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The Conference on Historic Site Archaeology Papers 1968 - Volume 3

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The Conference on Historic Site Archaeology Papers 1968

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THE CHAIRMAN'S REPORT

The Ninth Annual Conference on Historic Site Archaeology was held in Knoxville, Tennessee on November 7, 1968. This Volume III of The Conference on Historic Site Archaeology Papers contains a number of the papers presented at that conference, plus papers submitted to THE HISTORICAL ARCHAEOLOGY FORUM. Garry Stone's paper "Ceramics in Suffolk County, Massachusetts, Inventories - A Preliminary Study with Divers Comments Thereon, and Sundry Suggestions" was the focal point for this year's HISTORICAL ARCHAEOLOGY FORUM section.

Due to the change of professional position of the chairman during 1969, from the North Carolina Department of Archives and History to the Institute of Archeology and Anthropology at the University of South Carolina, and a subsequent nine months in the field, this volume has appeared later than originally planned. The change from mimeographed to Multilith printing should improve the product, though it is more expensive. With the sale of additional copies of Volume II, plus the increase in membership dues to $5.00 annually, the additional expense can be met. The typing cost has been furnished by the Institute of Archeology and Anthropology, as a cooperative gesture to the Conference.

Several papers presented at the Conference were never submitted for publication and therefore do not appear. Lee Spence's paper "Salvaging the Cargo of the Mary Bowers" has been held over to be included in Volume IV as one of the papers of THE HISTORICAL ARCHAEOLOGY FORUM. South's paper on the ceramic ware of Rudolph Christ is only summarized here since it has grown too long for inclusion, and will be published elsewhere. Iain Walker's paper did not arrive in time for the Conference.

The many plates accompanying the paper of J. Glenn Little, II, were furnished by Mr. Little, thus contributing considerably toward a saving for the Conference budget.

Thanks are due to those who participated in THE HISTORICAL ARCHAEOLOGY FORUM section of this volume, thus contributing toward the continued success of this section. I would also like to thank Carleen Regal for typing, Jane Gardner, Karen Lindsey, and Alan Shoemaker for proofreading the copy, and Jim Frierson for assistance with drafting.

Stanley South, Chairman
The Conference on Historic Site Archaeology

1
The emphasis of this article is the result of the analysis of approximately sixteen weeks of archaeological research on Fort Davis, Fort Mahan and Fort DuPont. It provides both historian and architect with data concerning the architectural features, construction methods and materials used in each fort; this data is also correlated with the known historical facts. All three forts are situated in the east of the District, across the Anacostia River within the city limits. During the Civil War, the area was known as the Third Sector of the Military Perimeter of Washington, D.C.

Between one-quarter and one-half mile east of the Anacostia River is a six-mile wooded ridge more than 300 feet above mean tide of the Potomac. This ridge, though extremely narrow and very contorted at some points, leveled off into a plateau at the south end of a deep valley of Oxen Run. The fortifications that were erected along this ridge were built with difficulty, and McCormick (1967: 24-26) enumerates these. Barnard (1871: 66) indicated that the forts on this ridge should have been large, well flanked, and self-sustaining.

"They should have been holding points well provisioned and capable of holding out for a few days in isolation while mobile forces utilizing interior lines could be brought to the relief of them or braked presumed simultaneous seige operations being carried out by the Confederates on some other sectors of the defenses."

Such was the theory of the defenses across the Anacostia (McCormick 1967: 26).

Fort Mahan, on an isolated hill northwest of the ridge, guarded the approaches to Bennings Road and Bennings Bridge and was near completion by the fall of 1861. Fort Mahan, the largest of the three forts, was a regular nine sided fort having a perimeter of 350 yards, and was located approximately 7/8 of a mile east of Bennings Bridge.

Although Fort Mahan was completed and ready for assignment of troops by December, 1861, the military engineers of the Army of the Potomac were far from satisfied; the archaeological investigations and McCormick's (1967) historical research indicate that the fort and its armament underwent extensive repairs and modifications for at least four years after 1861; however, we have been unable to date the modifications archaeologically. Because Fort Mahan was situated on top of a hill, its approaches are not visible from parapet position. Barnard felt that to strengthen this
weakness more than 400 yards of rifle pits needed to be constructed to cover the approaches. In 1864, engineers laid abatis and constructed rifle trenches toward the Anacostia in order to prevent a flanking movement from that side. These rifle pits exist in an eroded state today, and were easily identified archaeologically. Bastionette additions were projected from three angles of the counterscarp to provide covering fire for the exterior slopes. These bastionettes, along with rifle pits, are the only remaining evidence today of the actual Civil War fort construction, due to the fact that the interior of the fort and the main parapet walls and ditch were covered over in 1930 when the WCC camped in the fort. The filling operation took place probably to create make-work activity and also to provide recreation areas; i.e., today it is used as both ball fields and summer camp grounds. Archaeologically, we have found only the remains of the bastionettes.

In the summer of 1865, after the abandonment of many other forts, the 24-pounders and 32-pounders in Fort Mahan were replaced by field guns; and finally in October or November of 1865, Fort Mahan was abandoned. McCormick (1967: 29) indicates that the Engineers' drawing of Fort Mahan show that there was a guard house, a stable, and possibly some sheds, for a total of 12 frame and log buildings surrounding the fort. The officers' buildings and barracks were within the perimeter of the fort. Archaeologically, we were unable to identify the location of any of these buildings and we believe that they were destroyed by the WCC camp projects.

South of Fort Mahan was Fort DuPont, which was under construction in October of 1861, and according to Barnard (1861: 681; 1862: 699), was completed in the spring of 1862. McCormick (1967: 30) reports that it was built at an elevation of 303 feet above mean tide. Compared to Fort Mahan, Fort DuPont was much smaller, hexagonal in shape, and had a perimeter of only 200 yards. It also underwent modifications between 1861 and 1865; and archaeologically, we have been able to pinpoint one of these. Fort DuPont was never a strong work; the commanders of the fort had noted that a deep ravine passed very close to the east and west of the fort which should have been guarded by a block house or traverses to cover the sally port, and a magazine and rifle approaches had also been recommended but apparently were never built (McCormick 1967: 31). DuPont was abandoned in 1865 and subsequent reports indicate that the land was plowed and a tree farm begun on the property. Archaeological evidence confirms this activity. Within the walls of Fort DuPont there was a flag staff and a deep well. Two officers' quarters, 24' by 16'; a barracks, 100' by 20'; a mess hall, 50' by 20'; and a guard house, 24' by 18', existed just outside the fort. Unfortunately, funds were not available to excavate for these structures and indications are that a National Park Service road constructed some time in the early 1940's destroyed the majority of these buildings. However, within the walls of the fort, the well was easily identifiable, but we could not locate the flag staff position.

Approximately 3/4 of a mile south of Fort DuPont is Fort Davis, and McCormick states that this work was constructed at the same elevation (303 feet above mean tide) as Fort DuPont. It was larger than Fort DuPont, having a perimeter of approximately 220 yards. Chapman's letter to Lt. Col. J. A. Mardie, U. S. A., March 17, 1864, states that the shape of
the fort is that of an irregular heptagon and that the distance around the fort, measuring the inner crest of the spanment, is 230 yards. Along with DuPont, Fort Davis was abandoned earlier than Fort Mahan. Changes in armament between 1861 and 1865 are recorded by McCormick (1967: 33). A well of approximately 124 feet in depth and a flag staff were within the walls of Fort Davis. Outside the Fort were barracks, 20' by 100'; an officers' quarters, 16' by 100'; a mess house, 20' by 50'; and a guard house, 18' by 24'. McCormick indicates that in 1865 these buildings were dismantled, the lumber auctioned and the land on which the fort was built returned to its owner. To the west of the fort the ground drops away from the crest of 303 feet above mean tide. This decrease in elevation is probably a man-made contour, and thus the area where the buildings were once located has been destroyed.

FORT DAVIS: Archaeological research at Fort Davis was designed to recover information for restoration of the south parapet wall, the main glacis slope and the abatis as well as the interior gun platform, gun embrasures, Magazine No. 2, interior and exterior, Magazine No. 1, interior and exterior, and the entrance to the fort located on the north side (see Figure 1). The remaining areas would be contoured to accept these restored portions. Archaeological research provided enough data from six trenches (Figure 1) to conjecture drawings which allow complete restoration of the entire fort. When the 1862 Civil War Fort Engineers' drawings and a contour map drawn to the same scale are overlaid, there is only a small discrepancy on the north side. This discrepancy was caused by changing the angle of the north parapet wall as it met the apexes of the northeast and northwest side parapet walls of the heptagon. The discrepancy suggests that the main ditch is narrower than indicated on the north and south sides as shown in the 1862 Civil War Fort Engineers' drawings. Variations of the width, depth and angle of the parapet wall counterscarp were probably brought about by drainage problems that arose during construction. The angle of the pitch of the main ditch floor also varies. The positioning of Trench 1 (T-1) was based on the need to recover enough information to indicate elevations of the interior terreplein level, the nature of the slope of the gun platform, the height of the gun platform in relation to the terreplein level, the precise location of Magazine No. 2, the angle of the magazine sides and the elevation and nature of the corner mortar platform as indicated by the Engineers' drawings.

At the bottom of Figure 3 we see a cross-section of T-1 and it becomes quite evident that the red-brown clay-loam with a high concentration of pebbles, in some areas tightly packed, served as the terreplein. In the far west corner of this section, the elevation of the banquette tread when correlated with the Engineers' drawing cross-section is identical.

At the top of Figure 3, a conjectured drawing of the south interior parapet wall is shown as viewed from the inside of the fort looking south. The archaeological profiles at the bottom of Figure 3 are viewed from the inside of the fort looking north. The reversal of the profile in T-1 drawings was necessitated because of lighting problems, particularly the angles of the sun. To maintain vertical correlation between the archaeological
profiles and the conjectured restoration drawings of all sections of T-1, the reverse views of T-1 are presented in Figures 3, 4 and 5. All other profiles and conjectured drawings of the archaeological research are viewed from the same compass position. The superior crest of the parapet wall is at an elevation of 307.2' (as shown in Figure 3). This elevation was derived by analyzing the stratigraphic drawings, historical information, particularly Barnard (1861: 65-67), and the Engineers' drawings of the transverse profile of Fort Davis.

The location and elevation of a gun platform conjectured in the archaeological profile drawing (section C-D in Figure 3) is based on the stratigraphic data recovered from sections H-I and I-J, T-1. The conjectured restoration (Figure 3) places the top of the gun platform at an elevation of 302' and shows the relationship of the platform to the throat of the gun embrasure. Exact location of the throat and its dimensions are conjectured from information on the Engineer's drawings, transverse sections, cross-section and plan view. The type of sleepers or jacks used in the platform is unknown in Fort Davis. Neither gun platform on either side of Magazine No. 2 (see Figure 1) at Fort Davis was excavated for plan view data, as were Fort DuPont gun platforms of similar dimensions. Of course, for a valid cross-correlation, we are assuming a similarity in the techniques and plans used in the construction of Forts DuPont and Davis.

The configuration of the west edge cut slope (Figure 4) made during the construction of Magazine No. 2 can be interpreted as representing a leverage angle or stacking angle for the placement of horizontal logs allowing the compacting of earth against these logs as shown in the conjectured cross-section of the back room of Magazine No. 2 in the top portion of Figure 4. Archaeologically, we found a small horizontal cedar log with a three inch diameter, eight feet long.

Figure 4 (Architectural Restoration Drawing) does not show these half-round logs because information to indicate that they were used was not uncovered archaeologically; however, at a location near Stake F the position of the half-round logs can be conjectured. Outside of these logs, which would be to the west, Barnard states that these posts supported a revetment of small posts, placed horizontally and carried up uniformly as the earth was replaced externally. This arrangement afforded an air chamber around the magazine room (the air space is indicated in the conjectured Figure 4) with dividers in place as shown in the 1862 Engineers' drawing, which, in conjunction with the ventilating pipes, were found archaeologically; however, at Fort DuPont one section of a pipe was excavated. In analyzing whether to conjecture the restoration of the magazine room with large diameter posts laid horizontally or with small horizontal poles as indicated by Barnard (1871: 65-67) one archaeological find seems to answer the question. The archaeological evidence indicated that small diameter pole lengths were laid horizontally to the outside of the exterior wall supports of the room to make the air space watertight, as indicated by Barnard, and that the historical data in the form of the Engineers' drawings is incorrect on this point.

The conjectured restoration (Figure 4) of the interior magazine room
does not show how the roof structure joined with the interior wall supports or exterior wall supports. It simply shows how archaeological evidence correlates with the 1862 Engineers' drawings.

How the interior of the Magazine room was finished off is unknown archaeologically and will have to be conjectured from the historical data gathered by McCormick (1967) and the Engineers' drawings (1862). Barnard (1861) states that the interior width of the room was 12 feet, but archaeological evidence indicates that the room was only 10 feet wide. The 10 foot dimension is also found indicated on the 1862 Engineers' drawings, plan view and transverse sections. The front of the magazine room according to the 1862 plan view was 16' by 18'. In the center of section E-G is a drainage ditch on the north-south axis with two adjoining feeder drainage ditches on the east-west axis. The ditch location and configuration correspond to the drainage ditch profile shown in the transverse section of the 1862 Engineers' drawing.

The same configuration of the west edge cut made during the original construction of the magazine, as found in sections E-F, T-1, Figure 4, was conjectured and is shown by the dotted lines in section G-H. All three drawings of the profile of T-1, Figure 4, plan views of the sections and entire conjectural cross-section of the back room in Magazine No. 2, are vertically correlated.

Magazine No. 2's earthen crest is 4.4 feet higher that the superior crest of the parapet wall. This difference can be seen in the Engineers' drawings of 1862, particularly the transverse section. The elevation of the superior crest of the parapet wall is based on an analysis of the distance from the bottom of the drainage ditch to the crest of the earthen roof over the magazine room, and these dimensions in turn were correlated archaeologically with the historical information.

The elevation difference between the superior crest of the parapet wall at an elevation of 307.7' and the gun platform at an elevation of 302.5' was derived by taking the known dimension from the bottom of the drainage ditch in section F-G, T-1, and conjecturing from that point the elevation of the magazine crest which correlates to the elevation of the superior crest of the parapet wall in the 1862 Engineers' drawing. This is correlated to the elevation of the banquette tread which was analyzed in relation to the elevation of the gun platform to the elevation of the terreplein level. The results also correspond within one foot of each other to any measurements taken from the transverse section drawing. This analysis provides elevations for all of the conjectured restoration drawings T-1. Of course, historically McCormick has shown differences in construction measurements, and Barnard's report calls for specifications that were not followed. These differences have been demonstrated in the archaeological research thus far in T-1, sections O-H.

The gun platform level is identified because of the heavy concentration of gravel and the recovery of a large number of nails used to secure gun platform boards to jacks or sleepers. The analysis of the stratigraphy
did not provide any indications of beam location, such as sleepers or jacks to support the wooden gun platform. The gun platform measurement is 20' wide and this corresponds with the Engineers' drawings.

The terreplein level is 7.2 feet beneath the elevation of the superior crest of the parapet wall, for the east interior side of the fort. The gun platforms on both east and west sides of Magazine No. 2 are 2 feet above the interior terreplein level. The throat of the gun enclosure is two feet wide and cut to a depth of 2.3 feet from the superior crest of the parapet wall. The angle of the sole and cheeks of the embrasure can be conjectured from the analysis of the 1862 Engineers' drawings. Archaeologically, in T-1, Figure 1, an embrasure sole and the approximate angle of cheeks can be identified, but each gun embrasure had different angles; and we were unable, because of erosional forces, to determine the exact angles. In Figure 5, Section K-L was excavated to a depth where soil configuration matched the location of the "embrarrette" platforms seen in the Engineers' drawing plan view. The profile drawing of T-2, Figure 6, illustrates the south parapet wall and main ditch looking west. The conjectured restoration in Figure 6 is based on the correlation of data shown in the bottom portion of Figure 6 and matched to the 1862 Fort Davis Engineers' drawings with Barnard's report in 1871 and with Mahan's work in 1850.

McCormick (1967: 79) points out that the erosion of the counter-scarp was effected by rain which caused the abandonment of the 18-inch berm between the foot of the exterior slope and crest of the interior slope of the ditch and the substitution for it of a uniform 45° slope from the exterior crest to the bottom of the main ditch (Barnard 1871: 63). This historical note is in accordance with the archaeological findings.

T-2 was positioned on the center line of Magazine No. 2 to the south, and extended from the top of the present day remains of the parapet wall across the main ditch to the crest of the present day glacis slope. If the position of the two foot wide berm with a counter-scarp 7.3 feet high at a 55° slope is conjectured, then the archaeological evidence of a 45° slope from the juncture of the counter-scarp with the main ditch floor to the top of the exterior crest does not correlate with the transverse section of the Engineers' drawing.

Figure 7, A-B, illustrates the transverse profile of Magazine No. 2 and the rear of Magazine No. 1. This is a conjecture drawing based on archaeological measurements recovered as to specific features of the interior magazine rooms in sections A-B, B-C, C-D, and D-E, in T-4 as well as that recovered in T-1, sections D-F and F-G and G-H. The conjecture also incorporates some stratigraphic information of the interior terreplein level derived from an analysis of core drillings taken just south of Magazine No. 1. According to the Engineers' drawings of Fort Davis, 1862, the dimensions of the interior of the back room of Magazine No. 2 are 10 feet by 28.9 feet, the dimensions of the passageway connecting the rear room to the front room are 4 feet by 8 feet, and the interior dimensions of the front room are given as 16 feet by 16 feet.
Archaeologically, in Figure 7, it is impossible to conjecture the rear scarp angle of the southern-most portion of the magazine's earthen roof or the exterior slope of the parapet wall; but we have projected the scarp in Figure 7 at an angle of 62°. This is the same angle that is shown in the Engineers' drawing, transverse section.

A transverse section of the entrance of Magazine No. 2 is conjectured in Figure 7; the core drillings shown on the topographic map, Figure 1, help to identify the elevations that were needed in order to properly reconstruct the front entrance to the magazine. From the top step of Magazine No. 2 to the rear scarp of No. 1 is six feet, and the distance is wider than the distance shown in the transverse section of Fort Davis Engineers' drawing of 1862. Three factors based on archaeological evidence support this dimension:

1. The location of the sterile soil in Figure 8 indicating the farthest point north of the stairwell into Magazine No. 1 could have been constructed;

2. The location of two barrel molds and dimensions, the width of the bottom rims in the passage between the front room and the rear room (Figure 8);

3. Except that the dimensions from Magazine No. 1 given on the Engineers' drawings of 1862 is 16' by 16' for the front room with a passageway of 10' by 4' with the rear room dimensions of 10' by 20', then the rear scarp of the magazines' junction point with the terreplein level can be derived.

The conjectured interior (Figure 8) is based on a comparison with Fort Davis Engineers' drawing (1862) and the archaeological data uncovered at Fort Davis and DuPont, and this interior drawing appears in the upper portion of the figure.

Construction of the rear room of the magazine in Figure 9 is the same as discussed earlier and shown in Figure 7. The thickness of the earthen roof above the magazine top is the same as in Magazine No. 2. The architecture of Magazines No. 1 and 2 at Fort Davis is not the same as the architecture used at Fort DuPont. Conjectural architectural drawings in Figure 8 are similar to the drawings discussed in Figure 7. The conjectured architectural restoration drawing of the entrance to the fort is based on interpretation of the Engineers' drawing plan view and transverse section. T-6 is situated just north of the scarp angle of the main parapet wall. The projected horizontal plain of the parapet wall scarp on the north-south axis was based on control evidence uncovered in the excavations of T-5 as shown in profile and plan views of Figure 9. Conjectured dimensions of the ditch are based on analysis of the Engineers' and stratigraphic drawings. The main ditch counter-scarp angle does not correlate with the angle shown in the Engineers' drawings and is less steep, 62° (see Figure 9).

In reviewing the archaeological findings, the south parapet wall at
Fort Davis was 123' long on the east-west axis. Figure 2 shows that the north main ditch and parapet wall in the Engineers' drawings are farther north than they should be. Interpretative analysis places the location of the gun platform (Figures 3 through 6) corresponding to the topographic data shown in Figure 1. The angle of the south parapet wall exterior slope is different from the angle indicated in the Engineers' drawing. The west cheeks of the gabian line of the gun embrasures excavated in T-3, Figure 1 plan view, are indicated by the dotted line about six inches below the surface (see additional information, reference gun embrasures, Fort DuPont, T-5, Figure 19). It is hoped that the transverse drawings (Figures 6 through 9) on a north-south axis as well as the cross-sectional drawings on an east-west axis (Figures 3 through 5) will provide sufficient data for restoration and reconstruction of the south gun platforms, south parapet wall, main ditch, glacis slope, and the magazine interiors and entrance to the fort.

**FORT MAHAN:** McCormick indicates that from 1861-1865 Mahan underwent considerable repair and modification, and two of these modifications were recorded in the archaeological research. Mahan in 1865, indicates that bastionetes were projected from three angles of the counter-scarp to provide reverse and covering fire for the exterior slopes. Archaeological excavations in the southeast bastionete were designed to recover stratigraphic information for the stabilization and reconstruction of that bastionete. The southwest bastionete was identical, in our opinion, to the southeast bastionete; therefore, no archaeological research took place in the latter. A series of trenches, shown in Figure 10, T-1 through T-5, provide sufficient data to reconstruct a transverse drawing west-east, looking south from Point BB to AA, (Figure 12 and 13). An additional cross-section drawing of the southeast bastionete can be drawn from the data retrieved out of T-2 (see Point T-1, Stake B to CC, Figure 20). Archaeologically, the southeast bastionete might have been separated from the main fort's southeast parapet wall and encircled by a ditch. To confirm this hypothesis, extensive excavations of the bastionete would have to be undertaken, as well as a more extensive historical investigation as to the nature of the additions to Fort Mahan; at this time money is unavailable to pursue both of these items.

We have interpreted the stratigraphy of the trenches excavated as indicating that the southeast bastionete was used as a rifle bastionete. It mounted only two cannons; two small gun platforms are partially indicated by McCormick's historical information. Each gun position covered the main ditch and parapet wall of the fort, providing transverse fire for the entire length of the east and south sections of the main ditch. If the bastionete were separated from the main fort, movement between them could have been accomplished through a series of doors located in both scarps. Evidence to prove this hypothesis was not discovered archaeologically, it is simply an historical conjecture based on facts presented by McCormick (1967). Our research on Fort Mahan included surveying the eroded remains of the northeast rifle pits, as shown on the topographic map, Figure 11. The configuration of the topography in Figure 11 is similar to that found in the front of the bastionete in the southeast corner (Figure 10). Undoubtedly, some of the rifle trenches' construction was carried out in the area during 1863-1864, as
has been mentioned earlier. Figure 12 indicates the exterior and interior slope of the rifle trenches. The angles of the superior crest, to the interior crest slope, the superior crest to the exterior crest have been conjectured from the erosional remains of the trenches (see Figure 12). Excavation of T-3 provided the angle of the counter-scarp to the main parapet wall, which was 65°. The exterior slope of the parapet wall joins the scarp at an elevation of 165' and an angle of 25°, and it rises to an elevation of 169.2' at the exterior crest. From the exterior crest to the superior crest of the parapet wall there is an 8° slope and the superior crest elevation is 171.5'.

The revetment posts conjectured in Figure 13 as forming the breast heights are similar in height to those used in the construction of Fort Davis; however, the length of the posts is increased to 7.2'. Barnard (1871: 64) indicates that the post revetment was initially installed and breast heights did not prove durable. Where timber was available, vertical post revetments replaced them (McCormick 1967: 39).

The depth to which we have conjectured posts is based on archaeological data recorded during the excavations in T-1, section C-B (Figure 13). From the depth of the post molds, the elevation of the top edge of the conjectured banquette tread was 167'. There is a vertical rise to the superior crest of 4.5'. These three elevations were used as control elevations to conjecture the reconstruction (Figures 12, 13 and 20) of the southeast bastion, parapet wall, main ditch, scarp and counter-scarp.

The stratigraphic profile of T-1, Figure 13, is very complicated. Post molds found in sections G-A, T-1 also appear in sections F-D, T-1 but they are at a different angle. The stratigraphy to the east of Stake E, T-1, is a result of fort construction and the layers show uniform disturbance. Two horizontal post molds were found approximately one foot west of Stake F, T-1. These post molds are the remains of sleepers that were attached by wire to the revetments, 4.5' to 5.5' east of Stake F. The elevation of the banquette tread, 167', was conjectured by an analysis of the yellow-tan sandy clay and gravel and its slope with a drop of four feet east to Stake D, T-1.

In sections D-C beneath the top soil is a layer of dark red clay and gravel. This layer is the floor level of the bastionete. The exterior slope of the parapet wall between the superior crest and the exterior crest should have been between the elevations of 169.5' and 171.2'. The present bastionete floor was conjectured on a slope varying from 164.2' to 164' and these elevations correspond accurately with the stratigraphic drawings of section D-C, T-1, Figure 13. Bastionetes are shown in the Engineers' drawings dated 1863-1864. However, there are no precise cross-sections to transverse profiles to analyze in relation to the archaeological data. There are two plan views, however, that do provide a few clues to interpret, enabling architectural drawings to be conjectured as to the interior construction of the bastionete. These features are the superior crest line of the parapet walls as the top of the revetment posts; the revetment line represents the interior edge of the banquette treads and the exterior edge of the banquette tread as well as indications of the banquette scarps. The angle of the post molds.
shown in the plan view of T-1, Figure 13, sections B-C, C-D, D-E, and E-F also correlate with the Engineers' 1863-1864 sketch of Fort Mahan but a direct scale relationship cannot be assumed. Stratigraphic data recovered in T-2, Figure 21, confirmed in part our interpretation that the forward level of the bastionete was at an elevation of 164' and the fact that the two banquette treads served as rifle bastionetes or small gun platforms and revetments forming the bastionete tread.

