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ON CLASSIFICATION IN ARCHAEOLOGY^{*:}

Some form of classification is inherent in any research work, being either its point of departure or end product. However, it was only after publication of “De quelques formes primitives de classification” (1903) by Durkheim and Mauss that classification itself came to be considered a legitimate subject of research in its own right. This was the result of it being argued for the first time that “classifications generally *express* the societies within which they are elaborated” (Ellen 1979: 3). This is the same as saying that different societies use different classifications for the same phenomena. Anthropology, philosophy and historical studies were forced to redirect their endeavors towards the investigation of the classificatory process if they were to understand human society and the validity of human knowledge. Of the extensive literature on this topic, I will concentrate only on that which is directly connected to archaeology. I intend to present a fair summary of this material as this paper in fact represents a first attempt to bring the attention of Romanian archaeologists to the issue of classification. Despite the long history of this debate in other archaeological schools, it has as yet not been discussed in Romania. Its particular importance derives from the fact that most archaeological analysis begins by presenting an archaeological record by means of one or more typologies. Any conclusions the archaeologist might draw depend upon his or her point of departure: the typologies. Given the variability of artifacts, a series of questions arise: how can a type be identified? On what grounds can we state that an artifact belongs to a certain type and not to another? How does variability range within a type to be determined or, in other words, “How different is different”? Are there types/categories

* 2000-2001 Fellow. As her paper was not ready to be published in the yearbook of her series, it was introduced into the present issue.

in the “world out there” or types merely created by archaeologists? This last question asks whether there is an inherent structure to the world, which we can discover if we act correctly. What does a type really mean? Do the answers to these questions have a bearing on the results of archaeological analysis and if so, in what way?

The “typological debate” took place mainly in English and American archaeology. It leads to the realization of the existence of a close link between explanations of artifact variability (i.e., the way type has been conceived) and the main theoretical approaches in archaeological research.

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Why is Classification Worth Considering in its own Right?

Historically, and in some cases still today, archaeologists have not considered classification worthy of discussion. However, this does not presuppose the lack of a theoretical stand point on their part. It simply shows their thinking to be within the Cartesian frame which is dominant in modernism. For Descartes there existed a complete division between mind and body, between subject and object. Objects had constant properties, so, for the purposes of studying them, thorough and ‘objective’ (i.e., detached, impartial) observation was sufficient. This defined the philosophical basis for empiricism, a concept that dominated archaeology until the beginning of the 20th century (Thomas 1998: 150 - 154).

However, as R. Rorty has written,

On the periphery of the history of modern philosophy, one finds figures who, without forming a “tradition”, resemble each other in their distrust of the notion that man’s essence is to be a knower of essences. Goethe, Kierkegaard, Santayana, William James, Dewey, and later Wittgenstein, then later, Heidegger, are figures of this sort [...] These writers have kept alive the suggestion that, even when we have justified true belief about everything we want to know, we may have no more than conformed to the norms of the day (Rorty 1979:367 after Boast 1997: 182).

Today, the number of scholars who agree with Descartes is continually declining. There are several reasons for this.

Firstly, the information we receive about the world “outside there” is filtered through our senses. This is a fact. “Lacking a frame of reference it is impossible for the human mind to know the amount and kind of this filtering.” (Voorrips 1982: 95) Experiments have shown that every species has its own sensory and perceptual characteristics within which it perceives the world. For example, we have words for only those colors which fall within our visible spectrum; an octopus is unable to feel weight (Hinde 1998: 175 – 176). This means that we cannot perceive the world as it really is. Furthermore, the “making conscious” of observations is an activity that differs from individual to individual (Voorrips 1982: 95). On the other hand, it is also true that the impact of this upon human cognition should not be overemphasized: there are some general rules of how humans “construct their psychological world”. We tend towards achieving agreement between our view of the world and that of others. Otherwise we experience strong feelings of insecurity and lack of control. “The more our perceptions of the world coincide with those of others, the more willing we are to accept them as true.” (Hinde 1998: 176) This means that our representation of the world depends on the culture in which we were socialized. We cannot observe and describe the world independently of our culture. The simple act of using words presupposes the learning of categories. These are then recognized in the world around us. Different cultures categorize the same things differently.¹ Archaeologists, as bearers of a culture different from that of the prehistoric people, necessarily classify in a different way. Not only do folk classifications differ from their scientific counterparts, the latter also differ among themselves, such that truly objective classification (and knowledge) becomes impossible. Probably the most convincing demonstration was made by Michel Foucault (1966). In analyzing the evolution of scientific research from the Renaissance up to the 19th century, he was able to show that for the same domains of investigation (nature, language, etc.) scholars elaborated theories over time, which were totally incompatible with each other; knowledge hadn’t simply developed from the 16th till the 19th century by accumulation: it had been restructured. The ultimate basis of an époque’s knowledge is the “episteme”: a specific way of constructing categories and binding them together in structures. An episteme is a system that connects our eye to the world and makes a particular discourse of that world appear to be true; but this only applies in a given époque (Foucault 1966, throughout). Once a new episteme has been established, others – both previous and competing – are declared false. Legitimate knowledge

is a matter of agreement, and thus truth is more a matter of power than a matter of adequacy in respect of the subject of investigation.

These are some reasons that make the problem of classification worthy of consideration .

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The Main Phases of the “Typological Debate”

Before 1940. Initially, archaeologists published a large number of somewhat uncritically derived classifications (Adams 1988: 41). Their main purpose was to describe collections (Ford 1954:43), usually originating from unstratified sites. The smallest entities resulting from these classifications were called “types”. (Steward 1954: 54, quoting Ford). Among these, the most elementary is the “**morphological type**” owing to its basis on similarities and differences in form alone; it was of cultural significance. Other types had no cultural value, but were of chronological value; these were the so-called “**historical-indexes**”. The main role of these types was that of **time-space markers**, the absolutely necessary basis for every archaeological interpretation of every cultural historical development (Steward 1954: 54 – 55). In this stage classification was based simply on the archaeologist’s intuition and experience. Classifications were groupings of objects by direct comparison; types were defined through external similarities and differences as perceived by the analyst. The result of this was usually a plate illustrating the types as such, without explanation of the criteria on which they had been determined. Insofar as debate of the types existed, it was usually on a practical level: was a certain type consistent enough to be considered as such, or was it a variant of another type? Was a type sufficiently well described? Etc. The scope of the discussion was to elaborate an adequate vocabulary for the systematic description of the archaeological material. Epistemological issues were completely ignored (Adams 1988: 41).

