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DELAWARE



Further Work on the Pagan Creek Dike (Near Lewes)*

by

David Marine

In 1951 Mr. Frank Austin, Mr. and Mrs. Charles H. Robinson, Mr. Donald Dillon, Mrs. David Marine and uyself dug two test holes on the Dike. Our findings were reported briefly in the ARCHEO-LOG, 1955, Vol. 7, No. 1.

In this report I wish to record additional data that have accumulated since then. The location of the Dike is indicated in Figures 1 (cover) and 2.

It will make the subject more understandable and interesting, perhaps, if I give a little of the historical background of the area in the immediate vicinity of the Dike, and when more facts of history and data on the Dike become available, it may be possible to the the two parts together and come up with answers to the whens, hows and whys of the Dike.

The importance and purpose of this Dike may be indicated by the map of the eight Duke of York land grants in the area (See Fig. 3). These land grants were the old Dutch grants taken over by the English in 1664 with the identical property lines and acreage of the Dutch and redistributed by the English according to the rule, "To the Victor belongs the Spoils."

Going back six years (from 1664) may give us further insight. In 1658 there was a division of Dutch interests and activities on the Delaware because of the financial difficulties of the Dutch West India Co.

The Colony at Fort Casimir (New Amstel) (New Castle) was turned over to the City of Amsterdam to finance and administer while the activities at the Whorekill (Lewes) were retained by the Dutch West India Co. and one Peter Alricks was given the sole rights to trade with the Indians here. Referring again to Figure 3, it will be seen that the Dike is located on the I50 acre West India Fort tract of land and that Peter Alricks had the adjacent I34 acre tract to the northwest and Willem Claessen had the adjacent 50 acre tract to the southeast.

After the English took over there was a delay of 5 or 6 years before Duke of York grants were made and Helmanus Woolbanck (Wiltbank) made a trip to the Maryland Colony land office in Somerset County. On October 3rd, 1670, Jenkins - the surveyor in charge of Eastern Shore of Maryland land office - cane to the Whorekill and surveyed these three tracts of land. For this move, Mr. Woolbanck (Wiltbank) was charged with treason by local land owners, but the charges were squashed, and in 1672 Mr. Woolbanck was given a Duke of York title to this West India Fort tract. It is fascinating to speculate on why Mr. Woolbanck was so anxious and took such risks to get possession of this tract. He also purchased from Daniel Brown the 400 acre "Tower Hill" Duke of York grant directly across Pagan Creek and nore than one half of this tract was still owned a century later by a grandson - John Wiltbank. Thus Helmanus Woolbanck owned both tracts of land directly connected by the Dike.

*Footnote - (Our Society is greatly indebted to Mr. Samuel Russell, the owner, for his interest in and permission to make the investigation here reported.)



- (1) Great Dike (2) Old House Site (3) Russell Site (Indian Encampment) (4) Causeway (Parson's)

You will also notice that "The Old House Site" and probably the <u>Dutch Trading Post</u> were located on this West India Fort tract, as well as the <u>temporary Indian encampment</u> (known as the <u>Russell Site</u> in the records of the Sussex Archeological Association) (The ARCHEOLOG, Vol. 9, No. 1, 1957) and also that both are on or close to "The road over the Dike from Polit Town" (Pilot Town).

The earliest documentary evidence of the Dike we have to date is the Louis Chambers find of a plot by John Shankland dated Jan. 26, 1773. (See ARCHEOLOG 1952, Vol. 4, No. 2). This plot shows the Dike, marsh, Pagan Creek, land and road approaches essentially as they are today.

Briefly this is the historical background for the meagre data we have so far collected regarding the Dike.

Reference to Figure 4 will show the ground plan of the Dike as it exists today - its <u>apparent</u> length (about 700 ft.), curve, directions, width at the top (9-10 ft.), wooded and uarsh areas and the places on the Dike with and without groundsel or salt bush. The nine principal test holes are also indicated. The curve in the Dike requires notice. A plausible hypothesis would assume that construction of the Dike began at its northeast terminus on the West India Fort tract because (a) of the road approach from the Whorekill (Pilottown) and (b) of the probable source of the sand used, and extended on to the marsh on the same course as the road. If this course had been followed the whole distance across the marsh, it would have crossed Pagan Creek tangentially and also would have encountered considerable marsh on the southwest bank, but by curving the Dike more to the southwest, they were able to cross the creek at right angles and have firm high ground for the couthwest bridge abuttment,

In Figures 5 and 5a I have indicated present elevations in relation to the marsh level. These elevations are not drawn to scale, but are relatively accurate.