To retrieve data as to the nature of the banquette tread, breast heights and revetment at the southeast bastionete, T-2 was cut perpendicular to T-1. In Figure 21, upper portion, we have conjectured the glacis slope, the counter-scarp at an angle of 68°, the bottom of the main ditch with a southward slope of 3.5°, the main parapet wall scarp at 65° angle, and a portion of the parapet wall from its interior crest to the superior crest at a 6° slope. The bottom of the parapet wall slopes approximately one-half foot. It is 9.5' wide and the counter-scarp is 6.5' high rising to an elevation of 162.3'; the scarp slope rises from an elevation of 169.25' at the exterior crest to the uppermost elevation of 171.5' at the superior crest. The bottom of the main ditch at the superior wall is wider than the conjectured ditch width in T-3 (see Figure 12) by one-half foot. Location of the breast height revetment post is based on a correlation of stratigraphic data and the reconstruction conjectures as seen in Figure 13.

Five and seven-tenths feet north of Stake A, the disturbed material is interpreted as the location of the revetment works and breast height in Figure 21. From Stake A north to Stake B at an elevation of 164.6' is a layer of material that at one time formed the banquette tread. (See dotted line in Figure 21.) At a point three feet south of Stake B, T-2, underlying the reddish-tan, silty clay and gravel is a layer of gray, silty clay conjectured archaeologically to have been sod blocking. This clay may have served a function in the construction of the scarp or the banquette tread scarp and the floor of the bastionete as indicated by the dotted line and which is at an elevation of 164'. The dotted line in Figure 21 is very much a conjectured line to correlate with the information presented in Figure 13 (the conjectured architecture of T-1 and the interior of the bastionete). Stake B, T-2, is located on the south wall of T-1; and it is possible to view in Figure 13 the profile that would match Stake B, T-2. These two profiles incorporate all of the primary features of the bastionete at Fort Mahan and indicate our complete archaeological findings.

FORT DUPONT: Archaeological research at Fort DuPont was to be the most extensive, as compared with the investigations carried out at Fort Davis and Fort Mahan. The transverse profile labeled FF to EE (Figure 14) includes views of parapet walls and main ditches on the north and south sides of the fort, a gun platform not shown on the Engineers' drawings of Fort DuPont, the entrance to the fort, and the magazine (Figures 15 to 18). Cross-section profiles from HH to GG show views of the east and west main ditch and parapet walls, a double embrasure gun platform and a magazine. Stratigraphic profiles in T-A, T-3, T-5, and T-10 can be used to conjecture additional cross-sections and transverse profiles of the fort to aid the restoration programs.
T-6 provided stratigraphic data that enabled conjectured architectural drawings to be projected showing the exact position of the glacis slope, counter-scarp, and the bottom of the main ditch. The slopes, as well as the horizontal and vertical dimensions, do not correspond with those of the parapet wall and ditch drawn through profile GG and through the extension of transverse FF to EE on the north side with the data recovered from T-1, Figure 18. By establishing the depth of the juncture of the light gray-tan clay with the reddish-tan clay with cores, the scarp slope and upper parapet wall between exterior crest and superior crest slopes could be projected.

At the top of Figure 15, a conjectured architectural drawing of the main ditch is illustrated. It is vertically correlated to the profiles of T-6 and T-3C as well as T-3B looking west. The width of the main ditch is 9.5' and slopes southward. In section C of T-3, Figure 15, stratigraphic information in both the plan view and vertical view indicates the location of gun emplacements as shown by the conjectured revetment based on archaeological analysis of the plan view of T-3, section B, Figure 15. The apex of the revetment work also represents the junction of the south and southeast parapet wall. The profile section of B, T-3, also illustrates the numerous layers of clay and gravel constructed to form the breast height works south of the revetment. Revetment posts and breast height have been conjectured in a line with Stake B, T-3B, Figure 15. Stratigraphy indicated by the west wall of section B, T-3, Figure 13 and section A, T-3, Figure 16, illustrates that erosional forces moved the soil shown in profile T-3 in section A from the position south of Stake B, T-3B, Figure 15.

Mean elevation of the spike heads shown in Figure 16, T-3A, correlates with the conjectural position of the revetment works. There has undoubtedly been an alteration in the original gun emplacement as shown in the Engineers' drawing of Fort Davis in position T-3. The Engineers' drawing indicates a gun "embrayette", probably for a large 100-pound gun with small wheels. But, in fact, archaeological evidence suggests that the "embrayette" emplacement was changed to an embrasure emplacement. A center line in the plan view of T-3A, Figure 16, between the two beam molds is on center with the stratigraphic plan view of T-3C, Figure 15. Thus, Figure 16 correlates with our interpretation of the location of the apex of the parapet wall, southeast and south sections. A plan view of T-3, section A, Figure 16, indicates a series of seven beams or partial beam molds of decomposed beams that were uncovered during the excavation of the platform. Spikes were found in situ in five of the beams. The beams are not on the direct north-south axis nor are they parallel to each other. They apparently served as sleepers or jacks for the wooden gun platform to rest upon. McCormick (1967: 43) and Barnard (1871: 71) state that the gun platforms, both field and siege, were constructed on rammed foundations of earth so that the platform shoulders should not be less than 7.5 feet below the crest. Archaeologically we were unable to indicate that these dimensions existed at any time. The gun platform at Fort DuPont as indicated by the dot in T-3, is 4.5 feet below the superior crest of the parapet wall and not 7.5 feet. Also, McCormick (1967: 92) shows an 1862 photograph of a small fort, identified as Fort Massachusetts or Fort Sumner from the Brady-Eaton collection, with guns firing through and over the superior crest of the wall.
The platform as projected in T-3, Figure 16, was probably 14' by 16' wide with a gun ramp narrowing as it joined the terreplein level of the rear magazine. This platform is 4' narrower than the 20-foot wide one excavated at Fort Davis. In T-3, section B, Stake D, the interior level is indicated by the junction of the rear magazine scarp at 46° with the terreplein level.

Floor boards in the rear magazine room are at an elevation of 303.5' and were constructed out of varying width, 2 inch or 4 inch thick boards of unknown lengths. Undoubtedly the thickness of the boards was related to the size and number of powder barrels stored in the magazine. The conjectured reconstruction drawing of Fort DuPont (Figure 17) is based on the archaeological data recovered from the excavations of Magazines No. 1 and 2 at Fort Davis (see Transverse Profile, Figure 7).

In Figure 17 of Fort DuPont, the profile of T-1, section A, XY indicates that a ditch containing post molds sunken into the subsoil was used in the construction of the front magazine room, and it is only logical to assume that the same building methods would have been used in both rooms. The fact that the room supports were sunken at Fort DuPont does not correlate with, and is contradictory of, the archaeological data recovered from Fort Davis. Other than the dimensions of the rear room in the magazine and passageway, and the construction of the interior room ceilings, both the north-south and east-west axis ceiling beams are the same size as at Fort Davis and were covered with the same waterproofing. The rear room interior dimensions at Fort DuPont are 12' by 20'. These figures correlate with the information provided by Barnard (1871) as to the size room needed to stack three rows of barrels, but they are contradictory to the data uncovered at Fort Davis.

The passageway connecting the two rooms located on the west side is not centered between them as it was in Magazines No. 1 and 2 at Fort Davis. Archaeologically, we did not uncover facts as to the exact nature of the passageway except that we assume its measurements and methods of construction were similar to those used in the magazines at Fort Davis (see Figure 8). The rear magazine room was covered by compacted fill to a depth of 8 feet; in the front room the fill is not as thick, only 4.5 feet. The amount of erosional fill in the magazine room today resulted from the thickness of the compacted fill above the room as indicated by the Engineers' drawings of 1862, which give the correlation of angles of scarps at both ends of the magazines to determine the earth crest. Dimensions for the front magazine room are 18' by 20'. This is by far the largest of all the magazine rooms excavated.

Magazine earthworks extended on both sides of the passageway (see plan view Figure 17); archaeologically the post molds that supported the passageway roof opening were uncovered on the west side of the entrance way. Archaeological evidence on the east side of the passageway is more complicated; it indicates that the range of the ceiling structure in portions of disrupted sleepers are superimposed upon post molds that should be similar to those found to the west. Underneath the front portion of the magazine floor (plan view T-1, section A) a sleeper system of wood planks is shown.
by solid black lines, which represent partially burned and decomposed remains of sleepers. The dimensions of these sleepers are interesting to note. Archaeologically, on the top of the burned sleepers a non-structured pressed mash of additional wood was found in the west side of T-1; the floor boards were more or less intact when excavated. However, the east side of the floor boards did not exist. Only two parts of the wood floor along the west wall were available for analysis. Underneath the mash of wooden floor boards, the excavation was separated into two sections. South of the black line-enclosed boxes in the plan view of T-1, section A, the wood floor boards were laid on a north-south axis as indicated by the fibers of the wood grain. The floor boards to the north over the boxed in area were laid on an east-west axis. Boards south of the line were relatively thin. The average width was only one-half foot by 1 to 2 inches thick. (The reason for the east-west planking across the heavy sleepers was that loaded shot was stacked in this area.) The sleepers (blackened on the plan view of T-1, Figure 17) on a north-south axis are 3/10 to 4/10 of a foot wide with a distance between them of 2 to 2 1/2 feet. Nails excavated were 16-penny weight and were removed in situ from the sleepers. Three types of nails were found. The first type was a short nail perhaps only 1/10 of an inch long with a broad thin head. It was found only in the joints, and one wood sample was removed with the nails in situ indicating the structure of pine. The second type is a nail 2.5 inches long, thin, with a square-cut head. These were found along the sleepers, and particularly clustered at the corners, probably 10- to 12-penny nails. The third type is a nail of approximately 40- to 50-penny weight, found in only a few places, particularly at the juncture of sleepers where north-south sleepers adjoined the east-west joints. At the junction of one of the connections of the joints, 10- to 12-penny weight nails were hammered in from the bottom side of that floor indicating that the sleeper structure might have been pre-assembled, or that used lumber was incorporated. All nails are well-cut machined nails with well-made machine heads. Tar was found on the bottom sides of all sleepers, which was used to retard organic growth. This is similar to the information found at Fort Davis.

The Fort DuPont magazine was burned by freed men during their occupation. The vertical roof supports and ceiling supports collapsed and the magazine was allowed to cave in due to the fire destruction. Much of the earth compressed the original structure intact. The burned wood contained 60% water and it appears to be composed of layers sliced many times which indicates a high degree of compaction. Between the slices or layers of wood are large air spaces filled with water.

The exterior slope of the north parapet wall is at a 58° angle and descends from an elevation of 306' to the bottom of the scarp at an elevation of 289.3' (Figure 18). The seven foot, 4/10 inch width of the north parapet wall of the main ditch is considerably smaller than indicated in Figure 15, T-6. The counter-scarp slope is at a 60° angle. The location of the anchor beam and slope of the entrance bridge, the nature of its construction, and center support posts are projected into position according to analysis of archaeological data, the Barnard 1871 report, and
particularly the 1862 Engineers' drawings of Fort DuPont. Stratigraphy of T-10 (Figure 18) shows the bottom of the main ditch and counter-scarp and these are easily identifiable. However, a section of the upper counter-scarp had been destroyed and reworked by additional layering of soil which has recently accumulated; the clays were undoubtedly placed there during the construction of the asphalt road that encircles the present day fort.

From T-10 southward along the east of the main ditch, the east glacis and counter-scarp slope had been completely destroyed by recent activities according to archaeological findings (Figure 14), probably to provide fill for the road bed during the WCC work period. To prove the historical fact that work had been undertaken in the Fort in the form of landscaping since the WCC programs is impossible; it is our opinion that little or no landscaping had been accomplished since the WCC period.

Archaeological evidence found in T-5 (Figure 19) is interesting when compared to the evidence uncovered in T-3A, (Figure 16). The line of revetment posts angle is similar to the angle that was conjectured in T-3A. However, in T-3A stratigraphic information was not found on a horizontal plane to suggest the location of the wooden revetments and breast height's edge. In Figure 19 the angle of the gun embrasure cheek is conjectured from the location of the tan clay pockets seen in profiles of T-5. The clay probably served as a base to anchor gabians. In interpreting the plan view of T-5, a different angle for the gun embrasure cheek is projected than that on the plan view of stratigraphy. Cheeks of the embrasure, end in some cases slopes and magazines and traverses, were revetted with gabians. For gilling gabians, sand or turf or the trimmings of sod revetments, thoroughly rammed, were the best materials because the grass soon enveloped the breast work and formed a durable revetment even after the gabians themselves had decayed, McCormick (1967: 40). The turf-filled gabians were not affected by blasts of guns, whereas sand and earth were blown from them after a few discharges (Barnard 1871: 65).

However, when the embrasure angle is scaled onto Figure 14, (T-5 on the topographic map overlay) the angle matches the angle indicated in the Engineers' drawings of 1862, confirming the archaeological evidence. Also conjectured in Figure 9, T-5 is the edge of the banquette tread in a similar position when correlated to the Engineers' plan view drawing. Conjectured cross-section Figure 19 also illustrates the location of the revetment posts and the angle of the parapet wall from superior crest to the exterior crest as correlated with the stratigraphy. The scarp angle of 60° is based on the analysis of T-6, T-3, Figure 15, and the Engineers' drawings. An elevation five and one-half feet above the gun platform tread is the superior crest of the parapet wall, an embrasure sole is at a 4° angle. Excavations in T-5 did not produce any indications of jacks or sleepers to support the wooden gun platform treads or the platform descent angle, which was supposed to assist guns in rolling back after firing (Barnard 1861).

The excavation of the twelve trenches at Fort DuPont (see Figure 14) has provided stratigraphic data that enabled the reconstruction drawings; one transverse profile on the east-west axis, and one cross-section on the north-south axis to the fort.
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PROFILE OF MAGAZINE NO. 2
FORT DAVIS
LOOKING WEST

SCALE

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FEET

ONE FOOT DIAMETER CENTER SUPPORT BEAM

6' DIAMETER WALL BEAMS

FLOOR MAY HAVE CONTAINED
THAT DOOR FOR ACCESS TO BARREL

5 TO A HIGH FLICK PLANE OFobarrel
WASTE USED FOR FUELLING

PROFILE OF BARRELホール
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TRENDS IN HISTORICAL ARCHAEOLOGY: EDUCATION AND CONSERVATION

John L. Cotter

Education and conservation are inseparable in the future of historical archaeology, since they are the heart and soul of its purpose. If up-coming generations are not instructed and motivated in the study of the historical foundations of American civilization—motivated by active participation in research—then conservation will be a meaningless exercise in vanity on the individual, local, state and national levels. The purpose of this discussion is to present these aspects of historical archaeology in the light of historical perspective and present and future needs.

If historical archaeology involves the identification, interpretation and conservation of the artifacts of history, it can be observed that the United States and its antecedent colonial components began at a virtual zero point in the history of historical archaeology at the period of discovery. If there is one thing certain, it is that the discoverers and settlers were not concerned with the conservation of the cultural heritage of the Indians any more than they were interested in preserving the natural beauty of the wilderness about them. From the entrada of the Spanish into the Southwest to that of the Puritans into Massachusetts, the clergy were nearly unanimous in seeking to erase the artifacts and the memory of the religious and social heritage of the Indians, and the layman accepted and followed their lead. By the same token, the artifacts and landmarks of the settlers were held generally in low esteem by their immediate successors, and the colonists wasted little sentiment upon old structures and all but a very select few artifacts which could be regarded as heirlooms. In an expanding environment of new frontiers, new population and new settlements, that which was old was simply superseded and discarded when it was no longer useful. The conservation of objects symbolical of the past is practiced only by those who have these symbols about them and are moved to value them enough to conserve them. A notable exception, however, has been called to my attention by Stanley South, in the case of the Moravians at Bethabara in North Carolina who in 1770 put up a marker commemorating the original building on the spot, the 1752 log cabin erected by Hans Wagner which they tore down in 1767. When this marker wore out, they put up another one in 1801. In general, however, no sense of reverence, however occasionally applicable to their own heritage, was extended by settlers and frontiersmen towards the symbols of the pagan Indians any more than the Greeks or Romans conserved and revered the memorabilia and memorials of the barbaric Sythians or the Germanic tribes with whom the harbingers of Western Civilization contended.

Not that the young of the colonial period were not taught to value and cherish certain artifacts, but heirlooms were proportionately few and churches and public buildings were either in constant use or were ordinarily replaced and forgotten. Fire was a familiar agent of destruction and continual rebuilding was a way of life. There is no record of reverence at Jamestown toward the first two churches which gave way in 60 years to a third, all on the same spot, or toward the four statehouses that burned. In fact, when the Revolutionary War came to Williamsburg and passed, the destruction of the Governor's Palace and the courthouse left no will to restore them,
either for practical or sentimental reasons, for some 150 years, when the new nation had ripened into a mood for romance and nostalgia toward its founding.

Likewise, at Plymouth the ground upon which the Pilgrims first made it ashore prompted no one to consider Plymouth Rock until, in 1741, (G. F. Willison, Saints and Strangers, 1945) an old gentleman took it upon himself to proclaim he had heard that the "Rock" had received the first Pilgrim footsteps. This fact he had received at the age of six from a nonagenarian.

The first bustling years of colonization, conflict and the early decades of the new Republic saw no notable effort to conserve the heritage of the past in the archaeological sense. It was not until nearly half a century after the Declaration of Independence that the momentous occasion of the triumphal return of the Marquis de Lafayette as an elderly gentleman who was bourne through a triumphal arch over Chestnut Street, past the Pennsylvania State House, in 1824, quickened the popular will to regard the site of the signing of the Declaration and the adoption of the Constitution as the prime national landmark. It was not without due cause that architect Haviland proceeded thereafter to restore the Assembly Room to what he thought was its aspect during the signing ceremony, and William Strickland was commissioned to erect the present tower on the cracked and truncated foundation of the old one, as a fitting, if not historically relevant, embellishment to the old State House which had been patterned originally on the plan for an English nobleman's country seat.

Thereafter, national heritage landmarks lapsed into obscurity once more, and at mid-19th century the State House again was in danger of oblivion when it was put up for sale and removal, graced as it was by a dog pound in the cellar and a coffee bar in the hallway. Had not the City of Philadelphia purchased the entire square, the Independence Hall buildings would not have survived to come again into the patriotic consciousness during the Centennial celebrations of 1876. With this momentous commemoration the conservation of Independence Hall, together with that of the encampment site of Washington's Army at Valley Forge and the newly-hallowed Civil War battlefields, was assured.

At that time tourism had developed, with the vast spread of the railroads and fast transportation across the nation, so that not only the scenic wonders of Yellowstone and Yosemite and the Grand Canyon were becoming objectives for the adventurous and romantic, but also certain mysterious aboriginal ruins in the Rocky Mountain West and Southwest, such as Mesa Verde, Casa Grande and Montezuma Castle. By the same token, the stature of national heroes had grown in the public mind to the extent that Mount Vernon had been preserved as the first such private venture by an interested group of citizens—the Mount Vernon Ladies Society—to save an historical structure. As the 1900's approached, the lid was truly off the heritage pot, and it boiled away with rapidly increasing fury after the two world wars, aided by tourism and the enhanced value of historical attractions for the communities in which they were located. Combined with this manifestation was the desire for self-identification and proclamation that accompanied "manifest destiny," and the type of nationalism that has become the chief social phenomenon of the 20th century. The world-wide spread of the nationalist passion and the desire
to conserve heritage items is obvious in virtually every nation on earth, and need not be documented here.

The time is now at hand when the hundreds of millions spent to purchase, restore, preserve and exhibit historical sites is calling for a rapidly increasing supply of specialists—archaeological technicians, archaeological organizers, historical researchers, historical architects, museum curators and planners—together with a complement of special technical analysts: chemists, metallurgists, physicists; plus architectural historians, ceramic specialists, glass specialists, tureen specialists, and a host of others with unique and distinctive know-how and know-about in an endless series of fields, interests and specialties.

At the 1958 meeting of the American Anthropological Association in Washington, D. C., a symposium on historical archaeology was organized and chaired by the writer on the premise that the time was ripe for bringing to a focus the gathering interest in this branch of archaeology. It was at the summation of this meeting that he made the rash but prophetic statement that an organization would be formed to represent the interest here shown, and that it could be named the Society for Historical Archaeology with a journal, Historical Archaeology. It remained for Stanley South, however, to take immediate steps to organize the Conference on Historic Site Archaeology which held its first sessions at Gainesville in 1960.

By 1960 the writer had organized the first course in Methods and Problems of Historical Archaeology for the Department of American Civilization in the Graduate School of Arts and Sciences of the University of Pennsylvania. For this opportunity, thanks go to Dr. Anthony N. B. Garvan, Department Chairman, who had specialized in the concept of the artifact as cultural indicator at the University and during his association with the National Museum of History and Technology of the Smithsonian Institution. It may be appropriate to mention here some of the problems encountered in the teaching of this subject, and some of the methods used.

The course was initiated in the absence of any current archaeological investigations at local historical sites, with the exception of Independence National Historical Park. Since the course began in the fall, meeting once a week for two hours, the problem was to give a background in theoretical methodology, the history of technology and the relationship of historical research to planning and carrying out a field project—all before any field work was attempted. Logically, the presenter of such a course would hesitate to tackle a piece of field work after the class had been introduced to the theory of the subject at a time when the winter was closing in and the end of the term loomed before Christmas. The compromise was to devote weekends to visits to historical artifact collections, such as that at the Mercer Museum, Doylestown, and to such historical sites where archaeological work had been accomplished, as Independence, Hopewell Village, Morristown and Harpers Ferry—all National Park Service areas. Meanwhile the class initiated a world-wide tally of archaeological site conservation efforts by writing to embassies and foreign government agencies and universities. The returns indicated that the U. S. was by no means first in the conservation and research of historical sites.
In the spring term the opportunity was taken to offer special projects to certain students that would involve historical sites archival research and ground inspection. This was done at the site of the Jesuit Mission of 1613 on Fernald Point, Acadia National Park, and at St. Croix National Monument, scene of the settlement of DuMont and Champlain in 1604.

In time, it became apparent that students wanted more active field experience, and a summer course in field work was instituted, using Valley Forge State Park and two properties near there owned by the University, the DuPortail House and the Lafayette House. A winter field project was found in 1965 at the Man Full of Trouble Tavern in Philadelphia's Society Hill area near Dock Creek, where a private museum project welcomed the help of the class to investigate the floor of a basement passage. Literally a miniproject, this tiny area proved to have two brick floor levels above a deep fill, and contained datable artifacts ranging from a prehistoric aboriginal nether grinding stone to 20th century objects.

Several trends have by now become apparent in the training sought by graduate students in historical sites archaeology as offered in a department devoted to American studies. The classes tend to divide into those who intend to specialize in a related field, usually museum work or teaching, and those who resolve to make a career of historical archaeology. The latter are often people who have had previous training in field methodology and are sometimes from departments of anthropology at other universities. In the case of students in a similar class which I taught at Temple University in the Department of Anthropology at the invitation of Dr. Jacob W. Gruber the majority of students were those who had had previous field experience, were considering careers in archaeology, and wished to broaden the scope of their field into the historical scene. Others were education graduate students who wished to diversify and expand their frame of historical references. It is a small nucleus of highly-motivated graduate students skilled in archeological field techniques who form the cadre of future historical archaeologists.

Other universities now offer courses in historical archaeology. Steven Williams at Harvard instituted a seminar in the subject, but did not maintain it after he assumed other duties connected with the direction of the Peabody Museum. Bernard Fontana at the University of Arizona has continued to hold such a class, as have Edward Jelks at Illinois State University at Normal, Hale Smith at Florida State University, Edward Larrabee at Hunter College, Joel Shiner at Southern Methodist University, and several others. Collateral projects in the Northeast involving historical sites investigations have offered many opportunities to students to participate in field work. The University of Pennsylvania two years ago obtained a contract from the State Highway Department to initiate the Archaeological Survey of the Delaware Expressway, and so opened a field opportunity for some students from this and other universities. The National Park Service has employed students both to provide labor at projects through contractual agreements with universities and museums, and to assume direction of such projects for the Service, if they are qualified to work in Service areas, as temporary Government employees. At present, Temple University has contractual agreements to do archaeological investigations at Allegheny Portage and St. Croix Island areas of the Service.
In summary, a small group of highly motivated, well-trained and experienced students of archaeology has turned its attention to historical sites and the students are bent upon careers as historical archaeologists. They are preparing to ride the wave of historical sites investigation and conservation which is sweeping this and most other countries. Some of these students are working on doctorates, such as Paul Huey, Garry Stone, Barbara Liggett, Betty Cosans and Revell Carr at Penn, all of whom are members of the new Society for Historical Archaeology, and who are capable of setting up field projects and producing professional reports on their work which will include archival as well as ground investigation. Others are coming up from Temple University. The pattern is now being repeated at a number of other universities.
Allow me to preface my remarks by stating that I am carried on the payroll as an archaeological historian, a title which has certain implications in the context of this conference. Indeed, the present paper is a blend of cultural history, archaeology, and industrial archaeology. I hope that our purist colleagues will not be offended by the mixture.

Bricks are the commonest ceramics encountered in historical archaeology, but very little appears in the literature concerning their manufacture. Of all ceramics, brick is surrounded by the worst tangle of legend and folklore.

Brickmaking was the first industry attempted in English America, at Roanoke Island in 1585. At Jamestown, several large kilns produced brick early in the seventeenth century. In 1611, Sir Thomas Dale began building a city of brick on the upper James River.

Obviously, such a large quantity of brick could not have come from England. The sheer mass of brick used in Colonial houses precludes such assumptions. In addition to common sense, we have historical documents to support the thesis that most American brick was made on or near the construction site. The few bricks which were imported, probably were specialty items, such as fire brick and tile.

So much for the legendary "English Brick". A more persistent misconception centers about the use of brick dimensions as dating tools. Forman and his contemporaries failed elaborately to prove a relationship. Indeed, Richard Neve in 1726 catalogued no less than five different brick traditions then current in England. Miss Louise Bono, in a paper on nineteenth century brickwork, has suggested a correlation between brick dimensions and the changing styles of mortar joints. South and Harrington have proven conclusively that there is no simple correlation between brick dimension and absolute date.

The study of brick may therefore turn to more productive subjects, specifically, the study of brick manufacture as a vital industry in Colonial America.

Documentary sources on brickmaking usually relate to procurement of materials for specific projects. As a result, we have a good idea of prices and quantities, but we know very little about the everyday existence of Colonial brickmakers.

