

## MIMAKI AND THE MATCHING GAME

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Have you ever played The Matching Game? Take an archaeological situation and an historical document of roughly the same date. Juggle the pieces until a fit is obtained. Measure the goodness of fit by how many matches are made: famous people equal elaborate burials; palaces and centres equal well-developed settlement sites. The rules of the game require temporal and spatial congruence as well as similarity of features. The best juggler wins.

The Matching Game represents a particular approach to the archaeological record in which individual people, settlements, etc. mentioned in the documents are not only assumed to be identifiable or locatable among archaeological materials, their identification is assumed to be of primary importance. In fact, however, not many Troys exist to be equated so easily. Also, the illusion that the historically known can be equated with the archaeologically known generates interminable debates that are often a waste of time and effort. The debate in Japanese historiography on the location of Yamatai (Young 1958) is one such situation. This chiefdom, referred to in the Wei dynasty chronicles of China, has been identified by rival groups of scholars with locations in two widely separated regions of Japan: Kyushu and Kinai. The Kinai was first proposed as the location of Yamatai by the compilers of a Japanese chronicle in AD 720, and there has been a raging debate ever since between proponents of each of the two areas. The dilemma is still unresolved, though more heavily fuelled by archaeologists than historians these days: the identification of particular tombs with the legendary figure of Queen Himiko of Yamatai proceeds with a certain vigour.

The Matching Game also expresses a certain philosophy regarding the joint use of documentary and archaeological materials: it promotes full coordination of the two from the initial stages of research. Dymond, the foremost proponent of full coordination, states (1974, 99):

As historians, we have a moral duty to find out as much of the truth as possible and should therefore be prepared to use whatever evidence survives. If it is of different kinds, then we must use it in all of its variety and coordinate it.

Archaeological materials used in conjunction with the historical can be misused and abused if employed only to illustrate what is known from texts. But the particularist approach does not necessitate this; it can equally well employ archaeological materials "to embellish and correct the written record" -- the stated aim of historical archaeology in the United States prior to 1976 (Cotter 1976). Indeed, historical

materials can conversely play a secondary and supplementary role as in "text-aided archaeology", Charleton's characterization of research on the Classical world in general (1981). Nevertheless, these various uses of archaeology and texts remain particularistic and as such stand in contrast to the processual approach in historical archaeology currently widespread, at least in the United States, today.

In introducing the "new historical archaeology", Leone (1977) promoted the separation, rather than the coordination, of textual and archaeological sources of information so that each could provide its own perspective and then be tested against the other. Doubtless, Leone has moved beyond this processualist approach into more nether regions of archaeological thought since then, but his words were taken to heart by historical archaeologists in the United States in restructuring their approach to the material record. Now they not only separate texts from other material evidence, they also apply processualist method to both. As believers in general processes transcending the particular content of any historical sequence, processualists have given themselves to the search for pattern! Previously, this has meant patterning in the archaeological data, but now, patterning in texts is also pursued. Textual patterning, once elucidated, can be utilized as a model for investigating patterning in the material record, and these patterns can ultimately be contrasted, evaluated and finally integrated to provide the best approximation for understanding the past. In Cressey and Stephens' words (1982, 43):

The settlement and material patterns form two distinct, but analytically related, categories that are defined from different data sources -- documents and material culture. Both are derived from the same historical processes, and their integration is necessary for reliable generalizations.

This is a very different kind of "plea for reconciliation" of diverse historical and archaeological materials than that made by Dymond: it is a game of extracting patterns, not of matching individual items of data.

One of the earliest efforts systematically to extract patterning in textual materials to serve as a model for archaeological investigation is seen in Tobler and Wineburg (1971). This essay speculated on the possible placement of towns involved in the pre-Hittite network of Assyrian merchant colonies in Bronze Age Anatolia. The database was comprised of cuneiform tablets on which town names were mentioned without any other geographical details. The authors hypothesized that the mere juxtaposition of names implied a relationship between towns, and they translated that relationship by computer into geographical distance using the gravity model by incorporating the assumption that "places which are mentioned together frequently are probably closer together than are places which are not mentioned together frequently" (ibid., 39). The output of the analysis consisted of the predicted geographical locations of 62 important pre-Hittite towns vis-a-vis each

other. Unfortunately, the output contained a probable error of 50 km for town location when compared with a map of towns positioned with reference to records of itineraries. But the authors were optimistic, maintaining that both computer- and itinerary-based maps were not real but were products of speculation by the compilers on the skimpy information embedded in the cuneiform documents. Therefore, both could be used to hypothesize actual town locations to be tested archaeologically.