Since the variables were not defined, this kind of typology/classification cannot be checked, or reproduced by other analysts. The procedure requires direct comparison, so that the best fit was often only the least bad. As a consequence, through assignment of new artifacts, the initial internal homogeneity of the types was gradually lost. Pieces, which didn’t fit into either category, formed a new category. Later

inspection of the classification shows that its initial systematic character had been lost and that a new classification was needed. This **object grouping** serves only as a descriptive tool of a finite number of artifacts. It has no explanatory power, unless contextual information is added (mainly on chronology and place of production) (Voorrips 1982: 108 – 109).

Around 1940. Archaeologists became increasingly aware of the fact that morphologically different types could be functionally related to each other (Steward 1954: 56). The debate was centered on searching for the **functional or emic types**. Archaeologists tried to use classification to help them ‘get inside the heads of the artifact makers’ (Rouse 1939 and Krieger 1944 after Adams 1988: 41; Spaulding 1953: 305). Cultures characterized by them should correspond to social and ethnic units “that people themselves would have recognized” (Adams 1988: 41).

The debate was still dominated by field archaeologists who tried to validate their experience-based typologies such that a type was conceived as a ‘specific and cohesive combination of attributes’ (Krieger 1944: 277 after Whallon 1972: 14).

The Spaulding-Ford debate. In 1953, Spaulding published an article with a significant title: “Statistical techniques for the discovery of artifacts”. There he argues that “The artifact type is [...] a group of artifacts exhibiting a consistent assemblage of attributes whose combined properties give a characteristic pattern” (1953: 305). Accordingly, he proposed statistical methods for the “discovery of combinations of attributes favored by the makers of the artifacts” (*loc. cit.*), so that artifacts couldn’t fail to have historical meaning (*loc. cit.*).

This view that takes for granted that types do exist in cultures and that it only takes competent methodologists to discover them was strongly criticized by J. A. Ford (1954: 42). He argued that a cultural trait is an abstraction created by the archaeologist in order to analyze his material. Things look similar but not identical, as turned out by a machine, so that a cultural trait has a mean and a range of deviation. Their bearers were not necessarily aware of the existence of this ‘mean’ (*Op. cit.*: 45). A “type is formed by the observer at a chosen level of abstraction” and this is dependent on the purpose of the analysis (*Ibid.*: 47). Furthermore, the way a type looks like depends on the chosen space and the moment in time, which are analyzed. To illustrate his point, he analyses the houses of a fictional Gamma-Gamma culture from an equally fictional

Gamma-Gamma Island, as seen by ethnologists in 1940. There were several methods for building each part of a house such that the cultural concept of a “house” could be split into several other concepts (i.e., types), with neither of these levels of abstraction being any less ‘real’ than the other. However, seen together, all houses share common features, with themselves and with other aspects of the culture they belong to (for example, the type of family), for there is an “inherent organization [...] in culture at all times and places” (*Ibid.*: 52). If the chosen level of abstraction were a house, then the ethnologist would note that most houses are closer to the mean for the type, while some deviate from it. But if the chosen area is expanded to include the neighboring island as well, then those houses, which were considered deviations from the mean on Gamma-Gamma, now become rather typical of the neighboring islands; only a few houses will be still regarded as belonging to the Gamma-Gamma type. In other words “qualitative differentiation in culture is a function of distance” (*Ibid.*: 49). Artifact variability may be accelerated by different factors such as ethnic, political and social barriers, etc. These barriers, however, as well as the effect of these factors upon the cultural item, can not be known previously. Rather, they are to be discovered by the archaeologist, an endeavor that is dependent on the previously defined types (*Ibid.*: 49). The definition of the type depends on the point in space where the collection originates. It follows, therefore, that for the archaeologist working on collections stemming from two sites placed at a distance from each other, and who is unconscious of the nature of the problem, the differences between types will appear larger than they in fact are. Should new assemblages be discovered in the area between those initial ones, the limits between comparable types then become less clear due to the continuous character of artifact variability. Artifact variability is also continuous over time: the main types from 1900 will be different from those from 1940. The aspect of houses will constantly show minor changes over time such that the type, which presupposes limits established through similarities and differences, has no natural character. Only rarely abrupt changes encountered (*Ibid.*: 48 – 52).

Ford agrees with Spaulding in asserting that each culture has its internal cohesion. “The cultural type will, to a greater or lesser degree, be a reflection of the boundaries to one stream of ideas which the cultural bearers considered related” (*Ibid.*: 52). To this extent, statistical tests for the analysis of the consistency of the association of features can be justified. However,

types, as established by archaeologists, can not avoid dependence on the place and moment in time from which the artifacts originate and the chosen level of abstraction (*loc. cit.*). In summary: Spaulding was convinced that cultural types really existed and could be discovered by archaeologists, while Ford argued that, despite the internal cohesion of cultures, there is no such thing as a real type. Types are only tools devised by archaeologists and other anthropologists in order to analyze artifacts. Their variability, for both Spaulding and Ford, was the result of the putting into practice of the idea about how artifacts should look.

The Bordes–Binford debate came out of the explanation of stone tool variability in Mousterian (Middle Paleolithic), in southern France. Bordes argued that distinction should be drawn between the techniques and forms of stone tools. The former issue was conditioned by the availability of raw materials and consequently useless for reconstructing culture histories, while the latter was the result of different cultural traditions, of different ways of conceiving and manufacturing the same type of tool. With this in mind he developed a classification of stone tools, which became the most widely adopted system among paleolithicians. He created standard lists of tool types and used a quantitative technique for describing assemblages. The four different types of Mousterian – the Quina, typical Mousterian, Mousterian of Acheulean tradition, and denticulated Mousterian types – were described by cumulative curves; their distinctive shape was determined by different frequencies of tool type occurrence in an assemblage. The debate started from the question as to what these curves (i.e., assemblages) meant. For Bordes and Sonnevile-Bordes they were characteristic of different contemporary populations, each having their own traditional way of conceiving the “right” form for artifacts with otherwise similar functions. The long duration of these cultures was explained by the small dimensions of human groups at that time, for, Brodes argues, all humans are intelligent, but only a few are creative: the smaller the group, the more rare the innovations (Bordes and Sonnevile Bordes 1970: 72 – 73).

