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**THEORETICAL ARCHAEOLOGY IN THE MAKING:
A SURVEY OF BOOKS PUBLISHED IN THE WEST IN 1974–1979**

Abstract

A survey of the trends in archaeological method and theory during the second half of the 1970s is presented as perceived by the author in 1980, shortly before his arrest and imprisonment as a Soviet dissident. The paper was originally meant as a follow-up of his "Panorama of theoretical archaeology", published in *Current Anthropology* in 1977. Despite the time elapsed, the 10-year old paper has not lost actuality and bears witness to the sharpness of its vision. (FA)

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Turning-point

"A panorama of theoretical archaeology" (Klejn, 1977) covering a period of over ten years was brought up by me to 1973. It triggered a discussion in the course of which the very idea was questioned that world theoretical archaeology is in existence, or is taking shape, or, at least, is needed and practicable (reply to the opponents – see Klejn, 1980). In the meantime, between the end of the period reviewed and the late

seventies the emergence of that branch of science in the West became an obvious fact, not only conceptually but quantitatively as well.

Single infusions of theoretical monographs into the stream of theoretical articles suddenly gave way to an abundant flow of books (monographs and collections of articles) on theoretical archaeology. In the period treated in the "Panorama", none to five such books would annually come out (a greater number was only in 1972, namely 8 books) and afterwards, to the best of my knowledge, 8 to 10 books were published every year in 1974–1976, while the corresponding number for 1977 was 17, for 1978 – not less than 30 (!) and for 1979 (according to information available so far) – at least 10. Well then, theoreticians borne by the "Great Debate" between the Old and "New" archaeologies, those who previously published only articles, had matured since, reaching the level of monographic systematization of their views. In some centres groups of theoreticians had sprung up, and collections of theoretical articles were beginning to come out regularly.

The turning-point came in 1977. By a lucky chance it was the year my "Panorama of theoretical archaeology" was published; L.R. Binford's work "For theory building in archaeology" (Binford, 1977) came out in the same year – the name of the branch appeared on book covers, and three years later J.C. Gardin's book came

* Leningrad archaeologist Leo S. Klejn planned to continue his well-known "Panorama of theoretical archaeology" published in "Current Anthropology" (1977). But in 1981 he was arrested, spent a year and a half in prison and camp, and since then could not find a job in the U.S.S.R. However, since 1984 his articles began appearing again in Soviet professional journals, and now, during perestrojka he is afresh among leading archaeologists of the country, though he is retired and is busy now more with philology (Homeric studies) than archaeology. In a series of articles published in a popular Leningrad magazin with wide circulation (more than half a million issues) he describes his counterfeited trial, his adventures in Gulag and his professional observations made there – the "ethnography of camp". (See also Lev Samojlov, *Etnografija lagerja. Sovetskaja etnografija* 1/1990, 96–107.)

Here the continuation of the "Panorama", interrupted so dramatically, is presented or, to be precise, its part prepared for publication a decade ago. The survey was made until 1979 and written in 1980, but still remains interesting.

off the press under the plain title "Une archéologie théorique" (Gardin, 1979b).

Indeed, separating monographs from articles for the purpose of a survey is a somewhat artificial procedure (not unlike testing milk by skimming), yet it enables, though not without disruption of intrinsic links, to select and characterize brief by the more established and mature specimens of the current scientific output. As a matter of fact, leaving aside articles and confining the survey to books is a customary practice for a reviewer.

Expansion of the "New Archaeology"

As could have been expected, a certain lull followed the hard fighting of the late sixties and early seventies between the "New" and Old archaeologies. The "New Archaeology" stabilized and acquired substantial exegetics. Most of the space in a "Helinium" issue taken up by articles on the "New Archaeology" (Helinium, 1974, 2) and the whole number of "Prähistorische Zeitschrift" given over to the semihistoric - semiapologetic (though very thorough) M.K.H. Eggert's work on the "New Archaeology" (Eggert, 1978). The leaders of the "New Archaeology" Lewis R. Binford, Colin Renfrew and the prematurely deceased David L. Clarke became classics and, following the republication in a single volume of theoretical articles by Binford (1972), came out similar collections of papers by Renfrew, Clarke and Bruce G. Trigger, one of the leaders of "Settlement Archaeology", i.e. of a competitive branch of Taylor's "conjunctive approach" (Trigger, 1978; Renfrew, 1979; Clarke, 1979).

Either a direct realization of the "New Archaeology" demands or an impact of that movement can be discerned in a number of books whose titles frequently contain such word combinations as "new approaches" or "new directions" (Darvil et al., 1977; Kristiansen and Paludan-Müller, 1978; Plog, 1978). D.H. Thomas' introduction to "anthropological archaeology" is entitled "Predicting the past" (Thomas, 1974); its very title, like the subtitle of another book - "a predictive model" (Jochim, 1977), reflects confidence that the archaeological past is accessible to the hypothetico-deductive principle, i.e. to simple verification of hypotheses by checking expectations against new facts. "Anthropologicality" of archaeology is advocated by W.T. Neil

in his "Archaeology and a science of man" (Neil, 1978).

