table>

(Mendenhall 1969:345)

\[ \bar{Y} = 1.131 \]

\[ 1.131 \pm (2.179) \frac{3.8919}{\sqrt{13}} = 1.131 \pm 2.3520 \]

\[ \frac{3.8919}{3.6056} \]

\[ 1.131 + 2.3520 = 3.483 \]

\[ 1.131 - 2.3520 = -1.221 \]
This means that there is a 95% chance that the mean of the total population of differences (μ) will fall between -1.221 and 3.483. The mean of the total population of differences is the average number of years the ceramic mean date formula misses the historic median occupation date. The best estimate we have for this so far is 1.131 years (since this is the mean of the differences for the 13 cases). This number will vary some when more data is available but it should remain somewhere in the -1.221 to 3.483 bracket. We see then that the formula tends on the average, to overestimate the median historic occupation date by 1.131 years.

It is recommended, therefore, that to use the mean ceramic date for formula most accurately, Chinese Porcelain Types 26 and 39 not be used, and that the resultant date, on the average, will be one year greater than the median occupation date indicated by documentation. This is based on the present state of our knowledge, and may change as more ceramic formula dates are compared with well documented occupation dates.

Note: After the above figures were run the sample from Castle Hill, Newfoundland (eliminating Oriental Porcelain) was received, and it was discovered that Lyle Stone's two samples from Fort Michilimackinac, Michigan were inadvertently left out of the statistical treatment presented here. When these three samples are added to the 13 examples used here, \( \bar{Y} = 1.025 \), and the standard deviation is 3.49. This refines the mean of the population of differences from 1.131, to 1.025, the number of years the formula tends, on the average, to overestimate the median historic occupation date for the sixteen samples.

With this data in hand we can now say that there is a 95% chance that the mean of the total population of differences (μ) will fall between -.8343 and 2.8843. As stated above this bracket should include the average number of years the mean ceramic date misses the median occupation date, which is 1.025 years for the sixteen samples. Based on these fifteen examples then, the user of the mean ceramic date formula should subtract one year from the mean ceramic date derived without the inclusion of Types 26 and 39 (Oriental Porcelain).
### COMPARISON OF HISTORIC MEDIAN AND MEAN CERAMIC DATE MINUS 1.1 YEARS

#### SHOWING DIFFERENCE, MEAN, AND STANDARD DEVIATION

<table>
<thead>
<tr>
<th>Site</th>
<th>Median Historic Date</th>
<th>Mean Ceramic Date</th>
<th>CD-1.1-HD</th>
<th>(CD-1.1-HD)^2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Brunswick S7, N.C.</td>
<td>1755.0</td>
<td>1757.4</td>
<td>2.4</td>
<td>5.76</td>
</tr>
<tr>
<td>38AK4-15, S.C.</td>
<td>1731.5</td>
<td>1728.9</td>
<td>-2.6</td>
<td>6.76</td>
</tr>
<tr>
<td>38AK5-A, S.C.</td>
<td>1741.0</td>
<td>1745.4</td>
<td>4.4</td>
<td>19.36</td>
</tr>
<tr>
<td>38GN1-5, S.C.</td>
<td>1755.5</td>
<td>1753.5</td>
<td>-2.0</td>
<td>4.0</td>
</tr>
<tr>
<td>38PN1, S.C.</td>
<td>1760.5</td>
<td>1766.0</td>
<td>5.5</td>
<td>30.25</td>
</tr>
<tr>
<td>Brunswick S15, N.C.</td>
<td>1751.0</td>
<td>1749.6</td>
<td>-1.4</td>
<td>1.96</td>
</tr>
<tr>
<td>Brunswick NL, N.C.</td>
<td>1753.5</td>
<td>1750.9</td>
<td>-2.6</td>
<td>6.76</td>
</tr>
<tr>
<td>Brunswick S2, N.C.</td>
<td>1753.5</td>
<td>1749.3</td>
<td>-4.2</td>
<td>17.64</td>
</tr>
<tr>
<td>Brunswick S18, N.C.</td>
<td>1769.5</td>
<td>1776.8</td>
<td>7.3</td>
<td>53.29</td>
</tr>
<tr>
<td>Brunswick S10, N.C.</td>
<td>1803.0</td>
<td>1800.7</td>
<td>-2.3</td>
<td>5.29</td>
</tr>
<tr>
<td>Trebell, Va.</td>
<td>1797.0</td>
<td>1796.5</td>
<td>-1.5</td>
<td>.25</td>
</tr>
<tr>
<td>Tellico Blockhouse, Tenn.</td>
<td>1800.5</td>
<td>1801.7</td>
<td>1.2</td>
<td>1.44</td>
</tr>
<tr>
<td>Ft. Dobbs, N.C.</td>
<td>1759.5</td>
<td>1754.7</td>
<td>-4.8</td>
<td>23.04</td>
</tr>
<tr>
<td>SUM</td>
<td></td>
<td></td>
<td>0.4</td>
<td>172.20</td>
</tr>
</tbody>
</table>

#### Calculation

\[
\bar{y} = \frac{\sum y_i}{n}
\]

\[
S = \sqrt{\frac{\sum y_i^2 - \left(\frac{\sum y_i}{n}\right)^2}{n-1}}
\]

\[
\bar{y} = 13 \sqrt[13]{0.4}
\]

\[
S = \sqrt{\frac{172.20 - \left(\frac{0.4}{13}\right)^2}{12}}
\]

\[
S = \sqrt{14.348975}
\]

\[
s = 3.7880
\]

\[
s^2 = 14.3489
\]

From this data confidence factors can be determined for application when the mean ceramic date formula is used without Chinese Porcelain plus the adjustment of minus 1.1 years.

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Section Two

Centered around a Group of Papers on Pipe Stem Formulas
The purpose of this paper is to present a new regression formula for determining dates from kaolin clay pipestems.

Historical archeologists have been aware of the dating potential of pipestem borehole diameters since J. C. Harrington's observations in 1951 on the apparent regularity of change in borehole diameter over time.

In 1962 Lewis Binford produced a straight line regression formula from Harrington's original data; the 330 pipestem fragments from Jamestown, Williamsburg, and Fort Frederica. This formula supplied a researcher with a procedure by which he could supposedly produce a reliable date based on the pipestem bore diameter from his site (Binford 1962).

However, after the initial enthusiastic acceptance and application of Binford's formula, evidence began to appear which questioned the fit of the formula to the data. In her study of clay pipes at Williamsburg, Audrey Noël Hume demonstrated that the Binford formula was not applicable to samples dating earlier than 1670 or later than 1760 (Noël Hume 1963). Stanley South noted that there was a disconformity in pipestem dates derived with the Binford formula from the later ruins of Brunswick Town (South 1962). Binford himself noted that his correlation breaks down after about 1780 (Binford 1962:20).

In 1970, Lee Hanson also noted that there were discrepancies between the projected dates produced by the Binford formula and the time spans indicated by other evidence for some sites examined. Using Harrington's original data, Hanson proposed a series of straight line regression formulae which cover smaller segments of time. He felt that this method would allow the researcher to obtain a more valid date, and deviate less severely from the true relationship of the variables than a single regression line. Hanson also suggested that a curvilinear relationship might exist between the passage of time and pipestem bore diameters (Hanson 1971).

This suggestion prompted the co-authors of this paper to relook at the relationship of these two variables. Many persistent long-term movements or secular trends in the social and physical environments cannot be adequately approximated by a straight line. Quite often elements related to time, when graphed, will form nonlinear curves of either growth or decay type where change takes place steadily at either an increasing or declining rate.
To determine the best fitting curve for the relationship between borehole diameters and time, data from 14 sites, with a total of 26 stem samples were used. A mean date and a mean stemhole diameter for each sample was computed. Stemhole diameters were measured in 1/64" increments. The mean dates range from 1635 to 1775 and the mean borehole diameters range from .07/64th inches to .9/64th inches.

The sites from which the data was taken to produce the formula are listed in Figure 1. This study is limited to samples containing English clay pipes. Several other factors influenced our sample selection. First, the site had to contain a sufficiently large sample of pipestems, and second, it was necessary to select sites with a fairly reliable documented time range. Internal consistency in the site was another requirement. For example, the pipestem sample from Port Royal, Jamaica was not included because later, post-destruction material could have been introduced into this port situation with no means of control.

When the mean dates and borehole diameters were plotted on a graph, their relationship indicated a long-run tendency to decline in a geometric progression, or in the form of an exponential function, forming a second degree polynomial curve. The curve which best fits the data is popularly known as the Compound Interest Rate Curve. This is the form of the curve plotted on Figure 2. The general equation for this formula is \( Y = ab^x \). \( Y \) refers to the secular trend values, that is, the trend over time. The small "\( a \)" refers to the origin of the line, the small "\( b \)" to the slope, and the small "\( x \)" refers to the power to which the quantity "\( ab \)" must be raised. This equation is usually rewritten and solved in a general logarithmic statement, the standard form of which is

\[
\log Y = \log a + x(\log b)
\]

Our data were placed in seven classes with an inclusive range of 22 years in each class. This was done so that there would be at least one or more mean stemhole diameter values present within each class. An odd number of classes was also chosen to simplify the computation of the formula. These seven classes were given values of -3 through +3, with the year 1711 as the midpoint. (The point of origin.)

This resulted in the following computational equation:

\[
-\log Y + 1.04435 \\
\frac{x}{.05234}
\]

This equation must be used as the first step in figuring the date from a pipestem borehole diameter size. \( Y \) is the mean borehole diameter for the sample you wish to date, and \( \log Y \) is the logarithmic
conversion of this value.

After the general logarithmic equation stated above was solved for our data, the point of origin was mathematically shifted back from 1711 to 1600. This point of origin is used in a second computational formula. This formula must be used as the second step in the computation of a date. It uses the value X obtained from the first computational equation, and can be stated as follows:

\[ \text{date} = 1600 + 22X \]

X is a value you wish to date, expressed in terms of 22 year units, which resulted from our original analysis. It must then be multiplied by 22 and added to 1600, the origin date, in order to obtain the date for your particular borehole diameter size.