Binford supplied arguments for an alternative explanation of the French Mousterian. He conceived culture as an extra-somatic adaptive system that is employed in the integration of a society with its environment and with other socio-cultural systems (Binford 1965: 205). Function was the main cause of artifact variability. Explanation of the differences between

artifact assemblages was to be found in the different specialized activities carried out on those sites. For him, there was only one Mousterian.

Not even newer stratigraphic evidence, from the site where the four types of the Mousterian were found in layers one on top of the other, tipped the scales in favor of one of these hypotheses. Bordes considered that these sites to have been successively occupied by human groups with different traditions of artifact manufacturing. Binford answered that his own ethno-archaeological work, as well as studies made by American scholars on modern, native peoples in the New World, showed that the only factor that could be correlated with artifact variability was the kind of environment. "Variability among ... [social or ethnic] units tended to be graded across many groups, rendering the recognition of ethnically distinct groups a nearly impossible task" (Binford 1983: 93). However the discussion could not be carried forward as there was no answer to the question: 'Was the past so different from the conditions documented in the modern world?' (*Op. cit.*: 93).

This debate highlighted a conflict of views about the way cultural development was conceived: the organic and the cultural view. Both could account for the same range of artifact variability.

Important for the future direction of the debate around artifact variability is the distinction "New Archaeology" – later called processual archaeology – made between the style and the function of the archaeological material. According to this, an understanding of prehistoric cultures could only be achieved by focusing upon the function of the artifacts. This stance was adopted by some archaeologists and strongly challenged by others.

Evolutionary archaeology is the climax of those directions that stressed the relevance of function in understanding of artifact variability. Its critical point was an article by Robert Dunnell, "Style and Function: A Fundamental Dichotomy" (Dunnell 1977). Function was defined as being that component of an artifact that was devised as a response to the selective pressure of the environment. In opposition to style, function has an adaptive role (*op. cit.*: 199). Style is seen as functionally neutral: a bottle opener is made having in mind the idea that it should be efficient; the more efficient it is the greater the chance of replication, of persistence through time. Not all its traits are linked with efficiency. Color, for example. People could choose whatever color they wish, for colors in this case have equivalent functions. They don't influence the persistence

of the tool through time (Leonard 2001: 74). This is determined by its functional traits. Persistence of different traits through time can guide the archaeologist in his or her attempt to distinguish functional from stylistic attributes: the former being long lasting, the latter as whimsical as all trends are.

Another distinction concerning artifact variability was taken over from the evolutionary Darwinist biology: the distinction between homologous and analogous traits (*op. cit.*: 69). Artifacts from different parts of the world can, and do have common traits. Some have common roots, while others are simply the result of independent invention. The former were named homologous, the latter analogous. This distinction is of crucial importance if we intend, as evolutionary archaeologists do, to reconstruct the “polygenetic history” of artifacts (Lee Lyman 2001: 78).

Important progress is expected from this area in the long term. However, for the time being, its theoretical stance is more convincing than the case studies. The first objection concerns the method proposed for the separation of stylistic and functional attributes. These latter attributes may undergo a rapid decline² such that their correct identification depends on the moment in time the analysis is focused upon. Other case studies (see Leonard 2001: 80 – 92) have been left unfinished.³

Style had an adaptive function. This was the main conclusion H. M. Wobst came to after studying stylistic behavior. His starting point was an archaeological dilemma: “although style is integral to most archaeological research, it lacks meaning” (Wobst 1977: 317). Archaeological research conceived style as those aspects of artifact variability, which were congruent with specific areas, time periods, or social units, etc., and couldn’t be explained by productive advantage, mechanical factors or chance. No explanation had been given for this congruence so Wobst decided to try to clarify the issue. He argued that, as with every living organism, man needs to exchange information with his environment. One means of doing so is through style. Given the heavy contribution of artifacts to human survival, we can expect them to carry information. Unlike other means (e.g., verbal communication), they don’t require presence on the part of the emitter and can maintain a message for a longer period of time. Different styles are used for different receivers. Among these he distinguishes four social groups organized around the emitter: his immediate household, his relatives and close friends, a socially distant group and, finally, a very distant social group. For the first three groups, the greater the distance from the emitter, the more

complex the messages. Communication with the immediate household doesn't require any elaborate stylistic behavior as messages are familiar to all its members. A parallel argument can be brought forward for the next level, where stylistic behavior gains only slightly in complexity. Stylistic elaboration is requested for communication with the socially distant group, and it exists only in societies complex enough to consist of such groups.

As societies increase in size and complexity, more and more aspects of behavior become repetitious and anticipated. It is in the latter societies that stylistic behavior structures important aspects of artifact form (*op. cit.*: 326).

The most remote group is too distant to be considered a target, for it doesn't have much chance of encountering the message and cannot decode it. Wobst tested his theory of stylistic behavior using examples from southeastern Europe, and Yugoslavia in particular. This region was considered particularly good testing ground owing to its ethnic, linguistic and religious mosaic. Under such conditions, transmitting and receiving information about the social group of a person encountered before one gets into the gun range of one's enemy is of major importance. This is only possible by using items visible from a long distance (e.g., from the other side of a mountain, or from a long distance on a road). The most appropriate for this task is clothing. An example of this is given by the *struka* (a kind of cloak), which differs from tribe to tribe in color and color combination. After the state gained control over the tribes and local vendettas stopped, tribes began to reorganize themselves by ethnic criteria. Accordingly, headdresses and coats representing ethnic groups replaced the *struka*. Further changes occurred as a result of the official recognition of the Romanian-speaking minorities, for example, who gave up their former sheepskin kaplak headdress and adopted the hat of the Romania peasants as a sign of their identification with the Romania nation state (*Ibid.*: 330 – 334).

