WHAT WAS ARCHAEOLOGY DURING THE 1980s AND EARLY 1990s

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Abstract: An investigation on the workings of archaeology in Romania a few decades ago shows features of a culture-historical archaeology, with some particular traits, in part generated by the political regime of that time, which structure around defending accumulated tacit knowledge and the autonomy of the discipline. The reduced capacity of producing new thinking in the discipline is preserved after 1989, in changed social and political circumstances.

Rezumat: O investigaţie asupra felului în care funcţiona arheologia la noi acum câteva decenii arată caracteristici ale unei arheologii cultural istorice, cu trăsături proprii, în parte generate de regimul politic din acea vreme, care se organizează în jurul apărării unui fond de cunoaștere tacită și a autonomiei disciplinei. Capacitatea redusă de a produce gândire nouă în disciplină s-a păstrat și după 1989, in contextul unor condiţii sociale și politice schimbate.

Writing about how the discipline looked like decades ago is not an easy task for an archaeologist educated in Romania during the 1980s. This is something we were taught to believe we should not do. The public past of the discipline was imagined as inferior to its present and its scrutiny on anniversary occasions, when some sterile genealogy of the present could be performed, or some new one could be invented to please the powerful of the day, worthless. What I knew then and what I remember now are products of positions unsuitable for an all-encompassing view. I will not sketch a balanced, overall picture of archaeology in Romania. I have organized my recollections with the purpose of giving shape to what was believed at that time, in the office from the Macca House in which I started being an archaeologist in 1986, by being educated in a local variant of pre- and proto-historical archaeology, to be a coherent cluster of good practices, amounting to an implicit view of what archaeology was and of what it should be. I have recognized similar ideas in other places of archaeological research from Romania, during conversations with archaeologists from other offices and other institutes and museums, during symposia and annual meetings. I will not present other views, and there will be no attempt to reduce the diversity to the lowest common denominator or to show how bad archaeology looked like.

To the history student I was in the 1970s, archaeology presented itself in many ways: primarily as history of ancient art, then as access to ancient peoples through their cultures and chronologies, and as discovery during excavation campaigns. At that time I had no intention of becoming an archaeologist. This was partly due to a mystique of personal giftedness shared by those of my colleagues who had started their archaeological education by being close to an experienced archaeologist. Their knowledge was not a development of what we learned as students, it was something radically different, something no one was able to put into words other than the esoteric names they used for archaeological facts and those of the extraordinary tales about their masters. All that I learned from my professors in the history department was irrelevant compared to this knowledge.

The tales of my former colleagues from the history department and what my prestigious colleagues at the institute said had one thing in common: an archaeologist was someone entirely dedicated to the discipline. Nothing beyond the benefit of getting a particular kind of knowledge was expected from it. It was not a means for obtaining something else.

I had the privilege of being accepted as a researcher at the Bucharest institute of archaeology without the ideally long apprenticeship, which seldom brought the apprentice to such an important position as was then that of a researcher at the most important institute of archaeology in the country. This, together with my respect for knowledge coming from books, made some of my experienced colleagues very sceptical about my ever becoming a true archaeologist. I worked mainly with two masters, but my apprenticeship started late (I was 30), I do not know if too late to assimilate practical knowledge or because the eventual archaeologist

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in me should have manifested much earlier. As an apprentice one was disciplined into the discipline, slowly incorporating the normative ideals contained in the practices and learning to act in ways which made him or her immediately recognizable by other archaeologists and predictable. The relations between the masters and the apprentices were variable and evolving. From domination to friendship, usually taking the shape of a friendly domination in which unwelcome behaviours of apprentices were sometimes tempered by verbal violence,[1] these relations were long lasting, reducing the researchers of the condition of apprentices until the "youngsters" were in their fifties. Success at being an archaeologist, or failure to become one, were the only imagined outcomes. There was no choice of what kind of archaeologist one might have wanted want to be. Constant work was encouraged, speedy publication was not. "Stealing the trade" was the game, with its main drawback: major assumptions remained undiscussed and the possibility of developing better thinking belonged to the realm of the unimaginable.[2]

Most archaeologists lived through their commitment to an archaeological site, seldom to more, which had to be fully researched and published. Such a task was usually expected to take decades. Only the ambition of the researcher was encouraging productivity. The customary level of financing did not allow a more sustained pace and its structure supported excavation and little else, thus continuing a local "Anhäufungspolitik".[3] Most sites were subjected to "systematic research", put under the authority of one experienced archaeologist who, relying on his own social capital and improvising abilities, was expected to develop a camp made of lodging and storage facilities. Very little support was offered by the institute. For my last excavation campaign of the 1980s, the administrator gave me only one "engineer's notebook", which, together with the millimetric graph paper I had to procure on my own, were the traditional supports for recording an excavation.