To solve for a date based on a stem sample, using this formula, three steps are necessary. First, the Y value (the mean borehole diameter for the sample) must be converted to its logarithmic form. Second, the first computational equation must be solved, using the logarithmic conversion of Y, and third, the second computational equation must be solved, using the X value from the first computational equation. The application of this formula to a documented, dated stem sample is demonstrated in Figure 3.

This set of equations allows you to figure out a date which is much closer to the trend indicated by the pattern of our data than does the Binford formula. In other words, this exponential curve gives a better fit to the data. This can be seen in Figure 2. The straight broken line is the prediction by the Binford formula. The curved line is from our formula, and it can be seen that it fits the data, which is represented by the small x's. Our curve handles the upper and lower distribution of data much better than does a straight line.

It should be emphasized that this is an exploratory study. It is a further probe into the use of pipestem borehole diameters as an aid in dating an historical site. Hopefully it will provide stimulus for further research and study, beginning with a careful reporting of the data pertaining to pipestems in site reports themselves. For data to be most informative and useful to the researcher, the published data for pipestems should include

1. The size of the stem sample.
2. The mean diameter of the boreholes of the sample.
3. The standard deviation of the borehole diameters.
4. The upper and lower diameter sizes of the stem holes found.
5. The upper and lower time span limits in which the sample would appear.

Most of this information could easily be conveyed in a table of frequency distribution of the borehole diameter sizes, accompanied by the mean and the standard deviation values for the borehole diameters.

As more firmly dated sites become available, the curve and the formula presented here will be modified. In this way, it will be possible to develop the model with the best predictive power.
FIGURE 1

SAMPLE USED IN THIS STUDY

<table>
<thead>
<tr>
<th>SITE</th>
<th>MEAN DATE</th>
<th>MEAN DIAMETER</th>
</tr>
</thead>
<tbody>
<tr>
<td>Williamsburg</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Noël Hume, A. 1963)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>* Coke Garret I</td>
<td>1757</td>
<td>4.62</td>
</tr>
<tr>
<td>* Post Office Well</td>
<td>1730</td>
<td>5.25</td>
</tr>
<tr>
<td>* Post Office II</td>
<td>1760</td>
<td>4.54</td>
</tr>
<tr>
<td>* Coke Garret Goal</td>
<td>1750</td>
<td>4.96</td>
</tr>
<tr>
<td>Clay Bank</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Noël Hume 1962)</td>
<td>1695</td>
<td>6.11</td>
</tr>
<tr>
<td>Tutter's Neck, Va.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Noël Hume 1966)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pit A</td>
<td>1706</td>
<td>5.82</td>
</tr>
<tr>
<td>Pit D</td>
<td>1735</td>
<td>5.04</td>
</tr>
<tr>
<td>Jamestown</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Cotter 1958)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Refuse Pit 1</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Zone A</td>
<td>1660</td>
<td>8.6</td>
</tr>
<tr>
<td>B</td>
<td>1635</td>
<td>8.4</td>
</tr>
<tr>
<td>C</td>
<td>1635</td>
<td>8.6</td>
</tr>
<tr>
<td>Ice Pit (Structure 128)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Zone A</td>
<td>1662</td>
<td>8.5</td>
</tr>
<tr>
<td>B</td>
<td>1657</td>
<td>8.5</td>
</tr>
<tr>
<td>C</td>
<td>1657</td>
<td>8.3</td>
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<tr>
<td>D</td>
<td>1643</td>
<td>8.4</td>
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<tr>
<td>E</td>
<td>1643</td>
<td>8.7</td>
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<tr>
<td>F</td>
<td>1643</td>
<td>8.9</td>
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Table 1 (cont'd)

<table>
<thead>
<tr>
<th>Site</th>
<th>Date</th>
<th>Mean Stemhole Diameter</th>
</tr>
</thead>
<tbody>
<tr>
<td>Silver Bluff, S.C.</td>
<td>1748</td>
<td>4.91</td>
</tr>
<tr>
<td>(Eaton 1962)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fort Frederica, Ga.</td>
<td>1743</td>
<td>4.91</td>
</tr>
<tr>
<td>(Hanson 1971)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Archer Cottage, Yorktown</td>
<td>1769</td>
<td>4.31</td>
</tr>
<tr>
<td>(Griffin in Hanson 1971:3)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fort Michilmacinack</td>
<td>1768</td>
<td>4.55</td>
</tr>
<tr>
<td>(Maxwell and Binford 1961)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>*Fort Michilmacinack (Barracks)</td>
<td>1775</td>
<td>4.07</td>
</tr>
<tr>
<td>(Binford 1962:20)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>*Warrasqueoi</td>
<td>1688</td>
<td>6.50</td>
</tr>
<tr>
<td>(Binford 1962:20)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Brunswick Town</td>
<td>1751</td>
<td>4.88</td>
</tr>
<tr>
<td>(South 1962)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>James River Site</td>
<td>1635</td>
<td>4.4</td>
</tr>
<tr>
<td>(Harrington 1954)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fort Necessity</td>
<td>1754</td>
<td>4.4</td>
</tr>
<tr>
<td>(Harrington 1954)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Spaulding's Lower Store</td>
<td>1770</td>
<td>4.63</td>
</tr>
<tr>
<td>(Lewis Ms)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* Mean stemhole diameters values were obtained by working the Binford Formula backward from the Binford-derived date given.
FIGURE 3
APPLICATION OF THE FORMULA TO THE SAMPLE FROM
SILVER BLUFF, SOUTH CAROLINA

Formula

\[ -\log Y + 1.04435 \]
\[ X = \frac{.5324}{.5324} \]

Y = mean stem hole diameter
\( \log Y \) = conversion to logarithmic form of Y

B - date = 1600 + 22x

x = value resulting from Formula A

Solving for a Date using data from Silver Bluff, S.C. (1716-1780)

mean stem hole diameter = 4.91 (Y)

Y = 4.91 \( \log Y = .6911 \)

\[ A \]
\[ X = -.6911 + 1.04435 \]
\[ .05324 \]

\[ X = 6.635 \]

\[ B \]
\[ Date = 1600 + 22X \]

Date = 1600 + 145.97

Date = 1745.97

For further explanation and discussion of statistical processes involved, refer to: Parl (1967)
HISTORICAL ARCHAEOLOGY FORUM - Heighton and Deagan

BIBLIOGRAPHY

BINFORD, LEWIS R.

COTTER, JOHN

EATON, JOHN

HANSON, LEE H.

HARRINGTON, J. C.


LEWIS, KENNETH

MAXWELL, MOREAU, and LEWIS BINFORD

NOËL HUME, AUDREY

NOËL HUME, IVOR

PARL, BORIS

SOUTH, STANLEY
THE "BINFORD" PIPE STEM FORMULA: A RETURN FROM THE GRAVE

Lewis R. Binford

A recent article by Lee H. Hanson, Jr. has raised a number of issues regarding the appropriate use to be made of a regression formula previously suggested by me as having some utility over an inspectional method of estimating mean dates for the period of accumulation for archaeologically known tobacco pipe stem fragments.

He states

1. The Binford assumption that pipe stem bore diameters follow a straight-line regression is erroneous,

2. No formula can give results more precise than the data on which it is based;

3. I believe there were statistical and mathematical errors made in the computation of the Binford formula (Hanson 1971:2).

I think I can clear up at least two of these problems by pointing out the calculation of my pipe stem formula was not based on Harrington's published percentages but on the original data used by Harrington in arriving at his percentages. Harrington in his published description grouped data which in many cases included specimens, and in numbers of cases sets of specimens, which could be dated more accurately than his grouped data. Some information was being lost by his methods. There was also a problem of accuracy involved in using his published percentages since the numbers of dated specimens were not the same in all of his period groupings. In addition I found on working with the original data that the accurately dated pieces were not evenly distributed with regard to the period dated pipes. Since the use of means of the median dates for the pipes grouped into time periods was possible it was believed a more accurate representation of the data than the median date for the grouped data published by Harrington (1954) in which this differential accuracy was ignored.

I obtained the actual data from Harrington in 1957 and my calculations were made that year. Since there had been some controversy over the validity of Harrington's observations (Chalkley 1955a and 1955b and Omwake 1958), I experimented with the formula applying it to every sample of materials I could find in an attempt to evaluate its utility. This was done largely between 1957 and 1961. In 1961 it became obvious to me that the formula was of surprising accuracy and of great utility. Applications to the material which Maxwell and I were getting at Fort Michilimackinac made it clear that the publication of the formula was inevitable. During the summer of 1959, J. C.
Harrington visited our excavations at the Fort and I discussed with him the formula. I suggested that the publication of the original data might be worthwhile. My feeling was the publication of the original observations would serve as a basic inventory of the data which could be added to in order to increase the accuracy of the approach as well as allowing a clarification of the sources of error inherent in his published summary. He agreed that it might be worthwhile although he never got around to publishing, and I presented the formula in the Fort report (Maxwell and Binford 1961:107-199). Since there was a sub-rosa knowledge of my formula among historic sites archaeologists I orally reported on it at the meetings in Macon and a transcript of this report was published (Binford 1962). At the time of the publication in the "Fort" report I was aware that in my moves between 1957 and 1961 the original data obtained from Harrington had been misplaced. In fact, I am not sure that I even made any attempt to save it at the time I calculated the formula since I was toying with Harrington’s observations simply trying to evaluate their utility as a possible research tool. It was not until my work on the Fort Michilimackinac data that I was convinced of the utility and economy of my formula.

Since my work on Fort Michilimackinac I have not been engaged in historic sites work and in a recent survey of the literature was appalled at what had been done with this research tool of demonstrated value.