Other kinds of messages, such as an individual's position in a ranked scale used in smaller social groups, are emitted through items visible from shorter distances (jewelry, shoes, etc.). Finally, other items, not visible to people outside the household, show no differentiation (e.g., under-wear) and are produced with little costs (*Ibid.*: 334).

The further the distance from which a specific stylistic message can be deciphered, the wider its geographic distribution; and the more predictably the item is worn or visible, the wider the distribution of specific stylistic form carried by the item (*Ibid*: 335).

Artifact variability owes much to the fact that style has function. Wobst's article, as well as Bourdieu's sociological theories (Bourdieu 1972; 1980) about the role of artifacts in human life had a major impact on archaeology. One of its most fruitful directions, the post-processual archaeology, concentrated upon investigating artifact variability in relationship with social and power relations.

Artifact variability is only rarely determined by artifact function.

This was one of the conclusions Daniel Miller arrived at in his thorough study of modern pottery in central India. He started by examining the most common assumptions encountered in archaeological research at the time.

He selected 51 pottery shapes in use in an Indian village called Dangwara. Despite this great number, all of them could easily be recognized as being specific to the area under study. What made their recognition possible was style (Miller 1985: 35). This inclusion of a large number of categories in one style is possible due to particular exploitation during the production process of certain dimensions (i.e., characteristics) of the pottery. (In the Dangwara case, the most important dimension was the shape of the upper part of the vessels, the so-called *parti*). Dimensions are those which divide and, at the same time, unite categories. This is why artifact assemblages should be studied as a whole, as structuralists have done, and not as individual forms. Further, "this means that the identification of significant dimensions of variability is accomplished by precisely the same activity in ethnography as in the archaeology of prehistoric societies" (*op. cit.*: 50).

He also investigated the relationship between the function and form of pottery. Some of the forms were indeed well suited to their function (i.e., a clay disc used to imprint a decoration on the bodies of village cows at certain times of the year; a type of lid, etc.), but most were not. The oil lamp, for instance, had no form of provision for the wick, so that this often slid beneath the surface of the fuel extinguishing itself. Most rims played no part in the way vessels were carried or held. Vessels for water-storage had necks that were too narrow and no spouts such that obtaining water after the vessel was less than half full was a

near-impossible task. As in other parts of southern and southeastern Asia, a large number of vessel shapes were often used to perform the same function, while sometimes the same shape was used for different functions (*Ibid.*: 55 – 67). Thus it became clear that “the notions of function and efficiency play little part in any detailed explanation of the specifics of variability” (*Ibid.*: 67). Function has an impact on vessel morphology, but only in a distant way. The relationship between shape and function is established by convention. However, from villagers to academics, all people, when asked about the causes of vessel morphology, would answer in terms of function and efficiency.

Some of the attributes of the pottery could be explained by considering them in relationship with other dimensions of Indian culture. Typical Hindu oppositions such as those between water, seen as pure, and food, seen as polluting, or between red, as the color of the sacred, and black, the color of the undesirable, bear directly upon ceramics. Pots used in connection with water or with religious ceremonies are always red, while those used for cooking or meal consumption are black. Should vessels be used in private ceremonies, they must be different, i.e., “red and buff”.

Another issue investigated by Miller is that of the *emic* categories. Most archaeologists were convinced that knowing them represents the key to the understanding of past cultures. Miller found out that the Dangwara people had more names for ceramics than there were forms. Different people used different names for the same pots, each of them being convinced that all the others would name a particular pot as he himself did. On the other hand, some attributes of the pottery had no name at all (i.e., the “red and buff” surface), although they were used systematically. The contexts and the way vessels were manipulated had an evidently structured and systematic character, though the villagers were unconscious of this themselves. When stated, it was denied. Miller concluded that the idea of reconstructing the *emic* categories for understanding material culture was no more than an illusion. Users consider most material artifacts to be trivial and unimportant; they are mostly ignored and not currently objects of complex conceptualization. However, their manipulation is never mistaken. The reason for this is the ‘frame’. For the pottery, this can be the framework it is used in (a ritual, etc.), and the frame it constitutes itself for its content (which is usually given more attention) (*Ibid.*: 175 – 183).

Another question Miller’s study focused on is: do artifacts reflect social structure?

One of the examples he discusses concerns a piece of jewelry that the higher castes would wear around their ankle. As the lower castes began to copy this, the higher castes started to feel uncomfortable wearing it and gave up the habit. This example demonstrates how, if a process of emulation is at work and social hierarchy remains constant, dynamic social processes may be inferred from synchronic material (*Ibid.*: 185 – 191).

Other items are simultaneously connected with different social values. *Brahmans*, for instance, adopted metal vessels for religious reasons (metal vessels are seen as more pure than those of clay), while the *jat*, a notoriously unorthodox caste, would use the same as a sign of their wealth (*Ibid.*: 193 – 195).

Seen as a whole, artifacts don't simply reflect social structure as described in sociology. They play their own role in social life. The relationship between people and their material world can not be eliminated. There is no privileged 'real' society to be represented by artifacts (*Ibid.*: 202 – 203).

Both caste and the material world are constructs which capture and in turn constitute elements of culture, but within an array of alternative, sometimes complementary and sometimes conflicting representations. [...] The implication of this for archaeology is that categorization found in material form may be as fully constitutive of society as a normative articulated social categorization, equivalent to caste (*Ibid.*: 203).

Research on pragmatic aspects of categorization

Concepts are either **etic** (elaborated by the analyst) or **emic** (belonging to the analyzed culture) (Harris 1969: 568 – 604).

From another point of view, they can be either **monothetic** (built on a single attribute considered at a given time) or **polythetic** (built on several attributes). The former are more convenient as they are more easily identifiable, while the latter correspond better to what is actually seen. In the scientific classification, polythetic classification "is associated with increasing information; in the [... folk classification] it is a response to too much" (Ellen 1979: 11). In his "Philosophical Investigations", Wittgenstein argued that in everyday life semantic and perceptual categorizations are polythetic. Their meaning is heavily determined by contextual factors (after Miller 1985: 8).