Tobler and Wineburg's study has much to recommend it since it is essentially an effort to extract unintentionally-recorded data systematically from historical documents. Since the original record-keepers presumably had no reason to distort town location data, the interaction data available in their statements could be gleaned through indirect methods. It can be supposed that almost any historical records inadvertently contain such coded information and that if it can be extracted, the resulting pattern might be useful in archaeological investigation.

This use of historical documents -- as source materials for extracting systematically embedded data, especially information on settlement patterns -- is now routine procedure for many historical archaeologists working on urban sites in the United States. The search for pattern in documentary sources is for them ideally carried out "before putting a trowel into the ground" (Cressey and Stephens 1982, 53), but under rescue constraints this temporal separation of documentary and archaeological research is not always feasible. Indeed, research in these two areas are treated as independent until the time when they can be cross-checked and tested against each other and then integrated to provide as full an account of past society as possible.

Can protohistoric archaeologists learn anything from this approach in historical archaeology? Unlike the full historic archaeologist who may have direct access to bureaucratic documents such as modern landholding records or Cappadocian merchants' tablets, protohistoric archaeologists must often deal with narrative histories that indirectly cover their periods of interest. If a pattern-searching approach is adopted for the extraction of data relevant to past process from these narrative histories, we must first learn the textual critics' tricks for identifying patterns in historical texts which reveal biases in the recording process itself, rather than in the past which it is documenting. The search for pattern to expose collection and recording bias is the foundation of textual criticism. These patterns reveal genealogical fictions, duplicative or discontinuous sequences, etc. When linguistic or transcriptional difficulties are added to the processual and textual problems, additional levels of pattern-solving become necessary. Only when these basic transcriptional and textual patterns are known to our best ability can we transcend them to look at patterns which might actually reflect on historical process.

As a case in point illustrating how difficult this is to do, I turn to the Japanese chronicles. After first exemplifying some of the textual problems inherent in these documents, I take up published work by an

archaeologist who implies the existence of a certain settlement form in his use of name data from the chronicles. This work is evaluated in terms of pattern-searching within the stated objectives of using the documents to derive models for testing against the archaeological record.

### Japan: A Case Study

The early Japanese chronicles are political products of their time. The Kojiki, finalized in AD 712, and the Nihon Shoki, finalized in AD 720, are expert reworkings of previously extant records and documents that preserved genealogical descent relationships among the elite as well as anecdotal information about times past. The reworking of this material was undertaken to provide the imperial family with a continuous history of descent through many generations from the "Ages of the Gods" to their time, and it formalized the relationships between elite families while providing them also with genealogical connections to past rulers or gods. In fact, the clans of early Japan are commonly classified into shimbetsu (descent from the gods), kobetsu (descent from former emperors), or bambetsu (foreign ancestry). These specifically political aims in compiling the chronicles have contributed to innumerable textual problems, mainly concerning chronology and, not surprisingly, kinship relations. Some of these are fairly easily solved, as with the 'assignment' of ancestors to contemporary clans. In the Kojiki, for example, many of the genealogical specifications occur as interlinear glosses, clearly applied post facto to earlier documents:

There was born the child AME-OSI-TARASI-PIKO-NO-MIKOTO; next, OPO-YAMATO-TARASI-PIKO-KUNI-OSI-BITO-NO-MIKOTO. (Two children) The younger brother...ruled the kingdom. The elder brother... [is the ancestor of the Omi of Kasuga, the Omi of Opo-yake, the Omi of Apata, the Omi of Wo-no, the Omi of Kaki-no-moto, the Omi of Itipiwi, the Omi of Opo-saka, the Omi of Ana, the Omi of Taki, the Omi of Paguri, the Omi of Tita, the Omi of Muza, the Omi of Tuno-yama, the Kimi of Ipi-taka in Ise, the Kimi of Itisi, and the Kuni-no-miyatsuko of Tika-tu-apumi.] (Philippi 1967, 189).

In other instances, the narrative genealogies are inconsistent enough to give us an instant glimpse of their falsehood. In his footnotes to the Kojiki, Philippi states concerning one recording:

This marriage...is perhaps the most fantastic of all the marriages in the Kojiki: if we are to believe the Kojiki's own genealogy, Emperor Keiko is marrying his own great-great-grand-daughter (Philippi 1967, 229).