On reading Hanson's (1971) paper I rushed to my files in hope that by some coincidence the piece of scratch paper on which I had copied Harrington's observations in 1957 could be found among some of my notes on historic materials. Clearly the publication of that data would clear up Hanson's worries regarding my abilities to add, subtract and multiply (all that is needed to calculate a regression formula). That miserable little piece of paper has regrettably been lost. I then called John Cotter in Philadelphia who gave me Pinky Harrington's address and telephone number in Richmond where he currently lives in retirement. I caught him home which is rare these days for he and his wife have been enjoying many extended trips to Europe and elsewhere. We talked of the situation and his comment went something like this, "Are they still fighting over the pipe stem business?" Harrington cannot find his original observations. Cotter suggested that Jelks may have them since we worked up the pipes for the Jamestown report or they may be filed with the records at Jamestown. In any event at this writing I have been unable to locate the original data and thus I cannot prove to Hanson that I know how to add, subtract or multiply, but I have some confidence in my abilities along these lines. At this point until someone is able to locate (possibly at Jamestown) the original data, we are stuck with my formula as the best summary of that data.
HISTORICAL ARCHAEOLOGY FORUM - Binford

With this as background I want to describe exactly how my formula was calculated and then go into discussion of Hanson's treatment of the published data.

Harrington had a sample of approximately 333 pipes which he felt was representing different periods of production. Some were assignable to periods of production at different levels of accuracy. Some were referable to a twenty year period while others were referable to a period with less accuracy (I recall some as great as fifty years, etc.). Harrington himself describes this situation thus: "The particular periods used in the study were chosen, not to produce attractive distributions, but because these were the periods for which I was able to select an adequate sampling of datable specimens. It is quite true that many bowls could be dated more closely than the time spans used for the study. For example, several of those in the first group (1620-1650) are quite definitely earlier than 1640, and some can be placed without hesitation at around 1620" (Harrington 1954:7). (Note in Table 1 the calculated mean for the median dates represented in this period sample is 1626.2, clearly showing that most specimens included were datable to the early part of the period and some actually to a time prior to the period summary as published by Harrington.) What I did was to take each pipe assigned to a period such as 1620-1650 and record the median date for the production estimate, for instance, if a pipe was identified to 1610-1630 a median date of 1620 was recorded. The sum of all the median dates for pipes making up a period sample was then calculated and this result was divided by "n" the number of pipes in the sample resulting in a mean production date for the specimens represented in the sample. This date was then used together with the mean bore diameter to calculate the regression formula published by me (1961, 1962).

In Hanson's recent study he made two unwarranted assumptions (a) that I had used the data as summarized by Harrington in his 1954 publication, and (b) that I had used the median date for the grouped data published by Harrington in calculating the formula. Given these assumptions, Hanson concludes

(a) The Binford assumption that pipe stem bore diameters follow a straight-line regression is erroneous... (Hanson 1971:2).

(b) The Binford formula is statistically inadequate and mathematically incorrect! (Hanson 1971:6).

Hanson then goes on to offer an interesting piece of conjectural history as to how my formula was produced. He writes

Since I could not calculate the Binford formula from the Harrington data, what follows is pure conjecture
TABLE 1

<table>
<thead>
<tr>
<th></th>
<th>#1</th>
<th>#2*</th>
<th>#3</th>
<th>#4</th>
<th>#5</th>
<th>#6</th>
<th>#7</th>
<th>#8</th>
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</tr>
</thead>
<tbody>
<tr>
<td>1750-1800</td>
<td>1775</td>
<td>1768.9</td>
<td>18.9</td>
<td>3.05</td>
<td>21.95</td>
<td>4.26</td>
<td>.50</td>
<td>2.00</td>
<td>7.31</td>
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<tr>
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<td>1730</td>
<td>1739.8</td>
<td>10.1</td>
<td>4.90</td>
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<td>1695</td>
<td>1700.8</td>
<td>9.2</td>
<td>2.90</td>
<td>12.10</td>
<td>6.04</td>
<td>.53</td>
<td>1.88</td>
<td>4.03</td>
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<td>1650-1680</td>
<td>1665</td>
<td>1661.4</td>
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<td>1620-1650</td>
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<td>6.2</td>
<td>4.20</td>
<td>10.40</td>
<td>7.99</td>
<td>.70</td>
<td>1.42</td>
<td>3.46</td>
<td>4.91</td>
</tr>
</tbody>
</table>

NOTES:

Column Number:

(1) The median date for the grouped data as published by Harrington.

(2) The mean of the median dates of the specimens grouped into the designated time period.

(3) Difference between the mean date and the range value closest to the mean.

(4) One half the difference between the mean and the median dates for the samples.

(5) Total of Columns #3 and #4.

(6) Mean bore diameters for the appropriate samples.

(7) Standard deviations of the means for bore diameters.

(8) The number of observed standard deviations represented in a reference standard deviation of one sixty-fourth of an inch.

(9) Value obtained by dividing the values in Column #5 by the number of standard deviations in a sigle-tailed curve (3).

(10) The number of years represented in each sample expressed with respect to a constant standard deviation of one sixty-fourth of an inch. These are the values plotted on Graph 1.

*My formula for calculating age estimates is \( Y = 1931.85 - 38.26(X) \) when \( Y \) is the estimate sought, and \( X \) is the calculated arithmetic mean of bore diameters in the sample to be evaluated.
on my part as to how that formula was derived. To suc­ceed, one must ignore all but the earliest and latest of Harrington's five time periods and make three mathe­matical errors. First, one has to use .026 as the change factor per year, which is attained by rounding off the mean bore diameters of the earliest and latest of Harrington's time periods to one decimal place in­stead of two. The actual change factor without rounding off would be .02664. Using .026 will give a change interval from one diameter to the next of 38.46 years which must be altered to read 38.26. Finally, the number of years to a theoretical zero, again using .026 is 163.85 which must be added to 1768 instead of the proper date of 1775 to arrive at the zero point of 1931.85 that Binford attained. Either he added the figure 156.85 to the median date or did not use the correct median date for computation (Hanson 1971:6).

This piece of conjecture must make other assumptions, namely that I did not know how to calculate a regression, and secondly that I am unable to round numbers and correctly add, etc. This is the most patent kind of insult and, in addition, supplies us with a very nice example of the kind of methodological difference separating historians from scientists. Hanson had two bodies of factual data, Harrington's published material and my formula. In order to account for observed discrepancies between the two he assumed a connection between the two in the past and then proceeded to offer a historical interpre­tation to account for the discrepancies. He never questioned his assumption, preferring instead to question my intelligence and integ­rity. A scientist being fully aware of the role of assumptions in any warranted argument is generally interested in their validity. Historians seem blissfully naive regarding their own thought processes and even defend this innocence by scorning theory and philosophical problems of epistemology (Walker 1970). Hanson may well protest at this point that I too am making an assumption, that he is a historian. I don't know what his training has been but he behaves like one.

Having gone this far, Hanson then proceeds to analyze Harrington's published data and present a series of formulas for the relationship between bore diameter and age which are said to be "statistically more valid and will prove more accurate" (Hanson 1971:6). It should be borne in mind that my calculations were based on arithmetic means for both bore diameters and the date of the population summarized into a given time range. Hanson's calculations are based on mean bore di­ameters and median dates for the time period in which the pipes were grouped. The alleged curvilineality which Hanson notes in Harrington's data is the result of the difference between the actual arithmetic mean of the median dates for the pipes grouped in the sample and the median date of the time period in which they were grouped. In short,
there is a bias in favor of specimens dated to the later part of the time periods summarized by Harrington as 1680-1710 and 1710-1750. If Hanson had been trying to accurately work with the data available to him he would have not treated median dates as mean dates. Instead the only procedure available to him in which he need not assume accuracy not in evidence would have been to use median date estimates for the pipes of different bore diameter and follow a procedure similar to South's (1972).

This brings us full circle, Hanson has treated Harrington's published percentages as if they were equally reliable samples of identical structure with regard to the time periods summarized. They were not. He has alleged that the actual relationship between the median dates and the mean bore diameters is a curvilinear relationship. It is not unless one ignores the sampling error inherent in Harrington's data. Hanson has calculated a number of lines making use of various combinations of the five described "samples" which are alleged to be more accurate than a single regression summary of the data. Each of these lines is simply describing differences which arose as a result of numerical bias in Harrington's grouped data. This is illusionary accuracy gained by trying to summarize data in more precise ways than "the data on which it is based." Hanson should have written me regarding his questions about the data and my summary of it before engaging in "conjectural history" based on a false assumption.

If Hanson has erred regarding his own warning about the naive stretching of the data, I have certainly erred regarding the publication of the data upon which my descriptive summary was based. At the time the formula was calculated it was done as a personal expedient for testing the validity of Harrington's observations. Later it became clear that it was of general utility. By that time I had misplaced the original data and it appears that at the present time the original data is no longer available. For this I apologize. My short publications on the formula are clearly ambiguous as to exactly how the formula was calculated. Yet in no place do I refer to the use of a median date but always to the means calculated for both variables. I also state, "This I was able to do by using Harrington's original percentages and converting them to mean hole diameters for the given time period" (Binford 1962:19). I might further clarify this by pointing out the form in which I obtained the data from Harrington. He had tabulated the pipes numerically by thick marks for the estimated periods of manufacture and then drawn lines between this array thereby establishing his time periods. From this tabulation I was able to calculate the mean date of the summed medians for the pipes included in any given time grouping. The bore diameters were not tabulated on this array but were obtained in the form of percentages for the various bore diameters represented in each time grouping. Knowing the actual number of pipes in each sample I was able to reconvert these percentages back to counts and calculate the
actual mean bore diameter.

I must conclude at this point that my published formula is the only accurate summary of the control data used in Harrington's work. Hanson's reanalysis and its alleged greater accuracy is referable solely to discrepancies between the character of the original data and the form in which Harrington summarized it.