The excavations took place during from June to September, usually using 10 to 20 local teenagers as workforce. The archaeologist told them what do, showed them how a particular task was to be accomplished, and supervised them. He or she took care of recording of the excavation and of labelling and storing the finds. The rest of the year was spent working on them.

The most prestigious research theme, following a prototype developed in Germany and brought to us especially by Romanian Humboldt fellows, was the monographic treatment of a cemetery or of a settlement, composed of a massive descriptive part, with as many finds and features as possible carefully described and illustrated, and of an analytical one, in which each category of finds was split in types for which comprehensive analogies were provided, and with short conclusions, mainly on the chronological and ethno-cultural frames. Parts of this monographic dream could be presented as independent contributions on an artefact or feature category.

There was a important amount of generosity in our research tradition, stemming from the belief that what we did will be used by archaeologists from the near and from the distant future. An archaeologist had to know all the relevant information about the artefacts and features of his or her specialization and, a few believed, to make it public either in scientific journals or books, or in other forms, e.g., in card data bases. Scientific afterlife was to be attained by offering archaeological data, rather than by interpretation. However, some archaeologists, using their privileged access to the finds in order to get recognition with less trouble than that involved in publication and database making allowed other archaeologists, especially foreign, to use them only if they had something immediate to gain (from simple congratulations to scholarships and all expenses paid invitations to symposia).

Because each archaeologist was expected to focus on a particular epoch and even on a particular site, there was little scope for a discussion beyond what was recognized as the "specialty" of each of them. Typological and chronological problems being the main topics of any archaeological debate, eventually

1 Not at all uncommon in academic environments from Romania in the 1980s and still to be encountered in the 2010s, verbal violence was perhaps a way of asserting one's dominant position in the absence of its grounding in local explicit norms by which it could be decided whose arguments were better.

2 This was, again, an outcome of the emphasis on incorporated knowledge, which contained norms inaccessible to critique. See Bauman 2000, p. 208: "To create (and so also to discover) always means breaking a rule; following a rule is mere routine".

3 On this notion, see Marchand 1996, p. 331.

accompanied by details on contexts and discovery circumstances, the participation of archaeologists with other specializations was meaningless. For the acceptance of an interpretation, the credibility of the archaeologist was more important than a critical discussion, which was further made improbable by the master-apprentice system and by the reliance on tacit criteria of evaluation. Some archaeologists were even reluctant to participate in scientific exchanges confined to a single specialization: one of my colleagues was told, when asking for the permission to see some artefacts, "I will not show them to you because you will steal my ideas", thus expressing a central belief in our research tradition: all archaeologically useful knowledge comes from our finds.

The discipline was made of things one had to know without being taught and knowledge was imagined to be the privilege of those with inborn capacities for it.[4] Those capacities were never defined, never discussed, only alluded to and recognized. They were varied, from manly physical strength and endurance to the ability to see, in the excavation trench or in the publications, what good archaeologists saw, allowing them, e.g., to place an artefact in its right typological place just by looking at it (usually at its drawing), without having to make explicit the criteria. The discipline, whose soul was called by some "the method", was a sacred legacy from our masters, which we had the duty to preserve and, eventually, to pass on to people with the appropriate qualities, not to anyone fancying himself an archaeologist, during many years of intellectual and practical contact. I quickly learned that a true archaeologist was defined by specific knowledge and also by professed ignorance about what no archaeologist should know. No one was expected to bring any kind of knowledge from outside the discipline, although such knowledge was eventually appreciated during beer talks. Outside the master-apprentice relations, we were all autodidacts. There was no faculty of archaeology in Romania, only a few introductory courses taught in the history departments, which were mostly historical narratives based on archaeological research which provided very little access to how the research was done. The current situation is somewhat better, we have more courses, but a systematic archaeological education at university level is still missing.

Knowledge was disciplinary, with some prescribed openings towards other disciplines, to which the capacity of offering useful information was recognized. No alternative thinking was to be borrowed from them. Progress was extensive: the best interpretation was that which took into account the greatest number of finds according to the principles of culture historical archaeology. Therefore more excavations and more publications were supposed to be the main way towards more knowledge. Better knowledge was mainly imagined as compliance to methodological requirements which functioned as universal truths. With some effort, many of them could be found in the writings of German archaeologists.