In analyzing this figure, one notes a depression with an over all length of approximately 40 ft. beginning about 6 ft. back from the present field line. In Figure 1 (aerial photograph) you will notice a bulge of the tree line into the field and that the bulge reaches its maximum in the line of the Dike. I believe that this bulge indicates that sand had been removed from this area. Back in 1951 this depression was the subject of considerable speculation by Orville Peets and myself as to its significance. Our speculations would have eclipsed some of Baron Munchausen's Tales, but none of them were satisfying or convincing, so I consulted Mr. Samuel Russell, whose family has owned this property since 1836, and he was able to offer a very brief and satisfying explanation as follows: "My father sold fill dirt from this area to the Queen Anne R. R. Co. about 1896 for the causeway they were building across Pagan Creek a little upstream from the Dike". The old saying - "Truth is often stranger than fiction" can occasionally be modified to read - "Truth is often simpler than fiction".

In view of this fact and some circumstantial evidence, it is obvious that the original Dike began farther out, or, put in another way, that the actual length of the Dike is considerably





Fig. 6

Sand-mud junction; showing the sharp line of demarcation. Specimen taken from test-hole #2 with a spade. shorter than present appearances would indicate - possibly by as much as 65 ft. This is also suggested by the present width of the zone of larger trees (indicating higher ground) and by the width of the zone of scrub and brush (indicating wet ground) which terminates in the brushless marsh. It is further supported by our 1951 findings in test hole #1 which is 114 ft. from the present field line. As reported in the ARCHEOLOG (1955) this test hole is a trench cut from the castcrly side of the Dike to the midline and 43 inches deep to the original hard shore bottom. Forty one inches of this depth was composed of fill dirt consisting of marbled loam arranged in irregular areas of dark top soil and soil intermingled with yellowish clay. There was no mud below the fill dirt - only a dark line on the underlying sand indicating the original shore or beach surface. It was in the fill dirt removed from this trench that we found one intact notched arrowhead and several flakes.

You will notice from figures 5 and 5a that this trench (test hole $\frac{1}{2}$) is near the middle of the 196 ft. stretch where the present Dike top averages around $2\frac{1}{2}$ -3 ft. above the marsh level. It is safe to assume from the above data that test hole $\frac{1}{2}$ is close to or on the original shore or beach line, and that the original bank (if any) was toward the present field from this test hole. An attempt was made to find the line of demarcation (if any) between fill dirt and a possible original undisturbed bank.

To this end test holes "a" and "b" were dug 84 and 74 feet respectively from the present field line to a depth of 38 inches below existing Dike levels to a homogeneous light gray sand bottom. Above this sand each hole had contained only yellow-gray fill loan. Water rose in hole "a" to a depth of about 10 inches while none appeared in hole "b".

Test hole "c" was begun 65 feet from the present field line or 9 feet from test hole "b" and was extended 15 feet toward the field as a trench. This trench, at its beginning, was dug to a depth of 33 inches below the present Dike level which is at full height here at its beginning and 24 inches below present ground level at the field end, which is in the depressed area of the Dike.

At its beginning (65 ft. from field line) this trench showed fill dirt to a depth of 28 inches and then undisturbed yellowish sand. At the other end of this trench (15 ft.) yellow undisturbed sand began at 9 inches below present surface. There was no fill dirt here, but since this end of the trench was in the depressed area of the Dike, some fill dirt (loam) may have been present here before the dirt was sold to the Railroad Co. in 1896.