I turn now to a more important issue, that of expanding the utility of the technique as a research tool, estimating the time elapsed during the accumulation of the sample. I originally suggested that descriptive statistics regarding the character of archaeologically known samples might be utilized to offer an estimate as to the duration of the period of sample accumulation (Binford 1962). Hanson attempts to implement this suggestion but I fear from a very uninformed position. He suggests that, "It must be borne in mind that this second point (the use of standard deviations in calculating the time ranges of the samples) is based on the premise that all cultural phenomena are constantly changing...and that they each have an increase, a peak, and a decline in popularity through time" (Hanson 1971:11). In the case of the pipe stem data this is sheer nonsense. Harrington's data provides us with a model empirical population. We need make no assumptions about the character of culture change, all that is needed is a demonstration that the use of this model population as a standard for comparison is productive of information and a useful research tool for the recognition of variability in the archaeological record. In order to convert this empirical population into a useful model population we need but adequately describe that population. With regard to the relation between changes in bore diameter and elapsed time I have done that task. With regard to the form of the model population variability I have not, because at the time that I recorded the original data I did not calculate standard deviations for the variation in the median dates of the pipes grouped by time period. In order to estimate the elapsed time represented in a sample per standard deviation we must know several facts: (a) the relationship between measures of dispersion for both elapsed time and bore diameter in the model population and (b) the difference between the observed population and the model population. Given this information we may estimate the elapsed time during the accumulation of the determinants unknown sample by assuming the operation of common determinants. The best description of the model population demands that we know (a) the standard deviation of both variables being considered. In this case the standard deviation of bore diameter can be directly calculated but the standard deviations of the control samples cannot be calculated for year periods. Thus we must devise some method of obtaining an estimate of this value which can be defended as the best approximation of the real value currently obtainable. Hanson approached this problem as follows:
The principle on which this is based is that one standard deviation from the mean will encompass 68% of the sample on a normal curve. Two standard deviations will encompass 95% and three will encompass 99%. I consider two standard deviations to be sufficiently accurate for our purposes because of the slow rate of change. To convert two standard deviations of the mean bore diameter into a time span for the sample we must multiply them by one-half the average time span of the samples used in computing the formula (Hanson 1971:10-11).

There are several confusions here: (1) Hanson's discussion of the properties of the normal curve are relevant to evaluating the probabilities of a measured mean being representative of the population from which the sample was drawn, but are not relevant to an accurate description of the control population where the variability is only adequately summarized by three standard deviations. (2) Hanson assumes that variability in the control population is sampling error and therefore best summarized by the mean of that variability for any given combination of control samples included in the calculation of a mean date (his separate formulas). I will prove that the variability in the control population is patterned and describes a well-defined time trend, thus obviating the latter assumption. The former suggestion, that of using two standard deviations, is based on a confusion as to what one is doing, e.g. accurately describing a control population or evaluating the probabilities of the accuracy of a mean in that or any other population.

How can we evaluate the standard deviation of the means for the distribution of temporal medians included in the control samples when those data are no longer available? Several facts need be introduced at this time: (1) The control samples were certainly skewed in their distribution of temporal medians. I recall this fact and Harrington (1954:2) himself describes it. (2) In a skewed population the mean is affected by extreme values drawing it away from the mode. In the samples the degree that the mean value is plus or minus the median for the range is some indication of the direction in which the mode is located and hence a clue to the direction in which extreme values may be located (e.g., in the opposite direction of the position of the mean with regard to the median). The best estimate for one-half the time span represented in the control samples is therefore the difference between the mean for each sample and the range value closest to the mean. We may obtain this estimate by subtracting either range value from the mean or the mean from the range value, depending upon which side of the median the mean is observed to fall. This value is given in Column #3, Table 1. Since it is recognized that the actual samples were skewed and that there must have been some overlap between the time groupings, I have added to this value one-half the difference between the median and the mean (values given in Column #4, Table 1).
The reason for this step is complicated in that the known means sometimes diverge so much from the medians of the groupings that the actual time range of the samples in some cases must have been greater at one end than the grouped data would indicate. In my judgement the value suggested is the most appropriate estimate of this unknown. The sum of Columns #3 and #4 is then taken as the best estimate of one-half the time represented by each control sample (Column #5). This value is then divided by three, the number of standard deviations in one-half a normal course, giving us the number of years represented by the standard deviation of the control sample (Column #9). Having arrived at these data it remains to relate this variability to that in the hole diameters of pipes. Inspection of Table 1, Columns #7 and #9 demonstrates that there is not a simple linear or constant relationship between standard deviation in hole diameters and elapsed time, a condition assumed by Hanson. In order to describe accurately these relationships and at the same time produce a usable procedure for investigating unknown samples I have chosen to convert the information contained in Columns #7 and #9 into a single distribution which expresses the number of elapsed years with respect to a constant standard deviation in hole diameters of unity or one-sixty-fourth of an inch. This is accomplished by dividing the observed standard deviations (Column #7) into the value of one which then gives us the number of observed standard deviations included in a reference standard deviation of one-sixty-fourth. The resulting value (Column #8) is then multiplied by the corresponding number of years in one-half of each sample's temporal span (Column #9). This gives us the number of years represented in each sample by a constant standard deviation of one-sixty-fourth. (see Column #10). These values were then plotted on Graph #1 demonstrating the accelerating curvilinear relationship between the standard deviations for both bore diameter and time duration represented by each sample. The reader will note that I have added a plunging line at the end of the curve. This is necessary since the date, based on Harrington's data, when we could expect a mean bore diameter of 4.00 is 1778.81 and specimens of less than four-sixty-fourths are generally unknown. Therefore, the standard deviation must plunge to zero at that time.

Using the Procedure

In order to calculate the elapsed time represented by an unknown sample you first calculate the mean date using my formula. With this date you enter Graph #1 and read off the value at the intersection of the date line with the graph line. This is the expected elapsed years for one standard deviation of unity. You then multiply this value by the observed standard deviation for the sample you wish to evaluate and multiply the result by three (the number of standard deviations in one-half of a normal curve). The result is the best estimate of the number of years to be added to and subtracted from the mean pipe stem date of
the sample you are evaluating.

Contrast with Hanson's Methods

The procedure suggested by Hanson did not take into account the complex relationship between standard deviations of bore diameter and of elapsed time. His procedure recommends that the observed standard deviation be doubled and multiplied by the mean of two-thirds of the time ranges represented in the calculation of his various formulas. First, there are no constant relationships between the range and the temporal dispersion of the samples, and secondly the multiplication of his recommended measures of temporal dispersion by the observed standard deviation of bore diameters assumes a constant of unity for the standard deviations of bore diameters which he has not calculated. Clearly not a single one of the observed standard deviations in the control sample reaches unity (see Table 1, Column #7). Any accuracy obtained will only occur when his two errors tend to cancel each other out. That is where an observed standard deviation of bore diameter is small which when multiplied by his recommended temporal dispersion values reduces this value considerably because of the lack of any built-in description of the real relationships between temporal dispersion of the samples and size dispersion in bore diameters. In turn when observed standard deviations are large, greater than one, the temporal duration range will far exceed the accurately described duration because he assumes unit standard deviations in the control sample and they are obviously not so distributed (see again Column #7, Table 1). For these reasons we may justifiably dismiss Hanson's suggestions.

Evaluating the Degree to which Intervening Determinants have Operated on the Samples being Evaluated

The control population provides us with an approximation of the character of change through time in (a) mean bore diameters (b) variability in bore diameters per unit of time and (c) changes in the form of populations through time. The latter property of the control population is measured by the degree that there are changes through time in the control samples with regard to the shape and form of the distribution which they describe. One important property of these distributions is the degree that they differ from an ideal normal curve in their degree of skew or their divergence from the ideal in which the mean, mode, and median are all the same value. Since the actual data are needed to calculate the second and third moment values of the samples the best measure of skew is provided by a comparison of the means with the modes of bore diameters. The conventional methods of expressing this relationship is in standard deviation units. To obtain this measure of skew we calculate the difference between the mean and the
mode (see Table 2), Columns #1 and #2 and divide the result, Column #3, by the standard deviation, Column #4. The result, Column #5, expresses the degree of skew as a percentage of the standard deviation. The direction of skew is expressed by plus or minus signs depending upon the position of the mode with respect to the mean. The values given in Column #5, Table 2, demonstrate that the control population exhibits classic normal curves up until approximately 1740, at which time rapid change in the direction of a marked skew to the right (positive) is observed.

### Table 2

<table>
<thead>
<tr>
<th>Published Range of Sample</th>
<th>#1 Sample Mean</th>
<th>#2 Sample Mode</th>
<th>#3 Difference</th>
<th>#4 Standard Deviation</th>
<th>#5 Skew</th>
</tr>
</thead>
<tbody>
<tr>
<td>1750-1800</td>
<td>4.26</td>
<td>4.00</td>
<td>+.26</td>
<td>.50</td>
<td>+52.00%</td>
</tr>
<tr>
<td>1710-1750</td>
<td>5.02</td>
<td>5.00</td>
<td>+.02</td>
<td>.53</td>
<td>+ 0.03%</td>
</tr>
<tr>
<td>1680-1710</td>
<td>6.04</td>
<td>6.00</td>
<td>+.04</td>
<td>.53</td>
<td>+ 0.07%</td>
</tr>
<tr>
<td>1650-1680</td>
<td>7.07</td>
<td>7.00</td>
<td>+.07</td>
<td>.72</td>
<td>+ 0.09%</td>
</tr>
<tr>
<td>1620-1650</td>
<td>7.99</td>
<td>8.00</td>
<td>-.01</td>
<td>.70</td>
<td>- 0.01%</td>
</tr>
</tbody>
</table>

I have elected to discuss this property of the central samples since I fear someone will observe the apparent regularity in Harrington's published data and make unwarranted interpretive suggestions. It so happens that for all samples except the last one summarized by Harrington the mean value of the bore diameters is very close to a unit measure of bore diameter, 8, 7, 6, and 5/64 of an inch. When the mean is essentially identical with one of the units of measure we can anticipate that the mean and the mode will be the same and therefore the curve will exhibit little skew and conform well to an ideal normal curve. However, in any series where the mean is shifting because of changing proportional relationships between values, this change is manifest by a regular shifting of the form of the curve which generally describes a classic sine curve. We are measuring bore diameters with an ordinal scale and all cases are tabulated as members of a class interval 7, 8, 9, etc., 64ths, so that any change in mode will be from one class interval to the next. On the other hand the arithmetic mean is continuously distributed occurring in such forms as 4.65 or 5.23, etc. The convention of expressing skew either positively or negatively with regard to the relative positions...
of mean and mode insures in this case that measures of skew will exhibit sine curve-like distributions through time. As the mean approaches fifty percent of the mode we can anticipate a change in the mode to the next higher or lower interval as the case may be. These expectations are graphically presented in Graph #2. The scale is for the mean values of samples and our expectations regarding skew are plotted graphically with respect to positive and negative values of the mean with respect to the mode. As an example the interval between a mean of 5.00 and 6.00 we see that with a mean of 5.00 skew is zero since we anticipate the mode and mean to be near identity. As the value of the mean increases toward 5.50 positive skew increases since we anticipate the mode to remain 5.00 until the mean reaches approximately 5.50. At that point the curve plunges to the negative side since we anticipate a shift of the mode to a value of 6.00 meaning that the mean will be less than the mode and the distribution will be negatively skewed proportionately. It should be clear that the observed values of skew in the control samples published by Harrington are deceiving (see Table 2, Column #5). This results from the fact that most of his samples exhibited means close to a unit value for measuring bore diameters. The apparent unique character of the last sample is seen to derive solely from its uniqueness in the published array with regard to the mean-mode position and is not a feature characteristic of populations in the late period. We can expect samples to exhibit similar skew characteristics throughout the time span of the correlation between bore diameter and time.