Richard MacNeish who, according to his calculations, spent 5683 days in the field, was "growing up in and with American archeology" (such is the title of a section in his book) published in 1978 his meditation memoirs. In his informative and eclectic work MacNeish depicts his own and its past as a consistent and logical way from humanities to a social science, similar to natural sciences, - with verification of hypotheses, with system approach, ecologic paradigm, theory, etc. And, though actually the character of his research has been conventional, the bent for those new reputable mottoes is quite significant. Correspondingly introducing into the title the word "science" the author, nevertheless, ends the title with a question-mark ("The science of archaeology?"), thus admitting that neither he nor archaeology has quite attained the sought-for status (MacNeish, 1978, p.XII). Now the crown of development in the mainstream of American archaeology in G.R. Willey's and J.A. Sabloff's historiographic work (Willey and Sabloff, 1974) looks like the "New Archaeology", whereas previously Willey's attitude to it was much more reserved (Willey, 1968).

It is, indeed, the "New Archaeology" that is responsible for the emphasis on methods for gathering and selecting material now displayed not only in articles but also in three pamphlets (Redman, 1974; Mueller, 1974; Jelks, 1975) and three books (Mueller, 1975; Flannery, 1976; Cherry et al., 1977). None but adherents of the "New Archaeology" contended that not everything found in the field should be taken but only what is needed for verifying the hypothesis in question. Despite the dubiousness of that attitude, work on the problem is useful, inasmuch as it concerns representativity of samples, methods for objective registration, etc. Virtually excellent is the monograph (one can hardly call that integrated book a collection of articles) by a group of authors headed by Kent V. Flannery. In the brilliant introductory improvisations (based on actual events and images) Flannery wittily enacts field scenes and theoretical arguments between four generalized figures; R.M.A. - The Real Mesoamerican Archeologist, S.G.S. - his Sceptical Graduate Student, G.S. - the Great Syntesizer, and himself (the Ironizing New Archeologist). In those arguments R.M.A. was always defeated but did not usually realize it. Yet once, replying to S.G.S.'s philippic against the "Old Archaeology", an episodic interlocutor told him: "All you proved is that bad New

Archeology is as bad as bad Old Archaeology". "Worse" — R.M.A. corrected him. — "When the Old Archeologists write something bad, you could ignore their theory and use their raw data. When the New Archeologists write something bad, it doesn't even have any data you do want to use" (Flannery, 1976, p.285). I.N.A. made no objection. But, of course, he thinks that good New Archaeology is much better than good Old Archaeology. However, the point is — for how long the New Archaeology will remain new. And is it still new today?

No doubt, it was still giving rise to innovations in the seventies. The most interesting one was introduced by the talented and hard-working English archaeologist C. Renfrew. He attacked the problem of a qualitative leap in evolution, the problem of independent replacement of cultures. How do gradual quantitative accumulations lead to an abrupt qualitative jump? To reveal that mechanism the French mathematician René Thom developed a topological "Catastrophe Theory", the one that Renfrew took advantage of (articles: Renfrew, 1978a; 1978b).

The explanation lies essentially in realizing that gradual quantitative changes occur in one parameter whereas the qualitative shift takes place in another (e.g., in the case of boiling water: the quantitative changes in temperature and the qualitative shift in the state, in the structure of the substance). Thom represented the channels of changes in both inter-related factors as intercorresponding trajectories on planes with different curvatures. With that pattern, a gradual change in one factor is attended in some places (when projected to the other plane) by a sharp acceleration in the change of the other, if the other plane's curvature is drastically increasing at such places. This conception enabled Thom to graphically express mathematical relationships and to propose a matrix of all theoretically possible kinds of "catastrophes" or revolutions (there proved to be 16 of them of which, in fact, only a few kinds do materialize).

In the preface to Renfrew's collection of articles "Transformations" Thom writes that the application of his theory to archaeology is its first serious and systematic utilization, although it was originally intended for psychology (Renfrew and Cooke, 1979, pp.XVII–XVIII). Presented in the collection are very interesting and promising studies. It should not be forgotten, however, that they provide only a formal articulation of the mechanism of changes, leaving unanswered the question as to what determines, in each par-

ticular case, the curvature of the other imaginary plane, the question of the channels of the substantial process and of the matter links between the factors.