A fourth test hole "d" was dug 40 feet from the field line (10 feet toward the field from the end of the trench - "c") and near the deepest part of the depression. This hole - 24 inches deep - showed 8 inches of brownish sand and humus and below this undisturbed yellow sand.

below this undisturbed yellow sand. From these data it would appear that the original fill began about 65 feet out from the present field line and that this roughly represents the dividing line between the original high ground and the beach. The original layout of the Dike,



however, is complicated by the 1896 renoval of sand and the destruction of the roadway (Dike) in this area, which masks any evidence of sand removal prior to 1896. It might well be that the sand used to stabilize the marsh was also taken from this convenient Dike approach area and thus accounting in part at least for the present northeasterly bulge in the field line in this area (see Cover (aerial photograph Fig. 1). This Dike approach area is the most elevated of any ground along the field line and is composed entirely of yellow sand.

Test Hole #2 - This is the 2nd and last test hole dug in 1951 and is 122 ft. out from test hole #1. Here the top of the Dike at present is 14" above the level of the tidal marsh in contrast with 22-3 ft. at test hole #1. By punping out the water and removing some accumulated debris, I exposed the white sand noticed in 1951, which we at that time designated "beach sand". With a spade and bilge pump I was able to remove the layer of pure white sand 26 inches in thickness lying on a dense dark gray brown mass of compressed marsh grass, roots and marsh mud. (See Fig. 6). A probe was pushed down through the compressed grass and mud 22 inches to the hard sand bottom or 62" below the present top of the Dike. This was later confirmed by soil cores. None of the original clay superstructure of the Dike was present at this point. Since a 26 inch thick layer of bleached sand was proven for test hole #2 and was absent at test hole #1, it was necessary to determine where the sand layer began. I went back 40 feet from test hole #2, or 82 feet out from test hole #1, about half way down the steep slope of the Dike and took a core designated Test Hole #La. The top of the Dike here is about 16-17 inches above the marsh level. This core showed brownish sandy topsoil and a yellowish clay layer on a thin compressed layer of black mud and sand which was the original bottom and 32 inches below the present dike level. There was no white sand layer - only clay fill extending to the dark thin layer of compressed marsh mud.

<u>Test Hole #1b</u> was dug 20 feet back from test hole #2 or 102 ft. out from test hole #1. At this point the honey combed top of the Dike is roughly 15" above the marsh level. I took about 8"-9" of turf off and pushed the soil sampler down 28 inches further to hard bottom. This core contained no clay only a layer of white sand about 9 inches thick above the black mud, which here was mixed with the sand of the hard bottom which is approximately 36-40" below present top of the Dike. The data obtained for test holes Nos. 1a, 1b and 2 show that the sand layer begins somewhere in the 20 ft. between test holes No. 1a and No. 1b, or somewhat more than 20 ft. northnortheast of test hole No. 2 and somewhat less than 102 ft. south-southwest of test hole No. 1. The fact that the layer of "bleached" sand is 26" thick at test hole No. 2 and about 9 inches thick at test hole 1b (20 ft. apart) indicates that the sand layer probably begins very near test hole No. 1b. The Dike from test hole #1a on out contains many caved in

tunnels - probably of muskrat origin.

Test Hole #3 is 315 ft. out from the field line and 79 feet from Test Hole #2. The Dike here is now only 5 or 6 inches above the marsh level, while at Test Hole #2 it was 14 inches above. After removing a layer of brownish muddy turf about 12 inches thick, I exposed the white sand layer. This is at least 24 - possibly 30 - inches thick, but I could not dig to the bottom of the sand layer because the wet sand caved in as fast as I could lift it out. I then put a probe down and found 6 or 8 inches more of sand before getting to smooth mud. I put on an extension and pushed the probe down 8 ft. from the top of present dike, but touched no hard bottom - only mud.

Similar excavations were carried out in Test Holes #4, 5, 6, and 7 with similar findings. You will notice that Holes #3, 4, 5, 6 and 7 are all located where the dike at present rises only a few inches above the warsh level.