In order to evaluate the degree that an unknown sample differs from expectations derived from the control sample the following rule of thumb is offered: If the mean is greater than a unit number (4, 5 6, etc., 64ths) and less than one-half the value to the next higher number you will obtain a positive skew which will be in the expected range of the control populations. Similarly if the mean is greater than one-half the distance between units (5.5, 6.5, 7.5, etc.) and the mode is the next highest unit (6, 7, 8, etc., 64ths), you will obtain a negative skew which would be within the range of the control population. On the other hand, if a mean is the lower half of an interval (less than 5.5, 6.5, etc.) and the mode is at the upper end then you may be viewing a sample skewed more than the control population. This is true also for the reverse situation. The indications of skew are less likely to be significant if the mean is very near the half-way point in the interval. Graph 2 may be used to help evaluate this situation. Enter the graph with the observed mean of the sample and the intersection of the mean line with the graph is the expected value plus or minus the mode.

It should be pointed out that the measure of skew discussed here is very crude and some potentially useful information is almost certainly masked by the use of measures as large in proportion to the
total range of variability as 64ths of an inch. If it should become necessary to investigate this property (skew) more accurately the researcher may choose to use smaller measuring increments. This would present no problems in using the procedures outlined since measures taken with 128ths could be easily employed with my formula by simply dividing the value of "b" by one-half and then proceeding as usual. Similarly standard deviations calculated using 128ths of an inch increment could be easily converted for purposes of comparison to other samples reported in 64ths by dividing the observed standard deviation using 128ths by two. I feel that such a procedure might well be desirable because of the information potential inherent in the comparative study of skew.

The potential information which these evaluations may enlighten are numerous. We might expect a negative skew to appear in samples covering considerable periods of time if the rates of deposition for pipe stems had changed considerably over the time represented by the sample. If the population had been increasing at a fairly rapid rate we might expect a negative skew. On the other hand, if a settlement was being gradually abandoned we might expect a positive skew in the pipe population. Differences in logistical efficiency and the retention in the population of items manufactured at much earlier dates might affect the degree of skew observed in a sample. Other factors such as mechanical mixing of deposits might be expected to affect this property of the sample. When we observe significant skew in an archaeological sample we may immediately suspect the operation of some determinants such as those suggested above. Investigation of these contrasts between archaeological samples and the control population may be productive of information regarding the past.

Multimodality of Observed Samples

As can be seen from the data on the control samples they are all unimodal and for the period prior to 1750 almost classic standard normal curves. When archaeological samples are observed which do not meet these expectations then we may immediately expect the operation of determinants independent of those expressed in the control population. A good example is provided by the data reported by Deetz (1960) from the Joseph Howland Site at Rocky Nook, Massachusetts, where the distribution of bore diameters was observed to exhibit two modes. This situation is clear evidence that we may reasonably expect two occupations with some interruption in deposition of pipe stems between them. Such an interpretation is upheld by other evidence (Deetz 1960:8,10).

I hope that the role of an empirical control population in the investigation of unknown samples has now been made quite explicit. I hope also that these three dimensions of comparison will be utilized as
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a standard against which to evaluate observed variability in the archaelogical record in a search for its significance to the past.

A Demonstration

As a demonstration of the application of my recommended procedures and a comparison with the methods suggested by Hanson, I have prepared Table 3 which summarizes data from the Joseph Howland Site and Fort Michilimackinac (see page 246). We are now in a position to make some interesting observations. In the Joseph Howland sample the only known date is for the beginning of the sample. My estimate is closest with a discrepancy of 0.09 years! Hanson's formula #4 comes close to my mean estimate but misses the documented beginning date by 17.10 years. In the second sample only the terminal date is known and again my estimate is closest missing the known date by 0.16 years while again Hanson's mean is close to mine but misses the known date by 4.71 years. In sample three again only the terminal date is known and my estimate misses by 0.80 years while Hanson is close with the mean but misses the known date by 1.07 years. Samples #4 and #5 are known for both beginning and terminal dates so that we may check both means and sample span estimates. The mean date for #4 is 1774.95 and my estimate is 1773.10, a difference of 1.85 years, while Hanson misses by 8.30 years. With regard to the estimates for beginning and terminal dates my results are consistent, being 3.12 years too early and 0.32 years too late for the terminal date. Hanson misses the beginning date by 2.80 years and the terminal date by 16.6 years. This comparison demonstrates quite well the difference between my methods for estimating and duration of the sample and Hanson's, namely that Hanson's estimates are somewhat independent of his mean estimates while my methods demonstrate an internal consistency with variability referable almost exclusively to the difference between the known means and the estimated means. The final comparison is further demonstration of the internal consistency of my methods. My mean estimate is 1.28 years off with 1.88 years and 4.44 years of discrepancy for the initial and terminal dates respectively—all estimates being within the range of the known dates. Hanson misses the mean date by 7.66 years, the terminal date by 7.56 years and the initial date by 7.76 years. Both his mean and terminal estimates are outside of the known range of the sample. In these nine comparisons Hanson's methods show a cumulative discrepancy of 58.62 years with an average discrepancy of 6.51 years per comparison. My methods resulted in an accumulation of only 13.94 years of discrepancy with an average discrepancy of 1.54 years per comparison.

I hope the comparisons presented here will serve to emphasize the point that Hanson's methods are less accurate and based on less accurate information than are mine.

The purpose of these comparisons is not, however, to prove the
TABLE 3

<table>
<thead>
<tr>
<th>Samples (See Notes)</th>
<th>#1</th>
<th>#2</th>
<th>#3</th>
<th>#4</th>
<th>#5</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) Data</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(a) Mean</td>
<td>5.87</td>
<td>4.55</td>
<td>4.37</td>
<td>4.15</td>
<td>4.12</td>
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<tr>
<td>(b) St. deviation</td>
<td>1.33</td>
<td>.62</td>
<td>.15</td>
<td>.36</td>
<td>.12</td>
</tr>
<tr>
<td>(c) Mode</td>
<td>5.00</td>
<td>5.00</td>
<td>4.00</td>
<td>4.00</td>
<td>4.00</td>
</tr>
<tr>
<td>(d) Skew (ob)</td>
<td>65.41</td>
<td></td>
<td>41.66</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(e) Skew expected</td>
<td>-9.70</td>
<td></td>
<td>41.66</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2) Initial est.</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(a) Known date</td>
<td>1676.00</td>
<td></td>
<td>1768.00</td>
<td>1770.00</td>
<td></td>
</tr>
<tr>
<td>(b) Binford</td>
<td>1676.09</td>
<td>1734.70</td>
<td>1758.50</td>
<td>1764.88</td>
<td>1771.88</td>
</tr>
<tr>
<td>(c) Hanson #4</td>
<td>1658.90</td>
<td></td>
<td>1729.91</td>
<td>1757.03</td>
<td>1765.20</td>
</tr>
<tr>
<td>(d) Hanson #10</td>
<td></td>
<td>1729.91</td>
<td>1757.03</td>
<td>1765.20</td>
<td>1777.76</td>
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<tr>
<td>(3) Mean estimate</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(a) Known date</td>
<td></td>
<td>1707.20</td>
<td>1757.77</td>
<td>1764.65</td>
<td>1773.10</td>
</tr>
<tr>
<td>(b) Binford</td>
<td></td>
<td>1702.15</td>
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<td></td>
</tr>
<tr>
<td>(c) Hanson #4</td>
<td></td>
<td>1702.15</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(d) Hanson #10</td>
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<td></td>
<td></td>
<td></td>
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<tr>
<td>(4) Terminal estimate</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>(a) Known date</td>
<td></td>
<td></td>
<td>1781.00</td>
<td>1770.00</td>
<td>1781.0</td>
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<tr>
<td>(b) Binford</td>
<td></td>
<td>1738.31</td>
<td>1780.84</td>
<td>1770.80</td>
<td>1781.32</td>
</tr>
<tr>
<td>(c) Hanson #4</td>
<td></td>
<td>1745.37</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(d) Hanson #10</td>
<td></td>
<td></td>
<td>1785.71</td>
<td>1771.07</td>
<td>1797.6</td>
</tr>
</tbody>
</table>

NOTES:

#1 This sample is the grouped data presented by Deetz as two separate samples from the Joseph Howland site, Rocky Nook, Massachusetts (Deetz 1960:9).

#2 This is the total of kaolin pipes recovered from Fort Michilimackinac in the 1959 season (Maxwell and Binford 1961:108).

#3 This is a sample sealed below the soldiers barracks (deposited during its construction) Fort Michilimackinac (Maxwell and Binford 1961:74).

#4 This is a sample from a closet west of the south fireplace. This could only have accumulated during construction and use (Maxwell and Binford 1961:76).