In 1967 the Society for Historical Archaeology was set up in the USA with its journal "Historical Archaeology" which began orienting archaeological research in post-Columbian America first to realizing the need for theory and afterwards, after the dramatic 1971 "forum", to renouncing the old "particularistic", i.e. humanitarian-historical paradigm, aimed at the individualizing study of factors, and to replacing it with a "nomothetic" (law-establishing) paradigm of the "New Archaeology". Giving rather an ironical characteristic of this movement Stanley South places the histogram of its production onto an image of a snail. As shown by South's histogram (South, 1977a, Fig.3), the movement gave rise to a thin streamlet of articles (traced in the histogram until 1974) on the application of the hypothetico-deductive method to historical archaeology. In addition, two books came out at once in 1977: South's monograph "Method and theory in historical archaeology" and a collection of articles edited by him (South, 1977a; 1977b). In the histogram they would occupy the space where the snail raised its head. The following year they were supplemented with the collection entitled "Historical archaeology" with R.L. Shuyler as editor. "A new look at the American heritage" (subtitle) is promised by Th.A. Sande's book "Industrial archaeology" (Sande, 1978).

These works attest to the expansion of the "New Archaeology's" possessions in the USA: the primeval domain was joined by the historic province. Content with the conquest, Binford in his preface to South's monograph writes: "Rarely is a book published that is more than of passing interest. This is one of the rare ones." (South, 1977a, p.XI). In the meantime, the newly conquered region is not very large: as a matter of fact, "historical archaeology" of the U.S.A. is but a small part of the world archaeology of the later epochs whose foundation is the medieval archaeology of the Old World that has not as yet been conquered by the followers of Binford, D. Clarke and Renfrew. And farther ahead is the classical archaeology, the main stronghold of the traditional views... (cf. Bianchi Bandinelli, 1978).

Meanwhile the "New Archaeology" has not yet overcome some of its old difficulties, those involved in solving its basic problems. The American student M.B. Schiffer incriminates it with "profound neglect of many unresolved epistemological, theoretical, and methodological issues of past decade" (Schiffer, 1976, p.1).

One of its radical internal contradictions lies in the way it determines the record and the motive forces of cultural development. The "New Archaeology" renounces resorting to migrations and influences (Renfrew, 1979), it seeks to reveal the internal forces and immanent laws of development. Its apparatus of methods and concepts of system analysis (homeostasis, feedback, etc.) serves to account for the stability, tenacity of cultural systems but not their development, or growth, or progress.

Therefore, undertaking research in the internal processes of long-term cultural changes, Flannery and Renfrew as far back as the late sixties and early seventies gave up the hope cherished by Binford to discover the laws of development. They chose another modification of systemic approach, initiating a special line of investigation in the "New Archaeology", while Fred Plog explicitly announced his "departures" from the "New Archaeology" at all (Plog, 1974). Moreover, the "New Archaeologists" wish to avoid the "one-sidedness" of the monistic principles which bring the methodology too close to that of Marxism. Therefore, the source of development has to be associated with "kickers" capable of being engendered within any branch of culture, within any parameter of social life (Plog), it has to be placed outside sociocultural systems – into the natural environment (Binford), or else outside any subsystem of culture (Renfrew). This, however is at variance with the concept of the independence of cultural development and cannot account for the consistent trend in development. Furthermore, what has also to be explained is the discrete and leap-like character of development. In vain did theoreticians of the "New Archaeology" struggle with these problems.

Setting himself the goal of detecting the origination of "kickers" within a sociocultural system and finding that their distribution over subsystems was disorderly, Plog thus evaluated the result of his investigation: "neither a complete success nor a complete failure" (Plog, 1974, p.143). However, what he was checking in his search for "kickers" was not the chronological

and logical connections between shifts in concurrently changing parameters but a correlation between parameters within short-term (50-year long) periods assumed to be static systems, a sort of instant cuts, as it were. Several dozens of such periods have been distinguished, some of them periods of stable existence and others of transition. It is just in the periods of transition – and this has been detected! – the more noticeable changes occurred in technology (97%). Without expecting and realizing it, Plog received confirmation of one of the basic prerequisites of historical materialism.

Only in 1977 came out the proceedings of the 1970 Santa Fe symposium which reached the conclusion that it was impossible in principle to find the source of development within a system (Hill, 1977). The dialectical concept of internal contradictions as the source of development remained alien to the New Archaeologists.

Extensive application of mathematical methods and formalization techniques, as well as the expansion of non-traditional interests (in ecology, economics, social reconstructions), inspired the "New Archaeologists" fascinated with successes and engendered the idea among them that in the USA "New Archaeology became everyone's archaeology" (Leone, 1971, p.222) and this idea was communicated to other archaeologists as well (e.g. Trigger, 1978, pp. 195–196). The wide acceptance, of the New Archaeology, however, may well be due not so much to its successes as to the fact that the inability to resolve some radical problems has made the "New Archaeology" much more moderate than before and for this very reason (and in that form) quite acceptable to the wide circles of traditionally-minded archaeologists. The principal adherents of the "New Archaeology" have changed considerably, abandoning some of the important original positions which had determined its make-up.

In the circle of "Law and Order archaeologists", Binford's most orthodox followers, a curious shift in interests took place, evidenced by the collection of articles "The individual in prehistory" edited by J.N. Hill and J. Gunn and contributed to by F. Plog and Ch.L. Redman (Hill and Gunn, 1977). From the search for laws, regularities and stereotypes the "New Archaeologists" swung aside towards revealing the individual, unmatched and specific features stemming from the personal peculiarities, from the individual creative endeavour of people of the past. This was exactly what had been advocated for a long time by the British hypersceptical

archaeologists and the indeterministic scholar Ernst Wahle, the patriarch of archaeology in Germany.