The degree of abstraction implied in the classifying process and the **relevance of classification for a given culture** seem to be determined by everyday life conditions. This was the conclusion reached by D. Reason (1979) after having compared the modes of signification of (Polish) peasant families in pre-capitalist and capitalist economies. Forced to be economically self-sufficient, peasant family farms in pre-capitalist systems were oriented toward qualitative production, i.e., in order to survive they had to produce a whole range of products. These not being subjected to exchange, there is no need to elaborate such abstract notions like “value” or “labor time”. On the contrary, the capitalist system cannot exist without such abstract notions: in their absence, exchanging wares becomes an impossible task. This is why, argues Reason, “categorization” is a more adequate term for the classifying process in pre-capitalist societies, while “classification” should be kept for societies with a higher degree of abstraction (in this case study, the capitalist system) (Reason 1979).

Categorization solicits recognition of a kind which depends on no particular features of expression for its correct exercise, whereas classification is not only rule-governed (since it indicates a culture property), but these rules must be articulable in principle and constitute criteria of identity (*op. cit.*: 222).

In other cases, ethnologists noticed that even people living in comparable economic systems show different propensities toward classification (in its broadest sense). Thus the Navaho “love to categorize and are ready and willing to argue about hair-splitting taxonomic distinctions” (Morris 1979: 120), while the Malapantaram “showed a general disinterest in taxonomic concerns” (*op. cit.*: note 8).

Generally speaking, **classifications** can be **hierarchical, non-hierarchical but based on contrast, networks** and **non-hierarchical but lacking the idea of contrast** (Ellen 1979: 12-14).

- **Hierarchical classifications** order by inclusion of bounded semantic fields. This was the dominant case in the European scholarly tradition since Aristotle; however, it was by no means appropriate when used to examine folk systems. It does not account for “interlocking hierarchies, extra-hierarchic relations, synonymy, homonymy, polysemy, anomaly, cover categories and residual taxa [...] Rather [...] taxonomic trees are the result of classifying behavior” (*op. cit.*: 13). It has also been argued that elaborate hierarchical classifications

depend critically on the introduction of writing (*Ibid.*: 23). Illiterate societies are not able to handle too complicated and totally systematic classifications. “Whereas the inbuilt, rigorous logic of ethnographic method easily gives rise to anomalies, the informal logics of folk systems permit its avoidance” (*Ibid.*: 14). As far as the relationship between hierarchical classifying systems and social organization is concerned, there is no simple, one-to-one correspondence. While it is true that hierarchical European society developed a hierarchical classification system (of Aristotelian tradition), such highly hierarchical societies, as China, used a non-hierarchical classification (*Ibid.*: 25).

- **Non-hierarchical classifying systems** are based on the idea of contrast and of common points. For example, the Chinese Mohist philosophers’ system of the 5th and 4th century BC. Their categories include each other, some being broader than others (things>animals of four feet>oxen and horses). The common point between oxen and horses: they have four feet (*Ibid.*: 13).
- **Networks** are sometimes found to be more suitable for the complexity of reality. Rumphius, in his *Herbarium Amboinense*, grouped plants in networks, by similarities and differences, partially following the local, i.e., Malay, classification (*Ibid.*: 14; Peeters 1979).
- **Non-hierarchical, partial classifications** are where the idea of contrast is totally absent. These are the so-called “fussy sets” (Ellen 1979: 14). They can be more suitably explained by considering of a couple studies.

The mathematician, L. Zadeh (1965), analyzed situations in which objects are not, as in conventional logic, either members or non-members of a given set, but rather are seen as more or less representative for that set. This indeterminacy of categories is known as the “fuzziness of the sets” or as “fussy set theory” (after Miller 1985:8).

The linguist W. Labov, published the result of his research into expressions such as “A is a sort of B”. He used a set of pottery profiles. As he gradually modified their dimensions, he noticed that his informants requested contextual information in order to make distinctions, such as between a “cup” and a “bowl”. “In the world of experience all boundaries show some degree of vagueness, and any formal system which is useful for semantic description must allow us to record, or even measure, this property” (Labov 1973: 352 after Miller 1985:8).

Another study of the categorization of pottery, conducted by W. Kempton (1976), improved some aspects of Zadeh's fuzzy set theory. For this he asked eleven students from two American universities to classify an assemblage of Mexican pottery. Predictions made according to the fuzzy set theory were confirmed in thirteen out of fifteen possible taxonomical relationships between the different kinds of vessels included in the test. The two exceptions were concerning the categories "cup" and "mug", and "mug" and "coffee cup", respectively. The results showed that some of the mugs were considered to be a sort of cups and some were not (1976: fig. 15 and 16). This was quite surprising because all the informants agreed previously that a "cup" is a superordinate term for "mug" (Kempton 1976: 61). Kempton concluded that the fuzzy set theory is appropriate for judging the inclusion of an object in a set, but not for the inclusion of a set into another one. In the first case, inclusion is absolute, in the second it is gradual: members of one set are part of a superordinate set to a greater or a lesser degree, and some might turn out not to be members of the superordinate set at all. "There is a critical value of membership" (Kempton 1976: 60). This is how, though all the informants agreed upon the assertion "a mug is a sort of cup", it was still possible for some mugs to be more a sort of a mug than a sort of a cup and for other mugs to be more a sort of a cup than a sort of a mug. A parallel argument can be constructed for the relationship between "mug" and "coffee cup". Kempton's conclusion was that "Empirical facts require an adequate theory of human cognition" (*op. cit.*: 62). The fuzzy set theory should be improved (*Ibid., loc. cit.*).

On the evolution of the capacity of categorization. The aim of the recently developed branch of archaeology called cognitive archaeology is to investigate human cognition as mirrored by the archaeological record. One of its most important domains is concerned with the evolution of the human mind. Those working on this issue include not only archaeologists, but also psychologists, anthropologists and sociologists. Most of them agree that humans inherited the capacity to use concepts from their non-human ancestors (Donald 1998b: 184). This is based on the results of experiments made on chimpanzees – they are clearly capable of thought, though only when considering aspects of the social domain.⁴ It is also possible that they possess a (limited) capacity to think about the natural world. This is considered the starting point for the evolution of human cognition.