#5 This is a sample from the south fireplace accumulated during occupation (Maxwell and Binford 1961:75).
accuracy of my methods since it should be clear that they would have to be, given the differences between Hanson's and my methods in generating the recommended procedure for analysis. More important is the clarification of some points raised by Hanson regarding the limitations of the methods themselves. Hanson has alleged that in order to use the formula and the analytical procedure suggested, "we must demonstrate that the samples have a normal curve in order to use the means in calculating the formulas and the standard deviations in calculating the time ranges of the samples" (Hanson 1971:11). This is incorrect and most certainly derives from a poor understanding of the use of a reference or control population. The data summarized by my formula are a set of reference data, which can serve the investigator as a standard for comparison with unknown materials. Hanson seems to feel that one cannot make such comparisons and can only work with materials which are identical to an ideal population. As I have shown the control population itself can be expected to diverge markedly from an ideal normal distribution regularly at intervals of every thirty-eight years. Following Hanson's suggestions we would only be able to use samples whose means were close to unity. We would have to discard samples whose means fell around the half-way point between unit measures of bore diameter! More importantly the suggestion ignores the role which control populations may serve in science. Understanding, or clues to understanding regarding the past, derives directly from our abilities to explain differences which may be noted between archaeological samples and, in this case, the control population. If we demand that the archaeological record be identical with an ideal model which in this case is not realized in the control population, we will most certainly end up discarding most of the information in the archaeological record. Hanson implies that the standard deviation is not an adequate measure of dispersion and the mean not an adequate measure of central tendency if the sample is not an ideal normal curve! We may clearly calculate these values for samples which are no more divergent from the ideal than bi-modal distributions. The Joseph Howland sample (#1) in Table 3 is such a sample. As can be seen from the demonstration, the form of the sample has very little to do with the accuracy of the estimation of its duration using standard deviations. In highly skewed samples some accuracy may be lost since the mean is affected by extreme values. We can anticipate a tendency for the estimates to be slightly inaccurate in the direction of the skew. This does not prevent us from using those samples, and more importantly, it demands that we attempt to explain them. The properties of bi-modality and skew which exceed our expectations, arising as a product of the dynamics of change, are properties which must arise through the operation of determinants other than the dynamics of the production of pipes and the variability arising from that production. Factors such as these are our sources of information regarding the archaeological record and the past. Since our interest is in the site we are studying, then the degree to which we can explain such properties is directly proportional to the degree that we will learn from the archaeological
record about the past and the role of the site under study in the events of the past. Such divergencies in our data from an ideal form do not preclude the use of the formula and the standard deviations as a way of estimating mean dates and elapsed time. Furthermore, they are potentially the most informative cases regarding the past.

What are the limitations of the use of this technique? As far as I can see there are none when the technique is properly applied. Hanson appears to feel that the limitations are numerous. He makes much of the fact that one is evaluating samples of pipe stems and not sites. It should be perfectly clear that the control population is of pipe stems and not sites. The central problem of any archaeological investigation is the demonstration of patterns of co-variation and association. To point out that information gained from pipes is not generalizable to the site or any other unit unless proper association and correlation is forthcoming is to point to a general archaeological problem which is not specific to the use of this technique or any other for that matter.

Aside from the above confusion Hanson emphasizes the fact that as a dating technique the present model may be inaccurate. In fact he suggests that it is generally inaccurate. In this opinion I differ with Hanson, and the demonstrations provided in Table 2 should be sufficient on that point. I do not imply that cases of inaccuracy will not be found. They most certainly will since the distribution and association of pipes in archaeological sites is not just a function of the variability in pipes produced at a given time. Fortunately for archaeologists the patterns of co-variation and association remaining in the archaeological record bear some relationship to the patterns of behavior of men other than producers. The logistics networks, the users, and the processes of "times error" all affect these associations. Nevertheless Hanson seems to be demanding that we ignore these sources of information and strive for more accuracy in the definition of a control population.

...the formulas have the potential for refinement with new data. This alone makes it worth our while to continue using the formulas despite their present limitations (Hanson 1971:11).

We desperately need better-documented samples to replace Harrington's percentages so that we can study the regression curve in greater detail (Hanson 1971:12).

...a number of documented samples may enable us in time to abandon this method entirely in favor of some other method better-suited... (Hanson 1971:7).

I wonder what "documented samples" Hanson would like to have.
Does he want a truly representative sample of all the pipes produced in Europe during the relevant 200 years? I wonder how that would increase our accuracy? Certainly all the pipe makers were not equally and randomly contributing to a "pipe pool" which was then randomly redistributed in North America. Many factors were certainly at work which would tend to bias the pipes present in one area in favor of some manufacturers. I need only cite Walker's evidence for higher frequencies of Dutch made pipes in the Northeast (Walker 1965).

Logistics networks certainly changed as did the productivity of different producers through time and these changes may well be correlated. How could "increasing the accuracy" of the formula without a knowledge of the factors which would affect the samples to be dated by that formula possibly increase our historical accuracy? Only the use of techniques which are dependent upon the operation of processes which in the past were independent of the activities of men can be used with confidence that the results obtained are independent of those human activities. No amount of work attempting to increase the descriptive accuracy of the control population will insure that the target population (archaeologically known samples) will be identical to that population.

I suggest that we have a control population which is drawn from pipes present in North America, particularly the Virginia area. Numerous tests have demonstrated that the use of that population as a reference generally yields internally consistent results and the accuracy of those results is frequently at a level generally unknown in archaeological work. For example, if I had known the month during which the soldiers' barracks at Fort Michilimackinac were occupied, my discrepancies would probably have been reduced. What we need to do is begin the fascinating study of the distribution, temporally and spatially, of discrepancies between the age estimates given by the pipe stem formula and historically documented contexts as well as correlations with other classes of artifacts. Only in this way will we be successful in demonstrating the character of the determinants operative on the distribution of pipes in North America. Such determinants were certainly operative and they insure that the archaeological record will not be isomorphic with an undifferentiated universe of pipes viewed as they came out of the producers' doors. I fully expect that such discrepancies will cluster spatially with regard to the territories supplied from different ports and through different logistical contacts. I also expect discrepancies will vary with the logistical efficiency such that greater differences between pipe stem estimates and known dates will be shown to cluster during the initial periods of the establishment of settlements. I am suggesting that we do some archaeological research and analysis. Work is needed at the explanatory end of the procedure. No amount of refinement in the control population will obviate the need for or the desirability of purely archaeological analysis aimed at explaining variability in the archaeological record itself. Progress at this end of the research
continuum is what increases the utility for archaeology of such a tool as the pipe stem formula, which I published ten years ago. Such work is dependent upon a wide body of comparative data. We need greater coverage before valuable comparative studies can be made, and so I urge historic sites archaeologists to use the formula, describe their samples, giving means, standard deviations, and measures of skew even if they are not interested in obtaining dates.

Such work as Hanson's betrays an enormous weakness in the field of archaeology. It appears at almost every turn and it is very obvious in the current literature of historic sites work. His insistence on increasing the "accuracy" of the model population as a means to increasing the "accuracy" of results obtained from its use completely ignores the information inherent in the archaeological record, namely the factors which most certainly operated to differentiate associationally and co-variantly archaeological materials from the total population of pipes produced during the 200 year span of the demonstrated correlation. Much of Dollar's (1971) recent "clarification" of the character of archaeological data is of the same order. His discussion of statistics ignores completely the relevance of recognizing internal variability within a population and how it may be correlated. At least Dollar recognizes that such internal variability exists, but Hanson seems to think that if we knew accurately the character of all the pipes produced we would be able to accurately date all archaeological samples. This denies to archaeology the basic properties of organized variability in the archaeological record upon which it is dependent for generating knowledge about the past. Similarly Dollar's insistence that since we rarely know the exact parameters of the total population of variability represented at a site, which if known would generally only allow us to assert that an observed sample could have been drawn from that population (a fact we knew already since we in fact drew the sample) we cannot go about the archaeological task of investigating the character of differential patterns of co-variation and association. This is to say that we cannot do archaeological science. Instead, Dollar suggests a rather poor analogy between the archaeological record and "primary" and "secondary" historical sources. This is the historians' approach, looking for specific facts torn out of their organizational context and selectively used for interpretative arguments imposed on the data. The scientist is interested in the organizational properties of the external world, in this case the archaeological record and its contents. We seek to understand it, and the formation processes standing behind it, and thereby gain a better understanding of the past. All facts of the archaeological record are accurate with regard to some questions regarding the past. It is only when we establish subjective priorities as to what is important (I must say I disagree strongly with Hanson and Dollar in their implicit priorities) that we begin the distorting analytical game of selectively searching for certain facts to fit our priorities.
Archaeological data are not aggregates of undifferentiated potential observations like red and white balls in a bag, they are components in an organization derived from the organized patterns of human behavior and the actions of natural processes. Investigation of the organizational properties of the archaeological record is the archaeologist's job. The citation of all the organizational conditions which might have operated in the past to distort the archaeological record from the true picture should only inspire archaeologists to proceed as scientists to the investigation of those organizational characteristics. Dollar and Hanson appear depressed by these conditions and feel that they militate against the use of archaeological facts except in a very selective way.

I hope that this exposition of tobacco pipe stems will help to demonstrate some of the differences between the approaches of scientists and those not so committed.
HISTORICAL ARCHAEOLOGY FORUM - Binford

BIBLIOGRAPHY

BINFORD, LEWIS R.

CHALKLEY, J. F.


DEETZ, JAMES

DOLLAR, CLYDE D.

HANSON, LEE H., JR.

HARRINGTON, J. C.

MAXWELL, MOREAU S. and LEWIS H. (sic) R. BINFORD
1961 Excavation at Fort Michilimackinac, Mackinaw City, Michigan 1955 Season Michigan State University Cultural Series 1, No. 1 (whole number).

OMWAKE, H. G.
SOUTH, STANLEY
1972 Evolution and Horizon as Revealed in Ceramic Analysis in Historical Archaeology. The Conference on Historic Site Archaeology Papers, 1972 6.

WALKER, I. C.