Test Hole #8 - This is of interest in that the Dike here is preserved and rises about 2 ft. above the marsh level and below the turf there is still the layer of yellowish gray and marbled clay mixed with sand which was last noted in Test Hole #1a. This clay is laid directly on the white sand and served as the road surface. In digging, I found the top soil, grass and humus layer about 9" thick and the clay layer about 6-8" thick, down to the white sand layer. A core taken adjacent to the test hole shows 9 inches of brownish sandy soil, grass and brush roots, six inches of clay and 19" of white sand lying on black mud (peat). About 5 inches of this peat was included in the sample core. This mud is firm, compressed and relatively dry. The line of demarcation between sand and mud (peat) was quite sharp. The total depth of the dike from its top to the compressed mud at present is 34 inches. A probe put down 8 ft. showed no bottom, as was also found at Test Holes #3, 4, 5, 6 and 7.

Test Hole #9 is located where the Dike surface drops steeply to the soft mud level of the marsh and shows topsoil and clay absent, but the sand layer is about the same as in Test Hole #8. The white sand layer could be followed and mapped for 23 ft. beyond Test Hole #9, but no definite sand layer could be determined beyond this point.

Bridge abuttment - By probing in the soft mud toward the stream bed, one could outline timbers about 2 ft. down in the mud beginning at 8 ft. from the end of the sand layer. These timbers were arranged side by side and at right angles to the long axis of the Dike, Across the downstream ends of the cross logs is a 14 ft. log lying parallel with the long axis of the Dike, and on the downstream side of this log is a row of planklike pieces of wood driven into the mud, probably to hold the log in place just as there are at least 3 posts or small piles driven into the mud on the stream side to hold the transverse timbers in place. To thoroughly investigate this bridge abuttment will require careful planning, some engineering skill and considerable additional equipment. This is a project in itself.

South West Bank - Two test holes six feet apart were dug on the southwestern bank of Pagan Creck in the line of the dike and on the original "Tower Hill" Duke of York land grant. Here the stream impinges on the bank with little or no intervening marsh. Tost Hole #1 was dug on the sloping bank 18 inches back from the level of tidal water existing at the time. There was brownish sandy top soil to a depth of 9 inches and under this was a layer of pure coarse yellow sand about 8 inches in thickness. This sand was resting on very firm reddish yellow clay. I dug into this clay to a depth of 4 inches and the total depth of hole was 21 inches. Test Hole #2 was dug 6 ft. back from Test Hole #1 to a depth of 31 inches. The top 8 inches was composed of brownish sandy soil and grass roots. Then a layer of marbled gray and yellow sandy clay 14 inches thick was encountered, and below this I dug into a very firm packed reddish yellow clay similar to that encountered in hole No. 1 for approximately 9 inches.

No conclusions can be drawn from these few data beyond the fact that the reddish yellow clay at the bottom of each hole seems to be a natural undisturbed deposit, and that a planned exploration of this area should be made.

Discussion and Summary

I have probed the Dike extensively from Test Hole No. 1a to Test Hole No. 7; i.e., the section with the least elevation above the marsh level and therefore easiest to probe, and can find no evidence of underlying brush, poles, logs or other timbers except at the north eastern bridge abuttment mentioned above.

The question as to why the surface of the Dike between Test Hole No. 1a and Test Hole No. 7 is at present depressed nearly to the marsh level cannot be fully answered at the present time. I have already mentioned the rodent (muskrat?) tunnels in the steeply sloping sections around Test Hole No. 1a and beyond Test Hole No. 7, which might mean that in these areas the process of lowering the Dike level is still <u>active</u>, while in the intervening area (between Test holes No. 2 and 7) the lowering was accomplished much earlier by the same process, but is now <u>quiescent</u> or arrested because the rodents have entirely destroyed the clay fill portion of the Dike above the sand layer. Water action (tidal, rain, freezing, wave, flood) would necessarily hasten the destruction of the superstructure of the Dike after it had been extensively tunnelled by the rodents. In favor of this view as a major cause is the fact that the white sand layer throughout the depressed area is intact and overlaid only with mud and turf of ten less than a foot in thickness - indicating very little "<u>settling</u>" of the sand layer into the underlying mud over a period of 2 and possibly 3 centuries. Rodents could not tunnel the vet sand layer, and it would remain intact.

The outstanding and perhaps unique feature of this Dike or causeway, of course, is the 2 ft. thick (average) and at least 10 ft. wide layer of white sand (the full width of this sand layer has not been determined) beginning near Test Hole 1b, which is 216 ft. out from the present field line and ending approximately 23 ft. beyond Test Hole #9 near the stream - a distance of about 470 ft.