Temporary brick kilns, known as clamps, are almost inevitable features on the sites of eighteenth or nineteenth century structures. Such clamps were built for a specific project and abandoned. Sometimes they were outside the immediate site, as at the Lower Westover Church.
However, at the eighteenth century in Chuckatuck, Virginia, we found one clamp inside the church, and another clamp adjacent to the front door.

The clamp usually was built on the surface of the ground, although permanent kilns frequently were underground. A level clay bed was prepared, on which parallel rows of unburnt brick were laid as a footing. Bricks to be burnt were set on edge in a herringbone pattern along the benches, and the channels between the benches were filled with hardwood fuel. At about the fifth course, the brick was arched over the channels, and the clamp was built up for several more feet.

Sometimes, the entire floor of the clamp was paved, and the charge was placed on the brick floor. Very few clamps were built with such care.

The flimsy structure of an above-ground clamp is subject to erosion, ploughing, and other destructive forces, with the result that we seldom find more than a single fragmentary course surviving. This pitiful remnant usually consists of poorly-burnt bricks which further complicate the excavation by their tendency to fall apart.

Frequently, we find only a linear charcoal stain to indicate the fire channel, and hard yellow-clay strips where the benches stood. At Fort Randall, South Dakota, Mills insisted upon identifying the bench traces as strips of mortar.

Fortunately, we have a vast body of comparative material to help us interpret the remains by analogy. The art of making bricks in temporary clamps has survived in Brazil and other countries where labor is cheaper than transportation. In Williamsburg, bricks were made in such clamps until a few years ago, and a complete pictorial record was made of the process.

The first step in brickmaking is to refine the clay. Neve states that clay was dug in the autumn and allowed to weather all winter. The weathered clay, which has obtained some evenness by exposure to the elements, is then placed in a pugmill, where it is reduced to a uniform consistency. The pugmill usually consisted of a puddle of clay which was agitated by a rotating paddle pulled by a mule.

Clay from the pugmill is then forced into wooden moulds, one handful of clay being used for each brick. Large hands were definitely a professional asset. If two lumps of clay were used for a single brick, a weakness would occur where the two lumps joined. The excess clay is struck off the mould, and the bricks are stacked to dry slowly under cover.

Moulds were usually dusted with sand, which made it easier to remove the wet brick. Sand adhering to the faces of the bricks would melt during firing and form a protective coating. For this reason, early brick often display a sandy finish where the sand has not vitrified completely to form the glaze.
Air-dried bricks are then stacked in the clamp, and the fuel is ignited. A firing usually takes three or four days, but brickmakers differ in regard to the amount of time allowed for cooling.

In the clamps at Colonial Williamsburg, the ends of the channels were closed with steel doors. In Colonial clamps, the channels terminated at the shinlog, which was a pile of bricks stacked at the opening to control the draft (fig. 1, B, 7).

Decorative forms generally were fired with the common bricks, although they were more carefully placed in the clamps to avoid distortion. There is evidence that moulds were made especially for the decorative brick, but marks on some specimens indicate that special inserts were placed inside the standard moulds. In either case, all of the bricks, standard or decorative, from a single clamp, will exhibit the same general dimensions.

I do not mean to say that all of the bricks from a clamp will be the same size; merely that they were made from the same size moulds. Brick will vary tremendously within the same batch, according to their positions in the clamp. The soft brick, which did not shrink, were usually used in the body of the wall. Soft bricks are called soakers, probably because they will readily absorb moisture.

The smaller, harder bricks which had been fired near the channels were used on the exposed surfaces of the walls.

We are all familiar with the glazed headers common in eighteenth century brickwork. The headers were glazed by their proximity to the fire, although some reproducers of historic brickwork have helped the process by introducing ceramic glaze into the clamp.

The brick which spanned the arch of the clamp was glazed in its middle, and we frequently find glazed stretchers for this reason. Sometimes, the firing would run away, and the clamp would fuse into a mass of glazed rubble. Such a case was recently reported in Delaware.12

Each specially-made brick had a purpose and a name, which should be used whenever it is described. Large square bricks, used for hearths and paving, are more commonly called tiles. One such tile, found in the aisle of the 1624 church at Kecoughtan, Virginia,13 is nearly identical to one made in Surry County more than a century later. The Surry County specimen is significant because of the line across its middle. This line was produced when the divider was removed from a double mould. A mould with such a moveable divider has survived in Charles City County, Virginia.

At the John Halloes site in Virginia, we found a sandy, yellow sort of brick, which measured about 6½" long, 3" side, and 1½" deep. These bricks obviously were intended as a local substitute for the hard, yellow Dutch brick.
Whole bricks for construction were not the only products of a brick clamp, however. In at least two Virginia churches, the aisles were built of rubble from the clamps. In other cases, it is obvious that great quantities of rubble have been removed from clamps, probably for use as fill or road metalling.

Frequently, brickmaking sites will be found in woods, near ravines, or in places otherwise useless for farming or construction. They are frequently mistaken for churches and house sites, but the lack of mortar usually is a dead giveaway to their true nature.

What, then, would be the value of searching and excavating structures which are so unproductive and difficult to excavate?

**First:** They may help to confirm relationships between structures. A brickmaker probably would be an employee or slave of a master builder, and his techniques would be reflected in subtle features of his clamps.

**Second:** They complete the construction story of a structure, and may reveal otherwise-obscured details of a structure's history.

**Third:** Clay pits and excavated kiln pits were frequently used as trashpits. The claypit at Rosewell, in Virginia, is an exceptional example of such reuse of clay pits.

And, of course, they are part of the site, and therefore, cannot be overlooked.
Structures Associated with Working Brick Clamps

A. Base Structure: The arches at the ends of the clamps, and the benches on which bricks were stacked, were usually made of unburnt broken bricks, rejected during the drying process. The benches (3) and the channels (4) usually were 18", or two bricks, wide. They generally were about the length of two fuel logs, if they were fired from both ends. Most Virginia channels were 18' long; the number of channels would vary in proportion to the number of bricks to be fired.

B. Ready to Fire: The clamp has been prepared. The cover has been removed, but its upright posts (5) are left standing. Fuel (6) is stacked nearby, for the clamp cannot be permitted to cool during firing. Bricks (7) for the shinlog are stacked near the channel openings. The earthwall (8) was sometimes built up around the clamp ends, to retain heat and to strengthen the structure. This feature was omitted as often as not.

C. Section: The usual clamp was 5 or 6' high, but they sometimes were taller. All of the bricks to be fired were stood on their edges, and the brickmaker was careful to assure that there were air spaces between them. Fire would ascend through the air spaces in a fairly even distribution.

D. Excavated Detail: This drawing illustrates the sort of remains we usually find. The brickbat benches are frequently intact (3), although they could have been mined for fill material. The channel (4) usually is distinguished by charcoal dust and a black stain on the soil; even if all the brick has been displaced, the clamp outline usually can be traced by following these black streaks. Sometimes, brickmakers have left bricks (9) from the charge itself, although these bricks sometimes have deteriorated badly.
REFERENCES


13. The report of this excavation, with a chemical analysis of the brick and tile, is pending.
APPENDIX

Richard Neve's City and Country Purchaser and Builder's Dictionary is an indispensible reference for the historical archaeologist working in eighteenth century structures. The following notes on brick are from the 1726 Edition:

(entry "Bricks") ...in England, they are made for the most part of a yellowish colour'd fat Earth, somewhat reddish, (vulgurly called Lome) Mr. Leybourne saith, bricks are made of a reddish Earth, which ought to be digged before Winter, but not made into Bricks till the Spring Season....

...Now these which derive their Names from Accident, are Clinkers, Samel, or Sandal: Those from their Dimensions, are the great and small (or Statute) and Didoron, Tetradoron, and Pentadoron: Those from their Form and Figure, are Compass, Concave, Feather-edgy, and Triangular; those from Custom, Statute, and Cogging. Those from the Method of making, are Place and Stock Bricks, Those from the Place where, or by whom, are Dutch, or Flemish; and those from their Use, are Butteress or Pilaster, Coping and Paving.
WHAT ARCHEOLOGY CAN DO TO EXPAND HISTORICAL RESEARCH*

Stanley South

We might begin our examination of the relationship between archeology and historical research by looking at some of the areas where archeology is limited in its contribution to our knowledge. In the excavation of a church, for instance, the archeologist is able to determine specific information as to the architectural features of the structure at ground level and below, but his interpretations from this as to the religious belief of those who constructed the building and came to worship within its walls, must be based on analogy and historical research. Similarly, the excavation of a courthouse ruin would likely provide specific information as to its location, its size, the number of rooms, and the materials used, but would provide little toward our understanding of the social structure, the political organization, or the legal framework of the people who once used the courthouse building as an important center within their culture. The archeologist is often at a loss in interpreting directly from archeological data very meaningfully into these areas of culture; his evidence in this regard must be based on historical research, through the very thin thread of analogy from architectural features.

In the excavation of St. Philips Anglican Church ruins in the eighteenth-century English colonial town of Brunswick in North Carolina, details of interior construction were found in the form of floor and roof supporting footings, remains of paved aisles, window glass and wrought nails used in construction, and a few fragments of plaster moulding, but these will not, except through analogy on a very general level, carry the archeologist successfully into the ideology and beliefs represented by these remains. The courthouse ruin at Brunswick Town revealed the fact that it was the only square structure thus far found in the town, which would have set it apart from the house ruins and a partition wall pointed toward an unusual division of the area above. However, other than these clues, not a thing was found to aid in the interpretation of this structure as a courthouse; this information had to come from historical research, through correlation of the feature with an eighteenth-century map of the town. Now, without historical evidence the archeologist may have interpreted the structure as that of a public building, possibly that of a courthouse, and if through analogy he was able to do this, he then could, if he dared, project a general eighteenth-century colonial picture of political organization and legal and social relationships known to have been involved in courthouse generally; but the ice would be so thin without a firmer archeological support, that he would likely soon find himself swimming in the cold water of reality amid the jibes of his colleagues. This reality being that archeological data does not usually lend itself

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to interpretations extending very far into the area of religious, political or social organization.

What then, was archeology able to contribute toward the understanding of the situation relative to the courthouse at Brunswick Town? The historical references indicate that the courthouse was authorized with the creation of Brunswick County in 1764, and that it was blown down in a hurricane in 1769, and its location is shown on a 1769 map; nothing else is known from history. Archeology revealed that the structure was a building twenty-five feet square, with virtually no midden material inside the ruin other than a few fragments of eighteenth-century wine bottles. Outside the ruin, however, around the entire building, china from the 1790's to the 1820's were found, but none were found inside the walls of the ruin. Also found in abundance were objects from the period of the 1860's. These were buttons, bullets, percussion caps, etc., lost during the occupation of the site of Brunswick by Confederate forces at Fort Anderson, built over the ruins of the town. From this information the archeologist was able to construct the sequence of events as they apparently occurred at the courthouse site. It was used as a courthouse from the time of its construction (shortly after its authorization in 1764) until it was blown down in 1769. After this time its use is uncertain, but it is evident that it was covered by a floor during the period from around 1800 to about 1830, when the yard around the structure was used as a trash disposal area. This utilization could have come from someone living in the courthouse structure itself (from the fact that artifacts from this period did not find their way beneath the floor). The structure was possibly still standing in 1862 when construction of Fort Anderson began. At any rate, a structure was either built or added on the courthouse foundation at this time using cut nails of the period. It was used during the occupation of Fort Anderson, with midden and artifacts accumulating around it in considerable concentration. After the Civil War what was left of the structure may have rotted down, and for a hundred years no other artifacts found their way inside the foundation walls until the arrival of the archeologist.

This information still does not tell us a great deal about the courthouse as a public building, but it does reveal something of its use as a structure. The presence of occupation midden, therefore, has made a considerable difference in what the archeologist can say about the history of the structure. Through the recovery of physical objects, a sequence of events relating to the structure is now known that was not previously available. For example, buttons from the period of the Revolution, the War of 1812, and the Civil War, found in the yard, along with other artifacts, contributes toward a more specific knowledge than the two facts that the courthouse was built in 1764 and blown down in 1769.

In summary of the points made thus far, we see that archeology can contribute certain types of specific information relative to a particular place, such as the details of architectural features as well
as pinpointing their exact location, a temporal relationship, and something of the use in some cases to which the structure was put; but archeology is limited in its contribution outside the technological area. Archeology sometimes makes a considerable contribution in our understanding of the technology of particular crafts at various periods of time through the excavation of shops and industrial waste sites. The waste casting sprues and fragments of castings from a brassfoundry or silversmith shop, or the kiln waster dump of potters are valuable repositories for information relating to the evolutionary development of these technologies. Our attention tends to become focused on these sites due to their value to the archeologist. Such sites are those which he can "get his teeth into" as well as his trowel, in that they lend themselves to quantification and stratigraphic analysis as well as their basic "time capsule" character. However, there are other sites which do not so dramatically yield positive results. For instance, at the town of Bethabara in North Carolina, an eighteenth-century Moravian settlement, the maps and records revealed the location of the gunsmith shop, the Brothers' House, the blacksmith's shop, the millwright's house, the tailor shop, the Gemein Haus (church), the apothecary shop, the doctor's laboratory, and the pottery shop; with the exception of the pottery shop, the excavation of all of these ruins did not reveal a single clue that would have been sufficient to allow the archeologist to properly interpret the use of these structures! This would appear to be a somewhat dismal record for archeology, were there not other questions of interest than the limited one involving the specific function a particular structure served within the community of which it was a part.

One of these questions centers around the temporal relationship of the site, and is often one of the primary reasons the sponsors of archeological projects give as the reason for excavation. The means of determining this relate to the basic archeological method of stratigraphy, through which temporal relationships are most effectively established. This stratigraphic interest is also related to typology, through which the relative position of forms in time and space are studied. Therefore, besides its interest in establishing specific spatial locations and descriptions of historic sites and structures, historical archeology is also concerned with the temporal sequence that occurred on the site being studied. Studies of recovered artifacts in context from archeological sites can be made emphasizing the association of certain artifact types with particular individuals. This emphasis is frequently found in research for restoration, where the emphasis is often one historical figure associated with an historic site. There is a broader study, however, that is also of concern to the archeologist in terms of artifact analysis, and this is his concern for establishing general relationships between artifacts in time and

\[\text{\textsuperscript{1}}\text{A statement of the relationship of history and archeology is seen in: D. P. Dymond, }\textit{Archaeology for the Historian}. \text{ Historical Association. Pamphlet No. 71 (London: 1967).}\]
space which will be of value in future excavation interpretation by archeologists, and will have a feedback value on a broader level than that relating to a specific individual. The one relates closer to history in its concern with specifics, and the other to science in its general application.

The historical research on the colonial town ruin of Brunswick revealed a map of the town drawn in 1769, deed records for property in the town mentioning lot numbers, description of events that occurred in the town and of one house, the Royal Governor's mansion. The records did not include a description of the town, a description of an average citizen's house, a description or designation of any shops in the town, or data on the material objects which made up the household goods of the citizens. Through an analysis of the clues in the deed records, the lot plan for the town was reconstructed, thus allowing for the assignment of certain lots to individuals at particular periods of time. Through the correlation of a curved lot line on the 1769 map with a curved stone wall, a relationship between the lot plan and the ruins on the site was established, allowing for the association of particular ruins with individuals. Such relating of documentary data and archeological clues is characteristic of historical archeology, and constitutes a fascinating challenge for the archeologist, though such an emphasis is only one facet of the gem.

Through archeology at Brunswick, the positioning of lots and ruins in relation to each other was established, and ovens, smokehouses, wells, and major structures were located that were not shown or mentioned in any historical record. Architectural features of Brunswick Town houses were so well revealed that for the first time the type of houses built there was known, and conjectural drawings and models were constructed to illustrate this important information. The recovery of artifacts revealed that the major source of material objects for the Brunswick Town citizen came from England, with a small number of objects from Portugal, Italy, Boston and Philadelphia, China and the Malay peninsula, reflecting Brunswick's use as an important English colonial port. These objects were valuable as archeological anchors around which the interpretation of the eighteenth-century way of life in Brunswick was effected through exhibits in the visitor-center museum on the site.

The ceramics from an archeological site, as well as the glassware, brass objects, animal bone, plant seeds, and pollen all reflect the life pattern of the occupants who once lived there. In the case of Brunswick Town, where most of the homes were occupied only between the 1730's and 1775, this information can be closely related in some cases, to particular families and individuals. From an examination of the ceramics from the Brunswick Town houses on a more general level, through quantification, additional data has been recovered. By establishing the percentage relationship between various ceramic types from individual ruins in Brunswick Town and comparing these, it was established that these relationships were quite consistent in ruins that had a similar
time-span, and varied as the time-span varied. From this ceramic quantification data, it is now possible to arrive at a general temporal period for a ruin of unknown date on the Brunswick Town site. Such statistical studies are not often carried out under conditions controlled enough for meaningful results to be forthcoming, but the evidence is clear that statistical studies of ceramic types can produce data of value, and more such studies are being undertaken in order to further expand and test our data-recovery from historical sites. Bone, seeds, pollen, and cysts from human and animal parasites recovered from garbage dumps, privies, and cesspools have just begun to reveal their data through archeological recovery and analysis. Questions relating to social and health conditions, disease, parasites, diet, the source and availability of food in relation to the ecology of the area, as revealed through archeology and correlated with the historical references, are increasingly being asked by social scientists. Historical archeologists are trying to meet this broader challenge, allowing a more penetrating view into some of the areas of past patterned human behavior than has hitherto been possible through dealing with the traditional archeological materials. The historical archeologist has an increasingly expanding responsibility to inquire beyond the mere validation of an historic site through correlation with documentary evidence; beyond merely listing the presence or absence of artifact types for establishing the temporal position of the site; beyond the revealing of architectural features for the purpose of reconstruction and restoration; beyond exposing ruins for the entertainment of the visiting public to historic sites; and beyond the process of recovery and preservation of relics from the past hoarded into repositories and museums! His view must be as broad as the questions being asked by archeologists, sociologists, anthropologists, ecologists, biologists, archaeo-parasitologists and other scientists who are increasingly turning to historical archeology to reflect some light on their special problems and spheres of interest. However, although historical archeology is broadening its scope, the primary emphasis will continue to be in the area of material culture where so much must still be explored on the basic level of typology and stratigraphy in order to arrive at a better understanding, definition and temporal position of artifacts of many types found on historical sites. Too few historical archeologists are intimately involved with this basic level of the archeological process; this level of determination of the temporal position of artifact forms in time and space through typology, stratigraphy and historical research. Excellent examples of this basic approach are seen in John Goggin's classic studies of the Spanish Olive Jar and Spanish Majolica in the New World.


4John M. Goggin, Spanish Majolica in the New World. Yale University Publications in Anthropology, No. 72 (New Haven, Conn.: 1968).
where quantification, seriation and stratigraphy, basic archeological tools, were combined with documentary evidence to produce these masterful studies. Such work forms the backbone of historical archeology, demonstrating vividly the contrast between those studies oriented primarily toward a localized historical perspective, and the high-level, quantitative analysis of broader scientific applicability. Some practitioners see the historical archeology process as an extension of history, involved in the specifics of historical interpretation; others utilize the process as a search for broader goals involving the understanding of the evolution of forms in time and space as this development relates to a broad range of cultural and historical data. Neither the approach of history nor that of science should be emphasized as the orientation of historical archeology; rather the nature of the quest requires a utilization of the method and concepts of both history and science for the most effective execution of the process of historical archeology.

Although broader goals, such as those exemplified in John Goggin's work, are basic to historical archeology, the nature of many projects is such that the archeologist is often called on to elaborate more specifically an historical reference, and it is here that impressive results, from an historical point of view, are sometimes obtained. Historic shipwrecks, for instance, are ideal "time capsules" reflecting material culture at a specific time and place; the 1715 Plate Fleet wrecks off the Florida coast, for example. At the Moravian town of Salem, North Carolina, records dated 1793 include "A Collection of Faience-China Glazing Formulas," which had long been thought to be an item of passing interest to the Moravians, but not necessarily indicating that faience was made in Salem. However, during the excavation of the Fifth House in Salem in 1965, one sherd of tin-ash glazed ware was found, apparently of local manufacture. This discovery led to a greater interest in the 1793 formulas, and further research turned up an inventory of the Salem pottery in 1829 which listed white, blue, green and yellow faience glaze. Further excavation on the lot of the Fifth House carried out in the summer of 1968 revealed a deposit of six green-glazed faience bottles or vases thrown into a ditch as fill, having been taken from a kiln waster dump. Only a few more sherds of faience were found during the excavation, indicating definitely that the kiln for the manufacture of this ware was located elsewhere than on this lot, probably across the street where a kiln is known to have stood in the 1790's.

This example illustrates the value of historical archeology in expanding our knowledge of specifics in regard to historical research. We now know that not only was faience being made in Salem between 1793 and 1829,


but we can point to specific examples of the ware, and hopefully, when more excavation is carried out in Salem, the kiln waster dump itself will be found. An interesting fact in regard to this ware is that no known examples of this particular type ware is known to have survived in collections and museums.

A similar example is the evidence from historical research that the potter Rudolph Christ made a "fine pottery" at Bethabara from 1786 to 1789, but is was not until archeology revealed the kiln waster dump of this potter that the appearance of the "fine pottery" was known. Again no surviving pieces were known, but as a result of the knowledge of the ceramic forms revealed in the excavation, a mug of Rudolph Christ was recognized recently at an auction, and purchased by Old Salem, Inc.

An additional example of the way in which historical archeology can expand historical research through specifics is seen at Old Salem. The historical research revealed that William Ellis, an English potter, had demonstrated to Gottfried Aust, the Salem potter, the method of making "Queensware," and "tortoise-shell" ware on a visit to Salem in 1773. The 1968 excavation was carried out with the primary intention of locating a special kiln that was built for this purpose, and to, hopefully, find the kiln waster dump for this "fine pottery," either made by William Ellis himself, or by Aust and Rudolph Christ in later years. After a summer's excavation to a depth of from four to six feet over the entire Fifth House lot, a kiln was located, and nearby was a waster dump. However, the waster dump was older than the construction of the kiln, which was associated with an addition to the pottery shop known to have been added in 1797, but this fact did not diminish the importance of the objects recovered from the pre-1797 kiln waster dump. Here was found some of the finest pottery known to have been made in America in the eighteenth century, and the closest in form to that made by the Leeds and other creamware factories in England in the 1770's; here, indeed, was a dump of the "fine pottery" complete with double intertwined reeded handles and a variety of terminal and bisque spriggs, and fragments of true "Queensware" type plates with feathered edges. A mold for a sprigg was found by Brad Rauschenberg at the excavation of the First House in Salem in 1967,7 on the back of which were the initials "R. C.," indicating that Rudolph Christ was indeed capable of producing this finest of the "fine pottery," and pointing to this master potter as the creator of this excellent ware.

The role of archeology in expanding historical research in instances such as this is obvious; the historical research becomes the stimulus to open doors through which flows a wealth of data of inestimatable value, significantly increasing our knowledge.

These examples reveal an emphasis oriented toward the recovery, through historical archeology, of specific information relative to specific potters and their forms at a particular time and place; such a search is indeed an extension of history. However, if our goals relate only to these specific Moravian individuals and the ceramic wares produced by them in a limited period of time, then our concern is with the unique events of history. If we go beyond this particularized data collecting on the specific time and place level, and relate the study to the broader questions of direct diffusion, stimulus diffusion, acculturation, and social change within the Moravian community of Salem, we can see the light of the impact the English pottery making tradition had on a German ceramic base. If we see the specifics in reference to changing forms in time and space, then we are dealing with a process of broader scientific applicability; the process that is evolution. In either case, it is not the data that is the variable, but the theoretical framework through which we organize it that makes the difference between whether our results have an historical perspective or a broader generalized application.

Through historical archeology, we are indeed able to expand our research through an increased specificity as well as a broader understanding through greater generalization. If we limit ourselves and interpret our data only as it relates to specific individuals and events at a particular time and place (speaking only of the potters Aust and Christ and their individual wares), then we are distorting our view of the past by looking through a too narrow window of history, utilizing archeology only as a data collecting tool. In order to view the past fully through historical archeology, we not only utilize the specific theoretical tools of history, but also the broader generalizing concepts of history and scientific theory (speaking of the potters Aust and Christ so much as of the ceramic traditions each represents, and the expression of their forms in terms of these traditions seen against the background of the insulating culture in which they lived). It is through such a blending of the concepts of history and archeology, of the specific and the general, of pots and potness, that historical archeology makes its significant contribution in our continuing search for knowledge.
LETTER FROM MEXICO

Clyde D. Dollar

Ladies and gentlemen:

Once again I find myself sending you greetings via mail rather than being able to bring them to you in person. I must apologize for this impersonal but necessary approach, and please let me assure you that the distance separating us today by no means lessens my greetings to each of you or my wishes for the success of your conference.

One year ago, I presented to the members of the Conference on Historic Site Archaeology via means of a taped presentation a discussion of certain basic problems regarding theory and method in our profession. The response to this discussion was of such a nature as to indicate a professionally healthy regard for these topics. Feeling that the subject warranted further exploration, Mr. Stan South requested a number of experienced researchers in our field to write their critiques, remarks, criticisms, etc., on the subject of the discussion. The range and depth of these responses was most gratifying, and I understand that the original presentation, entitled "Some Thoughts on Theory and Method in Historical Archaeology," and the various critiques and my replies have now been published as a separate volume in the Conference on Historic Sites Journal series. In this form, the volume should serve as a source book and an important impetus for further exploration of the subject in the future. In the Epilogue section of this volume I suggested that further work in theory and method could perhaps best be fostered by the calling of a special conference on the subject, and I would like to repeat this suggestion at this time.