I appreciate the opportunity to comment on the two articles generated by my paper, but first I want to publicly apologize to Lewis Binford for impugning his intelligence in the method by which he devised his formula. The evidence he presents makes it perfectly clear that my own formulas are based on an unwarranted assumption and can therefore be dismissed as formulas from further consideration. Since they were unwieldy to use, particularly the part dealing with standard deviations, I won't shed a tear to their passing. However, Binford raises several points which I want to comment on further.

Binford criticized me for being too engrossed in "accuracy" and formulas for their own sake. This is not true but one would get that impression from reading my paper and so the blame lies with me. Since the Binford formula was first published 10 years ago we should have built up a body of comparative data by now that would have provided us with the means to make the studies of cultural processes he advocates (Binford 1972:17). To the best of my knowledge, only Binford and Iain Walker have published any effort to explain discrepancies in the expected dates in terms of cultural processes. Everyone else has been content to use the formula as merely a dating tool. There were a few "tests" of the formula made shortly after it was published but these were limited to judging the accuracy of the formula and little more. Since that time researchers have tended to polarize into two camps, one accepting the formula as Truth and ignoring variations in dates, and the other rejecting the whole concept out of hand and not using it. Lest I be thought a member of the latter group let me state for the record that I am actually a member of the former group, with reservations.

It seems to me that approximately 330 pipes (not pipestem fragments as Heighton and Deagan note) spread over 180 years is too few specimens for a control sample to be compared with English pipestem samples throughout eastern North America. In the intervening years since the formula was published research has produced better documentation on pipes, thereby reducing or altering the time range ascribed and a large number of documented pipes have been added to the inventory. This statement does not find fault with Binford; 330 pipes are all that was available to him 10 years ago. Recalculation of a new formula is timely if we plan to do more than date sites in the future. There has been little done thus far that will be affected by changing the formula. Specimens for a control sample could be acquired in one of two ways and this is a matter that is very much open to debate. We could (a) use everything possible from a limited number of sites to cut down
the variables as much as we can, or (b) use specimens from a wide geo-
graphical area on the assumption that the variables would negate one
another. The first of these alternatives was used by necessity, if
not choice, in computing the Binford formula but I feel the second al-
ternative has greater utility because it avoids the possibility that
the sites chosen for the first alternative might be anomalous them-
selves. As I say, this choice is open to debate and the best solution
would be to have two formulas, to be used as needed depending on the
questions to be answered. This does not mean that you would choose
the formula that gives you the closest date to what you want but that
you would already know the dates of your site and use the formulas to
solve hypotheses regarding the distribution of pipes and their cultur-
al significance.

If the reader will bear with me, two hypothetical examples will
serve to illustrate my point. Suppose that you had a small site in
Virginia and you wanted to determine whether the pipes came through
Williamsburg or from the same source. Using formula (a), assuming it
to be heavily weighted by Williamsburg specimens, you should get a
date close to the median of the site if the hypothesis is true. For-
mula (b) would be useful if you wanted to cluster sites according to
the degree of deviation in one direction or the other from a norm.
You could then examine the sites to see what else they had in common.

While my own application of a standard deviation to compute the
time span of a pipestem sample may have been a confusion, Binford's
application toward the same end overlooks the obvious; that the time
periods are of unequal length and therefore not comparable as he com-
pares them in Table 1. The greater length of the 1750-1800 time pe-
riod skews his calculations toward a greater time interval per 1/64th
inch. To get a comparable relationship between the time periods I
divided the figures in Column #5 on Binford's Table 1 by the number
of years in each sample range and got the figures .439, .375, .403,
.440, and .347 from latest to earliest for the five periods. There
is, in effect, no pattern, no trend, and no sine curve evident un-
less one ignores the variability of the time periods. In fact, the
1750-1800 and 1650-1680 periods are almost identical in the rate of
change. Although no longer important in the light of the foregoing,
we wonder why the line of Binford's Graph 1 starts its plunge toward
zero at 1768.9 instead of continuing on to 1778.81 as it should, in
theory, do? We note that this gives tighter dates to samples in the
1770's than it should if the formula is in fact breaking down by this
time.

Finally, Binford and others have set out to make archeology a
science. Archeology is no more a science than medicine. No amount
of statistical manipulation or reams of historical documentation can

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alter the archeological record. In the end we must either interpret this record in light of our manipulations and/or documentation or we must use our manipulations and/or documentation to support our interpretation of the record. The difference between the "new" and "old" archeology is simply a matter of whether the end justifies the means or the means justifies the end. The only room for improvement is the technology by which the archeological record is gathered so that it can be better utilized no matter which course is chosen.

This brings me to the second paper by Heighton and Deagan. While I applaud their effort to produce a new formula I note with dismay that many of their samples are, at best, guess dated and in some cases, like Archer Cottage, undocumented. Fort Necessity, on the other hand, is well documented but produced only nine pipestems. They acknowledge that they are willing to modify their formula and I hope they will do so by eliminating these questionably dated samples and utilizing pipes documented with makers marks and bowl shape (the latter being more difficult to pin down but so prevalent that to use only marked specimens might produce distortion). At the same time, I would hope that they would go to a 1/128th inch scale, not for greater accuracy, but to refine the method. It would be a simple matter to convert to a 1/64th inch scale for samples already measured.

I have prepared a table (Table 1) comparing the dates derived by Binford, and Heighton and Deagan formulas with dates derived from other sources. It can be seen that in most instances the Binford formula gives dates closer to the expected median date than the Heighton and Deagan formula.

I seem to have opened a Pandora's Box which delights me no end. We have had more heat than light thus far but I feel certain that out of all this will come a method that we can all accept and use. Most of the warnings I made regarding sampling and interpretation of dates in the original article are still true and unheeded. A review of the literature since 1962 will show how often the Binford formula has been misused and how interpretations based on it have been slanted (unintentionally, we hope) to conform to preconceived ideas.

So that I will not leave the reader with any doubts as to where I stand, I reject the Heighton and Deagan formula, in its present form, and reserve judgement on the Binford formula. I cannot accept the Binford method for obtaining dates from the standard deviation. I suspect that one day a formula along the lines Heighton and Deagan are pursuing will replace the Binford formula. I predict that it will be a flatter curve than the Heighton and Deagan formula but not a straight line and that samples will show the greatest dispersion in the late 17th and late 18th centuries.
Comparison of Dates Derived from the Binford, and Heighton and Deagan Formulas

<table>
<thead>
<tr>
<th>Site</th>
<th>Median Dates</th>
<th>Sample Size</th>
<th>Sample Mean</th>
<th>Sample Deviation</th>
<th>Binford Formula</th>
<th>Heighton &amp; Deagan Formula</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Fort Stanwix, N.Y.</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>North casemate</td>
<td>1769.96±11.46</td>
<td>1244</td>
<td>4.07</td>
<td>.2755</td>
<td>1776.13</td>
<td>1779.65</td>
</tr>
<tr>
<td>Moat bottom</td>
<td>&quot;</td>
<td>40</td>
<td>4.08</td>
<td>.2668</td>
<td>1775.75</td>
<td>1779.19</td>
</tr>
<tr>
<td>SW casemate-top floor</td>
<td>&quot;</td>
<td>543</td>
<td>4.09</td>
<td>.2879</td>
<td>1775.37</td>
<td>1778.77</td>
</tr>
<tr>
<td><strong>Fort Frederica, Ga.</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fort proper</td>
<td>1743.50±6.50</td>
<td>226</td>
<td>4.87</td>
<td>.4626</td>
<td>1745.52</td>
<td>1747.47</td>
</tr>
<tr>
<td>Town lots 25-27</td>
<td>&quot;</td>
<td>120</td>
<td>4.93</td>
<td>.4135</td>
<td>1743.23</td>
<td>1745.26</td>
</tr>
<tr>
<td>Barracks floor</td>
<td>&quot;</td>
<td>96</td>
<td>4.91</td>
<td>.4669</td>
<td>1743.99</td>
<td>1745.97</td>
</tr>
<tr>
<td>Clay Bank, Va.</td>
<td>ca. 1695.00±5.00</td>
<td>648</td>
<td>6.11</td>
<td>-</td>
<td>1698.07</td>
<td>1706.72</td>
</tr>
<tr>
<td>Jamestown, Va.</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Structure 128</td>
<td>ca. 1650.50±25.50</td>
<td>938</td>
<td>8.11</td>
<td>.5099</td>
<td>1621.56</td>
<td>1655.88</td>
</tr>
</tbody>
</table>

Table 1
Table 1. Five Average Data Points From Harrington For Deriving Equations Relating Date and Stem Bore Diameter

<table>
<thead>
<tr>
<th>Dates</th>
<th>Frequency in a Sample of 100 Stems</th>
<th>Stem Bore Diameter</th>
<th>Product 64ths inch</th>
<th>Average Stem Bore Diameter 64ths inch</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>f</td>
<td>D</td>
<td>f x D</td>
<td>fxD/100</td>
</tr>
<tr>
<td>1620-1650</td>
<td>20</td>
<td>9</td>
<td>180</td>
<td>799  = 7.99</td>
</tr>
<tr>
<td>1650-1680</td>
<td>25</td>
<td>8</td>
<td>200</td>
<td>707  = 7.07</td>
</tr>
<tr>
<td>1680-1710</td>
<td>16</td>
<td>7</td>
<td>112</td>
<td>604  = 6.04</td>
</tr>
<tr>
<td>1710-1750</td>
<td>15</td>
<td>6</td>
<td>90</td>
<td>502  = 5.02</td>
</tr>
<tr>
<td>1750-1780</td>
<td>3</td>
<td>6</td>
<td>18</td>
<td>426  = 4.26</td>
</tr>
<tr>
<td></td>
<td>20</td>
<td>5</td>
<td>100</td>
<td></td>
</tr>
<tr>
<td></td>
<td>77</td>
<td>4</td>
<td>308</td>
<td></td>
</tr>
<tr>
<td></td>
<td>799</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>707</td>
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<td></td>
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</tr>
<tr>
<td></td>
<td>604</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>502</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>426</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table 2. Kaolin Pipestem Bore Diameters and Corresponding Dates