Such investigations are, of course, necessary and, indeed the "New Archaeologists" carry them out in a much more sophisticated and exacting manner than the "old" ones. However, the significant point is as follows. In the sixties the "New Archaeology" was coming into existence against the background of youth riots and was linked with the sympathies of the left-wing intellectuals with the mass revolutionary movement, with faith in the natural progress. In the seventies, on the other hand, against the setting of declining mass rioting, when some of the recent rioters had sown their wild oats, while others turned to individual terrorism, the changing interests of the "New archaeologists" reflected, like a sensitive barometer, the change in the intellectual climate. This sliding down of the "New Archaeology" from the former positions is such that, properly speaking, some of its basic features are disappearing. And a question arises whether or not we are witnessing the degeneration of the "New Archaeology" into something different, in a word, the beginning of its end.

In June 1976, at the age of 39, died the "New Archaeologist" David Clarke, one of the founders of "Analytical Archaeology", who fundamentally applied to archaeological research in culture the neopositivistic methodological principles of Wittgenstein's "Analytical Philosophy", as well as the theoretical experience of "Analytical Biology" and "New Geography". The untimely death of Clarke is a severe loss not only to the "New Archaeology" but to theoretical archaeology as a whole.

The persisting influence of "New Geography" on the "New Archaeology" has led to the development, among Clarke's followers in Britain and other countries, of a sizeable mathematico-geographical trend, viz., Spatial Archaeology. Underlying this modern branch of "Analytical Archaeology" are two methods: "locational analysis" and "spatial analysis". The former applies geometry to comparison of the actual vs. optimal locations. The latter is more ambitious, mobilizing various mathematical procedures for expressing and evaluating spatial relations between archaeological finds, first of all between locations. Spatial Archaeology is not interested in the territories of archaeological cultures, local variants, etc. which were of great concern to "Analytical Archaeology". In the late sixties-early seventies Clarke experienced "emancipation from the cultural paradigm of V.G.

Childe", as his (Clarke's) disciples N. Hammond and A.G. Sherratt put it (Clarke, 1979, pp.9, 195). Already in his collection of articles "Models in archaeology" (Clarke, 1972) locational analysis was noticeably represented, including among others Clarke's own paper. Now other works are added to them: the posthumously published collection "Spatial archaeology" edited by D.L. Clarke (1977) and made ready for the press by I. Hodder, as well as I. Hodder and C. Orton's monograph "Spatial analysis in archaeology" (Hodder and Orton, 1976). Those are all very useful studies, yet here again, historiographically, the significant point is departure from investigation of evolutionary, developmental trajectories. "New Geography" is perceived here at every pace, but "Analytical Biology" has retired to the background. "Spatial archaeology" is "Analytical Archaeology" minus evolution.

Still another of the important positions no longer defended by the "New Archaeology" is equalization of systems revealed in archaeological material with the living systems of the past. Any change in the trajectory of such a system was regarded by the "New Archaeologists" as a direct projection of changes that had once taken place in the living culture. The archaeologists were trying to seek out possible causes for each shift: ecological changes, influences by neighbouring population groups, etc., ignoring the possibility of "posthumous" changes, differences in the state of preservation, in the state of our knowledge, etc. This permitted systemic approach to be used as it was employed in biology.

D. Clarke himself in one of his last articles "Archaeology: the loss of innocence" (Clarke, 1973, republished in 1979), questioned the validity of that equalization and proposed that the theory should be subjected to new divisions which recognize the complexity of relations between living culture and dead remains. Now Binford has published a collection of articles in which this proposal is materialized, true, only as applied to the natural fractions of archaeological material: osseous remains, flora, etc. (Binford, 1977).

Ethnoarchaeology and "behavioral archaeology"

It was precisely the demands for building up such divisions of archaeological theory, spontaneously felt even before, that gave rise, as early as the fifties, to experimental archaeology.

This is a specific branch of science at the junction point of archaeology and ethnology, a branch with tentative terms of reference, delimitation and name. It comprises "action archaeology" designed to reveal and register the components of a living culture which are to fall out into archaeological sediment, and to study the processes of such sedimentation; "ethnoarchaeology" (sometimes – "living archaeology", "living prehistory") – studies of the same kind oriented only to backward communities (ordinary ethnographic objects); "urgent archaeology" – the name should actually be applied to just abandoned nomad encampments, whose recent inhabitants are still alive and can amend the interpretations, to excavations of contemporary dumps, etc. ("urgent archaeology" is contiguous to "industrial", "historical" and "experimental" archaeologies). These activities, except "urgent archaeology" are integrated ever more often under the name of "ethnoarchaeology", since not only strictly ethnographic cultures, but also quite civilized groups of population, are investigated by it for archaeological purposes, using ethnographical means.

