ABSTRACT

An excavation by Philip Allsworth-Jones in a small mound at Aivoji Estate-Ganyingbo Sea Beach, Lagos State, Nigeria, revealed a dense concentration of potsherds with little other sign of domestic debris. Local informants suggested that the deposit was created by boiling salt from sea water. An experiment in salt-boiling with traditional technology, closely resembling the material assemblage recovered from the mound, indicated that salt boiling would produce a deposit much like the one excavated. The experiment also offered insights into the kinds and intensity of labor needed to produce salt in this way, and the productivity of the method.
About ten years ago, I was given the opportunity to join a small research project begun by my colleague Philip Allsworth-Jones, then of the University of Ibadan, Nigeria. He was investigating a site that became known as Aivoji Estate-Ganyingbo Sea Beach, on the coast of the Bight of Benin a little bit west of Badagry. Badagry was an important port in the eighteenth and nineteenth centuries, and belongs to the area inhabited by the Egun people, who area spread along the seacoast from southwestern Nigeria into Ghana.

The sea beach at Aivoji is characterized by a large number of mounds, littered with pottery. I’ve seen these mounds at three places along this stretch of the Bight of Benin, and it seems likely that they occur continuously in between. The beach here rises to a high dune, forming the crest of the barrier island, and the mounds occur behind the high dune, in a zone about a quarter of a mile wide.

Allsworth-Jones became interested in studying these mounds after a colleague, G. Paradis, obtained radiocarbon dates in association with ceramics across the border in the Republic of Benin. The pottery was very similar to sherds Allsworth-Jones was finding in the Badagry area, including those on the surface at Aivoji, and to ceramics currently being produced by traditional Egun potters in the same region. Paradis’ dates suggested that makers of these ceramics had been in the area for at least the last millennium, and Allsworth-Jones thought that the mounds at Aivoji had the potential of providing data to test this idea.

Rather prudently, Allsworth-Jones decided to test the smallest mound, which he called Mound 1. This mound was only about 60 cm high, and less than 12 meters in diameter. He completed a single 2 x 2 m test unit, from which he recovered more than 22,000 potsherds, plus a few other odds and ends. Allsworth-Jones is a Stone Age specialist and doesn’t even like
potsherds, so he decided he’d quit with the one unit and invite someone who did like pottery to help with the sherd analysis. That was me.

We were impressed with both the density and the lack of variety in the deposit in this mound. Ceramics were by far the bulk of the assemblage, nearly 95% by weight. Of the rest, there were a handful of cowries and other shells, one ball clay pipe stem, and the rest were various enigmatic lumps of fired clay. Some of the fired clay seemed to be architectural, others were cylindrical or square-sectioned but with no obvious purpose.

The presence of the ball clay pipe stem in an upper mound level places the deposition of the mound within the period of European contact. Its bore diameter is approximately 6/64". Harrington's bore diameter chronology would place a specimen of this size within a date range of A.D. 1650-1750, with a most likely range of 1680-1710.

We submitted one sherd to Alpha Analytic for thermoluminescence dating. The specimen was recovered the early zone of the deposit. The sample gave us a date of 290 +_ 20%, or A.D. 1660+58 (Alpha-3192). Neither date is particularly robust by itself, but they are consistent, and, provisionally, I see this as a late-seventeenth- to early eighteenth-century deposit.

As I said, potsherds were by far the biggest part of the assemblage. Even within the ceramic assemblage, the lack of variety is striking. We identified six rim types, but more than 95% of the typable rims belonged to one basic form, which I call the “standard” rim. Of twelve decorative types, mostly roulettes, 85% of the identifiable sherds bore the three
decorations associated with the standard rim. In other words, the ceramics were heavily dominated by a single vessel type, a medium-sized jar or bowl very similar to a cooking pot.

This severely restricted range of artifacts strongly suggested an industrial, rather than a domestic site. Consultation with the local people suggested that salt making by boiling down sea water was a likely candidate for the industry. This suggestion begged for experimental testing, especially when Allsworth-Jones was introduced to a local man, Mr. Abraham Avoseh of the village of Ganyingbo, who remembered boiling seawater for salt during World War II.

During the war, the imported salt that most Nigerians had come to prefer was scarce, and to supplement supplies people in the Badagry area had recalled something of traditional salt-boiling practices. Although Avoseh admitted that his people originally had used ceramic vessels, he had used large metal basins, since ceramic vessels broke much too regularly. Avoseh also thought that ceramic vessels would produce much less salt due to inefficient heating. After some discussion, he agreed to supervise a one-day trial using clay pots, although he was pessimistic about producing any worthwhile amount of salt.