Theoretical knowledge, understood, e.g., as an examination of important concepts whose meaning was usually taken for granted, although not always beyond debate, was absent and rejected as idle thinking, unless transformed into a usable methodology, i.e. one compatible with what archaeologists were accustomed to do on grounds they did not care to examine. Methodological improvements were accepted, provided that they offered tools for achieving culture-historical goals: chronologies and analyses of spatial distributions, leading to a more or less precise identification of cultural, i.e. ethnic groups, which provided the connection with the historical narrative whose main actors they were imagined to be. The best archaeologists were those who knew about the circumstances of discovery of a great number of artefacts and features relevant to their specialization in the investigation of a certain epoch and about similar finds from all over Europe. Knowledge about artefacts was mostly about morphological attributes, less about decorative techniques and patterns and even less about technologies, all used to place the artefacts in typological categories employed for chronological and cultural distinctions. When prestigious typologies, mostly to be found in archaeological literature from

[4] By no means an isolated way of thinking at that time in the Romanian academic environment, but perhaps more salient in archaeology than in other disciplines. Hard to tell what this had to do with what Vasile Pârvan, one of the founders of the local scientific archaeology, thought about education. See Pârvan 1920, p. 13, where the university is reduced to the task of de-animalizing mediocre minds and of procuring technical means for geniuses, the good professor being presented a gold and diamond prospector in the desert of human unwisdom. The method was that of cultivating and selecting superior souls, by testing each individual with "the touchstone of the Cult of the Idea". The rejected were to be forced back to the amorphous human heap to which they belonged, to serve as "pavement stones for building the new road to the upper spheres" (pp. 20-21).

abroad, were not available, the lack of theoretical grounding and even of methodological ability prevented local archaeologists from constructing new ones able to gain recognition from their colleagues, hence a proliferation of local typologies used only by their authors.

Occasionally, value to some ideas which could be imagined to function as archaeological theory was recognized. Such ideas could come only from great professors, dead or alive and well in German universities. Contesting them was unthinkable because that meant replacing them with something better, better meaning something coming from someone with a higher authority, not the outcome of a collective critical examination. To a few old and experienced local archaeologists expressing some general thoughts on archaeology was recognized as a privilege one should exercise with grace, i.e. without giving much importance to them. In more than one way thinking above methodological procedures and their application was inappropriate for an archaeologist. One calls for a core-periphery approach (theory for the West, method for the rest). Another one opposed masculine hard work and direct action to feminine lack of will and inability to accomplish anything palpable, associated with a propensity for beating around the bush, disguised as scientific activity. The rejection of theory had also something to do with the defence of the autonomy of the local archaeological research tradition, made of undisputable, incorporated truths, against principles of interpretation promoted by agents of the official Communist ideology. Knowledge from the social sciences was rejected for similar reasons: in Romania they were, to an important extent, legitimating political domination, and therefore associated, sometimes event conflated, with repugnant political ideology.[5]

Local archaeology was archaeology. Some information and some curiosity about other ways of doing archaeology existed, we even had a few books and journals in the library of our institute, many of them stemming from the library of Ion Nestor, the local grand master of pre-and protohistorical archaeology, but nobody recognized in them serious challenges to our ways. Some exotic ideas made their way into archaeological mainstream thinking, without having any impact. One spectacular example is that of the famous definition of culture promoted in the early sixties by Lewis Binford,[6] which was present in the archaeological teaching in the History department of the Bucharest University, its incompatibility with the local understanding of culture being unrecognized. Our way of doing archaeology was not one among others: it was the only way of doing archaeology confronted with marginal deviations promoted mainly by people from the United States and the United Kingdom who, despite their claims, were not true archaeologists, and to which only inexperienced or dangerously misguided people could pay attention. In 1991, at the end of a last discussion with the director of the institute before leaving Romania for a Fulbright scholarship at the University of Arizona, he wished me a good time during my absence from the institute and told me not to bring back home anything related to how archaeology was practiced in the United States. During my stay in Tucson I audited a statistics seminar taught by Barbara Mills, who recommended Tuckey's book on exploratory statistics,[7] which she tried to order for us at the campus bookshop, only to discover that it was out of print. The precious book was therefore to be found only in the Main Library, one copy put on reserve, so that we all could have better access to it. A few weeks after my return to Bucharest I have discovered a copy of this book, in very good condition, at my institute, in a bucket temporarily demoted to the function of a garbage bin. Apparently the book was part of a donation by the local American Embassy, which had taken the unfortunate decision to dismantle its public library. I do not know how the donors came to the idea that a statistics book could be more interesting for an institute of archaeology than for one of statistics or economics. In our system though, it had nothing to do with archaeology. This is not because statistical procedures were unknown to Romanian archaeologists. Indeed, some of them used seriation and cluster analysis, not only simple descriptive statistics. However, they were used only as providers of images of