In evolutionary psychology there are two basic theories about the evolution of the human mind. One considers this process to be completely modular: modules, sometimes also named domains or intelligences, evolved separately from each other, having their own rhythm and developmental phases, and were only later linked to each other by consciousness. As a result, cognitive fluidity was achieved (Mithen 1996: 55 – 92; Donald 1998a: 9 – 10).

The other theory describes an only partially modular evolution of the mind: beyond modules there must also have existed a kind of “central processor” (Donald 1998a: 10).

A somewhat different theory of the evolution of human cognition has been developed by S. Mithen, an archaeologist working mainly on the Paleolithic. He sees the human mind as having evolved as has the architecture of a cathedral (1996: 61 – 72). His model differs from other modular theories in the number of the domains it takes into consideration (only four) and the way it conceives their development. However, what is really unique about his study is the attempt to connect the archaeological evidence to the main stages of the evolution of human intelligence. Mithen states that every attempt to understand the early prehistory of the human mind must take into account the fact that people considered the social, biological, technical, and linguistical domains separately and behaved accordingly (*op. cit.*, in the first place 7 – 16).

Another research domain in cognitive archaeology studies the role of the memory in the development of human cognition. In order to get over the flaws and limits of their biological memory, humans devised means for external symbolic storage. Artifacts played a major role, although humans themselves can also serve as means for improving memory. In illiterate societies material artifacts can have multiple meanings from totally different domains. This was highlighted by ethno-archaeologists and is of crucial importance for archaeology. Thus, a village plan in Papua–New Guinea might simultaneously reflect such different information as the social structure of the local community, its relationship with other villages, its way of conceiving human anatomy and physiology and so on (Strathern 1998). It is apparent that archaeologist will never be able to infer these multiple meanings of things from the archaeological record.

A question that cannot be answered might be a wrong question. This seems to be the main idea of R. Boast’s investigation of style and function.

Archaeology, he says, considered style as “the bit that is most human, added beyond natural necessity. It is the bit that has most to do with the mind, with human intentions, with human communication. Style is seen as a key to the social” (1997: 173). He thinks “Wobst left style under-defined and treated the distinction between style and function as unproblematic” (*op. cit.*: 177). He argues that the whole discussion about style and function is the result of the Cartesian subject/object divide. Ontologically, however, style is not different from function: whatever exists must have both function and style. Style is not a “paint” humans added to the absolutely useful part of an artifact such that the artifact can be manipulated for the achievement of social goals. Objects are not only manipulated by humans; they themselves also determine human actions. Like humans, they are influenced by their own history.

Certainly objects refer to the objects of the past, just as we refer to our human forbears as resources in our current identity [...] To the degree that objects are passive surfaces on which we paint identities, so are we. To the degree that we purposefully determine actions, so do objects. [...] It is not that the concept of style is invalid, only that it is only valid within its own contingent categorization of the world – it is a way of describing the world that is dependent on a specific set of assumptions about how the social world works (*Ibid.*: 190 – 191).

In his opinion, it would be better to give up classification and replace it with networks of humans and artifacts. The reader is offered no case study. The article simply ends by raising a set of questions:

What happens if we extend agency into objects? How would non-human agency operate, how would it be delegated, how would it be inscribed and how would the inscriptions be challenged? What is the role of material form and context in prescribing action? What will happen to the way in which we study prehistory if we accept material agency? (*Ibid.*: 191)

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Numerical Taxonomy

Numerical taxonomy (unless specifically cited, the following is a summary of Doran and Hodson 1975; Shennan 1988; Baxter 1994;

Bernbeck 1995) consists of numerical methods of analyzing relationships between entities and grouping them into previously undefined classes. This direction of research in classification started in the 1960s in close connection with the development of computers. As a result, thousands of titles were published. Several reasons explain this situation:

- Archaeologists were not able to solve the problem of the selection of attributes relevant to the analyzed items. Most of the objects went out of use a very long time ago, raising the question: which of the theoretically infinite number of attributes should be selected for analysis? Thus, one of the tasks of numerical taxonomy was “to discover unexpected clusterings which may prove to be important” (Cormack 1977: 330).
- Archaeological material accumulated over time. Numerical methods were meant to turn this otherwise unmanageable mass of items into a coherent body of information.
- Most importantly: the wish to obtain “objective” typologies, to make classification independent from the other, i.e., verifiable and criticizable.

With this in mind, archaeologists adopted diverse methods initially developed in other fields (psychology, physical anthropology, ecology). A classic example is given by the work by Sokal and Sneath, *Numerical Taxonomy* (1963).

Basic principles. Numeric taxonomy relies upon two basic principles:

1. Relationships between items are conceived as distance. The intuitive concepts of similarity and dissimilarity are treated as numerical concepts of proximity and distance. Calculations rely upon the system of Cartesian coordinates. Units are represented as points in a multi-dimensional space. Distances between points are reckoned by using Euclidian or other concepts of distance.
2. The collection is treated as a statistical sample. The main advantage of this is that whatever the characteristics of the sample might prove to be, they must also be valid for the population sampled. Inference from the analyzed collection to the prehistoric population becomes possible.

Both these principles encounter difficulties in practice. The use of the concept of distance for measuring artifact traits is unproblematic only if the characteristics of the considered traits are compatible with a metric scale. When, for instance, nominal variables are investigated the result

becomes artificial. This happens because they can be registered only as presence or absence, being assigned the numerical values of 1, in and 2, respectively. Consider three nominal attributes say 'made of bronze', 'made of iron', 'and made of gold'. If calculation is based upon the Euclidian concept of distance, then the distance between each pair of units becomes larger than 1 (see Pythagoras's theorem: $d = \sqrt{1^2 + 0^2 + 1^2} = \sqrt{2}$). This result has no real meaning. The problem is difficult to overcome when nominal and numerical variables are analyzed together. There can also be other problems relating to distance, however conceived (e. g., Mahalanobis distance, etc.).