This sand is white and in the report in the ARCHEOLOG we called it "beach" sand. We should have called it <u>bleached</u> sand, since (1) we have no proof that it was hauled from a

beach and (2) because sea water would react with the iron oxide stain of the usual yellow sand, making iron chloride which, being soluble in water, would be removed.

There are also some differences in the sand found in the several test holes. In some it is coarse and in others fine. Also in one test hole (No. 4) the sand contains lumps of firm gray mud varying in size from 1/2 to 2 inches across and covered with adhering sand suggesting that this sand might have come from a beach with adjacent compressed mud strata in the bank. However, all told, I think the evidence available is insufficient to support a conclusion that this sand was taken from a beach. It would be more logical and in keeping with the available evidence to conclude that the sand was taken from the high ground on each side of the northern roadway approach to the Dike and was bleached by sea water after it was put on the marsh. Proof of where the sand, used on the marsh, came from, however, is further complicated by the 1896 destruction of this area of the Dike for sale as fill dirt.

The use of sand in this Dike was obviously for the purpose of extracting water, shrinking, firwing and stabilizing the adjacent underlying wud, which it succeeded in doing. I had never heard of such a use of sand (which does not wean much) but obviously it was not a new experiment for the builders of this early colonial Dike, or they would have tried it out first on a project involving less work and material. In addition, the builders of this Dike showed considerable engineering skill in delicately balancing the load or superstructure with the supporting capacity of the marsh.

I have been in touch with the Engineering Departments of the State Highway Commissions of Delaware, Maryland and the Federal Eureau of Public Roads. None of the personnel I discussed the question with had seen or heard of any instances or literature referring to the <u>horizontal</u> use of sand spread on a natural surface in road building to stabilize, dehydrate and harden marsh mud or other soft soils. (The <u>vertical</u> sand drain is a very recent development and was first used about 1925). I have spent considerable time in the libraries in Washington and Baltimore trying to find literature on the <u>horizontal</u> use of sand on soft soils to support roadways, but so far without positive results.

This use of sand in causeway building may be unique in Delaware, and if so, that would be important, but my research experience in another field (medicine) makes me feel certain that it has been used elsewhere, perhaps in the Low Countries of Western Europe by the forebears of those who built this early colonial Dike.*

(Footnote)

*The word "Dike" or "Dyke" probably has some Dutch significance. The earliest local record we have to date of the use of this word for a causeway is in John Shankland's plat of 1773, and he was only copying the local name commonly used in describing it. Why the word "Dike" instead of "Causeway" has always been used locally for this particular structure probably has historical significance. Nearly a century earlier (June 4, 1675), the Court at New Castle, Delaware, issued an order entitled: "To construct two Dykes or Highways along certain warshy lands." (Fernow, B., Documents relating the Colonial History of the State of New York, Vol. 12, 1877, pages 530-36; also -Scharf, J. T. History of Delaware, Vol. 2, 1888, page 860.) One of these Dykes was to be built with floodgates pri-

One of these Dykes was to be built with floodgates priuarily to keep out Delaware River water and was to be "10 ft. wide at the bottom, 5 ft. high and 3 ft. wide at the top." The other Dyke (specifications not given) was to provide a short cut for the traffic between New Castle and Swanenwyck (Swanwyck). These Dykes were completed in 1676 but no mention is made of the methods or materials used.

The Dyke between New Castle and Swanwyck, variously designated down through the years as "The Horse Dyke", "Broad Dyke" and "Wilmington Street" became part of the main highway between New Castle and Wilmington and has been repaired and rebuilt many times. An examination at the present time for traces of the original structure would therefore not be very promising.

The seven pages in Vol. 12 referred to above contain (1) Copies of several petitions, (2) a statement by the Magistrates and (3) a long letter by William Ton, Clerk of the Court regarding the opposition that had arisen. In these several documents the words "Dyke", "Dicke", "Highway" and "Causeway" are used interchangeably.