Far more important than these individual examples are two masterpieces of detective work by Kanda and Mizuno respectively concerning

entire dynasties of emperors. Pre-war Japan claimed to have the longest continuous record of descent among modern constitutional monarchies, reaching back to 660 BC when Emperor Jimmu, grandson of the god who descended from the heavens to rule the Central Land of Reed Plains (Yamato), allegedly took the throne. Mizuno demonstrated that the imperial line as recorded in the 8th-century chronicles even then incorporated three distinct lines of kings which he terms the Old (4th century), Middle (5th century) and New (6th century on) Dynasties (Mizuno 1952). Kanda, on the other hand, investigated the succession of the eight kings between the legendary Jimmu and the beginning of the Old Dynasty and decided that they were not successive at all but were probably contemporaneous leaders whose mutual marriage and kinship relationships had been reworked to show genealogical descent (Kanda 1959). Thus fell the claim of Japanese imperial continuity at least from these early periods.

The discontinuity between the 4th and 5th centuries and the postulated non-local derivation of the Middle Dynasty comprise one of the most difficult problems in Japanese archaeology today: determining whether the hierarchical political structure we see emerge in the early 5th century was a product of conquest, competition, or indigenous elaboration. This situation has previously been the subject of the Matching Game, in which the 'foreign dynasty' has been (unsuccessfully) equated with horseriding equipment in the elite tomb burials, giving rise to the Horserider Theory of continental invasion as the origin of the early Japanese state (Ledyard 1975; Edwards 1983).

The primary proponent of this theory, Egami Namio, implemented this theory by playing the Matching Game on a smaller scale as well. Taking the Japanese name of the 10th Emperor Sujin, which is Mimaki-iri-biko-iniwe-no-mikoto, he equated the 'Mima' element with the name Mimana from the southern Korean peninsula and the 'ki' element with a word for palace or castle. Thus, Egami says, "we can infer that [Mimaki] lived in a palace located at a place known as Mima" (1964, 60), and his movement from there across the straits as the first historically acknowledged emperor represents the conquest of Japan by Korea.

Egami's inference on the meaning of Mimaki's name was derived from analogy with later, 7th-century emperors who were often referred to in the chronicles by the names of their palaces (miya). Thus there was Ikenobe-no-miya-no-ame-no-shita-wo-shiroshimesu-sumera-mikoto (The august lord ruling under heaven of the palace of Ikenobe), or the equally august lord of the palaces of Osada or Toyura (ibid.). Egami extrapolated this name pattern backwards two dynasties and three centuries in time to apply to the name of Mimaki in order to support his theory of continental conquest. I have severe reservations about the historicity of his claim, but I am intrigued by his postulation of a certain settlement type, palaces, being reflected in 4th-century imperial names.

Egami's inference was presumably made on the basis of inscriptional data: the element ki in Mimaki is represented by the Chinese character



城 meaning 'walled city' or 'city walls' and is the same character used in modern Japanese for 'castle'. Assuming that this character was used in Mimaki's name for its semantic value (rather than purely for its phonetic value), then we can also assume that some sort of structure or settlement form was being indicated. However, as we have maintained above, a single match does not make a case, and if we want to accept Mimaki as the name of a palace, as Egami suggests, then we must find supporting evidence in the pattern of usage of the word ki and the corresponding character 城 in the early texts.

There are several ways in which to approach the search for patterning here; to begin with we can separate the various data on ki into categories:

1. its usage as a separate, independent word implying settlement;
2. its use in placenames; and
3. its use in personal names, such as Mimaki's.

There are at least four places that I know of in the Nihon Shoki where the element ki is used independently. One is in the 4th-century records of the 11th Emperor Suinin whose attempted assassin built a ki (apparently of rice stalks) for defence, fortified himself there against imperial troops for a month, but was finally killed when the structure was burned to the ground (Aston 1896, I:172; Sakamoto et al. 1967, I:264). The second occurrence, dating to the late 5th century, appears to be a duplication of this first instance in that a ki (made of rice stalks) was thrown up quickly as a fortification, but the ki, instead of being burned down withstood a siege and was said to be stronger than even the emperor's! The 21st Emperor Yuryaku sent out troops to look at this 'house' (ie) and had the booster put to death (Aston 1896, I:364; Sakamoto 1967, I:492-93). It is notable that in this latter passage, the ki is equated with a house and seems to be of more solid construction than that suggested by rice stalks. In the records of the 25th Emperor Buretsu, also of the late 5th century, the emperor ordered a high minister "to make a levy of labourers of the province of Shinano in order to build a [ki] in the village of Minomata. It was called Kinouhe" (Aston 1896, I:405; Sakamoto et al. 1967, II:14). Finally, another contemporaneous document, the Harima Fudoki (a local geography written in AD 713), is said to contain the passage "...the place where the ki was dug..." (Shinmura 1969, 510).