Experimental archaeology in the period under review was represented by two books (Ingersol et al., 1977; Keely, 1979); similar works by J. Coles, 1973; 1980) are outside the period. What experimental archaeology provides for the reconstruction of technological processes, ethnoarchaeology does for the reconstruction of socio-cultural processes. Thus, both branches are engaged, to a large extent, in parallel activities, yet ethnoarchaeology is closer to the principal problems of archaeology.

One of the leaders of that movement is L.R. Binford. In 1969–1972 he travelled for many months in Alaska together with Nunamiut Eskimos, cariboo hunters. Those explorations resulted in a monograph (Binford, 1978) of fundamental interest to archaeologists as the most comprehensive and systematic investigation of the kind. The same year saw the publication of I. Ghoneim-Graf's ethnoarchaeological monograph on Africa (Ghoneim-Graf, 1978) and J.E. Yellen's book "Archaeological approaches to the present: models for reconstructing the past" (Yellen, 1978) summarizing such research. Besides, three collections of articles on ethnoarchaeology came out in the seventies (Donnan and Clewlow, 1974; Gould, 1978; and Cramer, 1979), as well as one thematically close to them (Spriggs, 1977).

All studies of that kind accumulate empirical information, collect and summarize facts for de-

veloping a theory of archaeological reconstruction. "Behavioral archaeology" that emerged on that basis has undertaken the task of constructing such a theory. Work is conducted with due attention to detail, in the austere and impassive style inherited from the "New Archaeology". The adherents of "Behavioral Archaeology" are not yet many in number. Headed by Binford's disciples – M.B. Schiffer, J.J. Reid and W.L. Rathje – the movement separated from the "New Archaeology" in the early seventies, confronting it. Papers started appearing in 1972 (Schiffer, 1972a; 1972b; Lange and Rydberg, 1972), a collection of articles came out in 1974 (Reid, 1974), in 1976 Schiffer's monograph "Behavioral archaeology" (Schiffer, 1976) was published, and 1978 saw the publication of three collections (Schiffer, 1978a; 1978b; Grebinger, 1978) and of B.D. Smith's monograph (Smith, 1978).

What these authors primarily reproach the "New Archaeology" for is its "oft-repeated assumption that there is a direct relationship between a past behavioral system and its archaeological remains" (Schiffer, 1976, p.IX). Schiffer contrasts it with an opposite, undoubtedly more realistic assertion considering "these relationships" as "seldom being direct". He explains that usually structures observed in archaeological material are not an isomorphous reflection of the ancient structures formerly existing in the culture, social organization and behaviour of people. One ought to study the results of people's behaviour, the processes involved in the formation of a culture, in the depositing of remains and in their transformation into archaeological material. One should ascertain the laws governing those processes so that rigid deductions from archaeological remains would reveal the pattern of ancient people's behaviour. Due to the differences in one of the basic postulates, the general strategies of the "New Archaeology" and "Behavioral Archaeology" are antithetical.

The alternative views have given rise to somewhat identical controversies in Soviet archaeology, some authors accepting direct correspondence (Masson, 1976) and others rejecting it (Klejn, 1978). However, the arguments here and there are far from showing complete similarity. Anyhow, the positions of Soviet opponents to the simplifications do not coincide in many respects with "Behavioral Archaeology".

Calling their activities "Behavioral Archaeology", Schiffer and his associates do not, apparently, have in mind orientation to the methodology of behaviorism. They merely wish to stress

that they no longer want to glue together a priori the structure of archaeological material and patterns of ancient people's behaviour. They demand that those behaviour patterns be articulated separately as a special hard-to-reach goal of reconstruction.

However, Behavioral Archaeologists readily speak of the "behaviour" of cultural remains, meaning their transformations. That means they equalize all "mechanical" changes – people's actions and the ageing of things – as objects of scientific observation, registration and revelation of regularities and laws. Of course, it leads to simplifications. Archaeologists of that circle are interested in what usually occurs to things and what actually occurred to the things in each particular case, the problem of significance, of meaning being overlooked altogether. Disintegrating into elementary, superficially observed behaviour cells, the complex in most structures of cultural relations fall outside the range of archaeological objects. And this is exactly a feature of behaviorism which manifested itself (not so markedly, though) in the "New Archaeology" as well.

Behavioral archaeologists also inherited from the "New Archaeology" the conviction that archaeology alone was sufficient for complete reconstruction of the past. At least, the problem of integration of sciences, the problem of palaeohistorical synthesis, is posed in none of their works, which reduces considerably the efficiency of their method. Indulgence in the evidence of ethnoarchaeology and "action archaeology" has led Behavioral Archaeologists to the loss of discrimination between archaeological antiquities and present-day cultural objects, to depriving archaeology of its distinctive subject matter. "Archaeology, – states Schiffer, – is redefined as the study of human behavior and material culture, regardless of time or place (Schiffer, 1976, p.IX). "Expanding archaeology" is the title of his paper written jointly with Reid and Rathje (Reid et al., 1974). The trend towards expansion of archaeology beyond the scope determined by the specificity of its sources occasionally manifests itself not only in the USA (for criticism of such manifestations in the USSR see in: Klejn, 1978, pp. 48–60; Zakhruk, 1978, pp.12–13).