The completion of work on the "Thoughts" volume and the finish of the first season's excavation and research on the Chief Spotted Tail House site (at Rosebud, South Dakota) both occurred about the first of this past September, and I began work on another paper on the subject of theory and method. It was my intention to present this new paper, to be entitled "More Thoughts on Theory & Method..." etc. to the 1968 conference attendance. Unfortunately, work on all additional papers had to be shelved for the duration because of the development of yet another project. In early September, at the request of C.B.S. Films of Hollywood, I was loaned by the Rosebud Sioux Tribe, where I am currently employed as Consultant for Historical Resources, for work as Technical Advisor for history on the C.B.S. - Sandy Howard Productions of the film "A Man Called Horse," starring Richard Harris, Dame Judith Anderson, Manu Tupou, Jean Gascon, and Corinna Tsopei. The film is directed by Mr. Elliot Silverstein, holder of a Master's Degree in Fine Arts and recipient of numerous awards for film direction. This venture is so unique, and has such possibility for an interesting impact on certain aspects of the historical/anthropological professions in general, that I will take the liberty to describe some of its various aspects for your consideration.

"A Man Called Horse" is the story of a young Englishman who travels to America in the early 1820's. He hires three companions to take him hunting
in the Great Plains, and the drama opens with them on the outskirts of the Black Hills. There the small party is surprised by a group of Teton Dakota (Sioux), the three companions killed, and the Englishman taken captive. Because this particular group of Sioux have never encountered a white man before, and because the Englishman is discovered under circumstances that make him obviously more "white" than his three companions, he is taken captive, rather than killed, and made to live with the tribe as a "horse." From this point on (i.e., the first fifteen minutes of the script), there is only one white man in the remainder of the story, and the drama deals with his slow and sometimes very painful realization that these "savages" with whom he finds himself do in fact have a well developed sense of values, and they, in turn, come to look on their pet "horse" as a thing that slowly grows into a man (from the Sioux standpoint) based on his personal accomplishments (Sioux style) and not his wealth. The script is honest, brutal, and gives an excellent visual picture of these people's life.

The basic primary source materials for the movie are the journals and other papers of Lewis & Clark, Stephen H. Long, Bradbury, Brackenridge, Maximilian (Prince of Wied), George Catlin and Karl Bodmer. Even much that is contained in these journals, especially the later ones, must be used with great care as they describe encounters with Sioux who have had quite long trade associations directly with the white man—and our "Shunkawakan Dakota" Sioux have not as yet had these contacts. The existence of certain trade items, such as steel knives and axes, fragments of trade cloth, a few beads, and small amounts of vermillion dye is postulated on the basis of intertribal trading of very early trade goods. Our Sioux have not as yet received guns nor have they been exposed to the large amounts of trade goods that were to eventually be the basis for their wholesale cultural assimilation. In short, these are Indians the likes of which have probably not been seen since the year 1825. Those producing and directing this film have so far spared neither expense nor time in order to make it one of the most researched films about the early Plains Indians, and hopefully therefore, one of the most authentic dramas about such people. Ultimately, however, the most lasting measure of success of "A Man Called Horse" will not be the film's authenticity but rather its box office appeal, and ironically, this is one of the major factors that has led to the emphasis on the film's authenticity. If, through the use of historical research data, it will be possible to create an authentic picture of the Indians of the 1820's, then this in itself will be a drastically different and hopefully exciting visual experience for the movie goer as contrasted to the somewhat less than accurate versions of "Hollywood Indians" so frequently paraded across the screen. If "Horse" is a box office success, the way will be paved for a number of increasingly more authentic film dramas in the future. If it fails, then there seems to be every reason to believe that the normally seen "Hollywood Indians" will be with us for some time to come.

The making of the film has been a unique experience for me, to say the least, and already I have been given the opportunity to closely observe the inner workings of this world of fantasy. As of this early stage in the making of "A Man Called Horse," I can define at least two major professional problem areas. The first of these is determining just what is and what is not authentic and then filling in the "gray" areas of detail to achieve the realism necessary to make the film. The second of these is the problem of adequate and accurate transmission of this accumulation of historically
correct data to the score or more department heads and their assistants so that this information will then be translated into a visually accurate picture. I would like to point out that the historian rarely encounters either of these problems, but that the historical archaeologist frequently does albeit not on quite the same scale as for the making of a full feature length panavision color movie. I must admit that the scope of this project has kept my supply of midnight oil consistently low these past few months.

As already mentioned the drawings and journals of such observers as Catlin, Bodmer, Long, Bradbury, etc., have been used as primary source material for the film's research. Unfortunately, even without allowing for certain critical exclusions of some of this material (such as Catlin's drawings of Sioux who had had rather extensive trade contacts) by no means is all of this material sufficient to recreate the total daily life of a group of 1825 nonwhite contact Sioux. To fill in these gaps and gray areas in our knowledge, some original research has been necessary. For example, by the time this is being read to the assembled conference, we will have filmed (in color) the first shots of a sixty lodge tipi village. The major problem was what would these tipis have looked like? After critically examining all of the primary source data, I arrived at the conclusion that there was not sufficient evidence in the material to construct the tipi coloring and designs based solely on historical evidence, and I daresay that anyone else familiar with the primary sources will agree. Therefore, research into the means of obtaining both paint and dye colors was necessary, and this was done by actually obtaining the earth pigments, processing them according to the earliest known formulas and then devising a color chart based on the results. The same type of a color chart was constructed for dyed quill work by using early datable specimens of this type for color reference. Next, in order to get some idea of the design elements for such tipis, it was necessary to refer to the few fragmentary (and usually background) drawings of tipis found in the Catlin and Bodmer drawings. Unfortunately, the Sioux tipis drawn by these two artists are rendered in black and white and are far from being fully detailed—certainly not detailed enough to critically recreate and make into a believable tipi village living situation. Therefore, by taking the colors derived from the earth paints (in other words, those colors that could have been used by the Sioux of this place and period) and adding these colors to the historically derived design components, plus using imagination within a Sioux art framework, we were able to construct a large tipi village in the image and likeness of the 1825 Teton Dakota people. The result is quite striking, and if for no other reason but to see this one set, I would invite you to see the movie.

This was but one of many "gray" areas that required considerable research before sets, costumes, and props could be presented on film. In fact, the deeper I got into researching and planning for the specific shots of the film, the more I realized just how inadequately specific is the historical data contained in the primary source material for the period. Incidentally, a full bibliography of research material used in making the film will be available in about four months, and should anyone be interested in obtaining a copy, please write me in care of the Rosebud Sioux Reservation, Rosebud, South Dakota. Hopefully, I will also have available by that time a few copies of the annotated script which will
give source references for the specifics of the film.

The other problem area, that of being able to transmit historical knowledge into what the cinema world calls "visual reality," has been most enlightening to me. First, this requires that even the smallest detail must somehow be correctly and adequately transmitted to the Prop Master, the Art Director, the Costume Designer, the Wardrobe Director, the Make-up Artist, the Hairdresser, and the Special Effects Department. Unfortunately, the previous experience of all these individuals in making "Indian" movies was actually a detriment to their making this particular movie, but I soon found all of these various department heads to be most willing to cooperate and eager to "make it right" historically. In fact, this feeling has become contagious, and now even the camera technicians and set grips, who normally would not be the least concerned with such things, are alert to possible errors. However, the making of a movie is astonishingly complicated at times, and usually there are more than one hundred people physically on the set for any given shot. Under these circumstances, accidental error is almost certain to occur at one time or another. To help preclude as much error as possible, Mr. Silverstein, the film's director, has allowed me the privilege of being directly on camera for each shot as it is being made. As you can see, the historian's job in making a film certainly does not end with the preshooting research; he must also be Advisor, Censor, and Critical Scrutinizer of Every Detail each time the cameras roll.

The making of "A Man Called Horse" has been of great interest principally because it is a massive experiment in historical interpretation. Furthermore, it is of interest because the film represents an opportunity to correlate anthropological, ethnological, and historical data and then translate all of this material into a visual (and therefore emotional) image of a cultural group. The making of the film has also presented me with the opportunity to extend research on this period into very specific areas, and Mr. Silverstein and I are planning to coauthor a publication which will discuss the film and its historical and dramatic ramifications in more detail.

In the meantime, however, I sincerely hope that you will enjoy the film, and I will be looking forward to discussing it further with you in person.
THE CRISIS OF IDENTITY - HISTORY AND ANTHROPOLOGY*

Iain C. Walker

It would be unfortunate if the fire (and smoke) generated by my paper "Historic Archaeology - Methods and Principles" (Walker 1968a) and by the Historical Archaeology Forum 1968 (South (ed.) 1968) were to continue in a rebuttal-counter-rebuttal series of articles, for as the sage says, of the making of many books there is no end and much reading is a weariness of the flesh. I have no wish to indulge in what those of us who like polysyllabic and "neologistic" phrasology would call logomachy or hermeneutical declarations within a formally structured liturgical context - I had much rather talk clay tobacco pipes than philosophy - but it is clear that part of the controversy which has arisen has done so from a confusion of terminology and especially as to what constitutes history. As it appears that I have unwittingly been made the representative of some hypothetical history faction within the field I should like to clarify what I mean by history. I have adopted part of the title of the Cleland and Fitting article in the Forum 1968 publication (Cleland and Fitting 1968) because of all the papers there, this one most clearly represents an opposite pole of thought to my own and most obviously spotlights the problem, some parts of which I hope to resolve. I shall comment briefly on their concepts of theory, low and high level archaeology and history.

I was rather surprised to read (Cleland and Fitting 1968: 126) that I favour an anti-theoretical stance - I had thought that my paper to which these writers object was, in its own humble way, not utterly devoid of theory but presumably they at any rate do not think so. Presumably, they object to my criticism of Willey and Phillips' championing the phrase "American archaeology is anthropology or it is nothing" and the assertion that unless an American archaeologist is prepared to dispense with theory entirely he should take a stand on some of the basic questions of anthropological theory. Speaking as a (North) American archaeologist I find this assertion not only arrogant - were I to find it in an undergraduate's paper I should write "justify" in large red letters in the margin - but also fallacious, for I can take my stand on general British archaeological theory (cf Moore 1968: 57, 57-8) and entirely ignore general American anthropological theory. And if, as Cleland and Fitting suggest, I have been reading the wrong books, what are some of the "dozen or more better sources [than Taylor and Binford] for [American] anthropological theory in archaeology" - not, I trust, Chang 1967. I am quite prepared to read them (moreso, perhaps, than Cleland and Fitting and prepared to read books on historical philosophy, though if they want to read real anti-theory I suggest Higgs and Jarman (1969) on the origins of agriculture).

I also see (p. 136) I am supposed to advocate "low level" archaeology - well, maybe so, but I should rather use the artefact descriptions in the "low level" works of Noël Hume (1968a; 1968b - so defined by Cleland and Fitting, p. 134) than those in the "high level" work - assuming Fitting practices what he preaches - of Late Woodland Cultures of Southeastern Michigan (Fitting 1965). Further, it is malicious to compare, as Cleland and Fitting do, Noël Hume's reports with that of South on the Aust pottery at Bethabara.

*This paper was received too late to appear on the program.
(South 1967) - it is irrelevant to compare the interpretation possible from the excavation of waster dumps representing the products of one man over a period of years and the description of garbage, casual remains, and fragmentary structures which most of us are stuck with excavating. There is also an unwarranted implication that Noël Hume does not go on to interpret his evidence. That Cleland and Fitting lack familiarity with artefacts is strongly suggested by their definition of the fourth objectionable trend they see in historical archaeology (pp. 130-1) which, briefly paraphrased, says seeing is believing and pours scorn on those whose study has enabled them to identify differences not obvious to Cleland and Fitting.

With their second trend - work done in an intellectual vacuum - I am in complete agreement, having already emphasized this (Walker 1968a: 27). I also have some agreement with their fifth trend - that restorations have produced a carnival atmosphere unconducive to genuine research and where low-level work such as analysis of structural evidence takes precedence over more intellectual work. I doubt if anyone who has worked on a restoration would describe the atmosphere as carnival-like, but certainly the shortcoming of most such projects have brought disrepute to the field and certainly low-level work has been encouraged (cf Walker 1968a: 27, 30-1; 1968b: 116-21). Artefact study and publication in particular have been severely repressed, and the emphasis on low-level work has reinforced the attitude that an archaeologist can be hired whose experience has been entirely outside the historical field - the principle that once one can dig one sort of site, one can de facto dig any sort. It can only be said in some mitigation that not all of us work in the fields of Academe, that there are people on reconstructions who do fight a continuous battle to keep their work within professional and ethical standards, and that the production at Bethabara of high-level work suggests tourists-attraction projects can be made viable research projects. Further, without the stimulus of filthy lucre poured in to erect tourist-orientated restorations historical archaeology would not be in the state of rapid expansion and considerable potential that it is now: the very increase of work and informed opinion, by evolving the Historic Site Archaeology Conferences and the Society for Historical Archaeology for example, is tending to bring pressure to bear on organizations still doing unethical work.

Cleland and Fitting (p. 126) apparently regard me as an historian. This is not so, as I specifically stated (1968a: 23) that I had been trained in European prehistoric archaeology - I hope my subsequent remarks on historians (1968b: 120) have emphasized I am not one. I have excavated probably as long as have Cleland and Fitting, who are former biology and communications research students respectively (p. 126), and the historical bias I have is simply the reflection of British-taught prehistoric archaeology magnified to Cleland and Fitting by their own training being entirely devoid of history and historical methodology. The fact that I find it convenient to express myself within some of the general terms of historical philosophy does not make me an historian any more than does my quoting Beveridge, Collingwood, or Housman in my first article make me an animal pathologist, a metaphysical philosopher, or a classicist.

The real crux of the issue here is the definition of history. Cleland and Fitting (p. 132), in an excellent example of the neat and artificial
This chart betrays an unbelievably naive idea of what history is. If I outline the events of the American Revolution in terms of dates, battles, and treaties, then I suppose this is low-level history (though it is still based on a vast collection of sources); if I discuss the American Revolution in terms of British colonial policy, French intervention, war in Europe, military strategy and tactics, and political peace settlement, then I am writing high-level history. Similarly there is low-level archaeology — the data — and high-level — the interpretation. Interchange between these levels of archaeology and the equivalents in history are far more complex than the chart would like us to believe. The low-level archaeologist excavating a Revolutionary War site will use historical dates as a preliminary framework for his work; the low-level historian will mention the fort in his list of battles. The high-level historian will quote archaeological conclusions on points where documentary sources have not been clear but will not rehearse the stratigraphic argument; the high-level archaeologist will quote the historian's explanation of why the fort was built where it was instead of on more defensible ground half a mile away but will not repeat the ten pages of tedious detail wherein the historian examines masses of government papers and shows that the Treasury who advocated cheapness won an argument with the military who advocated a strategically-placed fort. Clearly, both fields have their own low and high levels and each field has considerable independence from the other — they do not merge into one organic whole at some lofty summit. Here Lies Virginia is subtitled "an archaeologist's view of colonial life and history" — an historian's view of the same field would be rather different (and, I suggest, less interesting, for archaeologists by the very nature of their work have to use more historical data than an historian needs archaeological data to write a 300-page book on the same subject).

That a limited definition of history is quite widespread among excavators of historical sites has only become clear to me after reading some of the papers contributed to Forum 1968 and a copy (generously supplied in advance of publication by its author) of South's paper "What Archeology Can Do to Expand Historical Research" elsewhere in this volume. South, who is certainly one of the three or four outstanding historical archaeologists, talks of the "unique events" of history beyond which if we go to study the broader impact and "direct diffusion, stimulus diffusion, acculturation, and social change, [as for example] within the Moravian community of Salem, seen in the light of the impact the English pottery-making tradition had on a German ceramic base; if we see the specifics in reference to changing forms in time and space, then we are dealing with a process of broader scientific applicability; the process that is evolution". I agree entirely with this definition.
of why we excavate - the only change I should make would be to substitute "history" for "evolution". To me, history is the study of the past, specifically man's past, and its relationship to the present: it is the end-product of a truly interdisciplinary approach. As Tervelyan says (1927: 26), historians not only collect facts - they think about them. The specific things and events which history means to South and Cleland and Fitting are to me only the raw data - "low level" if you like - of history. Thus the suggestion that anthropology must become history or become nothing is not just a smart remark to top 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. It is thus clear my aims, if not all my methods, are the same as those proposed by people in the field working from an anthropology basis.

Similarly, archaeology is to me not merely fieldwork - excavation and artefact analysis - as Cleland and Fitting define it (p. 132). It is not "non-temporal-particularizing", it is not even "sub-orbital-peculiarizing" or "post-prandial-perambulating" - it is the excavation, study, and interpretation of sites, as I have emphasized twice before (Walker 1968a: 24; 1968b: 106); and with this definition it merges without any straight-line divisions into my definition of history.*

Every excavation and every artefact study provides us with information which may rehabilitate old theories, discredit current one, result in new ones, or simply add one more enigma to our present knowledge. The relationships between the various aspects of our work have a fluency which evades analysis if we attempt to stop it dead at any given moment and dissect it, and it is this artificial dissection which I see as a great weakness in the theory of some American anthropologists. No scheme is sacrosanct, no terminology perfect (though if some social scientists achieved literacy it would improve matters) - when an English historian stated that "All accepted truths, just because they are accepted tend to become lies" (Hill 1965: ix) he was not merely indulging in paradox nor was he advocating nihilism: he was emphasizing that parrot repetition blinds us to fresh truths, that truth changes with knowledge, and that knowledge - which is presumably what we are all after - is a continuously-changing field.

South, to whom I am indebted for a discussion of terminology following my reading of his paper noted above and his reading an earlier version of this paper, has rightly pointed out that a problem obvious in historical works is the mixture of raw data and interpretation which is sometimes difficult

*Seven years ago Fontana, in an unpublished paper "History: Myth or Albatross" stressed that the artificial lines drawn between university subjects, history, geography, anthropology, and all the others, have a confining - and in the complexities of modern learning, an increasingly confining - effect on intellectual work. It seems to me that it is this solidifying of the fluid and variable that threatens our newly-emerging field. I am grateful to Fontana for sending me a copy of this paper.
for a subsequent researcher to separate. He also points out that one would not in the same breath mix the raw data of excavation—artefacts, stratigraphic relationships, soil types, etc.—and the interpretation of these features. This is true, but the division is never as complete as Cleland and Fitting's chart would imply. We keep our data in appendices or something similar, separate from our site interpretation, but this does not remove all interpretive contamination from this data, still less does it make it (to me data is an English collective singular, not a Latin plural) objective.

Presumably identifying pottery as pottery involves an interpretation of the raw data. Certainly to decide which pieces are stoneware, which porcelain, or whatever, involves interpretation—if it does not, then descriptions such as "white glazed pottery" appear. Equally, unless one knows a pipestem is a pipestem it can be listed as a tubular bead. If after describing a pipe-bowl we say it is of Oswald's class 9c we are taking the data another step farther. (If we comment that Oswald's dates for this type of c. 1680-1730 can be extended on North American evidence through variants to the end of the 18th century then this is yet another step—personally here I should remove the discussion to the interpretation section.)

In this section, however, I should be drawing on all the relevant information in my data section and repeating many of the observations made there—for example, the description of a pipemaker's mark—and relating it to available published and unpublished evidence, as well as interpreting this in the light of the totality of evidence from the site.

Thus while the report has a section on data and another on interpretation there is a continuous interaction between them. This is why I maintain my history (South's evolution) and my archaeology merge without any straight-line divisions.

Historians have far more complex problems of interpretation than those which so far face archaeologists, and this is why in history (in its orthodox sense) facts and interpretation can appear to be poorly separated. A much higher percentage of historical work is synthesis and interpretation of primary, secondary, and all sorts of intermediary material. If I make a study of a certain type of clay pipe found over a certain area and/or period (as distinct from discussing pipe evidence in a site report) my raw data is not included in separate appendices but becomes in effect references cited and in selected cases illustrations or verbal descriptions. Most historical works fit into this category. For historians their raw data is contemporary or contemporary-derived accounts whether monastic chronicles, treaties, newspapers, tape-recordings, trade directories, patents, hymnals, pamphlets, records of government enquiries, wills, poems, certificates of birth, death, and marriage, accounts by pet historians of medieval monarchs, acts of parliament, novels, and so on in endless lists. The utter lack of objectivity in all these is obvious—"facts" could well be added to "lies, damn lies, and statistics". Trevelyan noted that much of the business of historians is correcting and supplementing each other and in a memorable passage the purpleness or which approaches ultra-violet talked of the stray wreckage, corks, and broken planks cast up on the shore by Time from which we can argue much and guess more but which can never describe the great ship that has gone down into the deep (Trevelyan 1927: 7,8). The writings of Polydore Vergil,
Henry VII's pet historian have (together with his destruction of manuscripts disagreeing with his writings) contrived so to blacken the character of Richard III (Henry's predecessor killed in battle) that there are still two historians of some repute who flatly maintain, denying all evidence to the contrary, that Richard murdered the Princess in the Tower when in fact there is circumstantial evidence to suggest Henry was responsible.

The fact is that historians have such a vast amount of data available in many of their fields that it is impossible to digest it all. Anyone doubting this should read the 900-plus pages of *The Making of the English Working Class* (Thompson 1965), consider the author's criticisms of previous studies compared to the depth of his own sources used, and - in the 1968 revised edition - examine the 24 pages of further evidence rebutting statements of critics who had themselves produced evidence to disagree with the first edition. It is a sobering thought that as our own field expands we too are going to be faced with a rapid and continuous increase of knowledge and that the time will come when we will have to grapple with similar mountains of evidence and to attempt to synthesize and interpret it.

In conclusion, 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 with all their mechanistic chest-of-drawers concepts, their jargon, their disbelief in artefact evidence that cannot be put on punchcards, and their espousal of bogus objectivity, can re-create 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. We are all striving to reach the same goal, and only time will tell which if any of us have found the path which yields the least inaccurate conclusions.

The earlier version of this paper sent to South was communicated to Fitting, and I am grateful to the latter for sending me comments on it (Fitting was also sent a copy of the final version in advance of publication).

Only one of Fitting's comments calls for comment here. This concerns my remarks about the apparent lack of historical training on the part of Cleland and Fitting. Fitting indicates that though he has never written a report on an historical excavation he has had considerable training in history and historical methodology, and I therefore withdraw the imputation I made to the contrary. I must emphasize, however, that I disagree with the outlook on history as expressed in the article as strongly as ever: evidently historical methodology and philosophy in North America differ as radically from British tradition as does American archaeology from British. Here we will agree to disagree.
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THE CERAMIC WARE OF THE POTTER RUDOLPH CHRIST AT
BETHABARA AND SALEM, NORTH CAROLINA, 1786-1821

Stanley South

Excavations at the eighteenth century Moravian town site of Bethabara, North Carolina, in 1966, revealed that the potter Rudolph Christ was making a polychrome mottled glazed slipware using English type molds, between 1786 and 1789, during which time he operated a shop there. In 1789, after the death of the master potter Gottfried Aust, Christ moved to Salem and took over the management of the pottery shop, where he remained as master potter until he retired in 1821. He continued to make the "Tortoise-shell" ware, as well as milk pots, pans and other utilitarian forms.

Excavations at Salem in 1968, and documents found in the Salem Archives, revealed that Christ was making tin-enamelled ware, or faience in white, blue, green and yellow, after 1793, and fragments of white and green glazed faience were recovered. Some of these were painted with blue designs over a white glaze, on ware made on English type plate molds. The use of such molds was introduced by William Ellis into the Salem pottery shop in 1773, but it was apparently many years later before Christ was able to go into production with such plates.

Of particular importance in the 1968 excavations was the discovery of a kiln waster dump in which the finest of the "fine pottery" made by Christ was discovered. Here examples of sprigged ware with pearl beading around the rim and base, and double intertwined reeded handles with applied terminal spriggs were found. The close relationship to this polychrome glazed "Tortoise-shell" ware to examples made at the Leeds factories in England is remarkable.

The excavations at Bethabara and Salem have resulted in the recovery of "Tortoise-shell", mold made zoomorphic bottles, sauceboats, dishes, plates made on molds in patterns of "Queens", "Royal", and "Feather" designs. Also found to have been made was "faience", and coloring and figuring of porcelain, mugs and teapots with double intertwined handles and terminal spriggs, as well as white stoneware, and an olive-green glazed stoneware product. In the face of these discoveries the fact remains that the surface has only been scratched at Salem, and excavation in the years to come should produce more surprises from the pottery shop of Gottfried Aust and Rudolph Christ.

This statement is only an outline of the ware found to have been made by Rudolph Christ at Bethabara and Salem as revealed in excavations in 1966 and 1968. Since the paper dealing with this material was presented at the Ninth Annual Conference on Historic Site Archaeology in Knoxville, it has been revised and expanded so that it is too long for inclusion in this volume, and only this summary is presented. However, the complete paper is to be published as a technical paper of the Institute of Archeology and Anthropology at the University of South Carolina.
Ware of Rudolph Christ from the Christ-Krause kiln waster dump at Bethabara, North Carolina. The top center sauce boat is a reconstruction made from the plaster mold restored from fragments found in the waster dump. The handle is a form sometimes found on sauceboats of this type, though a double inter-twined reeded handle may have been used on such pieces. The polychrome "Tortoise-shell" plates and bowls are covered with a white slip, onto which splotches of manganese and copper glazing was applied, producing a brown and green coloration on a bright yellow or cream-colored background. The bowl in the lower right has a light buff paste, and is, therefore, closer related to the English examples than are the red-paste slip-coated pieces.
Figure Two

Ware of Rudolph Christ Recovered from Excavations at Salem, North Carolina.

Upper Left: Green tin-enamelled glazed "faience" vase, made by Christ after 1793.

Upper Right: "Tortoise-shell" ware teapot with pearl-beaded rim and base, with double intertwined, reeded handles with applied terminal spriggs; perhaps the finest Christ piece yet discovered.

Lower Left: Brown lead glazed mug with rouletted rim similar to those found on English scratch-blue salt-glazed stoneware; with double intertwined, reeded handles with applied terminal spriggs typically found on Christ's ware from Salem.