The concept of the sample, as normally understood by statisticians, is inappropriate for the archaeological material. For inferences from the archaeological sample to the population to be valid, it is first necessary to answer the following series of questions:

1. What is the relationship between archaeological items and the life of the human community that produced them? – This being the ultimate scope of the analysis.
2. What part of the material culture of a community entered and has been preserved in the earth?
3. Which of the artifacts preserved in the earth has been recovered for the research?
4. What selection of what has been recovered will be analyzed?

It is not possible to place these phases of judgment in a valid relationship with each other, with the exception of the last two. Still, the collection must be conceived of as a sample, lest the discussion be about some prehistoric material still existing in the present, and not about the past. Generalizations must rely on the analyst's experience and common sense, a situation also encountered in other disciplines (e.g., geology). To overcome this, other concepts have been introduced: target-population, case study.

General description of the methods used in numerical taxonomy

Some of the most commonly employed methods are presented here (for a general classification of all methods, see Voorrips 1982: 118). Procedures are normally either hierarchical or partitional.

Hierarchical procedures order classes into a hierarchy, resulting in a dendrogram. This can be obtained either by **agglomerative** procedures

(entities are organized in more and more inclusive classes) or by **divisive** procedures (the collection is divided into successively smaller groups until division is halted for some reason).

Partitional procedures produce a direct division of the sample into a given number of clusters, the relationship between clusters being of no interest.

Methods for grouping items

The monothetic, divisive procedure benefited from one of the few algorithms specially written for archaeology (Whallon 1972). Whallon set out to analyze a prehistoric pottery style, called Owasco (late Woodland; Center and East New York; 1000 – 1300 AD). Ritchie and MacNeish, who are thought to have used Krieger's definition of type, have previously classified the same pottery. They determined 16 types of the Owasco pottery, all having regional and chronological value.

In 1972, reanalyzing the same pottery, Whallon used Spaulding's statistical method (Spaulding 1953). However, only obtaining two types, he asked himself what had gone wrong. On the one hand, Spaulding's procedure was appropriate for Krieger's definition of type, also used by Ritchie and MacNeish. On the other hand, the types elaborated by Ritchie and MacNeish had shown regional and chronological value (the latter also tested by radiocarbon dating) and could be easily learnt and correctly used by archaeologists. This was a clear sign that was not merely the result of the author's imagination.

Whallon reanalyzed Ritchie and MacNeish's typology and noticed that, rather than being paradigmatic, as required by Krieger's definition, it was intuitive. It became clear that intuitive classification relies upon a non-explicit logic. But Whallon wanted a standardized method of producing such a classification, so he devised a monothetic, divisive procedure. As is usually the case in guides and classifications used by biologists, he considered attributes successively, one for each level of the division (hence the name monothetic). Their order is essential to the end result. Using a computer program, he managed relatively accurately to reproduce Ritchie and MacNeish's typology. This now became independent from the experience of their authors. The good news to come from this study was that intuitive classification was not illogical at all. The bad news was to come later: the replication of the intuitive

classification by using statistical methods was still a problem. The χ^2 – test is inappropriate for determining the frequency (i.e., succession) of attributes in a collection. The smallest of changes in its value influences the order of attributes.

Cluster analysis, at a first glance, seems more appropriate as classification method. This is because items assigned to one cluster are reunited by different attributes, such that every item has some characteristics in common with the other, but none of the characteristics is shared by all items in the cluster. This seems to correspond to the polythetic use of “type” in everyday life. There are lots of algorithms for obtaining clusters, each with its own advantages and disadvantages. In general, all make comparisons (i.e., reckoning of distances) between entities and cluster those which, depending on the algorithm used, appear more similar.

Methods for variable construction are built upon three basic ideas:

1. Space, distance, and dimensions. Archaeological units are considered points in the space. Distances between points are taken to reflect distances between units.
2. Configuration is referred to by coordinate axes (found at right angles to each other).
3. “Simplification”, i.e. reducing the number of dimensions to those necessary for the direct representation of the data. Here the problem is how to simplify the information without distorting it.

Principal component analysis starts with a multi-dimensional space with a large number of points (i.e., archaeological units described by many attributes). Axes are rotated so that they remain at right angles to each other and the first axis is placed in the direction of the greatest spread of point scatter, the next in the direction of the next greatest remaining spread, and so forth. These new axes are called *components*. As a rule, only the first few components are relevant; the rest can be ignored. These new axes are by definition uncorrelated. This allows the archaeologist to analyze the main attributes responsible for the variability in artifact of the assemblage under consideration.

Rotational fitting is used for analyzing two different configurations of points. The axes, fixed by a common origin, are rotated until the best overall fit between the corresponding points is achieved. The distances, i.e., the differences between these points, are then calculated.

This is a useful method when items are considered from two different points of view (e.g., pottery shape and pottery decoration) as it shows how closely 'related' they are to each other.

Discriminate analysis has been developed for situations where an initial grouping of the units already exists and analysts have searched for the main attributes that distinguished (discriminated) them from one another. The best example is that of the relationship between anatomical traits of humans and that of different species of non-human primates. It can be used successfully to discriminate between artifacts with known provenience (e.g., glass beads from different workshops).

Some comments on numerical taxonomy. In spite of the great enthusiasm taxonomists have shared for decades, it became clear that these methods could not eliminate subjectivity from the analysis. The idea of Doran and Hodson of calling them "objective methods" is misleading (Voorrips 1982: 111). All these methods presuppose that the analyst take a whole series of decisions. Firstly: which of the potentially infinite number of attributes is he going to consider? There is no way of seeing whether there were attributes that should have been considered, but were not (Bernbeck 1995: 218).

Hierarchical methods should be used only when there is reason to think that the relationship between units is ranked. If not, then that which we have obtained has no meaning (Voorrips 1982: 119 – 120).

The result of the analysis becomes meaningful only through interpretation. The most obvious case is that of cluster analysis. Clusters must be given a meaning. The criteria that brought them into being must be 'discovered' by the analyst after 'subjective' examination of their content. How many clusters there are in a collection also remains to be decided by the analyst after 'subjective' consideration (Bernbeck 1995: 217 – 220).