Thus, American "Behavioral Archaeology" expresses both a specifically American response to the requirements of today's practice, a response held in the traditions of American archaeology, and the world tendencies in the development of theoretical archaeology. These tendencies are partly – fruitful and meeting the de-

mands of present-day practice of research, and, partly – leading one away from them.

Mathematics, logic and computers

Mathematization of archaeology was going on in the course of the six years, yet in a different manner than before. Previously the sequence of steps was as follows: predominating first were experiments in devising methods and a search for their possible applications in archaeology, then followed adjustment of the methods to solving a number of archaeological problems, i.e. selection of appropriate approaches to specific problems. What prevails now is systematization of accumulated experience in manuals (Wilcock and Laffin, 1974; Doran and Hodson, 1975 – cf. Balonov and Sher's review, 1978; Bertilson och Ekblad, 1977; Ihm, 1978; Cowgill, 1979) and development of theoretical principles for application of mathematics to archaeology. People strive to gain a better insight into the nature of archaeological material and into the mechanism or – within the optimal, ideal limits – into the algorithm of archaeological research. What is needed is to refine upon the types of distribution of objectively recorded characteristics in archaeological material, and to further ascertain the character of inter-relations between them and the elements of a living culture and human activities, as well as to reveal the rules for strict interpretation reasoning in archaeological research. These are precisely the problems dealt with in the collections of articles initiated by some French archaeologists (Borillo et al., 1977; Borillo, 1978).

In addition to the general, greatly varied tasks of the mathematization and formalization of archaeology, the French have their own fad in that field, viz., development of descriptive archaeology associated with computer-based rationalization of collecting, storing, processing and retrieving mass information.

Organization of appropriate storage of a great body of archaeological information is dealt with in works on computer data banks (Borillo et Gardin, 1974; Borillo et Bourrelly, 1976; Le Maitre, 1978) on the basis of some specialized codes (formalized languages for description) developed by the French in the previous decades. It was at the time that J.C. Gardin laid down the principles of descriptive archaeology. Now he and his colleagues set out to formalize the ways of historico-archaeological processing that information, refining upon and streamlining the arch-

aeologist's interpretation reasoning (Gardin et Lagrange, 1975; Lagrange et Bonnet, 1978). At the same time this problem is also approached by theoreticians who experimentally investigate the potentialities of computer-based simulation of the course of archaeologist's reasoning (Hodder, 1978).

Summation of all that work on the formal logic of archaeological research was undertaken by Gardin. Oriented to that goal is his book whose English edition is entitled "Archaeological constructs: an aspect of theoretical archaeology", and the French, more detailed version – "Une archéologie théorique" (Gardin, 1979a; 1979b). Gardin divides the whole process of archaeologist's disciplined thinking into two stages: "compilation" (collecting and ranking of information) and "explication" (interpretative and explanatory operations). Underlying the first stage are the previously devised rules of descriptive archaeology, the second stage, a mirror reflection of the first, imparts to the whole construction elegance and simplicity, dangerous simplicity though.

The book will, undoubtedly, stimulate elaboration of this important theme. However, subordinating its subject matter to the principle of symmetry, the author invariably comes to excessive simplification in historical interpretation of archaeological material. Indeed, one can reveal, not without benefit, some logical schemes invariant for different lines of archaeologist's thinking, but that would be a study in certain logical operations in science generally, rather than in specific archaeological reasoning. Gardin came to investigating archaeological research from general work on formalization of research reasoning (in philology, history, etc.) and on informatics, and one cannot but feel it when reading his book. The specificity of archaeological material, of archaeological records, is not incorporated in Gardin's conception, nor are the specific features of the past being reconstructed, such as regularities in the early stages of the culture-historical process, ancient culture categories, etc. Without those, however, construction of an archaeological theory is hardly practicable. Thus, the French title of the book proves to be inaccurate, which, incidentally, is mentioned in the preface by the author himself.

The book is still an important contribution to theoretical archaeology; it will, undoubtedly, find adherents attracted by the simplicity of the solution. In point of fact, however, we need an algorithm of how to intergrete the archaeological material, and adequate and substantiated elabor-

ation of the algorithm is still ahead, not to mention systematization of archaeology as a whole.

Marxism and socio-economic problems

The revolutionary outbursts of the "stormy sixties" and the demoralization of the US establishment in the seventies gave rise, in the atmosphere of detente, to a distinctly rising interest in Marxism in the USA, the leading state of the capitalist world. Noting this feature in the themes of the American symposia in the early seventies, I wrote of the "Childeization" of Western archaeology (Klejn, 1977, pp. 21–22). "Childeization" is going on and spreading out.