From these references, it is fairly clear that a ki was some sort of habitational or fortified structure although its construction is variously represented as temporary or enduring, excavated or built up. But what justification do we have in equating the ki described as independent structures in these passages with the element ki occurring in Mimaki's name and translated by Egami as 'palace'? If the name Mimaki is truly a palace name like the 7th-century emperor's names containing miya ('palace') above, then there should be a pattern of occurrences in 4th century emperors' names as well. In fact, only one other, the 2nd Emperor Annei, has a name incorporating the element ki, Siki-tu-piko-

tama-de-mi-no-mikoto. There are, however, two direct references to actual palaces whose names incorporate the ki element. These are Tamaki, the palace of the 10th Emperor Sujin, and Namiki, the palace of the 25th Emperor Buretsu. In assessing the significance of these data, we immediately run into transcriptional problems. First, Sujin's palace is recorded as Tamaki ('jeweled ki') only in the Nihon Shoki; the Kojiki records it as Tamagaki ('jeweled fence'). It is difficult to ascertain which, if either, is more accurate. Second, Namiki is recorded in the Nihon Shoki with the characters meaning 'row of ki', whereas in the Kojiki it is written with the characters for 'row of trees' -- a much more logical compound!

Unfortunately, embarkation on this problem is beset with transcriptional difficulties. The major one is determining whether the ki element in these early names refers at all to a form of settlement or indeed to trees, since there were two words ki, homophones, with these respective meanings. The difficulty arises over the nature of the Japanese language and its representation by Chinese characters. When the Japanese began keeping records in the late 5th and 6th centuries, they had no native writing system and had to borrow one from China. These characters could be used in three different ways: (1) the text could actually be written in the Chinese language, using Chinese grammatical rules etc. -- and most of the Kojiki and Nihon Shoki are indeed written in Chinese; (2) the characters could be used only for their Chinese phonetic values, to transcribe Japanese syllables of similar pronunciation; (3) a character could be used for its semantic value, ignoring its original Chinese pronunciation and reading it as if it were the Japanese word of the same meaning. For example, the character 木 was used for the meaning 'tree' in pure Chinese sentences; it was also occasionally used to represent the sound mo in Japanese words (ignoring its semantic content); and it was often used to write the Japanese word ki 'tree' (ignoring its original Chinese pronunciation).

Examples of strategies (2) and (3) can be seen in the various ways in which the Japanese name Mimaki-iri-biko-iniwe-no-mikoto has been written in the chronicles. In the Kojiki song texts, this name is written using characters purely for their phonetic value (strategy 2): 美麻紀. But within the main Kojiki text, both phonetic and semantic values are employed (strategies 2 and 3):

characters used for  
phonetic values

pronun-  
ciation

characters used for  
semantic values

mi  
ma  
ki  
iri  
bi  
ko  
in(i)  
we  
mikoto

御 (honourable)  
真 (true)  
木 (tree)  
入 (enter)  
日 (sun)  
子 (child)

命 (lord)

印惠

The predominance of semantically used characters in this name suggests that the scribe thought he knew what the name meant and chose characters to represent those meanings -- except for one element, iniwe, whose meaning he apparently did not know and so chose to represent phonetically. But compare the characters used for the same name in the Nihon Shoki, finished eight years after the Kojiki, an example of strategy 3:

	<u>characters used for</u>	<u>semantic values</u>
mi	御	(honourable)
ma	間	(space)
ki	城	(walled city)
iri	入	(enter)
biko	房	(prince)
i	五十	(fifty)
ni	瓊	(bead)
we	殖	(increase)
sumera mikoto	天皇	(august lord)

The meaning of the name has changed with a choice of different characters. No longer was Mimaki the "sun-child who enters the honourable true tree, [unknown] lord" but rather the "prince who enters the honourable spacious walled city, fifty-bead-increasing august lord". Because of this apparently arbitrary shift between the two chronicles in the choice of characters to transcribe this and other names containing the element ki, how are we to know which one if either really reflects the original meaning, and do we have any evidence that the ki element in names ever can be equated with the independent word ki meaning some sort of settlement?