Lower Right: Bisque fired mug with "bead and reel" gadrooning in relief, with double intertwined handles with applied terminal spriggs. This particular form resembles closely early Leeds mugs.
PART 2

HISTORICAL ARCHAEOLOGY FORUM

Centered Around a Paper on Ceramics

by

Garry Wheeler Stone
In the seventeenth and eighteenth centuries the executors of estates were required by law to inventory the decedents' property. In the paper following this lengthy introduction, I have tried to show how these inventories can be used to understand the cultural context and importance of particular artifacts. Specifically, I have tried to show that in colonial Suffolk County, Massachusetts, these probate court records can be utilized to trace the introduction and diffusion of oriental porcelain, delft, white saltglaze stoneware, and the refined earthenwares. Even more important, the analysis shows that porcelain was introduced by Boston's mercantile elite, quickly was acquired by the commercial and maritime groups, but only slowly diffused among the artisan classes. In rural Suffolk County these extravagant imports were almost ignored until the eve of the American Revolution. The presence of porcelain is only partially related to inventory value, and reflects the life styles of the different groups more than their purchasing power. These relationships can be measured, and potentially could be a great deal of help to the curator's or archaeologist's understanding of his artifacts.

At the moment, however, this is a very preliminary study. Before working out the mathematical relationships between wealth, occupation, and the ownership of porcelain, it would be desirable to double the present sample. Moreover, this exercise has pointed out several areas in which it would be useful to sharpen the analysis.

The listed value of an individual's estate is only of limited usefulness in estimating his economic status, due to the wildly fluctuating values of colonial currencies. In the future, in order to make more meaningful comparisons of economic position between samples, notes on the types of property inventoried will be taken to allow the construction of rough economic stratification for each decade. For colonial Boston, real estate, shipping, and merchandise are key variables in reconstructing the economic hierarchy.

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1For Massachusetts see: The Colonial Laws of Massachusetts (Boston, 1887), 157-58.

2The author is a graduate student in the Department of American Civilization at the University of Pennsylvania where Dr. Anthony N. B. Garvan has pioneered in utilizing material artifacts as clues to cultural behavior. The original version of this paper was written for a course in colonial history given by Dr. Richard S. Dunn.

*A Preliminary Study With Divers Comments Thereon, and Sundry Suggestions
In the future, more attention will be paid to the position of ceramics within the inventories. In what rooms, and how, are they displayed? In many of the 1720 and 1730 Boston inventories, ceramics used for entertaining appear in upstairs chambers. Barbara Teller has found the same pattern in Providence, Rhode Island. Our present concept of a "bed room" is a nineteenth century development. Further work along these lines can reveal more about how colonial ceramics were used and displayed.

After expanding the sample, it may then be profitable to analyze the kinds of items represented by different ceramic types. They do not seem to have been considered equally suitable for all vessels. Delft was apparently not appropriate for hot liquids, while stoneware was widely used for mugs. Closer examination might reveal a great deal more about colonial evaluations of ceramic types. One result of this kind of research, though, is that it reveals not only what kinds of information can be extracted from the record under study, but what kinds of information must be sought elsewhere. The Suffolk County probate records enumerate expensive ceramics. They do not, however, describe the stylistic characteristics of any of the listed pieces, and they provide only the most fragmentary information about common earthenwares.

The inexpensive pieces of unrefined earthenware are poorly described in the records. This reflects their relative lack of economic importance, and probably also a lack of prestige. Their reporting is so fragmentary, that even their omission from inventories may not be certain evidence of their absence from the decedent's household. This is particularly true of rural areas where there was a cultural bias against careful enumeration of ceramics. Whereas for Boston residents the making of detailed inventories was a natural result of commercial training, seventeenth and eighteenth century agriculture did not develop similar habits. Thus in the countryside, while land was usually carefully described, listings of household goods are frequently very abbreviated. Only rarely are milk pans, milk pots, or butter crocks listed. These items and other common earthenware forms -- pans, pots, platters, pitchers, porringer, and mugs -- are enumerated in mercantile inventories. Study of these lists, custom records, advertisements, and kiln sites may determine what earthenware forms were available, and perhaps even in what quantities, but they can tell us very little about how they were distributed. Archaeology will apparently remain an essential technique in determining what kinds of earthen kitchen and dairy utensils were present in representative households.

The listings of fine ceramics are more helpful. Most eighteenth century Boston inventories give their number, function, and type. Representative of the more detailed inventories is this 1730 listing of

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4 Suffolk County, Massachusetts, Probate Records, 1650-1850 (Film: Graphic Microfilm of New England, n.p., n.d.), XXII, 48; XXVII, 442; LXIX, 80. Hereafter cited by volume and page only.
the ceramics in William Welsted's front chamber.  

<table>
<thead>
<tr>
<th>Description</th>
<th>Price</th>
</tr>
</thead>
<tbody>
<tr>
<td>9 Blue and White China saucers, 5 Cups, 1 Stone Tea and Milk pot</td>
<td>£ 1:10-</td>
</tr>
<tr>
<td>2 small China Bowls with Covers</td>
<td>£ 1:16-</td>
</tr>
<tr>
<td>2 Blue and white China stone Basons</td>
<td>16</td>
</tr>
<tr>
<td>2 China Images @ 10/</td>
<td>1</td>
</tr>
<tr>
<td>8 Blue and white China Cups @ 1/</td>
<td>8</td>
</tr>
<tr>
<td>5 Brown China Ditto and 1 Cracked one</td>
<td>10</td>
</tr>
<tr>
<td>1 large Blue and white Earthen Bowl and a sugar pot</td>
<td>10-</td>
</tr>
<tr>
<td>1 Doz. Delph pickle saucers</td>
<td>8</td>
</tr>
<tr>
<td>1 Doz. Blue and white Delph plates</td>
<td>2</td>
</tr>
<tr>
<td>6 Blue and White Earthen Dishes</td>
<td>2</td>
</tr>
<tr>
<td>1 Doz Blue and white Delph plates</td>
<td>1</td>
</tr>
<tr>
<td>8 Odd Ditto 10/ and 14 saucers 10/ and 3 cups and a Laddle 2/</td>
<td>1:2</td>
</tr>
<tr>
<td>11 small White stone Mugs and c.</td>
<td>6</td>
</tr>
<tr>
<td>1 small White and Blue China plate</td>
<td>5</td>
</tr>
<tr>
<td>4 Juggs, a sugar pot and c.</td>
<td>10</td>
</tr>
</tbody>
</table>

Detailed as the inventory is, it does not describe the design painted on the large earthen (delft) bowl, the shapes of the tea cups, or the style of the teapot finial. Even the 1770 description of a plate as tortoise-shell ware, only describes the technique of decorating. Neither the shade of the mottled coloring — green, brown, or purplish-grey, the form — queen shape, shell edge, feather, or royal, nor the manufacturer is revealed. This information can sometimes be gleaned from letters and diaries but usually must come from the study of dated specimens or archaeologically recovered fragments. However, by quantifying important variables about fine ceramics, the inventories provide crucial information not available from other sources.

Though the type of information recorded in inventories is limited, it is readily available. Five hundred inventories can easily be analyzed in the time required to excavate one site and process the ceramic material recovered. The fruitfulness of even the following limited essay into inventory analysis, emphasizes the need for more research of this type, and for the publication of archaeological material which has only rarely been made available.

The uniqueness of Boston — the foremost colonial port — restricts the usefulness of material derived from the Suffolk County inventories. The analysis of its inhabitants' ceramics can only be used to reconstruct their tastes. The archaeological relevance of this information is very limited. Most of the archaeological remains of colonial Boston and Suffolk County have been destroyed in subsequent urban development. However, it would be extremely interesting to be able to compare the patterns prevailing in Boston with those prevalent elsewhere. The research of Teller and South suggests that delft was far more important in colonial

5 XXVII, 443.

6 LXIX, 64.
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Providence, Rhode Island, and Brunswick Town, North Carolina, that it was in Boston. Ideally, analyses should be conducted for all the important geographical and ethnic areas. Such studies would be relevant to the writing of historical American ethnography, and only by having locally valid comparative material available, can an artifact, an inventory, or a kitchen midden be understood within its original cultural framework. However, before projections can be confidently made from archaeological evidence, we need to understand more fully what kind of a sample it represents.

Probably the most useful way to determine the relationship between the original ceramic population -- imperfectly recorded by the inventory -- and the archaeological sample, will be excavating documented sites. At the moment, very little is known about the average life span of historic ceramics, and without this information it is impossible to know how far such archaeological evidence can be pushed. How random or regular was the creation of this evidence? Were the coarse kitchen wares broken more frequently than the fine tablewares? Was delft more brittle than porcelain? Through the excavation of garbage pits, privies, and other trash deposits from households for which inventories have survived, answers to these and other questions can be obtained. In order to compare closely archaeological and inventory evidence, however, it is necessary to quantify both.

One result of the present exercise has been to reemphasize the importance of both qualitative and quantitative analysis of ceramic and other archaeological material. By this I mean analyzing not only the types present -- their forms, pastes, glazes, decoration, and origins, but also in what proportions the different types are present. The analyses of the materials recovered from Brunswick Town and the Custer Road dump site have shown how easily consistent patterns and subtle shifts in these patterns can be demonstrated. Without similarly measured data, it will be impossible to project social and regional differences from archaeological material.

Archaeological evidence of material artifacts may never provide as firm a basis as inventories for projecting the cultural system they represent, it frequently must serve as such. While inventories are available for a large enough proportion of the total population so that their evidence can be used with a great deal of confidence, they do not adequately record all types of artifacts, and specific inventories frequently are not available. Sometimes, in interpreting a particular artifact, individual, or house site, only archaeological evidence is available. At the moment utilizing this material is very difficult due


then summarized for each individual, for each decade, and for the entire period.

The probate records pose various problems. A large but unknown percentage of the inventories represents the possessions of the aged. Generally it can be assumed that these terminal households have had curtailed purchasing power for some time. Moreover, particularly in the country, many of a family's assets were distributed among the children as they came of age.

A second major type of problem is that the inventories were recorded to facilitate settling the estate, and were not taken with the intention of illuminating future researchers. Not infrequently several categories of goods are lumped together and given a value, such as "Pewter Brass and all Iron Ware £2:8:-." Descriptions are often ambiguous. "Pot," "teapot," or "punchbowl," could refer to articles of metal as well as ceramic. The inventories are most useful for analyzing valuable and clearly distinguishable items. Unfortunately ceramics had neither of these qualities until the third decade of the eighteenth century.

At the end of the seventeenth century, Englishmen had a wide variety of ceramic types available, many of them visually exciting. In Staffordshire and North Devon potteries were turning out large quantities of colorful slipwares. At London, copies of Chinese porcelain were being made in tin enameled earthenware; which also was being used for large decorative dishes, or "chargers." Good English stoneware had recently become competitive with German exports, and Dutch, French, Spanish, and Portuguese tin enameled ware was also available. Sherds of all of these types have been excavated at Jamestown. However, the Suffolk County inventories seem to indicate that the New England Puritan was generally satisfied with cheap utilitarian earthenware.

Unfortunately, the inventory descriptions of earthen ceramics 1680 - 1710 are very vague. Almost all the references are only to "Earthen Ware," usually lumped with woodenware or glass bottles so that even the value is unknown. Descriptions such as "6 cups and 4 muggs earthen 3d," giving number and type, are infrequent. They occur only twice in 1680, four times in 1690, and three times in 1700. Over half the inventories in this period contain no mention of ceramics at all. To what extent this reflects their absence, or their economic insignificance, is uncertain. Whereas the iron, brass, and pewter owned by even an average family represented an investment of several pounds, most of the earthenware listed was worth only

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10 IX, 12, 25; VII, 176, 179, 186; XIV, 122, 146, 188.

77
to the paucity of published material for comparison. Very few historic sites have been adequately reported. At Hopewell Village National Historic Site, two massive nineteenth century privies have been excavated, and the carefully recovered material catalogued, but not analyzed. Most archaeologists working in the field are probably familiar with similar situations. Full analysis and reporting of such material are time consuming and expensive, but without that, historical American archaeology will never realize its potential.

In the following section, I hope to demonstrate how a start can be made in understanding artifacts in their historical context. I have already indicated what a limited beginning it is, but the productivity of even such a preliminary trial has convinced me that much more needs to be done in this area. It has also convinced me that by combining inventory and archaeological evidence, great progress can be made in reconstructing the material accouterments of vanished American cultural systems, and to some extent, these systems themselves.

A hypothesis of this paper is that under many circumstances, the ground is the last place to look for information on historic ceramics. For those areas and periods for which probate inventories contain detailed listings of the decedent's ceramics, these records can be an invaluable resource. Through them, the introduction of new types into the cultural system can be plotted. By watching the speed by which exotic items are defused among different groups, the student of the society can describe the various taste patterns prevailing, and gauge their rates of change. This information is directly relevant to the historian's understanding of the society, or of particular individuals within it. This information also should be a great help to the archaeologist. A comparison of the distribution of sherds from a stratum, with the distribution of pieces in probate inventories, should complement qualitative analysis in providing clues to the date of deposition, and, within important limitations, provide clues to the former owner's cultural system and his place within it. Particularly during periods of innovation, ceramics promise to be a sensitive index to differences between social and cultural groups.

This is true of Suffolk County, Massachusetts, during the half century preceding the American Revolution. Analysis of 318 probate inventories dating from 1680 to 1775 shows that for the second half of this period the consumption patterns of residents of Boston can be clearly distinguished from those of surrounding rural areas. Even within Boston, porcelain can be used to differentiate between the values of various groups.

Beginning in 1680, at 10 year intervals thereafter, and with a final sample drawn from 1775, from 30 to 50 probate court inventories were read and all occurrences of ceramics noted. The total value of the inventory and the decedent's place of residence and occupation were also recorded. From these groups all inventories not representing "households" were eliminated. Households were minimally defined as groups of furniture containing provisions for sleeping and cooking. Also eliminated were all inventories where the place of residence was uncertain. The data was
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a few shillings or pence. It frequently may have been ignored while inventoring an estate or included in the "& sundries."

Despite the lack of description in the inventories, it is obvious that most of the ceramics in Suffolk households of this period had little decorative or ceremonial importance. Almost invariably earthenware appears in the kitchen or with the cooking and dining utensils. A few exceptions do occur. In a 1680 inventory four earthen platters are listed with the pewter in what was obviously an entertaining area of the house, and in 1700 "6 cups & 4 muggs earthen" are listed in a "Hall." The most important exception is the inventory of Mr. Peter Butler, a substantial Boston mariner, who had "A glass case and Earthen Ware & glasses in ye sd Case & on the mantle piece" of his parlour, and more earthenware on the mantle of his parlour chambers. However, since these and an ambiguous reference to "Chargers £1:13/" are the sum of the exceptions among 114 inventories,11 it might appear that either the Puritan, unlike other Englishmen, was not interested in colorful ceramics, or that the Puritan, unlike the Pennsylvania German, did not succeed in transplanting this taste to the New World. This hypothesis receives some support from Laura Watkins. The products and kiln waste of 17th and 18th century New England potters reported in her volume are quite uninspiring.12 However, it is a conclusion of which I am dubious. Further research into the customs records, like that begun by Malcom Watkins, or of mercantile records and newspaper advertisements may reveal additional information, but final resolution of the question will probably await archaeology.

To what extent tin enameled earthenware is present in Suffolk County inventories of this period is uncertain. It may have been intended by the 1700 listing of "2 Crewets, 3 white pottingers, 5 coffee cups, 1 mustard pot a saucer 28," and a 1710 reference to "12 Coffee Dishes 4/" probably refers to that or porcelain, but tin enameled ware was obviously relatively unimportant.13 Unfortunately, "delft" is a relatively late misnomer, and the unequivocal designation "blue and white earthen ware" does not appear until 1730.

About 1720 the lack of emphasis on ceramics in Suffolk County inventories began to change rapidly. Chinese porcelain had been present in western Europe in limited quantities from the late middle ages, and it was not unknown in seventeenth century New England. In 1671 Peng Heath of Roxbury died in possession of "6 cheny Dishes 38," and a dozen years later the estate of Mr. Joseph Rock of Boston included "one china Bason."14

11XVII, 158.


13 XIV, 131; XVII, 48.

14 VII, 174-75; IX, 181.

79
In England, Chinese porcelain apparently became both more available and more in demand at the beginning of the eighteenth century. In London the possession of china became so important to the stylish that the fad was regularly satirized by Addison and Steele in their *Spectator*, 1711-1714. If their reporting is to be believed, every unfortunate gentleman of London was about to be bagged by his spouse's mania for collecting the expensive china. Even worse, their homes became so encumbered by the delicate imports, as to become veritable porcelain booby traps within which the poor men dared navigate only with the greatest caution. This state of affairs was not reached in Boston until 1730.

Porcelain first appears in my sample early in 1721, when the inventory of a Boston shopkeeper was filed. His "China Sett" was followed by others in 1721. In 1730 porcelain was present in a quarter of the Boston inventories — in truly opulent quantities in one. William Welsted's dwelling contained over 120 itemized pieces scattered around his front lower room, middle lower room, and front chamber. He also owned over 70 pieces of delft and several dozen pieces of fine stoneware. Porcelain continued to increase in popularity after 1730, and by the outbreak of the Revolution was probably present in a majority of Boston households. As fine tableware was rarely listed in rural Suffolk County inventories throughout the period being studies; the rest of this section will deal primarily with the urban sample, 1730-1770, a group of 88 inventories.

What factors are involved in the increasing use of porcelain are uncertain. Among them are a probable rise in the standard of living, combined with an ever increasing desire to keep up with the Joneses. Another factor may have been a decline in the price of tea. Rodris Roth has pointed out that until the middle of the century tea was priced out of the reach of all but the wealthy. Whatever factors are involved, the spread of the tea ceremony is highly correlated with the increasing use of porcelain. Of 38, 1730-1770 Boston inventories which had ceramic equipment for tea consumption, at least 33 included porcelain. Only 5 inventories contained porcelain but no tea utensils. Three of these exceptions were mariners' estates valued at less than £100, but which included one or two porcelain bowls. Presumably these bowls were used for serving


16 XXII, 48; XXVII, 443.


18 LVI, 122; LXVIII, 482; LXIX, 31.
INVENTORIES CONTAINING CERAMIC TYPES AS PERCENTAGES OF THE TOTAL NUMBER OF INVENTORIES

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Porcelain --  Stoneware ----
Delft ---  Earthenware ...........

RURAL

BOSTON
punch, and as center or mantelpieces. The increasingly democratic diffusion of porcelain was also the popularization of the tea ritual.

Porcelain was clearly the dominant ceramic type in Boston during this period, despite its relative costliness. In 1770, whereas cream-colored, tortoise shell, or white saltglaze plates could be had for about 4 shillings a dozen, porcelain plates cost a shilling or more apiece, enameled china 2 or 3 times that. Nevertheless, in the 41 urban inventories of this period that contain porcelain, 1096 pieces are listed. For the same sample, only 242 pieces of delft and 206 pieces of stone tableware are enumerated. This dominance was true in all economic classes. In only 4 inventories was delft present where porcelain was absent, and fine stoneware appeared in only 2 inventories where porcelain was not present. To some extent these figures are weighted in favor of porcelain, as it was the most carefully enumerated. However, this is the culture's own bias and reflects the prestige of "china."

The most prestigious porcelain was that usually described as "burnt china." This term has been interpreted by Roth and Teller as only another designation used to emphasize the difference between porcelain and earthenware, reflecting the higher firing of porcelain and its resulting vitreousness. However, it is almost certain that burnt china is actually porcelain decorated with overglaze enamels. The term probably owes its origin to the practice of describing the metallic compounds used in pigments as burnt metals. This hypothesis is sustained by both the consistently higher values placed on burnt china, and the appearance of the adjective enameled only twice in the 41 inventories listing porcelain.

Altogether, 184 pieces and several sets were specifically described as burnt or enameled. Though less than a fifth of the total number, enameled pieces comprised as much as 50 percent of the larger porcelain inventories. William Welsted owned 54 pieces; Captain John Hubbart, a wealthy feltmaker, had 46, including basins, dishes, plates, and cups and saucers.

Three of the inventories list a total of more than 100 pieces of porcelain. All these had been owned by individuals worth over £3500. For persons of middling means, 20 or 30 pieces was a normal holding. The urban median was 26 items. A representative inventory from this group is

19 LXIX, 63, 53; LXVIII, 470.


22 XXVII, 443; XXXIV, 630.
that of Mrs. Grace Knight, Widow, who had owned 27 pieces of china: 10 cups and saucers, a large cup and saucer, and 5 broken bowls. She also had possessed 2 white saltglaze teapots, 2 saltglaze bowls and a dish, and a flower pot, 2 plates, 2 bowls, and a dish of delft. Her inventory was filed in 1750. Another representative list, recorded in 1770, is that of Captain Samuel Butt, Mariner, who had owned 6 coffee cups and saucers, 8 burnt china tea cups and saucers, a half dozen blue and white cups and saucers, 2 china bowls, a delft teapot, cream pot, et cetera, a half dozen blue and white delft plates, and a parcel of earthenware and empty bottles. In small groups of porcelain like these, teaware usually outnumbered items for dining. The reverse was true in the largest inventories, where dozens of plates and dishes, sauce boats, relish trays, butter plates, and bowls are listed.

Virtually all the porcelain items enumerated in the inventories, large or small, were functional, and used in serving foods or beverages. A porcelain wash basin appearing in a 1760 inventory was a rare exception. While all these items had ceremonial and decorative importance, purely decorative items were infrequently listed. China flower pots or vases are enumerated in one inventory, and images are present in another. These exceptions represent less than one half of 1 percent of the porcelain recorded by the appraisers.

The popularity of "delft," as tin glazed earthenware came to be called, seems to have paralleled that of the porcelain it imitated. Cheaper than china, it may have occasionally provided a less expensive substitute for true porcelain, as in the case of the Boston widow of modest means who had owned "2 delph jugs." Its popularity, however, seems usually to have been based on its own appeal since it almost always appears in inventories containing porcelain, and not separately. One reason it failed to compete more successfully with porcelain seems to have been its unsuitability for cups for hot liquids. Few delft cups and saucers are present. It was more appropriate for larger objects, though, and as many delft as porcelain flower vases are listed. Generally it is present in smaller parcels than porcelain, as well as in fewer inventories.

Utility stoneware -- jugs and pots -- is included occasionally in seventeenth century inventories, but it did not become common until the 1720's and 30's, at the same time that stoneware for the dining table and tea board was appearing. In an inventory filed in 1721 "1 white Stone

23 XLIII, 368.
24 LXIII, 511.
25 LXVII, 470; LXVIII, 503; XXVII, 443.
26 XLIII, 489.
Mug" is listed. If this was white Staffordshire salt glaze, the time lag between the production of new wares and their introduction into America appears to have been diminishing. This would only postdate its first production in England by a couple of years. Stoneware increased rapidly in popularity until circa 1750-1760. However, except for mugs, it always was of decided tertiary importance. In only a few inventories was it the dominant type. The most unusual of these was that of Captain Rueben Stevenson, Mariner, which contained 98 pieces of stoneware tea and dining items. His few pieces of porcelain, however, were worth almost as much, and were displayed more prominently. In the 1770's white saltglaze was still carried in Boston shop inventories, but its importance was rapidly declining.

The revolution in ceramic technology that English manufacturers were implementing in the 1760's appears clearly in the 1770 and 1775 samples when "Cream Col." and "Tortice Shell" plates, a set of "yellow earthen ware," and dessert china of "pencil earthen ware" appear. Unlike the common earthen wares, these new types were carefully enumerated. The presence of mass-produced refined creamware probably accounts for the declining importance of stone.

The use of porcelain as an index of status is perfectly straightforward for the rural areas surrounding Boston. The first mention of it is in the inventory of an extremely successful Weymouth weaver whose estate was valued at £1938. With only three exceptions, all the other occurrences of porcelain were in the estates of men listed as "Gentleman" or "Esquire." The exceptions were very minor -- a Milton widow, former owner of a china bowl and 5 delft plates, and 2 yeomen whose estates in 1775 each contained a few shillings worth of unitemized "china." In my 1730-1775 rural sample, 4 of the inventories of esquires or gentlemen contained porcelain. Only 3 of the 50 other inventories of rural males contained "china."


28 LVI, 91.

29 LXVIII, 503; LXIX, 64; LXXIV, 270.

30 XII, 533; LVI, 47; LXXIV, 257, 258.
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TABLE 1

THE CORRELATION OF STATUS AND THE POSSESSION OF PORCELAIN FOR RURAL SUFFOLK COUNTY, 1730-1775.

(The Inventories of Males Only)

<table>
<thead>
<tr>
<th>Porcelain</th>
<th>Status</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Gentlemen</td>
</tr>
<tr>
<td>Present</td>
<td>4</td>
</tr>
<tr>
<td>Not Present</td>
<td>4</td>
</tr>
</tbody>
</table>

Of the rural gentlemen possessing porcelain, only two owned more than a few pieces. It is significant that not only were these the two most valuable country inventories, but that both included Boston real estate. This suggests that these cases represent the transference of urban wealth and taste to the countryside, rather than the assimilation of urban values by the Suffolk County yeomanry. By and large, the values of the rural farmer seem to have centered around land, and appear to have been little influenced by their urban neighbors.31

A similar analysis of the distribution of silver has recorded the same phenomena. Between 1680 and 1720 the possession of silver plate became widespread within Boston's middle classes. In 1720 it was present in 60 percent of the urban sample. In the countryside, however, in both 1680 and 1720 silver plate was included in the inventories of only a small minority of wealthy yeomen. This strengthens the conclusion that two radically different consumption patterns coexisted in colonial Suffolk County.32

In Boston the use of china as an index to social status is more difficult. Table 2 clearly shows that while the use of porcelain was initiated by the wealthy merchants and mariners, it then spread to the rest of the population. In 1770 porcelain appeared in 71 percent of the urban sample. However, despite its wide distribution, two distinct correlations are present. One is the obvious direct relationship to

31 LVI, 145, 182.

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inventory value. The wealthy inventories contain the first appearances of porcelain, the largest holdings, and the most enameled pieces. The second is related to contact with the larger world.