In 1976 an imposing manual, J. Smith's "Foundations of archaeology" (over 560 pp. in 8°), was published in California, based on the principles of Childe's Marxism, or else cultural materialism (Smith, 1976), and prefaced with sympathy by Robert MacNeish, a leading figure in traditional US archaeology, the author of the manual being his disciple. The book more than once notes the importance of the works of Marx, Engels and Morgan for understanding the development of society and culture and for the construction of a general theory of archaeology. Smith's book is in use as a manual in a number of American universities.

In a letter of Aug.4, 1980, Smith wrote me that in the late 1960s and the 1970s he expected, having received his Ph.D., to appear as the only Marxist archaeologist in North America. However, to his surprize he found that a some other half dozen graduated the same year at Harvard and other Northeast Universities. Starting in 1975, the Marxist journal "Dialectical anthropology" is coming out in New York, it carries articles on theoretical archaeology as well. The journal "Soviet anthropology and archaeology" that was a New York publication for several decades specializing in translations of Soviet papers, divided in 1980 into two magazines (to expand the space for publications), with the Marxist archaeologist Ph. Kohl at the head of "Soviet archaeology".

A symposium on "Marxist approaches to archaeological research" was held in Mexico City in 1975 and another one on "Prehistory in the U.S.S.R: theory, method, problems" was to be held in San Diego, California, in 1981.

The development of the British ecological school in archaeology (or the Crawford-Fox – G. Clark trend) took an unexpected turn. Rising and acquiring much influence and many fol-

lowers was E. S. Higgs, G. Clark's associate studying non-artifact remains. He proposed to explore within the area of a settlement the production resources accessible from it in antiquity. The term "Site-catchment" to denote the whole complex of such resources was introduced by the geographer Vita Finzi (1978). According to Higgs, such resources and the technology of their utilization predetermined unambiguously the whole life of the inhabitants: their social structure, ideology, etc. Assimilating in his own peculiar way and simplifying Marxist concepts, Higgs adheres to rigid economic determinism. In the last years of his life D.L. Clarke, too, fell under Higg's influence (cf. Clarke, 1979, pp. 199–201).

These events could have been assessed as accidental episodes brought about by temporary factors, had they not manifested themselves against the background of the generally quickened interest of Western archaeologists in such theoretical subjects as archaeological studies in economics (Sabloff and Lamberg-Karlovsky, 1975; Pastoral production, 1976; Earle and Ericson, 1977; Welinder, 1977; Cohen, 1978) and reconstruction of social structures (Lorenzen-Schmidt, 1975; Redman et al., 1978; Green et al., 1978) in connection with demographic and ecological evidence (Zubrow, 1975; Evans, 1978; Welinder, 1979). A collection of articles in honour of Grahame Clark, the author of "Prehistoric Europe: the economic basis" is entitled "Problems in economic and social archaeology" (de Sieveking et al., 1976). The term "social archaeology" had originated earlier but gained in popularity in the seventies (Renfrew, 1973; Gjessing, 1975), the term "economic archaeology" appearing in the seventies. Along with them came into being the terms "archaeology of government" (Trigger, 1974) and "archaeology of trade" (Kohl, 1975).

All this stream of studies indicates that Marxism, sometimes openly, sometimes implicitly, constitutes one of the fundamentals of theoretical archaeology as it is presently developing in the West, true, solely in its division oriented to culture-historical process, to the objects of reconstruction. As for its division concerned with archaeology proper, with archaeological cognition of the past, with the research process, i.e. as regards metarchaeology, Marxism is not resorted to in theoretical investigations in the West, and such potentialities and prospects of Marxist scientific methodology are considered there only in papers sent in from socialist countries.

Conclusion: change of paradigms

Generally speaking, the ways and means of cognition remain the least developed province of theoretical archaeology. The failure of the neopositivist aspirations of the "New Archaeology" in that field is most tangible, while the loss of credit by the simple schemes of hypothesis verification is most perceptible. The turn to computer-based simulation of the natural human brain, involving its characteristic indeterminacy component, disappointed those who had believed in the near reign of the computer-embodied "analytical machine", cleverer than human intelligence, and in the need to diligently liken the scientist's natural intelligence to it when carrying out research. The subject of the limits and potentialities of archaeological cognition (Ghoneim-Graf, 1978) acquired a new stimulus.

The disappointment of the simplifiers, their despair in the face of the arising complexities were superimposed on the traditional agnosticism of sceptically-minded archaeologists, on their denial of laws and regularities in culture. In that atmosphere, with the motley pattern of philosophico-methodological convictions and the instability of the natural-science picture of the world in the minds of many Western intellectuals, there appear works (professional, at times) attempting at irrational cognition of the past: by means of telepathy, etc. – "psychic archaeology" (Goodman, 1977; Jones, 1978). As regards the range of tasks and the respectability of presentation, they leave far behind the former pastime with search-fork. Those works, full of scientific phraseology and methodological guarantees, may be looked upon as the extreme and grotesque expression of the intuitivistic tendency. However, they are not an extreme in theoretical archaeology but its alternative. And in theoretical archaeology this is a symptom (an unhealthy symptom) of a sound realization of its intricacies.