Seen in a broad perspective, this alternative use of the 'tree' 木 and 'walled city' 城 characters may turn out not to be all that irrational. If we acknowledge that the most prominent feature of a Chinese city was its walls and if we know, as was the case (Reischauer 1937), that the 'walled city' character was being used in 8th century Japan to indicate wooden palisades (see below), then we might come to the conclusion that what is being referred to in the texts is actually a sort of settlement or habitational form surrounded by rows of upright timbers to form a palisaded enclosure. The use of the 'tree' character would then imply timber rather than living trees, and Namiki as a palace name might seem quite reasonable. So would other ki compounds that occur in a variety of personal and place names in the texts. Almost all of the below are written with the character for 'tree' in the Kojiki but with the character for 'walled city' in the Nihon Shoki.

HISIKI	diamond-shape timber/walls
INAKI	rice stalk [-thatched?] timber/walls
IMAKI	current timber/walls
IWAKI	rock timber/walls
IPOKI	500 timber/walls
KAZURAKI	vine timber/walls



KINASI	without a timber/walls
NUNAKI	marsh timber/walls
OSIKI	push timber/walls
PUNAKI	boat timber/walls
SAKI	? timber/walls
SIKI	rock timber/walls
TAKAKI	high timber/walls
UBARAKI	wild rose timber/walls
UMAKI	horse timber/walls
WAKAKI	young timber/walls

However, analogy with a contemporary form of palisade brings a new level of complication into understanding the nature of ki. In the 7th century, there were a whole series of fortifications built on the northern frontier of the Yamato state. These were bases from which expansionist operations were launched against the aboriginal inhabitants of the northern forests. In its own version of Manifest Destiny, the Yamato state defeated these aboriginals, the Ezo, and incorporated the whole island of Honshu into its territorial administrative structure. The fortifications bear names such as Nutari-no-ki (built in AD 648), Idewa-no-ki (operative in the early 8th century), Nakayama-no-ki (operative in the early 9th century) and Okachi-no-ki (Reischauer 1937; Shinmura 1969, 286 and 1717); most of these are written with the character for 'palisade' but Okachi-no-ki is also referred to with the 'walled city' character (Reischauer 1937).

Do we now have a case where the compilers of the Nihon Shoki, aware of a contemporary form of fortification called a ki, unilaterally assumed that all ancient names containing the ki element really were fortifications also and so transcribed all of them with the 'walled city' character? Or was it the other way around, the word ki being taken from the earlier documents and applied to contemporary structures? And what do we do with the linguistic evidence that ki is actually a Korean word deriving from the Paekche state, whereas another Korean word cas meaning the same thing also worked its way into 8th-century Japan as the word for the Ezo fortifications chashi (Sakamoto et al. 1967, 1:640)? Why were the Japanese fighting from ki while the Ezo were fighting from chashi? And where does Egami's inference fall in all this, based on a single occurrence of ki in an emperor's name? Would that it were all so simple!

### Summary

The determination of pattern in documents to provide models for archaeological investigation is a necessary and worthy exercise, but an extremely difficult task. Protohistorians who choose to deal only with archaeological data without reference to the texts can almost be sympathized with for their sensible abstinence, more so than archaeologists who use the texts without questioning the structure and content of the material. A simplistic approach like Egami's is attractive, but it cannot withstand scrutiny. Matching only has limited utility,

although it can be a convenient stimulus for finding patterns that substantiate sophisticated equations of documentary and material evidence. In an admittedly inexhaustive investigation such as this, pattern has been indicated, but it is patterning at the basic transcriptional and textual levels. Much more work is required until we can approach the patterning that may bring to light the settlement form that Egami has suggested.

In line with the recommendations for separating documentary and archaeological data, I have not yet mentioned that there is tremendous evidence in the material record for ditched and/or stone-walled hilltop fortifications in protohistoric Japan and Korea as well as limited evidence for walled house compounds among the elite stratum (Barnes 1983). The presence of patterning in the archaeological record for these two forms of settlement makes it plausible that regular names for such settlements might be preserved in place-name and personal-name data in the chronicles. Thus it is worthwhile pursuing this problem, trying to overcome the transcriptional difficulties surrounding the word *ki*. However, not until some sort of patterning can be recognized in the *ki* data can we begin to hypothesise relationships between the archaeological and documentary materials. Meanwhile, the difficult part is resisting jumping to conclusions or making facile equations which disregard the complicated nature of the historical materials bequeathed to us.

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