Nevertheless, these methods are still very useful as exploratory methods (Baxter 1994: 1-23) since there are characteristics of artifacts which cannot be distinguished by use of human senses, their being either too numerous or having prohibitively small dimensions. By way of example, we can consider a test made using three potters, all requested to manufacture ten cups of a certain shape. For the analyst, all the cups produced looked very much alike, but after measurement and analysis by statistical methods, both computer programs identified tree clusters.

These corresponded, as expected, to the three handworkers (Impey and Pollard 1995).

However, the importance of these methods shouldn't be overemphasized. When this happens, the archaeologist risks not thinking sufficiently about his or her classification.

Probably the best idea with which to end is that of Doran and Hodson:

We do not believe that intuitive classifications by archaeologists are necessarily invalid. On the contrary, the visual appreciation of complex morphological patterning is a major human ability, which it would be perverse to discount. [...] However, it is clear that the innate abilities for pattern recognition that archaeologists may possess are rarely controlled sufficiently for consistent, communicable classifications to result (Doran and Hodson 1975: 186).

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On the Consistency of the Archaeological Measurements

Among all stages of archaeological research, classification is probably that most influenced by human perception. Only a few studies have investigated its role in the recognition of types. Are classifications fully replicable?

The Kayenta Tusayan pottery (south-western USA) has a well-established and long-known typology and as such was chosen for a case study by Fisch (1978). There were four participants in the test, of which three learnt the typology in the same period. The classification depended on a combination of nominal variables. The final results showed a discrepancy of between 22 and 30% between any two of the participants; discrepancies were somewhat evenly distributed between categories (no single type occasioned consistent bias); some attributes turned out to be understood differently by different analysts.

A second test carried out by Fisch (*op. cit.*) was designed to verify the replicability of measurements in time. The same person remeasured a collection of flint tools after three years. Some of the measurements yielded 0% discrepancies as compared to the previous, while others showed a bias of more than 30%. The cause was identified as the broad definition of some of the attributes considered (e.g., the amount of cortex

preserved on the artifact). As noted by the author of these tests, archaeologists must show caution when interpreting material from publications.

Beck and Jones (1989) sorted 40,000 lithic tools from southeastern Oregon into functional categories. Ch. Beck showed a constant tendency toward conservatism (by recognizing fewer categories); Jones, on the other hand, tended to be liberal (by constantly recognizing more categories). These tendencies remained constant throughout the test.

Whittaker, Catkins and Kemp (1998) carried out a consensus test – borrowed from ethnology – in a well-known pottery (North Sinagua, Arizona) to test the coherence of classification by 13 persons of different levels of experience. The results showed the professor's tendency to be different from the general tendency of the analysts, which also included the students with little experience.

In conclusion, it has become clear that if consistent classification is to be achieved, the following must be observed:

- Classes should be clearly defined;
- A set of rules for ambiguous situations should be elaborated (how straight is a straight line? how straight should the active part of a flint tool be that it be considered 'straight' and not 'concave' or 'convex'?);
- The classification should be verified during the classification process;
- Several analysts should work concomitantly if types are to be correctly recognized.

Nevertheless, the problem of systematic bias can not be totally eliminated and will always influence the possibilities of comparing archaeological materials.

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How I See the Typological Problem in Romanian Archaeology Today

In the following discussion I will refrain from quoting any particular work except where I find it absolutely necessary to the understanding of this text.

Seen as a whole, Romanian archaeology still uses only intuition as the method of classification. Items are grouped as the analyst sees fit, classes only described, but never justified, and archaeological works have only one, multi-purpose typology: that which should serve for the investigation of time-space relationships, as well as economic, social and ethnic (or other) phenomena. The correctness of a typology is not evaluated in the context of the aim of the research; rather it is evaluated in terms of the ideal of the best general homogeneity, though it is never stated as such. A typology may be judged to be wrong simply because it is found to have too many types or varieties, irrespective of the problem under consideration. The dominant idea is that the “real type” should be discovered in the material. This is typical of culture-history, the dominant research direction in Romania. Other directions in the world archaeology are not represented and, as a consequence, the typological debate is still absent. This is due to the way in which the whole archaeological process is organized (though, first and foremost, the lack of specific and systematic university education in the field archaeology is to blame). However, I will not elaborate on this matter further here.

As an effort to update research and given the scientific impression people generally hold about statistical methods, attempts to apply mathematics in archaeology have, in fact, been in evidence. The first of these probably dates to 1971 (Nestor and Vulpe), while the most recent uses computer-based statistical techniques as considered useful for archaeology by the authors (Lazarovici, Micle 2001). There is no continuity to these works and no discussion of the validity of the methods. As with the case of intuitive classification and numerical taxonomy, techniques (sophisticated to a greater or lesser degree) are implemented haphazardly and in the absence of a theoretical background, the input is not conceptualized and the algorithm used is determined more on the basis of the availability of a computer program than by its suitability to the problem under consideration (typically the use of seriation, instead of cluster analysis, or specific cluster-analysis algorithms with no explanation as to the way clusters are built). There is no easy way to remedy this situation; the use of statistics requires specialists, and they will not appear any time soon in Romanian archaeology, where even the most traditional skills are self-taught.

In summary, the typological debate, which is at the heart of present concerns in the world of archaeology, is at its very beginning in Romania.

NOTES

- 1 By way of example: American children living in an urban environment classify flora in only four categories: trees, plants with or without leaves, bushes, and grass. In their time, children living in a tropical forest recognize about 600 species of plants (Daugherty 1978: 74).
- 2 For instance, there are all kinds of tools and weapons we don't use any more (cf. O'Brian and Leonard 2001, especially fig. 1.1).
- 3 Reanalyzing ceramics from North Mexico from the point of view of an Evolutionary Archaeologist, Leonard obtained clusters which differ from those of other analysts before him. This meant the previously stated type of relationships were incorrect, owing to the unawareness of the distinction between homologous and analogous traits (Leonard 2001: 85). In my opinion, though this might well be the case, his demonstration would nonetheless have been more convincing if he had made the effort to explain to what extent these differences might have been caused by the algorithms used for cluster formation.
- 4 This characteristic is usually called "Machiavelic behavior".

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