Generally, only the prosperous craftsmen and contractors owned porcelain, suggesting that those whose horizons were largely limited to Boston still structured their conspicuous consumption on a hierarchal basis. Thus it was not noteworthy for an individual like Daniel Ingersol, a rich shipwright, who obviously contracted the building of vessels, to own a porcelain punch bowl and blue and white tea cups. However, it would have been unusual if shipwrights John Ingleby or John Foreland -- common artisans whose estates were valued at £27 and £31 -- had owned porcelain.33 This was in accordance with the late medieval belief that the luxuries owned by individuals should reflect their social and economic class. During the seventeenth century in old and New England it was both presumptuous and illegal for the lower classes to ape their betters. In 1653, for example, the Ipswich Quarterly Court fined two women from households of modest means for wearing silk scarves.34 Although after 1675 New England sumptuary legislation was rarely enforced, the mores on which it had been based seem to have lingered among the artisan groups. The merchants, mariners, and gentlemen, however, had the values and tastes set by different standards.

Porcelain was obviously an important ingredient of elegance in mid-eighteenth century Boston. All of the merchants and two-thirds of the gentlemen in the 1730-1770 sample had some. The increased propensity of those connected with trade or government to possess china was based on more than economic considerations. Though only the rich could afford a large collection, a punch bowl or a few cups and saucers were within the reach of most. The urban craftsmen and contractors in my 1730-1770 sample had a median inventory value of £350. Twenty-three percent owned porcelain. During this same period, the mariners' estates, whose median valuation was only £263, contained porcelain 63 percent of the time. This actually understates the case. Three mariners who owned porcelain were eliminated from the 1760 and 1770 samples because the furniture of their rented quarters did not contain implements for cooking.35 Being fashionable was apparently an important cultural value for the cosmopolitan seafarer. It was also important for individuals like Peter

33 XLIII, 296; XXXIV, 608, 633.


35 LVI, 245; LXVIII, 455; LXIX, 10.
<table>
<thead>
<tr>
<th>Inventory</th>
<th>1730</th>
<th>1740</th>
<th>1750a</th>
<th>1760</th>
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<tr>
<td>T</td>
<td>MERCHANT 129+T</td>
<td>FELTMAKER 118T</td>
<td>MERCHANT 113T</td>
<td>GENTLEMAN 12+T</td>
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<tr>
<td>L</td>
<td>MERCHAND 34T</td>
<td>Limeburner</td>
<td></td>
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<tr>
<td>to £ 3500 +</td>
<td>MERCHAND +T</td>
<td>MERCHANT 34T</td>
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<tr>
<td>to 2000</td>
<td>Inqholder Carter</td>
<td></td>
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<tr>
<td>to 1000</td>
<td>Cordwainer Baker Mason</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>to Housewright Joiner Yeoman</td>
<td></td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>to 500</td>
<td>Carpenter</td>
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<tr>
<td>to 400</td>
<td>MARINER 21T</td>
<td>MARINER 29+T</td>
<td>MARINER 36T</td>
<td>Print</td>
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<tr>
<td>to 300</td>
<td>Housewright</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>to 200</td>
<td>GENTLEMAN 10T</td>
<td>WIDOW 26T</td>
<td>MARINER 13T</td>
<td>SUGAR BOILER 7</td>
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<td>to 100</td>
<td>Cordwainer Blacksmith</td>
<td>TIDE SURVEY- 40T</td>
<td>WIDOW 26 T</td>
<td>TAYLOR +T?</td>
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<tr>
<td>to 1</td>
<td>Shipwright</td>
<td>Wine Cooper</td>
<td>Shipwright</td>
<td>Rigger</td>
<td></td>
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<td>Book- KEEPER 14T</td>
<td>INNHOLDER 1T</td>
<td>MARINER 1</td>
<td>MARINER 2</td>
<td></td>
</tr>
<tr>
<td>to 4</td>
<td>Shipwright</td>
<td>Widow Carpenter</td>
<td>Widew</td>
<td>WIDOW 18+</td>
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<tr>
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<td>Widow Taylor</td>
<td></td>
<td></td>
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</tr>
<tr>
<td>to 8</td>
<td>Shopkeeper</td>
<td>Mariner Cordwainer</td>
<td></td>
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</table>

a There was severe upward revaluation of the currency between 1740 and 1750.

b Besides making wigs, he also ran a small shop which specialized in ceramics.
Braner, Tide Surveyor, a minor customs official who had a china service for twelve, or Jeff Johnson, a bookkeeper of very modest means, who had a half dozen china cups and saucers, milk pot, and sugar bowl. This suggests that innovations in taste were made by those whose business bought them into contact with European fashions. Economic dependency led to cultural imitation. Being au courant became much more important to this class and those connected with them, than it was to locally focused groups. The Boston artisan apparently felt far less need to indulge in conspicuous consumption.

The use of archaeologically recovered evidence to determine status is limited by the uncertainty of what the sample represents. For one site, too many variables influence the number of sherds recovered to place complete faith in quantitative sherd analysis. A clumsy maid or an old cook could radically skew the results in different directions. Even so, for Suffolk County, the presence of porcelain in an early or a rural site would be clear evidence of elite values. In later urban sites its presence would be less conclusive, but along with other indices might suggest middling or better economic status, or involvement with the mercantile or maritime community. Its absence would shift interpretation towards modest economic means and basically local preoccupations.

An inventory provides the historian with a much more accurate measure of an individual's taste. However, the most exciting use of this index will be in making intergroup comparisons. Porcelain would seem to be an ideal index for use in comparing the value systems of eighteenth century Philadelphians and New Yorkers, or the New England yeomanry with the Pennsylvania Dutch.

36 XLIII, 380, 313.
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COMMENTS ON
MR. GARRY WHEELER STONE'S
PRELIMINARY STUDY OF "CERAMICS IN
SUFFOLK CO., MASS., INVENTORIES, 1680-1775"

J. Jefferson Miller II

My reaction to Mr. Stone's most interesting preliminary study of Suffolk County inventories is ambivalent. On one hand I heartily agree with his general thesis that these records constitute valuable documentary sources and are extremely useful in attempts to reconstruct changes in the material culture of a given time and place. Further, Mr. Stone's concentration on one aspect (ceramics) of these inventories has produced workable data that is enhanced by its established geographical bounds and by clearly determined end (but not beginning) dates. On the other hand, I question certain aspects of Mr. Stone's interpretative methodology and the accuracy of some of his tentative conclusions.

First, let me praise Mr. Stone. The value and pertinence of inventories has been appreciated for many years. Inventories, account books, correspondence, graphic materials, and newspaper advertisements have comprised the usual assemblages of documentary materials utilized by social historians working in the traditionally accepted manner for reconstructing the past from contemporary written and graphic sources. Inventories have proved of great importance in attempts to furnish so-called "period rooms."¹ The painstaking detail of Mr. Stone's study, in conjunction with its clearly defined geographical and temporal limits, contributes significantly by demonstrating the value of a concentrated analytical investigation of inventories. The utility of such detailed work seems - at least to me - undeniable. The fields of historical archaeology, social history and economic history all stand to benefit from investigations of this type. Mr. Stone's accomplishment in tracing, from inventories, the rise and distribution pattern of porcelain in the Boston area has added pertinent information to one area of American studies still confounded with many uncertainties and misconceptions. Further, this study indicates a number of possibilities for future investigations that may prove to be of great importance. For example, a correlation between ceramic and pewter table wares would be of great interest as the practice of melting down and re-using pewter limits the scope of archaeological data in this particular aspect of colonial life. But, and this is a very large "but", the necessity for valid (or at least non-misleading) interpretation is essential. Having praised Mr. Stone, I will now try to indicate a few areas in which his admittedly preliminary study is subject to criticism on interpretive grounds.

An especially appealing facet of historical archaeology lies in the complimentary utilization of anthropological and historiographical approaches. In practice, this seemingly straightforward interdisciplinary method is complicated by a third, less clearly defined element - the necessity for a hopefully realistic understanding (based on documentary sources and objects
themselves) of the so-called "material culture" of a given period and place. Of course, Mr. Stone is well aware of this factor and quite clearly has some background in this elusive area which, I fear, is frequently treated with far more certainty than is merited by our limited knowledge. Mr. Stone properly states that he is cognizant of this problem in his frank acknowledgment that inventories are of only limited help in reconstructing information concerning the use, quantity, and type of coarse household earthenwares. Here, historical archaeology dovetails nicely with inventory studies in that many excavated sites have produced volumes of these coarse earthenwares. Having indicated an awareness that his synthesis of the bits and pieces of information from inventories involves special problems, Mr. Stone proceeds, in a number of instances, to adopt an attitude of certainty that requires, I believe, more considered justification or explanation.

It is when Mr. Stone reaches the true nuggets in the inventories he has uncovered that I begin to have some specific doubts concerning the interpretation of his data. For example, on page 4 of his paper he gives an inventory of 1730 containing a list of ceramics from the front chamber of William Welsted. Two items so listed are a "large blue and white earthen bowl and a sugar pot." Mr. Stone then writes, "Detailed as the inventory is, it does not describe the design painted on the large earthen (delft) bowl,..." Why is this bowl delft? The inventory quite clearly lists a number of items as "Delph" while others (including the bowl) are termed "Earthen." In fact, the compiler was meticulous in his distinctions: "China," "China stone," "Earthen," "Delph," and "White stone." Given this firm degree of categorization, can we assume a "Blue and white Earthen bowl" is "Delph"? It could be, but the weight of the evidence as supplied by the terminology of this particular inventory requires some reasoned explanation for the distinction between "Earthen" and "Delph" which, if Mr. Stone is correct, becomes meaningless. Mr. Stone might well ask: if not delft, what was the bowl? I cannot answer that question, and I agree that quite probably it was delft, but I am not willing to assume it was delft. On page 13 Mr. Stone states that in 1730 blue and white earthenware is an "unequivocal designation" for delft. Perhaps so, but, for example, these blue and white pieces might have been French faience or Spanish tin-glazed earthenware from Talavera or some similar place.

Thus, the overall aura of confidence in his material that pervades Mr. Stone's study should, I believe, be tempered. Continuing this thought, I challenge the idea (p. 7) that "Archaeological evidence of material artifacts may never provide as firm a basis as inventories for projecting the culture system they represent,..." I suspect that some of the archaeologists commenting on Mr. Stone's paper will also take exception to this statement. Conceding their great value, I do suggest that the imprecise and variable nature of many inventories results in uncertainties at least equal to the statistical uncertainties presented by the artifacts recovered in a given dig. Without belaboring the point, I would suggest that Mr. Stone's hypothesis that "...under many circumstances, the ground is the last place to look for information on historic ceramics." (p. 9) might be reworded "under some circumstances, the ground is the last place to look for information on historic ceramics."
Another questionable interpretation upon a rather significant point appears on page 17 where Mr. Stone writes, "However, it is almost certain that burnt china is actually porcelain decorated with overglaze enamels." As the term "burnt china" appears in many inventories, its exact meaning (if we can determine it) is of importance. Mr. Stone advances a hypothesis that "burnt" is synonymous with "enameled." but there is a good deal of evidence to the contrary. Contemporary newspaper advertisements placed by china vendors in the major colonial cities tend to indicate there was a definite distinction between "burnt" and "enameled" china. For example, a 1774 New York ad offered:

"To be sold at Rhinelander's Store...chinaware, blue and white cups and saucers of all sizes; burnt and enamel'd ditto;..."

In a like manner a Philadelphia dealer advertised:

"China tea table setts of various patterns, cups and saucers, enamel'd, burnt, blue and white ditto."

Many similar advertisements can be found in 18th century newspapers. Evidently, there was a rather clear differentiation between three types of decoration on porcelain: blue and white, enameled, and burnt. Sometimes the adjective "pencilled" also appears. This presumably refers to the delicately drawn scenes (often religious or mythological) in overglaze black or, infrequently, red. Though I am not able to explain exactly what was meant by "burnt" china, I do feel there is ample evidence to flash a yellow light of caution and to refrain, until more information is gained, from considering "burnt" china as overglaze enamel painted porcelain. In elaboration of this point, it is interesting to note that Josiah Wedgwood (1730-1795) differentiated between enameling and burning. In his correspondence (though I have not by any means read all of it), Wedgwood seems to use the term "burning" in reference to fixing gold (gilded) decoration to his wares. He writes of "some gold powder such as is burnt in upon china" and "making experiments in burning gold."

I do not mean to imply that "burnt" is synonymous with "gilded" or "gilt," but this is a possibility. At any rate, there probably was an important difference between "burnt" and "enameled" china as indicated in the enumeration of the various types of porcelain for sale, and I find it difficult to accept this repeated duality as nothing more than a redundancy.

Before continuing with this criticism, which is sincerely meant to be constructive, I think it important to state that these cautionary thoughts do not represent nitpicking or undue concern for trivial points. Rather, they represent the degree of care we should all take in the interpretation of a past material culture. Archaeologists and historians working side by side in the field of historical archaeology must each learn from the other. The specialist in some aspect of material culture is, at times, overly inclined to rely on that mysterious (sometimes practically intuitive) element we loosely call "expertise." The orderly precision of sound archaeological method should, in the general sense, influence for the better the thinking of the specialist in material culture. In the field of material culture, many past errors, changes in dating, attributions, etc.
attest to the need for more restraint. Conversely, and perhaps this is wishful thinking, the traditionally trained archaeologist can benefit in certain circumstances from the "expertise" of a specialist in material culture, and in doing so avoid elaborate taxonomies and statistical presentations that sometimes are time consuming, unnecessary, and even misleading.

So much for the sermon. In returning to Mr. Stone's paper, I would like to comment on his interpretation of the word "china," which he seems to equate with Chinese export porcelain. For the period c.1680-1755, I find no serious quarrel with this application. But during the 1750's, porcelain making took hold in England and some of this porcelain was exported to the colonies. This factor, not considered by Mr. Stone, raises socio-economic questions of some importance. Archaeological investigations of colonial sites to date have indicated that most of the imported English (soft paste) porcelain was of the simple, cheaper variety decorated in underglaze blue. Much of this blue and white English porcelain found in colonial sites is Worcester or Liverpool (a generic term covering a number of small porcelain factories in Liverpool). This fact has significance, as Bristol (near Worcester) and Liverpool were two of the leading English ports serving the North American colonial trade. At any rate, the English (and presumably the colonists followed suit) used the term "china" to describe English porcelain as well as Chinese export porcelain. An early example of this usage appears in a 1756 advertisement from a Liverpool paper:

"Liverpool China Manufactory--Messrs. Reid & Co., Proprietors of the China Manufactory, have opened their warehouse in Castle Street, and sell all kinds of blue and white china ware, not inferior to any make in England, both wholesale and retail."8

Of course, it is not surprising that the great majority of inventories seem to have made no distinction between English and Chinese porcelain. At times the term "India China" was used, and this clearly seems to have meant Chinese porcelain. Nevertheless, a number of archaeological investigations (those being conducted at Williamsburg by Ivor Noël-Hume for example) have confirmed the fact that English porcelain was not uncommon in the colonies during the 2nd half of the 18th century. The archaeological evidence indicates that the Chinese porcelain heavily predominated, but the very fact that in most cases the inventories fail to pinpoint this material distinction provides another example of how carefully they must be interpreted and delineates another area in which information from inventories can be of maximum utility when used in conjunction with archaeological evidence. Further, there is another complication presented by evidence that the term "china" was used for non-porcelains in the final quarter of the 18th century. Ivor Noël-Hume has pointed this out in a recent article on pearlware.9 At this time we do not know for certain exactly when the more general usage for "china" began.

Turning to the statistical information, I must say that I question its applicability as well as some of the conclusions Mr. Stone has drawn. On page 17, on the basis of 41 urban inventories, Mr. Stone found listed 1096 pieces of porcelain, 242 pieces of delft and 206 pieces of stone table ware.
While acknowledging that "to some extent these figures are weighted in favor of porcelain, as it was the most carefully enumerated", Mr. Stone concludes, apparently on the basis of these (and other?) statistics, that "Porcelain was clearly the dominant ceramic type in Boston during this period." I seriously question this conclusion. First, are these statistics valid? Mr. Stone concedes they probably "are weighted in favor of porcelain" and I agree.

Before becoming a curator of ceramics, I practiced law for a number of years. During that time I watched many inventories being compiled by estate appraisers. Invariably, the better ceramics received scheduling that was reasonably accurate, but everyday wares were frequently listed as "1 cabinet of kitchen china," "1 closet of bric-a-brac," or "miscellaneous kitchen china." Mr. Stone concedes that this casual approach probably explains the small quantities of coarse earthenware in the colonial appraisals. I agree, and further suggest that this human factor also affected the sample of 41 inventories (p. 17) that scheduled 1096 pieces of porcelain and only 448 pieces of delft and stone table ware. Without flatly asserting this ratio tabulation to be non-indicative of the true situation, I do submit that archaeological investigations to date have not indicated such a marked preponderance of porcelain. Thus, Mr. Stone's unequivocal conclusion (p. 17) that "Porcelain was clearly the dominant ceramic type in Boston during this period, despite its relative costliness," seems unwarranted even when considered from the historical point of view.

Here, I must say that I found the certainty of many of Mr. Stone's opinions (especially as repeatedly expressed by his use of the word "obviously") objectionable. Of course, his paper is presented as a "preliminary study." I do think that in its final form, less certainty would be advisable in areas where the evidentiary materials are relatively vague, complex, or incomplete. In this respect, I might peripherally take Barbara Teller (cited several times by Mr. Stone) to task. In her generally excellent study of Rhode Island inventories, she also is, at times, far too certain of rather uncertain things. For example, she feels that "French Delft" in the inventories is "undoubtedly Rouen Faience" (p. 573) and that a reference to "openwork" indicated cream colored earthenware (p. 576). This could be true, but there are alternatives in each instance. Both English white salt-glazed stoneware and Chinese export porcelain were made at times with pierced designs (open work). Rouen was an important faience center, but other French areas made and exported faience. It is possible that prolonged exposure to inventories serves, at least partly, to blindfold the researcher to the manifold options and uncertainties presented by these documents.

CONCLUSION

Mr. Stone's paper is a valuable preliminary study that provides impressive documentation concerning the diffusion of porcelain in Suffolk County, Mass. In his attempt to apply these findings to the more comprehensive problem of analyzing the material culture of the place and period (insofar as ceramics are concerned) some of his interpretative methods and some of his resulting determinations are questioned.

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attest to the need for more restraint. Conversely, and perhaps this is wishful thinking, the traditionally trained archaeologist can benefit in certain circumstances from the "expertise" of a specialist in material culture, and in doing so avoid elaborate taxonomies and statistical presentations that sometimes are time consuming, unnecessary, and even misleading.

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1. Unfortunately, the modus operandi of many museums and restorations has been to consult inventories for the type of material culture, but to ignore the quantitative evidence provided by the inventories. Thus, we frequently find period rooms furnished with the (probable) correct type of artifacts - but far over-furnished in terms of the quantity of artifacts that might reasonably have been found in any given room.


3. Today, tin-glazed earthenwares from Holland or England are given the generic term "delft." French tin-glazed earthenware is called faience, while Italian and Spanish tin-glazed earthenwares are usually designated as majolica. Talavera, in Spain, was long a major center of tin-glazed earthenware manufacturing. Writing of 18th century Talavera production, William B. Honey stated, "The contemporary Chinese blue-painted porcelain was in this period imitated at second-hand through wares of Delft and Savona." European Ceramic Art, Vol. 1, p. 606.


7. Porcelain (soft and hard paste) was made commercially on the Continent from the early 18th century onward. Little seems to have been imported into the colonies, so the factor of continental porcelain can be excluded as a practical matter.

8. Williamson's Liverpool Advertizer and Merchantile Register, Nov. 19, 1756. Reprinted in H. Boswell Lancaster, Liverpool and Her Potters (Liverpool; W. B. Jones and Co., Ltd., 1936). The term "china" was also used in the early 1770's to describe the porcelain evidently being made at the time by Bonnin and Morris of Philadelphia.

Mr. Miller's comments amply repaid my desire to have a ceramist on the panel. In his comments, I am chided in three areas: sloppy language, lack of ceramic expertise, and hasty conclusions -- the first two points I readily grant; the last I dispute.

I am rightly taken to task for failing to define my terms. My four categories of porcelain, delft, stoneware, and earthenware were meant to be general and inclusive. By delft I meant any form of tin ash glazed earthenware, whether originating from England, Holland, Italy or Portugal. The "porcelain" category includes items of both Oriental and Western manufacture. Only rarely are the inventories specific enough to enable distinctions to be made within these categories.

Mr. Miller's hypothesized explanation of "burnt" china is quite persuasive. His reasoning here shows the decided advantage of expertise. I would only suggest that since the term "enamelled" appears so infrequently in the Suffolk County inventories, "burnt" may have been used by the appraisors as an abbreviation to describe porcelain that was both gilded and enamelled.

The most serious question Mr. Miller raises, however, is whether or not I was justified in concluding that porcelain was the dominant ceramic type in mid-eighteenth century Boston. An alternate explanation would be that most of the other fine ceramics (less valuable) went unrecorded. I cannot answer this challenge at this time. It is my impression, though, that the inventories are not that inaccurate. An answer to this question should be readily available through content analysis. One tactic would be to compare the proportion of porcelain in detailed inventories, to the proportion of porcelain in abbreviated inventories. A complementary approach would be to contrast the frequency with which delft and stoneware were casually listed in "lots" or "parcels" with similar listings for porcelain. Such analysis should indicate whether or not porcelain was indeed predominate, and should also provide one means of gauging the reliability of my statistics.

Merci, Monsieur. In the future I shall be more careful.
COMMENTS ON GARRY WHEELER STONE'S "CERAMICS IN SUFFOLK COUNTY, MASSACHUSETTS, INVENTORIES 1680-1775"

Iain C. Walker

The Historic Site Archaeology Conferences held since 1960 have well maintained Goggin's hope that they would get down to brass tacks. As historical archaeology has evolved, so have the Annual Conferences, and with the introduction of the Forum papers - perhaps the most valuable single contribution yet made to the field - brass tacks have expanded from artefacts, which was what Goggin had in mind, to theory.

A glance through previous papers presented at the conferences indicates the second Forum will again break new ground, this time considering a specifically historical source of evidence and its potential application to archaeology. The evidence in question is inventories, made on the death of the owner, of household effects in a restricted area of colonial Massachusetts in the 95 years preceding the American Revolution, with specific reference to pottery. The problem examined was to see whether certain types of pottery, an item ubiquitous on most archaeological sites (but not at South's Charles Towne site!) could be correlated with the standard of living as indicated by the overall nature of the late owner's effects. The possible archaeological use of such information is noted. As Stone repeatedly emphasized (e.g. pp. 3, 13, 17, 26)* there are a great number of variables and imponderables in a study of this type: some are general problems, others are related to the specific area studied. The original version of Stone's paper was done for a course in colonial history at the University of Pennsylvania, but as the course was under the department of American Civilization, there we are mercifully spared, whatever the drawbacks of that title may be, the arguments which would have undoubtedly arisen had it been done purely for a department of History, or for a department of Anthropology.

If most of my comments appear an exegesis of the variables and imponderables in the light of my own experiences and views, it is not to be taken as an adverse criticism of Stone's own cautions. Rather, I hope that each commentator will explain how each possible use of studies such as Stone's strikes him from his own experience: if we finish with a hundred additional caveats from people of varied backgrounds and trainings we shall stand a chance of obtaining a realistic coverage of the problems as they affect archaeologists who are going to have to apply Stone's suggestions to their own excavations.

As Stone says (p. 6), we need to know what our samples represent. Archaeology is likely to be essential in determining earthen kitchen and

* Page references to Stone's paper are given in brackets throughout.
dairy utensils not only because the documentary sources are likely to be unhelpful on such items (p. 4), but probably because these items are among the cheapest, most hard used, most likely to be broken, and least likely to be mended items of pottery and thus most likely to be found when excavating refuse deposits. Each discipline has its own biases - they may or may not help each other out. The worst mistake we could make would be to assume historical data, because it involves the written word, is thus accurate. I remember the consternation among historians when excavations at Louisbourg revealed in a certain area four buttresses where there should have been seven. The documentary evidence for the latter number was there in black and white - the contractor had submitted a bill (and been paid) for building seven - but the fact was this enterprising individual only built four.

That a luxury item such as porcelain was introduced into the area by the Boston merchantile elite, that it was quickly acquired by the commercial and maritime groups, that it only slowly diffused among the artisan classes, and that it was almost totally ignored in the surrounding rural area until c. 50 years after its first appearance among the elite (p. 1) comes as no surprise, but it is useful to have it proved. The elite felt a need to prove their social equality with the English haut monde, and the commercial and maritime groups all hoped to make it to the top eventually and were thus permanently keeping up with the Jones. To this one should add that those involved in trade and commerce were those who could obtain porcelain most easily, not solely because of their money but because their business brought them into contact with those importing high-quality wares - discount purchases among friends, for example, may have made obtaining such items easier. The fact that mariners, whose inventories recorded a markedly lower total value than those for urban craftsmen and contractors, had porcelain much more frequently (pp. 24 and 26, cf. 16) suggests to me ease of purchase - in this case during their voyages - was an important factor, rather than simply being fashionable was an important cultural value for this class, which is Stone's suggestion. The artisan classes and still more the rural classes had less money to spend on such items and less need socially for them; but they also had less opportunity to obtain them in the course of their work (cf p. 22).

The desire, reflected in the upper Boston classes, to imitate the English society to which they still emotionally belonged is only to be expected (cf p. 26). The same ties appear in the trade catalogues and advertisements in Canada early this century which announced with pride items "Straight from England" and "As used in London". Again, the Boston upper classes, unlike the lower classes, were able to have direct and repeated contact with England including visiting and receiving visitors of comparable class. It may therefore be suspected that the division between Stone's two upper and two lower classes was greater than the difference between the two classes making up each group.
A point which should be studied carefully is the effect of personal contacts between the merchantile elite of an area and regions in Britain. For example, there appears to have been relatively little contact between Liverpool and colonial Virginia (Noel Hume 1963: 284), but in the early 18th century James Blundell of Virginia sailed twice or thrice a year with tobacco to Liverpool where his brother Nicholas arranged for return cargoes (Berry 1963: 5). Again, Robert Dinwoodie, Lieutenant-Governor of Virginia from 1751 to 1758, had been a founder of the Delftsfield Pottery in Glasgow in 1748, and he and his brother James (Lord Provost of the city in 1742 and 1743) had a large family business. In 1771, the year after Robert's death, over 37,000 pieces of earthenware and over 25,000 pieces of stoneware were shipped from Glasgow to Virginia alone; and Maryland, Philadelphia, Boston - stoneware only - and Antigua in the West Indies also received pottery from Glasgow (Fleming 1923: 91). Granted Glasgow was Scotland's chief port and that not all its exports would come from the city itself, how much of this trade was attributable to Dinwoodie's influence? Were there any Boston counterparts to these personal contacts?