Since the above account of the Dike was written, I have received the following important information from Dr. L. R. W. Soutendijk, Financial Counseler, Notherlands Embassy, Washington, D. C., in a letter dated February 27, 1958:

"With reference to your letter of November 30th, I am now able to give you the following information which I received from the Netherlands:

It is indeed correct that the Dutch did not use wood, unless it was absolutely necessary, for the construction of dikes and roads. This contrasted with the English who used wood (e.g. in Romney Marsh).

Evidently the Dike you referred to was used as a road. Since the Middle Ages it has been the custom in Holland, if possible, to have a foundation of sand for road construction or otherwise at least to construct a road of which the bottom layer consisted of sand. One can still find traces of these layers of sand in the old roads which no longer exist because of peat digging and reclamation.

There is very little literature on this watter [I can confirm this statement - D.M.] Our knowledge concerning old dikes and roads is increasing because of the growing interest and the fact that more attention is given thereto when excavations take place."

Report on a Pleistocone Shell Deposit Henry H. Hutchinson

This unusual shell deposit was reported by W. J. Stoakley of 116 Clayton Avenue, Laurel, Delaware, July 14, 1957. It is located 1.3 miles west of meridian 75° 35' W., and 1.9 miles south of parallel 38° 35' N. The elevation is 20-25 ft. above sea level (U. S. G. S. map, Seaford quadrangle) and is on the farm of Emory Spicer, Portsville Road, Laurel, Delaware.

In order to obtain water for irrigating his crops, Mr. Spicer had excavated a trench measuring 325 ft. long, 25 ft. wide and 20-29 ft. deep. At a depth of approximately 8 ft. below the natural ground surface, there is a stratum of bluishgrey clay impregnated with fragile oyster (and other) shell. This deposit is 12 to 24 inches thick and extends beyond the length and breadth of the excavation and is on the line of the present ground water level. Most of the shell is broken into small pieces, but occasionally one can find a whole half or complete bivalve. The same type of shell deposit was reported to have been found at about the same depth when the C.C.C. was digging a drainage ditch on the farm of Columbus Phillips a number of years ago. The Phillips' deposit is about 0.4 miles west of the Spicer excavation.

On July 14, 1957, Henry H. Hutchinson was shown the Spicer deposit and examined and troweled through much of the exposed clay stratum at one end of the trench in an attempt to discover artifacts of human make but with negative results. He also took samples of the clay and shell for expert examination by the Marine Laboratory, geologists and others. On August 22, 1957, Dr. Shuster, of the Marine Laboratory, and his assistant went to the site with Hutchinson and spent several hours examining the deposit and looking for specimens. Some of the samples he took were sent to the Delaware Geological Survey.

Dr. Carl N. Shuster, Director of the Marine Laboratory, University of Delaware, reports that in addition to the common Chesapeake Bay oyster he found two prominent brackish water forams - Streblus beccarii and Elphidium. These forams have a distribution from Pleistocene to Recent in geological time so they alone cannot be a definite time marker. However, he thought that from the depth and circumstances the deposit could not have been a Recent deposit and therefore would be of Pleistocene age.

Chesleigh A. Bonine, Professor of Geology (retired) Pennsylvania State University, gave as his opinion that the deposit was laid down during the last interglacial period, probably 35,000 to 50,000 years ago.

Robert M. Germeroth, Geologist, Delaware Geological Survey, reports that in addition to the common oyster he found fragments of shell of the genus Arca - a clan. Also examination of the clay or mud revealed well preserved microfossil fauna - namely, Streblus beccarii and Elphidium genteri which range, geologically, from Pleistocene to Recent and are characteristic of strongly brackish water. He also saw good evidence of a Pleistoceno age from the stratigraphy and from the discoloration of the shell.

Crook and Harris in an article in <u>American Antiquity</u>, Jan. 1958, give two radiocarbon dates in excess of 37,000 years for material obtained from a Pleistocene terrace in Texas. <u>Conclusions</u>. Since the above authorities are in essential agreement that this is a Pleistocene deposit and the C-14 dates from Texas confirm Bonine's estimate, we can conclude that this deposit is 35,000 years old or more and was deposited when this area was covered with brackish water.