5 In a public occasion, a local conference held in mid 1990s, a very experienced archaeologist remarked, to the benefit of the few archaeologists who were inclined to look outside the discipline, that anthropology was created by the French Communists during the 1950s.
6 Binford 1962, p. 218.
7 Tuckey 1977.

organized data, authoritative because they were "scientific" images already used by important archaeologists. Such procedures were not to be taken from statistical books, but from other archaeologists or, occasionally, from scientists who were not archaeologists. This is just an instance of a still active conviction that even if archaeologists might cooperate with other scientists, they should not venture into understanding what other disciplines were doing, something imagined as impossible for an archaeologist, and should limit their interest to the results useful for archaeology, i.e., understandable only with what archaeologists usually know.

Nothing coming from the outside of the discipline was allowed to have a say in deciding what archaeology was and who was a good archaeologist. This was happening in a totalitarian dictatorship, in a discipline used for nationalist propaganda. There were archaeologists with good connections with people in power, but usually whatever they got as rewards for their cooperation was used for archaeology. Some of them were good professionals, most of the time ready to recognize when and where they have made "compromises" and anxious to separate them from scientific work. Others were mediocre or worse, and were a constant source of ridicule. In either case, the support from the outside did not impose any of them as a leading scholar.

There was one particular circumstance in which the locally recognized quality of the researcher and the recognition from the authorities outside the discipline were entangled: that of the participation to foreign conferences and symposia which invited, all expenses paid, archaeologists from Romania. The contribution of the Romanian authorities was to allow the archaeologists to exit the country. Archaeologists were concerned to keep aside was what due to personal scientific achievement and what was due to the favours of the political establishment. An exemplary tale is that of Gheorghe Diaconu, who was offered by the local authorities the opportunity to go to such a conference in the place of an invited colleague whose participation they did not approve. He refused to go, telling his friends and colleagues that he could not have done that because the hosts might have asked him: "Who are you? Maria Comşa?"

As culture-historical archaeologists, members of research tradition associated with nationalism since its beginnings, more than a century ago,[8] which rested on the assumption that cultural groups are ethnic groups, we were all methodological nationalists. However, an important distinction should be made – and was made during the 1980s and the early 1990s – between those archaeologists who were using what seemed the only viable methodology at that time and those archaeologists who were willing to sacrifice any methodological constraints in order to bring to the nation-state whatever justification it needed. Without it, we would not be able to tell who were true scientists and who were those willing to subordinate knowledge to current political imperatives. The distinction is easy to make in the archaeological interpretations used in the most important research task, that of documenting the origins and early history of the Romanians.[9] Culture-historical archaeology naturalized the nation as a primary form of human association and was indeed a suitable framework for legitimating nationalist claims of territorial antiquity, but it imposed constraints on the interpretation and evidential requirements which had to be satisfied if one, as an archaeologist, was to recognize our ancestors in the archaeological evidence. However misguided, the archaeologists following the idea that a clearly bounded group of artefact and feature types indicated a people were acting within a living scientific paradigm, one which did allow failure, and indeed, in the search for such a group associated with the local Romance speaking population during Late Antiquity, had to admit it, even while devising more or less ingenious ways to circumvent the problem. Still, by doing so they escaped the fate of nationalists, as described by Z. Bauman: the scholarly search for truths known by everyone before the beginning of the inquiry.[10] Other archaeologists ignored the constraints of the admittedly nationalist archaeological paradigm only to proclaim that one type or another was enough to recognize the evasive ancestors and keep silent about the evidence to the contrary, thus abandoning their professional obligation to take into account all the finds which, as stated above, was the main trait qualifying someone as a good archaeologist.

8 On the early phase of the development of culture-historical archaeology, see Trigger 1989, pp. 148-206.
9 See Popa 1991, for what he viewed as the ungrateful task of eliminating the dross from Romanian research.
10 Bauman 1992, p. 685.