Another point to consider is how specific colonial exports brought in certain goods traded in return. In the 17th and 18th centuries Barnstaple and Bideford in North Devon had a very considerable tobacco trade with Virginia (William 1938: 168, 1967 ed.), and the appearance of North Devon sgraffito ware on sites restricted to (with one exception in Massachusetts) Virginia, Maryland, and Delaware in the later 17th century must be connected with this trade. Yet North Devon coarsewares reached New England during that period in large amounts both on documentary evidence - an entire shipload to Boston in 1688, for example - and archaeological evidence from three sites in Massachusetts and one on Long Island (Watkins 1960: passim): was sgraffito ware really so expensive that Bostonians could not afford it and only tobacco magnates could indulge in it and other ceramic luxuries (p. 11) or did puritan New England really have a different taste in pottery (cf pp. 12-13)?

Another field which must be studied is how far prestige china reflects not just those who could afford it, but those who had to afford it. The commercial and maritime groups obviously included some of the latter; but what, for example, of clergymen? In England the general clergy were relatively poor - in 1688 Gregory King estimated their yearly income per family as between £50 and £72 depending on status, which was about the same range as freeholders; while merchants and overseas traders had family income ranging between £198 and £400 (quoted in Trevelyan 1944: 277, 1946 ed.). Yet at least Anglican clergy hold a position which made entertainment of higher and richer classes necessary (and desirable - cf Inglis 1963: 41, a reference to a 19th-century observation) and did hold a position of certain respect. Were these factors reflected in their having better household goods than the freeholders whose annual income was comparable to theirs?

The suggestion (p. 20) of analyzing kinds of items represented by different ceramics is intriguing. Why was delft apparently not appropriate
for hot liquids - was it a basic weakness of the material or was it tradition based on some other factor real or imagined? Stoneware for mugs makes sense, but time was when beer had to be drunk from pewter (and connoisseurs still say so today). Stoneware mugs suggest practical expediency (minimum breakage despite hard usage) - pewter mugs, while equally having a low breakage rate, suggest an original snob value. Can we obtain more precision on the use of treenware (p. 11) in earlier times and rural areas - archaeological evidence would be difficult to find unless preservation conditions were suitable. It is strange that stoneware and delft first appear so late in the records - both were being made in England long before the earliest date Stone finds them and in Virginia, English delft had taken over the market from Dutch and Portuguese delft by the end of the 17th century (Noël Hume 1963: 290). Bristol was manufacturing delft by 1647 (Pritchard 1926: 262) and Liverpool by 1710 (Charleston 1954: 101) and both these ports had major trade with North America. Certainly Bristol tobacco pipes of the second half of the 17th century and the earlier 18th century were traded to North America from Hudson Bay to the Caribbean (Walker MS). One suspects here that the records are simply inadequate; but it may be that New England was poorer than Virginia, and that treenware supplemented earthenware to some degree.

A related point to the analysis of items and wares is the longevity and basic breakability of wares (pp. 6-7). Stone asks was delft more brittle than porcelain; we can also ask if delft from different sources had different breakage rates; and if so were the differences enough to be likely to reflect in either the historical or archaeological records? Bristol, Glasgow, and Liverpool all made delft and all were major ports exporting to North America. If delft from source X was found to break more readily than that from source Y, would this mean less material from X would be bought or that it would be sold more cheaply and that more might be bought (at least in certain classes)? Would the upper classes buy superior pottery - for display or entertaining, for example - and use cheaper ware in everyday use and still cheaper wares for their servants? Did those who could afford replacements easily break more than those who could not - presumably those who used porcelain for more everyday use tended to break a higher percentage than those who used it for tea parties only. Certainly porcelain dinner services belonged to the well-to-do; tea sets belonged to those less well off (p. 19). Would the clumsy maid or old cook (p. 26) be likely to work for a less well-off family and perhaps have a similar effect to lessen care taken by a well-to-do family? It seems likely that porcelain would be a more delicate item than delft and thus break more easily, but if porcelain were used less and when used, used more carefully, would an archaeologist expect to find more porcelain fragments than delft when excavating, assuming an equal number of items of each kind had been used at the site ignoring other variables such as whether certain shapes of items tended to break more readily than others?

In what conditions might one expect pottery to be mended and reused? South (1968) indicates that all classes of pottery from earthenware to overglazed enamelled porcelain have been found with rivet repairs on 18th-century sites. The Louisbourg lead-glazed earthenware he notes was probably repaired because even coarse pottery was so scarce there. French coinage
at Louisbourg, because it was so scarce, was used and re-used so much that many coins were worn smooth; English coinage from the British occupation there of 1745-49, on the other hand, was generally found in good condition. The reasons for the scarcity of French pottery and coinage at Louisbourg appear to have been entirely historical — the French had less effective contact with and less trading interest in their colonies than had Britian. On the other hand, the fivefold mended over-glazed enamelled teacup from Russellborough must surely have been kept for some sentimental value, for even such expensive ware cannot have been so rare in the colonial governor's house as to warrant such extensive repairing in what was after all a far from invisible mend. Thus archaeological finding of such mended material does not indicate a simple correlation with wealth or lack of it. Stone asks (p. 6) how far archaeological evidence relating to the lifespan of ceramics can be pushed and how random or regular was the creation of this evidence: these are crucial questions, and the fact that there are no general answers, nor will there ever be, does not prevent us from considering the validity of specific answers, given specific data from specific sites.

Pipes can also be considered under the question of basic breakability. At present in the British pipe-industry, pipes for export are those from the hotter parts of the kiln, as these pipes are stronger and less likely to break in transit (e.g. Walker and Walker 1969: 136). Was this the practice in earlier times or were colonists shipped the poorer products because they were less able to register complaints? — evidence suggests that inferior North Devon sgraffito wares were frequently shipped to the Colonies (Watkins 1960: 11). (The absence of Oswald's 18th-century type 10 pipes from North America is so total that it is difficult to attribute this solely to pipes for the colonies being shipped only from a few limited centres, and there is evidence that Bristol was producing "export only" heel-less pipes specifically for the New World market, so it appears that colonials had definite views on items they wanted and therefore probably would not accept inferior goods passively.) Would stronger pipes last longer than others, and if so would this make any archaeologically significant difference? And if they were used longer in North America than in England would this reflect the stronger pipe or their probably higher price (and possibly lesser availability) in the colonies? Many pipe fragments from Louisbourg and elsewhere in Canada seen by this writer suggest prolonged use — teeth-marks, bowls with their stem remains pared to take some makeshift stem, and the like — whereas he has seen no such examples while studying in England. Indeed there is evidence to suggest a reasonable consumption figure for pipes in England was several a week (Walker MS).

Modern Dutch pipes are fired at a rather higher temperature than are British pipes (Walker MS) — if this obtained 200 years ago they would be stronger than English pipes and might be expected to last longer. On the other hand, Dutch pipe bowls were much more delicate than were English so that this, the most important part of the pipe, might break more readily than an English one. But if Dutch pipes were difficult and expensive to
obtain, the smoker who valued his superior Dutch pipe would treat it more carefully - unless he had enough money not to worry about such things. Such speculations are not necessarily pedantic - there must be some reason why it appears (admittedly on initial evidence) that stem bore diameter dating of deposits of pipe fragments in England is not nearly so invariably accurate as its application to English material in North America.

The association of porcelain with tea drinking (pp. 16-17) raises some interesting speculations. Has any archaeologist found tea leaves on his excavation? There seems no reason why this should not be possible as tannin is a preservative - certainly they should be easier to find than coffee grounds. And what effect did tea drinking have on the use of other beverages? There seems no doubt that in England the increasing popularity of tea (and coffee and chocolate) in the 18th century resulted in a marked increase in temperance among the lower classes (George 1925: passim, 1966 ed.). If this was the case in the American colonies then the number of "wine" bottles an archaeologist might expect to find might decline as the century progressed. This feature might effect all social classes if tea replaced beer and spirits for the ordinary man and wine for the elite. And this in turn might conceivably effect the number of beer mugs or wine glasses likely to be found on an excavation.

An excavation I have longed to do, and still hope to do one day, is that of a house site whose last occupant is still alive. This would have been possible at Louisbourg, had circumstances permitted, with a 19th-early 20th century house site within the fortifications. Even if belongings had not been moved when the site was left and vandals had not subsequently looted the building it would probably be difficult for an archaeologist to identify the use of each room (assuming a one story basement-less house) but it would be instructive to compare personal reminiscences as to use with the archaeological evidence (cf Stone's suggestion [p. 2] that in future documentary research should include noting which rooms held various types of ceramics). More instructive would be the comments on material found in the rubbish dump - did a sailor relative bring home exotic objects from abroad, did a shipwreck provide items which normally would never have reached local inhabitants?

In Nova Scotia the place where people went to seek their fortunes and where everyone has a relative was not Upper Canada but the Boston States. The influence of this can be seen in curious ways - in Nova Scotia a driving permit is spelled "license", the American way; in Ontario "licence", the English way. A 19th century farmstead on Cape Breton Island might have more American made items than one in southern Ontario for all that the latter might be much nearer the United States border. How much New England material was brought be those returning to Nova Scotia? Did those who went to New England take anything distinctive with them? In the case of the former probably a certain amount, some of which may appear in the documentary or archaeological record; in
the case of the latter perhaps little or nothing, for immigrants, particu-
larly poor ones, bring little tangible with them. Yet in the case of the
latter, influence from immigrant tradition may be immense. Irish fleeing
the Potato Famine in the later 1840's almost certainly started the
Montreal clay pipe industry (Walker MS). The "Moravian" settlers at
Bethabara, or more specifically their potter Gottfried Aust, almost
certainly imported a central European tradition of "two-piece" pottery
pipes, a tradition which still survives in the United States. To me, as to
Stone (pp. 6, 27), the most exciting application of studies such as his
seems likely to be in comparing different geographical and ethnic areas.
Historical geography is a field so far little used by archaeologists.

How did taxation affect certain goods? Because Dutch pipes were
superior in manufacture to English, the 17th century saw repeated attempts
to prohibit their importation into England; but by the end of the century
they were certainly available, for a very heavy tax was imposed on them
at that time for at least a short period. By the beginning of the 18th
century, however, Dutch pipes were being bought - apparently legally - at
a price a little higher than that of good-quality English pipes (Walker
MS). Did Dutch pipes come to the English American colonies and if so did
they come legally or were they smuggled? Dutch pipes were so highly
regarded that not only English smokers preferred them. In the 18th
century Prussia and Sweden both banned the importation of Dutch pipes in
order to protect their inferior native industries, which suggests smokers
were prepared to pay for superior products (Walker MS). Dutch pipes
found on English colonial sites might indicate occupants of some financial
means or at least discerning smokers, rather than Dutch immigrants.

Stone's documents record the possessions of an owner at the time of
his death. One wonders if the compilers took the opportunity to have a
massive spring clean, either of chipped crockery or of the deceased's
unwanted possessions, before or after taking inventory. If so, might this
appear in the archaeological record as a sudden concentration of whole or
nearly-whole items, provided the rubbish-dump was a well-stratified
deposit such as a disused shaft or pit or a fairly small hollow? If this
were the case then there might well be heirloom problems, old possessions
thrown on top of more recent material and inverted stratigraphy. At the
same time, inventoried goods might be sold or disposed of to friends and
relatives, especially of the house was passing out of the family, so that
only a limited amount of the recorded items might be found by excavators
of the site (cf p. 10). Are there any significant differences between
inventories of women's possessions and those of men, and if so, could this
in the case of single sex households, conceivably appear in the archae-
ological record? These problems may sound artificial and academic, but
they should be examined first to see whether indeed they are.

Historical restorations have been described (Cleland and Fitting 1968:
131) as hotbeds of "low level" archaeology and other vices, sacrificing the
advance of intellectual research to the fleshpots of constructional and
other tourist perversions. It is true that most restoration (more accurately reconstruction) projects have relegated research qua research below work necessary to justify money expended in terms of tourists attracted, but such projects hold out some of the best prospects for large scale comparison of remains found by the vagaries of archaeology and those found by the vagaries of documentary research such as Stone's work. Some idea of the correlation (if any) between these two grossly inexact research fields will certainly come from the excavation of a number of sites whose contents have been recorded in inventories such as those studied by Stone; but the total excavation - not sampling - of whole series of sites of the same period in the same place supplied (presumably) by the same village stores and a correlation of pottery types and other artefacts, and a correlation of this with suitable documentary evidence is what is likely to give us the most accurate estimate of how far archaeological and documentary evidence will have to be "translated" to make a mutual dialogue intelligible. Only large scale excavations seem likely to provide this sort of large scale evidence, and at least at present such projects appear limited to the commercial (whether private or government) enterprises which ironically are those least interested in the intellectual aspects of the work. However, the work done by South at Brunswick Town (South 1962; 1964; 1967b) and Bethabara (1965a; 1965b; 1967a; cf also Rauschenberg 1968) is perhaps a hopeful pointer to a time when large scale sponsored excavations will no longer be synonymous with a tourist first research second outlook.

In the meantime, we are left with the "carefully recovered material catalogued, not not analysed" from sites such as the two massive 18th-century privies at Hopewell Village National Historic Site noted by Stone (p. 8). But how has this material been catalogued, and with what purpose - if any - in mind? Does the cataloguing involve irrelevant categorizing (cf Tunnell 1968:125) or mindless measuring to the millimetre of dozens of features which may or may not be relevant (cf Fontana 1968:126) on the principle that it is better to measure everything within sight now because it will be 15 years before we can hope to make a meaningful study; or does it consider what the object was, what it did, who made it and when, and who used it? Recording without reasoning makes nonsense of the intellectual field of archaeology. What good is the scientifically determined length of an English made pipe to the accuracy of a millimetre when the pipe was made to a length of inches and wear and tear of the mould and trimming and smoothing of the pipe after moulding would make such accuracy meaningless in any case? Why should an 18th century French plate be measured in the metric system or English inches when its manufacturer thought of it in terms of French inches? Does an excavated beer bottle hold an Imperial pint or a U. S. pint? Excavations which involve the cold-storage of artefacts until the excavator - or worse still, someone else - can get round to looking at the material years later should be forbidden: as Stone says (p. 8), without full analysis and recording of such material American historical archaeology will never realize its potential. A discovery, as General Pitt-Rivers, the father of modern
British archaeology put it last century, dates only from the time it is put on record by publication.

If we can combine archaeological and documentary evidence into a valid end product we should be able to contribute materially to what is at present known, largely from statistics taken from mass-surveys - about standards of living (cf p. 16). When an economic historian notes (Ashton 1948:158) that while the cost of living in England in 1831 was 11 percent higher than in 1790 urban wages had increased by 43 percent, it might appear that when the extremely rapid rise in the cost of living during the period 1790-1815 had subsided to a more stable and somewhat lower level the average worker was better off than he had been at the end of the 18th century. But if this is so, and we accept that the standard of living in England rose steadily throughout the 19th century, how do we explain the incredible and ever-worsening poverty in the industrial slums, such as Mayhew in the 1850's and Booth in the 1890's recorded in London? In fact, urban wages were largely based on those of skilled workers in full time employment and their use disguises the growing number of chronically under employed and the sweated labour in the appalling slums of the industrial cities (Thompson 1963: chapter 8, 1968 ed.).

Each site an archaeologist excavates is unique - it is not the product of an anonymous class of people but the product of individuals. Through it the archaeologist sees, however dimly, how individuals lived and worked. For all the limitations of documentary and archaeological evidence about a site, an archaeologist is nearer those individuals than if he were an historian simply averaging the standards of living from tables of food prices and typical wages. I doubt if we should, or even could, deduce meaningful mathematical relationships between wealth, occupation, and ownership of porcelain or anything else, as Stone suggests (p. 2) we may be able to do; but with a proper interdisciplinary approach we should be able to do something more important - reconstruct something of the life of ordinary people whose homes and ways of life have not been preserved among the historic mansions and national monuments.

Elsewhere, in trying to define what I mean by history, I have stressed the endless resources open to those studying the past. Stone (p. 13) notes some of these, and there must be diaries, travels, and novels which describe pottery and many other objects of material culture in terms other than those of the legal inventories of the dead owner. We are really only on the threshold of vast research possibilities. How much we will ever be able to make use of this material depends in large measure on how we apply what we already have available to assist archaeological work. Certain archaeologists, notably Noel Hume and South, have made excellent use of documentary resources to produce integrated studies, but few if any conventional historians have tried to look at historical evidence through the eyes of an historical archaeologist. Stone's work is therefore important because he has set out to attempt just that. It is up to all archaeologists to help strengthen and widen this bridge, and it is good to know that at
least one university appears to realize the value of such interdisciplinary study. If this particular commentary on Stone's work seems unduly rambling it in fact reflects credit on Stone's paper, for each reading of it and of my own drafts has prompted fresh thoughts in my mind. This, as much as the historical information in the paper, contributes to its bridge building qualities.
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ON THE RELATIONSHIP BETWEEN HISTORY AND ARCHAEOLOGY

David S. Brose

Garry W. Stone, in his paper "Ceramics in Suffolk County Massachusetts Inventories 1680-1775," has raised some extremely interesting questions about the relationship between historical documentary research and historic sites archaeology. Stone's thesis seems to be that a quick diffusion of ceramic styles among certain groups indicates a rather similar status among the members of those groups. Certainly other interpretations are possible. Perhaps an interpretation with a more economic viewpoint might be relevant. One might regard the length of time required for the acceptance of new ceramic types as inversely proportional to the involvement in those trading activities dealing with the china itself. Certainly the economic classifications proposed or dealt with by Stone do not preclude this possibility.

As Stone has noted (3) the inventory samples of probate records seem rather skewed toward what I would call luxury goods. I would not, however, agree that this reflects a "relative lack of economic importance" (Stone: 3) for the common earthenwares. These were, from all historical evidence, much more important to the colonial economy than the china which entered as trade-wares. Stone clearly appreciates the role of archaeology in delimiting the distribution of local earthenwares, and I would also add, since historical records are so poor in this regard, archaeology will eventually be the only way to produce a clear chronology of stylistic replacement of those types of common earthenware which figure so prominently in colonial histories. This will be especially important since, even for those types of china which are included in inventories, the inventory seems more concerned with the probate estate. That is, prices (and therefore the values) are more adequately treated than are the descriptions of the actual materials.

The relationship between archaeology and probate inventory analysis is far from clear. It seems to me that they are much more complementary than Stone has indicated. By this I mean that they do not represent two very different methods of dealing with the same data, but rather represent two different methods for dealing with two mutually interrelated aspects of the same phenomenon. An example of what I mean might be found in a single dwelling where archaeological analysis could produce a detailed and comprehensive list of materials broken during the occupation. Probate inventory analysis on the other hand, would produce a list of "heirloom pieces" which the archaeologist will not find at the site. These pieces will have not been broken in everyday use and will be passed on to the succeeding heirs. Both types of analysis must be used to reconstruct the total ceramic inventory of any one occupation.

This does bring up the problem of the life span of particular ceramics. It seems to me that coarse earthenwares will be broken more frequently than will fine tablewares. This seems evident for three separate reasons. First, common ceramic earthenware is lower fired than is high quality porcelain. This must mean that it has a somewhat poorer quality. Secondly, common earthenwares are much cheaper than imported oriental porcelain and therefore, one would expect less care to be paid to their use. Lastly,
the common everyday non-prestige item utility wares are certainly more in use than are the finer oriental porcelains. One would therefore, expect them to be subjected to more opportunity for breakage, and therefore one would expect more breakage to occur.

These three hypotheses could be tested but by very different methods. The first hypothesis is clearly born out by physical tests which have been made by many ceramic engineering laboratories (e.g. the Ohlin Materials Research Institute of the Case Western Reserve University). Common experience would also tend to bear out this difference in materials strength. The second hypothesis could only be tested by a historical reconstruction of economic purchasing patterns for the relevant time period. These documents, of course, will be difficult to obtain, and one might in their place use modern "ethnographic" analysis of value-related activities in urban and rural areas. The third hypothesis could be tested by time-motion studies using tagged ceramic pieces in any one of a number of modern households having both coarse earthenwares and fine porcelains. Common sense suggests that all three are correct. To some extent the life span of various types of ceramics can be correlated with the particular function to which they are put (Brose, 1967). In no case, however, will "the excavation of garbage pits, privies, and other trash deposits from households for which inventories have survived, ... (provide) answers to these and other questions ..."

Archaeology will, in fact, provide a much better basis for projecting cultural systems, than will inventories. This is true because to some extent, all materials employed at a site will probably be represented in an archaeological sample. On the other hand, Stone (pages 2-5) indicates probate inventories will not do so. However, both provide the most accurate picture. This, I take it is the ideal collaboration of history and archaeology described by Cleland and Fitting (1968).

I do not have some question as to just what is being measured by the diffusion of these items. While taste patterns (Stone: 9) are clearly one factor, they are no means the only one. They may not even be the major one.

The distance from the source of the material being introduced and the communication networks over which it must travel, certainly will have a major effect on the adoption of new ceramic styles. The utility of the objects will also be somewhat important. In this light, one must consider the different use of objects in different societies, or sub-societies. That is, porcelain is a common utility ware in China and Japan. This has been true since the 16th century (personal communication: Kamer Aga Oglu). They are still a prestige item as much as anything else in Occidental societies. It is also clear (as Stone notes, pages 1-3 this volume) that the economic status of the individuals may have as much role in determining the acceptance of new ceramic styles as will their particular taste.

Indeed, the use of probate records creates problems. One would in fact need a rather long series of lists from a single family to verify Stone's claim (page 10) that individual households curtailed purchasing power as the inhabitants reached senility or that the goods were distributed among the children as they came of age. A long series of such lists would
allow us to determine what, in fact, within a ceramic inventory represented heirlooms, and what recent purchases.

Stone's paper represents one of the first original attempts to clarify problems of material culture on the basis of historical documentation. For this I can only offer my congratulations. No, I would also offer several suggestions: In describing several small inventories of ceramics (pages 18-19) Stone has indicated that variations in functional use of vessels can be seen. It might be instructive to draw up correlation tables based on the shape or presumed function of the particular ceramic type, testing this against the particular distribution in terms of the distance from the source of such materials. This might account for cases where tea wares out number dining items. On the other hand, it might not, and new information might be derived. A simple statistical test, such as a chi square, seems ideal for this situation.

The application of such a test to Table 1 in Stone's paper, gives a chi square value of 12.5812, and a p < .001. Clearly indicating that there is a very significant correlation between the status of gentlemen and the presence of porcelain. While this is, in fact, Stone's thesis, the information does not present itself unequivocally in the table as shown. This does bring up the question of what constitutes a "gentleman". Presumably something other than personal desire on the part of the titleholder was required. These additional criteria might prove quite useful in predicting the presence or absence of particular ceramic types in the dwelling of such a person. This criterion might also prove to be much more significant than position within trade network, economic status, or taste.

To return to the problem of small samples, Stone (page 20) has stated that one reason delft failed to compete successfully with porcelain was its unsuitability for cups for containing hot liquids. This does not seem to be a real functional absence, since some cups and saucers are present of delft. It was, therefore, functionally equivalent to porcelain cups and saucers. Perhaps the answer is its price, or its displayability as status indication. In the same vein, Stone indicates that utility stonewares do not become common until the 1720's and 30's in the inventories. This brings up the question as to whether these items are not common, or their inclusion in inventories does not become common practice, until this time. These are important distinctions.

It would also seem to be at this time (Stone: 21) that new types of earthenware were being enumerated in the inventories. These, however, were generally imports. This may in fact have relegated them to a position analogous to the oriental porcelains as status symbols.

Stone has perceptively seen that innovations in taste were made by those whose business brought them into contact with the new fashions. This brings us back to the original question of dependence of new styles on economic position, social position, or inclusion in ceramic trade networks. While Stone seems to feel (page 26) that archaeological evidence is limited in its ability to determine status due to the uncertainty of what the sample represents, he feels that a clumsy or old person could skew results in a different direction, and therefore he places little faith in quantitative sherd analysis. True. True also of inventories, for the same
factors are at work, but reversed. If a clumsy cook drops several pieces of porcelain to increase the apparent popularity of porcelain for the archaeologist, she by the same act, removes from the probate inventory several pieces of porcelain and therefore, decreases its popularity for the historian. This clearly indicates why these two approaches to the same data cannot be used individually, but must be used together for the total picture. Stressing again my pleasure at seeing this piece of historical research in print, I would ask Mr. Stone several questions. Why double the present sample of inventories as opposed to tripling it (page 2)? What would American ethnography be like (page 6)? Also, why do ceramics promise to be a sensitive index to differences between social and cultural groups, particularly during periods of innovation (page 9)? With these questions, and my chagrin at having my paper attributed to Stanley South (page 7, footnote 8), I would take issue only with one major point. I disagree with Stone that an inventory provides the historian with a more accurate measure of an individual's taste than does competent archaeological analysis. I strongly demure also, from the suggestion that this index of ceramic type by social status can be used for making intergroup comparisons. Porcelain seems to me a poor index for use in comparing value systems of 18th century Philadelphians and New Yorkers, or New England yeomanry or Pennsylvania farmers, since it is not at all clear that status and taste are significant factors in the presence or absence of certain ceramic types in different areas. While Stone's paper clearly documents their significance within a single Massachusetts county, factors (such as I have already suggested) of communication network economic status, functionally or utilitarian use of the ceramics, and distance from manufacturing centers as well as involvement in the trade involving such ceramics, seem to me equally, if not more significant factors in the distribution of porcelain or earthenware. Until such factors can be clarified, quantified, and separated, it seems premature to attempt interareal correlations by the use of ceramic indices such as Stone envisions. With this denial, I would again congratulate Garry Stone on a very fine paper which cannot help but point the way for future collaboration between archaeology and history in this nebulous young discipline we call historic sites archaeology.
In reading Mr. Brose's comments I have found myself alternating between wholehearted approval and violent disagreement. Of the two, the areas in which we disagree are by far the more significant. By forcing me to sharpen some of my own thinking in order to state these objections, David Brose has performed a favor, which I hope will lead to more sophisticated future analyses of both archaeological and historical data.