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Book Review

Ceramics for the Archaeologist by Anna O. Shepard (Publication 609 Carnegie Institution, Washington, D. C. 1957)

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In this book of 414 pages we have a much-needed manual by one who has long been recognized as the person best qualified to The intimate mixing of the advanced sciences of the write it. White man with sympathetic observation of aboriginal potters makes the unique quality of this book. A few quotations will illustrate this: (p. 86) "Potters have different bases for judging when firing is completed. They may burn a predetermined amount of fuel, or judge by the appearance of the fire or of the pottery, or be guided by time. Maria Martinez of San Ildefonso, famous for her lustrous black ware, follows a careful routine and times the different stages. On the other hand, a Santa Clara potter explained that she knew when the pottery was hot enough by its color. On two different occasions, her firing temperatures were measured with a thermoelectric pyrometer, and in each case her maximum was 750°C. It is not uncommon to judge temperature roughly by color. This can be done because light emitted by a black body is proportional to the fourth power of its absolute temperature. its absolute temperature. Pottery is near enough to a theoretical black body for rough temperature estimates to be made from color. * At the bottom of the same page under the sub-head: "Control of firing atmosphere" we read the following: "Since archaeologists souctimes classify pottery with respect to the kind of atmosphere in which it is believed to have been fired, it is important to know to what extent potters who did not have kilns could control firing atmosphere." This is an example of the very gentle but sometimes devastating reproof of inexact statements by persons supposing them to be self-evident but who are unaware of all the considerations involved. On the following page there is an accurate table giving firing data from Pueblo and Guatemalan potters but this is given a further analysis which must be read carefully if we are to attempt to base conclusions on this table. Persons who write of a craft from book knowledge alone are prone to errors that the worker in the craft reads with a feeling of dispair, for to correct then he would have to take up unfamiliar tools. Pottery is a craft of an extra-ordinary couplexity but with an appearance of simplicity, and also it is one on which a vast amount has been written, so the student who has worked with words but not with clay can easily come to feel that he has a learning that is proof against error. It may be a very small part of the purpose of this book to show the danger of one-sided knowledge, but it does show this very effectively, and it warns almost as often against unsafe deductions

based on contact with primitive methods as against those made on a plane too far removed from such contact.

The primitive potter is treated as a conrade but with no abdication on the part of the White scientist. Here is an example of this cooperation: (p. 90) "Everything that would be required was assembled near-by at the start, and the stages of firing were timed by clock. A small amount of fuel, dung and juniper chips, was added when the temperature had reached 300°C (fig. 6:1-2), but a constant rate of heat increase was maintained. The ware was intentionally fired at low temperature to avoid the risk of impairing high luster. After 25.5 minutes, at 650°C (fig. 6:3), two washtubs of manure dust, used to smother the fire, were thrown on all at once." And again: (p. 92) "I well remember the first time I put the long thermocouple tube of a pyrometer in a Pueblo dung "kiln." I expected the Indian to blame any accident that might occur on this new, strange contraption. Unfortunately there was a loud snap just as the fire was beginning to burn briskly. When the firing was completed and the "kiln" was opened, we found that a beautiful black jar had a large spall on the side. Without a remark, the potter examined it carefully; then after a moment she exclaimed, "Oh, I left a grain of corn in the clay." This explanation may have been the correct one, and was no doubt a welcome one under the circumstances, but the warning of the next paragraph must be quoted: (p. 93) "Sometimes the potter may attribute a firing accident to a natural but incorrect cause. It is important that the ethnologist who is making a record of firing understand the causes of defects in order to judge whether or not the potter is giving the correct explanation. This means knowing the working and firing properties of the clay."

A not unexpected criticism has come to me from the first of our members to read this book, and the objections made are certainly valid. For us the book is too much concerned with Southwestern, Mexican and Meso-American pottery. We are especially sensitive on this point, for we have been frequently irritated by the confident assumption, by persons who have watched pottery making in the Southwest, that they have seen pottery made as it must have been made by all Indians everywhere. And perhaps with less reason we are irritated also by the display of pots with elaborate designs in paint and slip, when our Indians used neither. The latter half of the book seens at first glance to be en-

The latter half of the book seens at first glance to be entirely devoted to ceramics of the Southwest, but there is a great deal in the text of the widest application. However, there is justification for the feeling that our Eastern Indian pottery has been somewhat neglected. We could easily find excuses for this neglect, but it may be better to insist that there is here a lacuna that should be filled. And we should go further; we should present specific problems and ask that they be answered. With our questions we should try to contribute information that would help in finding the answers.