(Calculated from a Least Squares Straight Line Relating Date and Logarithm of the Bore Diameter)*

<table>
<thead>
<tr>
<th>64ths Date inch</th>
<th>Date year</th>
<th>64ths Date inch</th>
<th>Date year</th>
<th>64ths Date inch</th>
<th>Date years</th>
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<tbody>
<tr>
<td>4.0</td>
<td>1777.6</td>
<td>5.5</td>
<td>1713.1</td>
<td>7.0</td>
<td>1664.2</td>
</tr>
<tr>
<td>4.1</td>
<td>1772.6</td>
<td>5.6</td>
<td>1709.4</td>
<td>7.1</td>
<td>1661.3</td>
</tr>
<tr>
<td>4.2</td>
<td>1767.7</td>
<td>5.7</td>
<td>1705.8</td>
<td>7.2</td>
<td>1658.5</td>
</tr>
<tr>
<td>4.3</td>
<td>1762.9</td>
<td>5.8</td>
<td>1702.3</td>
<td>7.3</td>
<td>1655.7</td>
</tr>
<tr>
<td>4.4</td>
<td>1758.3</td>
<td>5.9</td>
<td>1698.8</td>
<td>7.4</td>
<td>1652.9</td>
</tr>
<tr>
<td>4.5</td>
<td>1753.7</td>
<td>6.0</td>
<td>1695.4</td>
<td>7.5</td>
<td>1650.2</td>
</tr>
<tr>
<td>4.6</td>
<td>1749.3</td>
<td>6.1</td>
<td>1692.1</td>
<td>7.6</td>
<td>1647.5</td>
</tr>
<tr>
<td>4.7</td>
<td>1744.9</td>
<td>6.2</td>
<td>1688.8</td>
<td>7.7</td>
<td>1644.9</td>
</tr>
<tr>
<td>4.8</td>
<td>1740.6</td>
<td>6.3</td>
<td>1685.5</td>
<td>7.8</td>
<td>1642.3</td>
</tr>
<tr>
<td>4.9</td>
<td>1736.5</td>
<td>6.4</td>
<td>1682.4</td>
<td>7.9</td>
<td>1639.7</td>
</tr>
<tr>
<td>5.0</td>
<td>1732.4</td>
<td>6.5</td>
<td>1679.2</td>
<td>8.0</td>
<td>1637.1</td>
</tr>
<tr>
<td>5.1</td>
<td>1728.3</td>
<td>6.6</td>
<td>1676.1</td>
<td>8.1</td>
<td>1634.6</td>
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<td>5.2</td>
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<td>6.7</td>
<td>1673.1</td>
<td>8.2</td>
<td>1632.1</td>
</tr>
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<td>5.3</td>
<td>1720.6</td>
<td>6.8</td>
<td>1670.1</td>
<td>8.3</td>
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<td>5.4</td>
<td>1716.8</td>
<td>6.9</td>
<td>1667.1</td>
<td>8.4</td>
<td>1627.3</td>
</tr>
</tbody>
</table>

*Equation: \( Y = 2058.41 -466.47 \log X \) (Line B, Figs. 1 & 2)

Note: The equation in Table 2 is based on Harrington's 5 average data points.
Table 3. Comparison of Harrington's Dates with Dates Predicted by Several Different Equations

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>7.99</td>
<td>1635</td>
<td>1626.15 -8.85 78.32</td>
<td>1632.81 +2.19 4.80</td>
<td>1637.37 +2.37 5.62</td>
<td>1635.01 +0.01 0.00</td>
<td>1635.24 +0.24 1</td>
</tr>
<tr>
<td>7.07</td>
<td>1665</td>
<td>1661.35 -3.65 13.32</td>
<td>1664.14 -0.86 0.74</td>
<td>1662.19 -2.81 7.90</td>
<td>1668.89 +3.89 15.13</td>
<td>1663.87 -1.13 2</td>
</tr>
<tr>
<td>6.04</td>
<td>1695</td>
<td>1700.76 +5.76 33.18</td>
<td>1699.22 +4.22 17.81</td>
<td>1694.10 -0.90 0.81</td>
<td>1706.81 +11.81 139.48</td>
<td>1696.77 +1.77 5</td>
</tr>
<tr>
<td>5.02</td>
<td>1730</td>
<td>1739.78 +9.78 95.65</td>
<td>1733.96 +3.96 15.68</td>
<td>1731.55 +1.55 2.40</td>
<td>1744.36 +14.36 206.21</td>
<td>1738.33 +8.33 8</td>
</tr>
<tr>
<td>4.26</td>
<td>1765</td>
<td>1768.86 +3.86 14.90</td>
<td>1759.84 -5.16 26.63</td>
<td>1764.81 -0.19 0.04</td>
<td>1772.34 +7.34 53.88</td>
<td>1774.91 +9.91 10</td>
</tr>
</tbody>
</table>

Sum of Squares 65.66

Sum of Squares 16.77

Sum of Squares 235.37

Sum of Squares 414.70

Table 4  Method of Computation and Example of Equation for Least Squares Straight Line

EXAMPLE: Computation by Cresthull of Least Squares Straight Line Through Harrington's 5 Data Points

<table>
<thead>
<tr>
<th>DATA INPUT:</th>
<th>X</th>
<th>Y</th>
<th>X+Y</th>
<th>Y=Date (years A.D.)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>7.99</td>
<td>1635</td>
<td>1642.99</td>
<td>X=Bore Diam. (64ths inch)</td>
</tr>
<tr>
<td></td>
<td>7.07</td>
<td>1665</td>
<td>1672.07</td>
<td>N=5 Data Points</td>
</tr>
<tr>
<td></td>
<td>6.04</td>
<td>1695</td>
<td>1701.04</td>
<td></td>
</tr>
<tr>
<td></td>
<td>5.02</td>
<td>1730</td>
<td>1735.02</td>
<td></td>
</tr>
<tr>
<td></td>
<td>4.26</td>
<td>1765</td>
<td>1769.26</td>
<td></td>
</tr>
</tbody>
</table>

Average: \( \bar{X} = \frac{\Sigma X}{N} = 6.076 \)

Average: \( \bar{Y} = \frac{\Sigma Y}{N} = 1698 \)

<table>
<thead>
<tr>
<th>PRIMARY COMPUTATIONS*</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. ( \Sigma X = 30.38 )</td>
</tr>
<tr>
<td>2. ( \Sigma Y = 8,490 )</td>
</tr>
<tr>
<td>3. ( \Sigma XY = 51,276.50 )</td>
</tr>
<tr>
<td>4. ( \Sigma X^2 = 193.6546 )</td>
</tr>
<tr>
<td>5. ( \Sigma Y^2 = 14,426,600 )</td>
</tr>
<tr>
<td>6. ( \Sigma (X+Y) = 8,520.38 )</td>
</tr>
<tr>
<td>7. ( \Sigma X(X+Y) = 51,470.1546 )</td>
</tr>
<tr>
<td>8. ( \Sigma Y(X+Y) = 14,477,876.50 )</td>
</tr>
</tbody>
</table>

CHECKS ON ACCURACY OF PRIMARY COMPUTATIONS

|   | 1 + 2 = 6 | 30.38 + 8,490 = 8,520.38 | 3 + 4 = 7 | 51,276.50 + 193.6546 = 51,470.1546 | 3 + 5 = 8 | 51,276.50 + 14,426,600 = 14,477,876.50 |

SECONDARY COMPUTATIONS

<table>
<thead>
<tr>
<th>Expression</th>
<th>Substitution</th>
<th>Result</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \Sigma xy = \frac{\Sigma XY - \Sigma X(\Sigma Y)}{N} )</td>
<td>51,276.50 - 30.38(8490)/5</td>
<td>-308.74</td>
</tr>
<tr>
<td>( \Sigma x^2 = \frac{\Sigma X^2 - (\Sigma X)^2}{N} )</td>
<td>193.6546 - (30.38)^2/5</td>
<td>9.065720</td>
</tr>
<tr>
<td>( \Sigma y^2 = \frac{\Sigma Y^2 - (\Sigma Y)^2}{N} )</td>
<td>14,426,600 - (8490)^2/5</td>
<td>10,580</td>
</tr>
<tr>
<td>Slope ( b = \frac{\Sigma xy}{\Sigma x^2} )</td>
<td>-308.74/9.065720</td>
<td>-34.05576</td>
</tr>
<tr>
<td>Intercept ( a = \bar{Y} - b(\bar{X}) )</td>
<td>1698 - (-34.056)(6.076)</td>
<td>1904.92</td>
</tr>
<tr>
<td>Equation: ( Y = a + bX )</td>
<td>( Y = 1904.92 - 34.056X )</td>
<td>-</td>
</tr>
</tbody>
</table>

*\( \Sigma \) (Sigma) indicates sum of all values. For example, \( \Sigma X \) is the sum of all X values.

Note: X and x are not interchangeable. x and y are symbols for computation

** A least squares straight line has the following property: The sum of squares of vertical deviations (Y) of observations from this line is smaller than the corresponding sum of squares of deviations from any other line (See Figure 2).
TWO LINES FITTED TO HARRINGTON'S DATA POINTS

Line A: Least Squares Straight Line (Date Vs. Bore Diameter)
Line B: Least Squares Curved Line (Date Vs. Logarithm Bore Diameter) (Figure 2)

Figure 1
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LINE B: LEAST SQUARES STRAIGHT LINE ON SEMILOGARITHMIC PAPER
FITTED TO HARRINGTON'S DATA POINTS
(Data Vs. Logarithm Bore Diameter)

Figure 2

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CERAMIC ANALYSIS TOOLS
FOR THE INTERPRETATION
of Eighteenth Century British American Sites

An Application of the Mean Ceramic Date Formula

Legend

The Ceramic Types Used to Construct the Analysis Tools

Application of the Analysis Tools

The Ceramic Analysis Data from Census Sites Using Independent Occupation Studies, Presence and Absence, and the Mean Ceramic Date Formula Compared to the Documented Site Occupation Period

The Marker-Type Model

An Application of the Analysis Tools

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