Summing up salient features of archaeological thought and practice I have encountered during the 1980s and the early 1990s, some of which devalued what I knew then, I am surprised to recognize in them many of the characteristics of the scientific worlds described by the sociologists of knowledge: specialization and professional blindness (Max Weber), organized scepticism (Robert Merton) and the interest of being disinterested (Pierre Bourdieu). Even the master-apprentice system, with all its drawbacks, is appropriate for performing the task of educating by exemplars, as described by Thomas Kuhn.[11]

Many things are now better: instruments, technologies, thematic variety and so on. Most importantly, we can write whatever we think, assuming only risks also assumed by our Western colleagues. But things are not going as they should. There is a loss of that autonomy the best archaeologists of the 1980s were trying to preserve and strengthen, combined with the survival of a propensity towards making things fit and of one towards acting as we should without knowing why. Archaeology in Romania was, to a certain extent, a periphery which resembled the position of long-term student, which, however subaltern, was still one of a member of the scientific community. We are still a periphery, without any peripheral wisdom, understood as a capacity to compare and evaluate centres,[12] but these are less interested now in educating peripheries. We are alone responsible, individually, of our choice of a peripheral position, of which direction to embrace and of what centre to recognize in a world in which archaeological knowledge quickly becomes a periphery of other ways of thinking. The long-term apprenticeship is replaced with the short-term building of knowledge claims, which are proliferating with such speed that none can keep up with them. Actually no one should. We are in a competition arbitrated not by our sceptical masters but by enthusiastic bureaucrats from the outside, some of which believe to know more about current science than the researchers. What can be a progress, e.g. the opening to new ways of doing archaeology, is not necessarily one. The opening is frequently not critical. It is just a part of complying with what seems better knowledge because it seems promoted by archaeological traditions more prestigious than ours, perpetuating, among others, the old belief that any product of exact and natural sciences is better than anything we can offer as archaeologists. It could be objected that this is what the archaeologists from the 1980s were doing, but their long apprenticeships offered the advantage of bringing them inside research traditions which offered them analytical tools, they were not just borrowing statements, theoretical *membra disiecta* to which archaeological data are added as illustrations.[13]

While appearing to be doing something completely different, we are doing the same thing, as it happens in many aspects of our long transition from a dominated past to an liquid future: we are complying (compliance with Western ways of doing science is repeatedly advocated by the people doing our research policy),[14] we are still developing our sensitivity to what the power might want, although is it no longer clear where the power is.

The extensive development of archaeological knowledge is still going on, at a rate higher than that of the 1980s, due to rescue excavations, but the engine generating new ways of interpreting the archaeological record is still missing. This engine is the confrontation of different interpretations, between researchers whose knowledge can be appreciated only by their competitors, in which conservation and subversion strategies become manifest.[15] What was not something one could argue is now debatable, but critical discussions are few and they use arguments mostly borrowed for their perceived authority. The immortal Gambetta appears once again on the stage. The vacuous proliferation of foreign authorities makes it more difficult to examine their work. The reluctance to assert the responsibility of choosing an interpretation frame, which was then a sign of being an

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[11] Weber 1922, pp. 530-531; Bourdieu 1975, p. 26, *cf.* Merton 1973, pp. 275-277; Merton 1973, pp. 277-278; Kuhn 1996, p. 186.

[12] For this notion, see Fernandez 2000.

[13] However, it must be noted that many such legitimate imports were not free from misunderstandings and were not kept up to date with what happened in the environment which had produced them after the import was made.

[14] E.g., our National Council of Scientific Research, wants "to implement in the socio-humanistic sciences from our country the current way of the international thinking in this field, a mechanism that is not new and which has numerous examples of success in the history of Romanian science and culture" (retrieved from http://www.cncs-uefiscdi.ro/viziune on March 12, 2014, my translation).

[15] Bourdieu 1975, pp. 23, 30-31.

archaeologist in the local tradition, accompanies now the desire to belong to a better one. The old research questions die out without being explicitly dismissed. The new ones are frequently imported with their answers. Now, as then, our creative capacities are employed in applying ways of thinking over which we seem to have no control. We can choose what to apply, but, with institutional research priorities still missing, the consequences of such choices are individual, they are not creating a local tradition of research with better chances than the old one to be a part of a peer-to-peer network of international archaeological research, in a world in which our discipline, split among prestigious scientific practices, mostly consequences of a will to adapt, to obtain recognition and appropriate funding from outside authorities, loses its capacity to stick to its own problems and confront the outsiders with knowledge about the past of humanity which is not what they expect.

Bibliographical abbreviations:

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Bauman 2000	Z. Bauman. *Liquid modernity*, Oxford, 2000.
Binford 1962	L. R. Binford, *Archaeology as anthropology*, American Antiquity 28(2), 1962, pp. 217-225.
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