We commissioned pottery from a traditional potter whose work Allsworth-Jones had already studied, Madam Aihaji Dauda. Since we had not completed our sherd analysis and did not know what the original pots might have looked like, we asked for boiling vessels similar to the metal pans Avoseh had used during World War II, and also vessels to use as supports for the pans,
following traditional styles. Aihaji agreed that she would make the pots “strong” to stand up to intense heat, and produced two sets of support pots and four boiling pans. The boiling pans averaged about 50 cm in diameter. Three of them were about 24 cm deep, while the fourth was noticeably deeper, about 33 cm.

Before arriving at the site, we also arranged for the delivery of a pile of firewood and for a couple of laborers to help out. The laborers were more of a social necessity than a logistical one.

We arrived at the site shortly after 7 a.m. on January 25, 1986. The first order of business was to set up the fire. Our co-workers took charge, filling the support pots with sand for stability and laying the firewood so that the fire could be fed from all three directions, through the legs of the tripod. We got the fire going about 7:30, put on the first pan, and added two buckets of water straight out of the ocean.

The pot began to sweat almost immediately, the porous earthenware absorbing and leaking water. By 8:00 the outside of the pot was black with soot, but the water continued to sweat through the vessel walls. We filled up the backup pans with water to let them do their soaking ahead of time.

By 8:45 the water level was down about two inches, and there was a film of white, presumably salt, along the water line. The water was just beginning to boil. A little after 9:00, the drip of water from the bottom increased visibly as the pot began to crack. The leak increased further, so we emptied the first backup pan, put it on the fire, and poured the hot water into it.
Since one of the boiling pans had cracked on the trip to the site, we were left with only one backup. At this rate, we expected to go through our last pan by about noon.

The second pot held up, though. We added another bucket of seawater at 10:40, and another at noon. The fire was stoked frequently with wood and coconut husks. Several times, as the water level dropped, we tried to scrape off the white film or to splash it and redissolve it into the brine, but there did not seem to be any effect.

About 2:15, we went into a discussion of whether or not to add more water. We decided on another quarter bucket, as a compromise between getting as much salt as possible, and hoping to have the salt cooled and everything packed up by dark.

Final boiling went much more quickly than expected, however. By 2:50 the brine was so concentrated that it was boiling away at an almost visible rate. Within ten more minutes, it turned into a greyish sludge, and then a paste, and we took it off the fire by 3:00.

The final product was about 465 grams of salt, slightly greyish but with a very light, almost fluffy texture. Avoseh told us that the salt should be washed in a basket to get rid of the greyish tinge and the small bits of trash in it. He thought, however, that this was only done in recent times, to make the color more attractive to buyers used to the purity of imported salt.

The second boiling pan, the successful one, was crusted with soot on the outside and a thin film of salt on the inside, but
seemed perfectly fit for additional duty. Of the funnels, one was visibly cracked, but appeared fairly solid. We turned the first pot, the cracked one, into sherds for a comparative sample for our archaeological assemblage.

The next morning we returned to the site for a last inspection. It turned out that our successful boiling pan had cracked badly while cooling, and would be not be usable again. The funnels were still very warm at the base, and two were fairly thoroughly cracked. With the sand in them, though, they looked pretty solid, and they probably could have been reused if left in place.

The immediate results of the experiment were quite satisfying. We showed that it was feasible to make salt by boiling seawater in ceramic vessels, and that the process would incidentally produce a lot of potsherds that closely resembled the ceramics excavated at Aivoji Mound 1. Most of the sherds would be from the boiling pans, with a few funnel sherds mixed in.

We don't, however, have an exact match. On analysis, the ceramics excavated from Mound 1 belong to much smaller vessels than the basins that Aihaji Dauda made for us. The excavated “standard” vessels are only about 30 cm in diameter, and relatively deeper. Intuitively, we’d like to think of boiling vessels as wide and shallow, to maximize surface area for efficient heating and evaporation. The Egun do use shallow ceramic pans, but as frying pans and griddles, not as boiling vessels.
The earliest description I have of West African saltboiling is by William Bosman in 1705, a very appropriate time for comparison to the Aivoji deposit. Bosman did not mention any unusual size of boiling vessels, but described another way of increasing water capacity: “these, I say, use Earthen-Pots; which they set ten or twelve next another; thus making two rows, being all cemented together with Clay as if they have been done by a Brick-layer” (Bosman 1705:309). Some of the clay lumps found in Aivoji Mound 1 have positive impressions of the potsherd rouletting, and may well be the cement as described by Bosman.

I’ve spent most of my last 13 years in Mississippian archaeology, on a site of circa A.D. 1100-1350 in the central Mississippi Valley. There are a number of issues relating broadly to salt-making by the Mississippian people, to which the Aivoji data make interesting comparisons.