As recently as the first half of the twentieth century the world of the past under investigation seemed quite simple. The adepts of the "New Archaeology" have already realized its complexity. "Processes and systems, – they asserted, – should no longer be considered simple until proven otherwise". And they replaced that motto with another: "Systems explanations are complex until proven otherwise" (Watson et al., 1971, p.69). But until recently the conclusion drawn from that statement was along U.R. Eshbi's lines: "Whenever systems become complex, their theory consists, practically, in finding ways

of their simplification" (Eschbi, 1964, p.78). Thus, tracing changes in a few very particular parameters (number of dwellings, number of some types of tools, etc.) and concluding from them transformations of a culture, Plog realized that his approach may be considered as "over-simplification". But he waved aside the reproach right away: "For this simplification I do not apologize" (Plog, 1974, p.162).

Now a different way out, a different task, is presenting itself to the researchers: the construction of a theory complex enough to be adequate to the complexity of the object.

The mid and late 1970s in the West was a period when the formation of theoretical archaeology as a special branch was taking place. Characterized in this survey are the principal directions and peculiarities of this process. Indeed, what has found its way into the survey does not exhaust the multifarious events in theoretical archaeology around these few principal subjects, even if one confines himself to books, without turning to the stream of articles.

A profound analysis of the conventional methods of relative chronology was proposed by the Swedish scholar B. Gräslund (Gräslund, 1974). H. Jankuhn's collection of articles on the principles of archaeological study of settlements rate (Jankuhn, 1976) continues the tradition of Wahle and the Hamburg "Archaeologia Geographica" of H. Eggers. A collection of articles entitled "Stone tools as cultural markers" (Wright, 1977) came out in Australia, the subjects covered include typology, ethnography and many other fields, F. Bordes and L.R. Binford being among the contributors. In P.J. Ucko's new collection (Ucko, 1978) comparison is made of the forms of Australian primeval art and those of prehistoric Europe. The late Norwegian scientist G. Gjessing in his book (Gjessing, 1977), unfortunately, his last book, advocates humanitarian understanding of primeval archaeology. Will he have young successors or will that position remain a feature of the "departing generation"?

Thus, there is a lot of interesting material in the collections of articles, motley as regards subject matter, by friends and disciples of Irving B. Rouse (Dunnell and Hall, 1978), Epigoni of taxonomism and adherents of "settlement archaeology", among the authors are J.B. Griffin, A.C. Spaulding, R.C. Dunnell, Chang Kuang-chi, B.G. Trigger, and others. Taking up "sociology of archaeology", D.W. Schwartz considers the interaction between the principal role types among men of knowledge viz., Searcher,

Genius, Advocate, Systematizer, Contributor, Eclectic and Teacher (no Telepathist, thank goodness). Schwartz is, possibly, right maintaining that the appearance of a Genius is determined not so much by the scope of individual gift as by the conditions of the epoch. Each period has its specific demands for man of knowledge of a definite type, specific distribution of roles by their importance, and specific appointment of the leading role.

What stage are we at now? Has the hour struck for the Geniuses? Or has the time come for the Advocates? Or else nowadays the Eclectic, Contributor and Systematizer are called upon to be the leading figures? The innovator Geniuses seem to have uttered their word. Have just done so. A change in paradigms has taken place. The neopositivistic paradigm of simplification has given way to the paradigm of realizing complexities, close to structuralism yet not coinciding with it. Whereas the trend to simplification enquired development of theory, the realization of complexities called for the creation of a special branch – theoretical archaeology. And, indeed, what it needs is systematization.

R.C. Dunnell, the author of "Systematics in prehistory" (Dunnell, 1971) in his contribution to the collection of essays in honour of Rouse, set himself the goal of formulating principles of archaeological reconstruction and interpretation. This is a long-standing aim of theoretical archaeology. J.B. Griffin in his time proposed 12 principles (Griffin, 1956). C.F.C. Hawkes thought that 2 would suffice (Hawkes, 1957). Dunnell confers this status to 7 rules, which is, probably, insufficient. Presumably, they are in reality more than 12. The important point, however, is this: Dunnell presents those principles as a system of axioms, theorems and corollaries, and that is an important step in systematization and formation of discipline.

However, if we ask what underlies the axioms, we shall find that Dunnell provides no distinct answer. He hopes that in a wiser context (e.g., of archaeological discipline as a whole) they will, all the same, prove to be theorems (Dunnell and Hall, 1978, p.43, footnote). The possibility and necessity of philosophical, ideological substantiation for a Branch of knowledge does not occur to the American Systematizer. And one is beginning to feel that Schwartz's list, reflecting in a way the realities of Western archaeology, lacks an extremely important figure, that of a Thinker.

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