The ceranics of the Middle Atlantic States have received a vast amount of study, but almost exclusively as sherds. The fact that these sherds were once pottery vessels is made to seen almost irrelevant. A photograph of a restored vessel is often shown, but the problems of its construction are seldom treated. Here another quotation is a propos: (p. 183) "Ideally, construction should be included among the criteria for pottery classification, for it is a fundamental part of technique; moreover, basically different methods of vessel forming are employed by prewheel potters, as we have seen in the review of practices of presentday potters. But unfortunately, as the archaeologist is well aware, potsherds are generally quite uncommunicative on this matter.^m

Several years ago I attempted to collect reports of pottery naking by Indian methods and the first I found was a paper by Theodore Stern "Panunkey Pottery Making" issued as Vol. III of Southern Indian Studies, 1951, at University of North Carolina, Chapel Hill, N. C. This is an excellent study of pottery as made by a few present day Indians, but the case for the survival of old traditions is very weak. The intervention of White archeologists in bringing prehistoric pottery to the attention of these modern Indians, and the influence of White teachers using examples and methods derived from the Southwest, obscures whatever there may have been of authentic in this manifestation.

All the pots illustrated or described seem to have flat bases, though in some the flat area may have been small. In North Carolina prehistoric pottery, flat bases are as rare as with us, i.e. less than one in a thousand.

Not making much progress in collecting material under this head, I wrote to the Ceranic Repository. Dr. Griffin replied that not much seems to have been published, but he kindly sent me a reprint of one of his own articles which, in intention at least, was exactly what I had hoped to find. It was entitled, "An Experimental Study of the Technique of Indian Pottery Making," by James B. Griffin and Carleton W. Angell. (Reprinted from Papers of the Michigan Academy of Science, Arts and Letters, Vol. XX, 1934. Published 1935). In the first paragraph we find this statement (p. 1) "However, we found that there were a number of tricks to this trade with which we were not familiar and that our work was merely a small beginning in the amount of technical investigation which will have to be done before this phase of aboriginal industry is understood." In the last paragraph (p. 6) we have this: "We shall also attempt detailed examinations of sherd cross-sections in an endeavor to determine the method of construction. There are, of course, many questions yet to be answered concerning the nature of the materials and the technique of manufacture of Indian pottery."

the technique of manufacture of Indian pottery." As to the experiments themselves, they seem to have been made by a sculptor under Dr. Griffin's direction, and we note that a saucer was used to form the base. On this a square of cotton cloth saturated with water was laid. This, of course, would have left a sharp imprint of the cloth on the base. We find such imprints of textiles or matting on the bases of some flat bottomed pots, but not often on conical bases unless the imprinting is an integral part of the treatment of the sides. Hence, we have no trouble in accepting the statement that there is room for further experimentation. But if there was further experimentation, it does not seen to have come to the attention of the Ceramics Repository.

When I mentioned the small number of good reports of experimental pottery making to a prominent Eastern archeologist, he replied that Anna Shepard had made every conceivable experiment in this field. A great many statements in the present volume could be cited as implied denials of any such claim, but the book as a whole would suggest that no useful experiment we could make would fail to involve some part of her experience or fail to derive help from it.

A final word of praise does not relate to the substance of this volume, but it is inspired by a sort of colophon (though on the first and not the usual last page) which gives valuable information to societies with publication problems. (back of title page) "Composed on Photon Machine by Machine Composition Company, Boston, Massachusetts. Printed Offset by The Mullen Printing Corp., Woburn, Mass., 1956. Reprinted Offset by The Kirby Lithographic Company, Inc., Washington, D. C., 1957." The text is as clear and sharp as letterpress and the many

The text is as clear and sharp as letterpress and the many drawings are superior to average letterpress. The original volume was not expensive at \$6.00, but this reprint is sold for \$2.00.

O. H. Peets

- Page 14 -

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