On a very shallow level, we can compare vessel shapes. One Mississippian vessel is known familiarly as the Wickliffe funnel, with a wide opening at one end, usually assumed to be the top, and a small hole at the other, thought to be the bottom. Identified in the 1930s, this vessel has always been a puzzle. Suggested to be a funnel, juice press, or moss-packed filter, its function remains unknown.

One of the surprises of the Aivoji project was finding a pot almost exactly like the Wickliffe funnel in the Badagry market, whose use was well known in the local community. This pot is still used in sets of three as a tripod support for a cooking vessel, exactly as you saw in the Aivoji boiling experiment. James Bellis reported similar pots in southern Ghana, where one function of the hole in the top was to allow a cook to insert a stick and move it under a roof in a sudden rain. Similar hearth pots are in use elsewhere in Ghana. Is this a good analogy to the Mississippian funnel?

Barring recovery of a tripod set of funnels in situ, an experience so far denied to us, evidence for such a pattern of use may be difficult to document. Tim Pauketat has pointed out
that funnel pots used as tripods should have been manufactured in sets, which should occur in household assemblages. He was not able to demonstrate any evidence for matched sets in his American Bottom sample.

Another possible approach is to look at the characteristic sooting effect, in which an arch-shaped band of soot surrounds the area of the pot wall most directly in contact with the fire. David Hally has shown that sooting patterns are a significant source of data for studying vessel function, and we’ll have to look at Wickliffe funnel sherds for similar traces.

There are, of course, well-known problems in the use of ethnographic analogy, especially when simple morphology is the basis of comparison. Clearly, a superficial vessel resemblance cannot be used to affirm that the Mississipians used the Wickliffe funnel as the Egun use their vessel.

More useful analogies have to do with the idea of salt-boiling, the techniques, logistics and archaeological contexts involved. I think it’s useful to compare the Aivoji data to Jon Muller’s work at the Great Salt Spring in Southern Illinois, which is clearly a salt-boiling site. Muller describes “an unusual archaeological assemblage of considerable uniformity.” He notes three characteristics of his assemblage: limited categories of artifacts; uniformity of materials, particularly vessel type; and the lack of domestic refuse. These points, plus the sheer density of ceramics in the deposits, compare very closely with the data from Aivoji Mound 1.

Muller also argues that the assemblage at the Great Salt Spring indicates site specialization, that is, a limited activity site, but not production or personnel specialization. At Aivoji, the production experiment adds a new perspective.

The labor involved in making the salt actually was fairly light. The fire was stoked every few minutes throughout the day, but did not require constant or concentrated attention. It probably received far more attention than it really needed, since our laborers had to make some attempt to justify the wage we paid them, and must have been fairly bored as well. A single laborer could easily tend half a dozen or more fires.
On the other hand, our experience suggests that the boiling site can be something of a social center, if it is close to a village. Part of the social activity, of course, resulted from the bizarre spectacle of having a pair of Europeans going to the incredible trouble of boiling a bit of salt out of sea water, in clay pots no less. But the fire was also convenient for boiling eggs for lunch and as a source of flame to light a bush lamp, and small boys took turns at stoking the fire because they happened to be hanging around. A village could perhaps keep a dozen boiling fires burning, and tend them almost incidentally during the day.

Whether tended incidentally by villagers, or more deliberately by one or two designated laborers, the use of a dozen boiling fires could be a very productive activity, measured both in salt and in potsherds. In fact, our experiment probably indicates only a minimum salt productivity level. Sea water contains about 3% salt. European salt springs can contain up to 26% salt concentration, potentially an eight or nine times increase in salt production per volume of brine.

The primary labor actually is in making the boiling pots, and gathering the firewood. At Great Salt Spring, Muller noted evidence for making salt pans on location, and obviously there would have to be laborers gathering firewood for boiling fires. A properly organized group, concentrating on making pots, gathering firewood, and tending boiling fires, could produce a significant amount of salt in a relatively short time, even using sea water. With this kind of production, I would agree with Muller that full-time specialization in salt production would be quite unnecessary for a non-urban population. A village near an ocean or salt spring, or a short-term visit of perhaps a couple of weeks by an organized crew, could produce a goodly amount of salt.

I’d like to do some more work at Aivoji. One 2 x 2 meter test unit does not begin to do justice to the archaeology of the immediate environs. The sherd analysis did create the first glimmer of a ceramic sequence for the area, but the short time span and the specialization of vessel form in the Mound 1 deposit will make for a sequence of very limited applicability. We can’t even say for sure that Mound 1--let alone the bigger mounds--was created by salt boiling.
The results of our experiment, however, are consistent with this explanation, and provide some useful comparative data on salt production with traditional tools and materials.