I would like to continue the dialogue in two areas. The first of these relates to the utility of historical and archaeological data. The second is our relative valuation of social versus economic factors in the diffusion of ceramic types.

Both Mr. Brose and I agree that the fullest information about a site is produced through synthesizing documentary and archaeological information. In specific areas, however, we disagree rather substantially. In discussing the relative life spans of particular ceramics, Brose indicates strong reservations about determining this from the excavation of documented sites. Instead, he opts to attempt this through logical deduction. However, in doing so, he is projecting from one cultural system to another. Such hypothesis generation is useful but dangerous, and must always be tested against data from the subject culture. This is well illustrated by Brose's arguments and proposed tests for establishing the shorter lifespan of coarse earthenwares vis-a-vis porcelain. Admittedly, earthenware is weaker than porcelain, but is this directly relevant? The question is whether an earthenware pot is stronger than a porcelain teacup. Usage patterns of historic ceramics - which strongly influence mortality figures - will not be determined by studying usage patterns in modern households, which have quite different cultural frameworks. The documents strongly suggest that Brose's statement that non-prestige utility items were more in use than Oriental porcelain is not correct for some households. In preparing a meal, perhaps only half a dozen pots and pans might be involved. Serving and consuming it, however, might involve 30 or 40 pieces of fine ceramics.

Brose's reservations about the use of inventories stem in part, I believe, from a misconception about the nature of inventories. Inventories are not composed merely of "heirloom pieces." In a family with sustained purchasing power, inventories record heirloom pieces and replacement purchases. Thus while the creation of archaeological evidence is accidental and irregular (within the regularities created by physical properties and usage patterns), the inventories represent groups which are, in part, purposeful creations. They then vaguely represent an "ideal" group of items. Through excavating dated trash deposits from the same household and comparing them to the "ideal" represented by the inventory, some rough notion of ceramic lifespans may be gauged.

Consecutive inventories, of course, provide much better evidence. Two inventories printed by Teller suggest that in at least one Providence
Inventories, in fact, are much better evidence than archaeological data for reconstructing vanished material culture. Archaeology will supply important qualitative information missing from inventories. It will also fill in substantial gaps in inventories, such as the coarse earthenwares. However, where in the archaeological sample will be found pewter, silver plate, furniture groupings, or oil paintings? The information contained in inventories is biased, but a full probate record presents a more vivid room-by-room description of an 18th century dwelling than archaeology can ever recreate.

David Brose and I disagree on the relative importance of social and economic factors in the diffusion of Oriental porcelain. Brose suggests that an interpretation with a more economic viewpoint might be more relevant, particularly when making comparisons between different areas. He thinks social status and taste might be secondary to such factors as distance from source, trade routes, involvement in commerce, and utility. As neither of us has done the research which would answer this question, it is impossible to judge between our hypotheses. Certainly I shall have to keep such factors in mind when I extend my research. However, I would like to offer information from two areas which suggests that social status and taste outweigh economic considerations in the diffusion of porcelain.

Within the limited area of Suffolk County, Massachusetts, social factors clearly outweighed economic considerations in the possession of porcelain. While inventory size — purchasing power — was directly related to the possession of porcelain, the most important economic factor was indirect. This was participation in commerce. Here the crucial ingredient would not appear to be greater access to porcelain — Americans were not yet involved in trade with the Orient — but the creation of a lifestyle in which conspicuous consumption was important. Only by accepting the primacy of social motivation is it possible to explain why three distinctly different consumption patterns — agrarian, industrial, and commercial — coexisted within the ethnically homogeneous population of colonial Suffolk County.

I question whether distance from place of manufacture and other logistical factors are as significant as Brose suggests they may be. For coastal America, water transportation would appear to be the great leveler. Distance from source certainly did not prevent the rapid diffusion of Oriental porcelain.

I am now starting a study which should differentiate between our hypotheses — a comparison for 1755-1764 of a coastal North Carolina county and a frontier piedmont county. Until the results of this are available, I can only offer two sites which strongly suggest that transportation and

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utility were secondary to social considerations. These are Fort Ligonier and Fort Dobbs, where refined ceramics have been found in the construction levels. Both were erected during the French and Indian War on the raw frontier, far from water transportation. At Ligonier, a stream silted up during construction produced a variety of fine ceramics including Chinese porcelain. This, however, was a major fortress. Fort Dobbs - postage stamp sized post on the North Carolina frontier - has produced a similar range of ceramics, although its normal garrison was only two officers and thirty men. In both cases, pewter would have been far more practical. The conclusion seems inescapable that refined ceramics were an important part of the equipage of an officer-gentleman.
DIVERSE COMMENTS AND SUNDRY SUGGESTIONS CONCERNING

CERAMICS IN SUFFOLK COUNTY, MASSACHUSETTS, INVENTORIES 1680-1775:

A PRELIMINARY STUDY WITH DIVERSE COMMENTS THEREON, AND SUNDRY SUGGESTIONS

Charles E. Cleland

In his paper *Ceramics in Suffolk County Inventories 1680-1775*; Garry Stone purposes to explore a number of problems.

1. How can probate inventories be used to understand the cultural context and importance of particular artifacts?

2. How may records be used to trace the introduction and diffusion of ceramic types?

3. How are various ceramic types related to social use patterns as gauged by levels of economic and/or social position?

4. How do probate inventories serve as a means of understanding the spatial distribution of ceramics within dwellings?

5. What are the utilitarian functions of particular ceramic types?

Stone is exploring cultural problems by using historic data, as we would expect, his conclusions are stated in cultural terms. Stone is led to observe for example, that Boston mariners of 1760-1770 placed a higher social value on porcelain than other members of their economic class. This observation is not a historic fact, but a cultural generalization drawn from historic fact. Stone is thus trying to discover the cultural principles which account for the facts of history. Such a method is bound to draw the wrath of historians who seem to harbor a congenital revulsion for this type of approach, preferring instead, to think that history determines the course of culture. It seems, however, that the process of working from particular fact to general cultural proposition is the only meaningful way to understand our data whether it be historic or archaeological. The general philosophy which Stone expounds in this paper is not only reasonable but is in fact the only realistic way of explaining the spatial and temporal distribution of artifacts.

While Stone's method is a superb example of imaginative historic research, some criticism could be made of the way his study was carried out on a more specific level. His categorization of ceramic types is bothersome in that white salt glaze, often specifically mentioned in the sample inventories, is apparently included in the stoneware category while delft, a tin-glazed earthenware, is listed separately from the earthenware category. In addition, Stone's tabulation of results by percent of inventories which include a specific ceramic type does not seem to be particularly meaningful. No doubt Stone chooses this method because of the lack of specificity in the inventory listings. It is admittedly difficult to determine numbers of vessels from descriptions such as "a set of china" or "one large parcel of delft." Faced with a similar problem, Teller (1968)
calculated the average number of vessels of each type per house; this system (which Teller does not explain) is more useful but undoubtedly less accurate than Stone's.

Another problem seems to involve a misunderstanding of the use and potential of archaeological data and the relationship between archaeology and history. Stone seems to despair in what he sees as major limitations in the use of archaeological data. For example, "the use of archaeologically recovered evidence to determine status is limited by the uncertainty of what the sample represents. For one site, too many variables influence the number of sherds recovered to place complete faith in quantitative sherd analysis" (p. 26) or "archaeological evidence of material artifacts may never provide as firm a basis as inventories for projecting the cultural system they represent, it frequently must serve as such." (p. 7). It seems rather self-defeating to regard either archaeological or historical data as better or worse for solving certain kinds of problems. It must be kept in mind that archaeology and history produce different kinds of data, that these data are both subject to certain kinds of biases and that there are always situations where one or the other method produces the most informative data. Perhaps we could turn to the site of Fort Michilimackinac for an example of the kind of data produced by the historical and archaeological method and what this data can tell us about a problem.

Fort Michilimackinac, near Mackinaw City, Michigan, was founded by the French in 1715 and inhabited by French until 1760. At that time the site became a British military post and remained so until 1780. Extensive excavation and historic research has been undertaken at the site by Michigan State University in conjunction with the Mackinac Island State Park Commission. The historic records of the site contain a single inventory of the property held by John Askin in 1778. John Askin was a wealthy trader who by his own accounting was worth 12,882 New York pounds. While Askin's inventory is probably complete, the problem of precise determination of ceramic type from imprecise descriptions is troublesome (Table 1). It seems reasonable to determine the following ratio among Askin's ceramics:


The absence of delft from Askin's list may be because he had none, because he listed delft by form only, or because delft and porcelain were included in the category "china".

Both Teller (1968:572) and Stone observe a similar distribution of types in households of wealthy individuals. Succinctly stated; wealthy people could afford and did acquire porcelain in preference to other less expensive ceramic types. Whether the secondary forms are predominantly stoneware, white salt glaze or creamware depends upon whether the frame of reference is the early, mid or late 18th century.

With that principle in mind, we return to Fort Michilimackinac and examine the excavated ceramics which were studied by Lyle Stone (nd.). This study clearly shows that at the time of Askin's inventory (1778) creamware is by far the most common ceramic type on the site, delft and porcelain are of about equal frequency while white salt glaze is rare. We see equally
<table>
<thead>
<tr>
<th>Stoneware</th>
<th>Salt glaze</th>
<th>Porcelain</th>
<th>Creamware</th>
</tr>
</thead>
<tbody>
<tr>
<td>11 small stone dishes</td>
<td>18 white plates</td>
<td>1 large china bowl</td>
<td>2 large Queen ware jugs</td>
</tr>
<tr>
<td>1 flint sugar dish</td>
<td>5 large white cups and saucers</td>
<td>1 small china tea canister</td>
<td>8 Queen ware dishes</td>
</tr>
<tr>
<td>1 small white bowl</td>
<td>1 set new china</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>1 box with 2 sets of china</td>
<td></td>
</tr>
</tbody>
</table>

**Probable Additional Ceramic Forms**

- 1 large painted sugar canister
- 1 quart mug
- 1 butter plate
- 10 cups and saucers for all
- 6 tea pots
- 2 mustard pots
- 3 tea pots
- 1 sugar dish
- 1 butter dish
- 2 japaned candlesticks
- 1 large japaned tea board
- 3 milch pots

**Table 1.**

Ceramic Pieces Mentioned in the 1878 Inventory of John Askin, Fort Michilimackinac, Michigan
clear that Askin's inventory has practically no relevance for understanding the distribution of ceramic types at Fort Michilimackinac. Askin's tableware would be as much in style in Boston or Providence as it was out of place in Michilimackinac. The fact that a discrepancy exists between the quantities of various ceramic types in Askin's inventory and the types recovered in archaeological excavation does not mean that Askin's inventory isn't helpful in examining ceramic problems at Michilimackinac.

Archaeological excavation of two dwelling units of a British row-house at the site by James Brown during the summer of 1968 provides an example of how archaeology data was used to examine status problems and why Askin's inventory is important in interpreting the ceramics of the site. Brown's excavations demonstrated that the frequency of various ceramic types varied from one dwelling unit of the row-house to the next. Moreover, the same ceramic distributions even extended to the garden plots associated with each dwelling. One dwelling in particular produced an inordinate amount of porcelain and fine earthenware ceramics as well as other elaborate artifacts types. It seems quite clear that the difference in the distribution of ceramic types between the two row-house units reflects a status difference between the occupants.

It is true that we don't know who lived in each unit of the row-house, what the residents did for a living or the amount of their net worth. These are historic facts that are really irrelevant to the interpretation of the archaeological data. The pertinent information includes: (1) what are the differences in the spacial distribution of ceramic forms; (2) are the differences significant; and (3) what forms of cultural behavior account for distribution differences? These are, of course, the same basic questions Garry Stone is asking in his paper. The facts of history are of no help to the archaeologist. The generalizations drawn from historic and ethnographic facts are, however, vital in interpreting archaeological data. Thus, to the extent that one could generalize about the kinds of materials held by wealthy men, John Askin's inventory could help us understand the occurrence of a great quantity of porcelain in one unit of a Fort Michilimackinac row-house.

Thanks to Garry Stone's more solid research, we are able to look at the spacial and temporal distribution of ceramic types more intelligently.
REFERENCES

Stone, Lyle M.


Teller, Barbara G.

Charles Cleland has put his finger on the weakest point of my paper—poor categorization of ceramics. This is true for two categories, stoneware and earthenware, where coarse cooking and storage vessels were lumped with dining and tea wares. In the future these will be separated. My mistake was that in devising adequately general categories for generally described items, I choose categories that were largely technological. I am becoming increasingly convinced that while technological analyses of ceramics are useful in recording changes in technology, commerce, and taste, they yield little information about cultural behavior.

Information retrieval can be improved by creating ceramic categories which correspond to values perceived by the culture within which the ceramics were used. Delft was deliberately separated from the other earthenware since it functioned culturally as imitation porcelain. The difference in cultural significance between creamware and white saltglaze stoneware is rather slight, and is probably not as great as the difference between plain blue and white porcelain, and the gilded and enamelled variety.

Even more information can be recovered by analyzing the forms rather than the ceramic types represented in the distribution. Form analysis—in combination with historical research into behavior—brings us directly to cultural actions. In the future a determined effort will be made to separate tea wares, dining wares, and cooking and storage vessels.

The problem of developing adequate categories is not simple. This can be illustrated by comparing Cleland's and my conclusions drawn from the sample inventory he reproduces. Cleland determined the following ceramic ratio:


I would lump all the refined stonewares together—white saltglaze, scratch blue, bisque red, lead glazed—since it is frequently impossible to distinguish between them, while noting in the discussion of stoneware that at least 29 pieces were white saltglaze. I would also eliminate the "1 box with 2 sets of china" as being part of his mercantile inventory. This would give the following ceramic distribution:

porcelain 2: refined stoneware 4: creamware 1.

Within the ceramic collection the ratio of dining pieces to tea wares is 3:1. However, this would obviously shift toward tea wares...
if some of the "Probable Additional Ceramic Forms" were included. It would be important to reexamine the original inventory to try to segregate through context the personal and commercial possessions in this list.

Both of these analyses are rather crude and do not wrestle with some of the finer points. What do the distribution of forms indicate about the actual processes of serving a meal or pouring tea? What does the mixture of types indicate about breakage and taste? Should a "display" function category be created for the large porcelain bowl and the 2 creamware jugs? The problem of creating categories for inventory analysis is quite parallel to the problem of creating appropriate categories for sherd analysis. The categories must fit the data, and should be both broad enough and specific enough to recover most of the information available.

My comments about the categorization have not been directed in any way at Charles Cleland. His remarks afforded an appropriate starting point from which to indicate problems in dealing with the data and possible ways of improving the analysis. There is one area, however, in which I would strongly differ with him on theoretical grounds. This is the statement that "the facts of history are of no help to the archaeologist."

By this Cleland does not mean that historical research into material culture is not relevant to the interpretation of archaeological data. Cleland is as convinced as I am that "generalizations drawn from historic and ethnographic facts are...vital in interpreting archaeological data." What he does state, however, is that in analyzing ceramics from an historic household, the occupation, economic status, etc., of the occupants are antiquarian details "irrelevant to the interpretation of the archaeological data." He considers as relevant: (1) the distribution of ceramics in comparison to other sites; (2) the statistical significance of any differences; and (3) what forms of cultural behavior account for the differences. In brief, his argument is that archaeological data can only be understood in terms of cultural generalizations. True, but it is also true that without "particular" data cultural generalizations cannot be applied. The occupation, wealth, social status, and ethnic background of a household is of the same class of information as the statistical distribution of their archaeologically recovered trash.

Cleland has worked himself into the logical and theoretical corner of implying only generalizations about material culture are relevant in interpreting archaeological data. This is patently false. Social, ethnic, and economic generalizations are equally relevant, but these can only be applied if the equivalent details can be determined. Frequently archaeological data—especially in non-historic situations—must be interpreted without such information, but this is an undesirable situation. This can be illustrated with Cleland's own example of the two Michilimackinac row houses that produced markedly different ceramic inventories. Without information on the occupants of the two houses we can only assume along
with Cleland that the difference in artifacts reflects some sort of difference in status. Should documentary—or archaeological—data become available which would indicate the actual economic and social statuses of the occupants, the analytical situation would be quite different. The analysis could then incorporate generalizations about the normal behavior of these classes, and could then attempt to isolate the significant variables. The direction of the interpretation could then shift from a description of the archaeological findings and projection of possible explanations, to an illustration of how economic or social factors effect cultural behavior. This, I trust, would be one type of interdisciplinary analysis towards which Cleland and Fitting pointed in last year's Forum.
COMMENTS ON GARRY W. STONE'S PAPER:
"CERAMICS IN SUFFOLK COUNTY, MASS., INVENTORIES, 1680-1775"

Ripley P. Bullen

Mr. Stone's very interesting paper makes some excellent points and introduces problems which can be solved with proper archaeological data. His suggestion that the better ceramics of Colonial times were concentrated in the large centers such as Boston and Providence is undoubtedly correct. Probably Salem—an important shipping center of that period—should be added to the list of New England "centers."

That certain items—China bowls for example—were less common in the rural areas seems correct. Their presence in rural Essex County (Andover) before 1781 is indicated by the inventory of Job Foster's estate, originally filed May 18, 1782, and available in the County Courthouse in Salem, Massachusetts. An addition to the inventory of this estate, filed in 1785 after a protest by Foster's son, included "11 stone plates and 1 dish, 5 turtle shell plates, 2 glass mugs, 2 wine glasses, 2 'delf' bowls, 3 China dishes, 8 two dung [tung] forks, 1 hay ditto, and case of drawers." Quantities are not any such large amounts as Stone presents for William Welsted's front chamber of 1773 and the date is a little bit later but some of the same high quality ceramics are included. This data supports Stone's age and area concepts.

In raising the question about the "average life span of historic ceramics," Stone lets the archaeological cat out of the bag. To date an historic structure accurately by archaeology, i.e., by the associated artifacts, it is necessary that all the specimens belong in a limited continuous time span and that they be in sufficient quantity to lend credence to the apparent dating of the structure. Heirlooms can usually be detected, when present, by their relative scarcity, but end dates are more apt to be accurate than apparent dates of construction. However, this method of dating, when used judiciously, is accurate enough for most purposes.

Stone does well to emphasize the quantitative as well as qualitative analysis of historic ceramics, as I will show shortly. He is undoubtedly right that the presence of China (porcelain) and Delph vessels in quantity indicated a fairly high status, at least economically. The important thing here is the quantity, not the quality alone.

An example, albeit of a slightly later date, of status ceramics in a non-status household and also some indication of the possible life of such objects can be gleaned from the excavation in 1943 of a cellar hole and its adjacent deposits in Andover, Massachusetts. This excavation produced parts of three China bowls (one beautifully decorated with painted flowers in a basket), a blue-on-white bowl, innumerable examples of blue feather edge and of Staffordshire transfer printed blue-on-white including a Clews "The Landing of Lafayette" (post-1824) and a green "Canova" plate (circa 1829), marbleware mugs and mochaware bowls, many
decorated cups and saucers, T. Meyer and Stoke queensware, four different kinds of drinking glasses, including footed tumblers, glazed mugs, some New England black-glazed redware teapots, earthenware milk separating pans, and a 1829 U. S. penny, as well as six matching brass escutcheons with drawer pulls, five table knives and eight two-tined or "tunged" forks all with rivited wooden handles, pewter spoons, glass bottles, much tinware, tools, gun flints, binding wire, buttons, smoking pipes, eye glasses, and arm tips from three umbrellas or parasols. The admixture of 18th and 19th century objects is evident with those of the 18th century in the minority.

There is documentary evidence that the cottage (called a "cot" in the documents, its cellar measured only 10 by 11.5 feet) was built in 1815 and burned down in 1845. The three China bowls and probably the Delph vessel and a chest with three drawers (the six escutcheons were decorated with incision and similar to mid-18th century specimens) can be traced to Job Foster's estate of 1782. This suggests a useful life of China bowls, in this instant, to be at least 35, possibly 65, years.

The owner and builder of the "cot" was known as "Black Lucy," originally a young slave girl in Boston in 1770 when she was purchased by Mr. Foster for his wife. Like other Negroes in that area, she became free shortly after the end of the American Revolution. However, she continued to live with the Fosters and when Mrs. Foster married Philemon Chandler, after Job's death, Lucy went with her to the Chandler home. Certainly Lucy could not be called a person of high status and the relatively large quantity of 19th century materials from her home clearly indicates the heirloom attribute of the China bowls. However, if only a limited inventory had been examined—such as might be secured from a small test—the residence of a late 18th century person of high status might have been suggested.

There is one other comment to make and that is that Lucy, although she was a person of low status, possessed a large collection of ceramic materials, much larger than might be expected. Analysis of her collection indicated that, in general, she owned only one of each type, i.e., of each decorative pattern. For example, if she had 12 Staffordshire blue-on-white cups, 12 different pictoral patterns would be represented. This is consistent with the local tradition that she, after Mrs. (Foster) Chandler's death in 1812, supported herself by day work. Undoubtedly, her extensive collection represented to a large extent damaged specimens given her by those for whom she worked. It indicates that the assessment of a person's social or economic status by the items found in their cellar hole or well must be done discretely. Obviously, quantification of archaeological material is important.
Bullen, Adelaide K. And Ripley R.

Knowledge of the types of ceramics used in America prior to the nineteenth century has come largely from studies of surviving specimens, archaeological remains, manuscript records, mercantile papers, and advertisements. Extant ceramics with documented histories relating them to an individual are so rare that it is primarily from archaeological excavations that sufficient information has been derived with which to analyze the cultural significance of ceramics. One can turn his attention to only a limited number of excavated sites, due to the expense and difficulty of carrying out archaeological expeditions. The scholar, forced to make use of trade and ownership documents, and seldom able to relate surviving specimens to those listed, is often limited to a general rather than a specific approach.

Mercantile records and advertisements, while providing valuable data for the economic historian, are of less interest to the cultural historian. While often giving specific descriptions of ceramics, and suggesting the quantities and relative prices of available types of wares, they generally do not identify the market for which the wares were intended. Such documents are also of limited value in establishing overall trends, since they cover short time spans.

It is necessary to consider the question of the value of this type of study of ceramics as cultural documents. Obviously, ownership represented a specific choice, for the functions served by ceramics might have been fulfilled as well by other materials, especially in terms of availability, price, and durability. The selection and continued use of certain types of ceramics and the frequent appearance of mended ceramics in inventories reflect definite attitudes about the function of the objects and about their social overtones. These attitudes, when identified, lend cultural meaning to surviving specimens, and prevent them from remaining simply curiosities to the cultural historian.

Mr. Stone considers American-owned ceramics from 1680 to 1775, using data derived from estate inventories, which have only rarely been used by ceramics scholars. He demonstrates the inventories' usefulness in gaining clues to cultural values. Although he is in apparent agreement with Barbara Gorely Teller, his study must be considered tentative, since his conclusions were based on such a limited number of samples.

Mr. Stone's graphs showing the frequency of appearance of porcelain, delft, stoneware, and earthenware in inventories are useful indices of the relative popularity of specific wares during the period under discussion. The peak- and trough-magnitudes are probably the result of limited data as well as changing popularity, for Mr. Stone notes that in the case of his urban sample for the period 1730 to 1770, only 88 inventories were used. That number evenly distributed over these forty years would result in data points determined on the basis of less than eighteen inventories each, with a minimum increment of five-and-one-half per cent. Even this figure is
deceptive, for examination of the "Boston" graph shows only two data points beyond the sixty per cent level, thereby increasing the minimum increment to just over nine per cent for each itemization of a specific ceramic type. More data would probably not change the curves dramatically. The relative growths and declines of popularity would doubtless remain the same although the curves would probably be considerably smoother. The sample size, however, does not detract from Mr. Stone's important conclusions based on the preponderance of ownership of porcelain by wealthy gentlemen, mariners, and merchants.

One of the most interesting points mentioned by Mr. Stone is the "status value" of various wares, which he derives from the location of the wares in the home. He notes that porcelains were usually displayed in the best room, while older or less elaborate wares were relegated to less public rooms. An especially important observation is that ceramics were often displayed, not stored in closed cupboards. These are of great interest in determining cultural values, and emphasize the value of estate inventories in cultural research. Unfortunately, specific identification of the wares and their price or value is today frequently subjective, if not impossible.

An inventory, of course, represents one man's estimate of another man's property. It is often impossible to determine the assessor's expertise in his duty, and it is presumptive to believe his knowledge was equally sound for all materials he was called upon to appraise. Reliance on today's definitions of the terms "delft," "earthenware," and "china" is risky, and other descriptive adjectives frequently used are difficult to define today. One debated term, "burnt china," is interpreted by Mr. Stone as meaning "enamel-decorated porcelain." Caution must also be exercised in comparing itemized values for ceramics in inventories. Comparisons of wares in a single inventory may be valid, but the variables introduced in using other inventories make conclusions suspect. Nevertheless, it is reasonable to infer that high valuations are indicative of elaborately decorated wares where "enameled" or other such adjectives are used. Until a common denominator is identified which will eliminate such variables as monetary equivalency and personal taste, specific prices must be considered as indicators rather than precise data points. Only the identification of surviving wares or fragments which can be tied directly to an inventory will help solve this problem.

A matter not discussed by Mr. Stone, and one which must be approached from sources other than estate inventories, concerns the relative popularity of European and Oriental ceramics in America. One wonders if one or the other conferred greater status to the owner, or if American buyers were even aware of the sources of the wares they purchased. This is assuming, of course, that there were no significant differences in their price of serviceability (Mr. Stone notes that delft tea wares were apparently unpopular--and probably unsatisfactory for that purpose).

Despite frequent ambiguities in colonial inventories, Mr. Stone has isolated specific information related to ownership of ceramics, and has applied that information to an understanding of cultural values in America. The accessibility of inventories recommends their use in the manner he has employed. The ownership of certain types of ceramics is only one aspect
of the broad subject of cultural history, but with increasing application, knowledge of that specific aspect may result in its identification as a sensitive index to changing values.