

RE-READING BRAIDWOOD.....

BRAIDWOOD'U TEKRAR OKUMAK.....

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Nasıl ki insanın teknolojisi, kültürü, ekonomisi ve çevreye uyumu üzerine geliştirdiği son derecede akla yakın projesinin bir yenilik getirmesi gibi, Braidwood'un çeşitli bilim dallarının katılımıyla oluşturdukları araştırma ekipleri de tümüyle bir yeniliktir. Kendisi bütün bu çalışmaları, değişken bölgesel öğeleriyle, çok geniş bir bölgeye uygulamaktaydı; onun yaptığı arazi çalışmaları basit bir veri toplama anlamında değil, varsayımları doğrulamak veya reddetmek için denediği bilimsel bir araçtı. Şimdilerde bizim genel bilgimizi oluşturan birçok fikir daha önceden beri Braidwood'un yapıtlarında ifade edilmişlerdi. Braidwood tarafından yapılan standartlaşma çabalarının bir kısmı, hatta tümü, özgün bölgesel bir sıra düzeni takip ederek, terminolojisi ve kronolojisiyle birlikte, bizim düşünce sistemimize girmiştir.

Çeşitli eleştirilere rağmen, onun fikirleri her çalışmada izlenir ve onun en önemli anlatımları artık çok yaygın olarak kullanılmakta ve anlaşılmakta ise de bazı kavramları günümüzde yeniden gözden geçirilmelidir. Kültürel farklılaşmaya uyum sağlamanın getirdiği ivme değişiklikleri ve tüm kültürel değişimler için muhakkak olması gereken 'kültürel olgunluk' kavramı üzerine Braidwood'un fikirleri bu varsayımın zorunlu adımlarını oluşturur; bu kavramda, kültür içindeki tersdüşmeler ve savaşımalar gitgide artarak değişimi getirmekte, bunda da sosyal ilişkilerin ekonomik ilişkilere olan zıtlığı ege-men bir rol oynamaktadır.

My first encounter with R.J.Braidwood coincided with my initiation to Near Eastern prehistory, at the University of Rome, in the mid sixties. Braidwood's work was perceived by Professor Salvatore M. Puglisi, whose pupil I was, as the substantial archaeological evidence which complemented Childe's general theoretical enquiry: not only did the beginning of agriculture have to be ascribed to a concrete space and time dimension but, more generally, the role of economic systems in the characterisation of social struc-

tures and cultural expressions had to be tested against field data.

I have, ever since those early years, been carrying out field work and teaching in this perspective, referring to outlines laid down more than half a century ago by these scholars. If we consider the amount of field work and methodologies that have been developed in these decades, it is astonishing how Childe's and Braidwood's ideas still need but minor corrections, with new

data and ideas being, more often than not, added to the old ones, lending more substance to the framework, rarely modifying it. R.J. Braidwood emerges not only as the earliest Near Eastern pre-historians, and to this day one of the most eminent and open-minded, but also as one of the founders of modern archaeology, with most of his principles having been fundamentally adopted in a worldwide archaeological perspective: environmental reconstruction has become a customary investigation, with ecology being considered a crucial - though non-deterministic, in Braidwood's view - component in the development of ancient civilisations; the role of culture in this process, first proposed by Braidwood, has been increasingly emphasised; the sequences in the Amuq Plain and at Çayönü are still the reference point for the surrounding regions, though with some corrections and additions, and expressions such as "nuclear zone", "incipient agriculture" and "village farming communities", all coined by Braidwood in the fifties, have entered the modern archaeological language along with all their implications.

Besides these quite commonly shared considerations, I perceive Robert Braidwood as one of the important guides in my life, to whom I owe, as many others of my generation do, a large part of my scientific formation as well as part of my personal approach to archaeology. For various reasons, and in different ways and times, Braidwood has underlain the entire course of my scientific life: from my initial interest in the beginning of agriculture, passing through my research for alternative food production models in Africa, which I had the honour of discussing with him long ago, my becoming involved in the Çayönü project fifteen years ago, my teaching Near Eastern Prehistory, for which my first stock of slides, selected from among those from the Jarmo project, Braidwood sent me personally, and ending with my excavating the Amuq-related site of Mersin-Yumuktepe in the last ten years.

Although I never worked with R.J. Braidwood in

the field, I met him on several occasions. The first was in Rome in 1984, when he was invited by Professor Puglisi to receive an **Honorary Degree** at the University "La Sapienza" and, subsequently, when he stopped off on his way to and back from Turkey: we spent hours discussing the results of my study on the lithic materials from Çayönü in minute detail (!) with Linda, his wife, inseparable companion and collaborator. I also met Linda and Bob in Turkey while working on the Çayönü materials at Istanbul University, and lastly in Chicago, in 1994, during a short stay at the Oriental Institute. I had the pleasure not only of getting to know them, but also of seeing and working where they worked, always in places which very much reflected their personality: the impressive Çayönü site and its field house, the Oriental Institute, and their private house in the marvellous wild park in La Porte. On each of these occasions I re-discovered, besides Robert Braidwood's unquestionable intelligence, his scientific honesty, his great human simplicity, his unlimited interest in life, his inoffensive sense of humour, his capacity to listen to anybody, always with the same degree of interest... What can be done to honour such a person? When I first became involved in the Çayönü project, he sent me copies of most of his articles (his "fossils", as he defined them), with his own handwritten notes and comments which we would discuss together. I am once again going through them and wish to contribute to this volume by humbly attempting to re-read what he wrote, in the light of years of debate among archaeologists and of my personal experience in the same field, with our lively encounters and the sound of his voice during the informal telephone conversations we would have, still clear in my mind.

Despite his considerable age, Braidwood's field work at Çayönü continued until the end of the eighties, well after his retirement, with his lecturing and writing going on even later. His death therefore came almost as a surprise to his collaborators and was perceived to be a huge

loss by many Near Eastern prehistorians.

Braidwood started his field work in the Near East in 1930 when he participated in the first expeditions of the Oriental Institute in the Amuq Plain. He always remembered that time as a formative period not only for himself, as a graduate student, but for oriental archaeology in general: *"It is fascinating.....to have spanned archaeology's transition, in southwestern Asia, from field practice as it was in 1930 to that of today"* (1981). To simply "span this transition" is an understatement which is typical of Braidwood: he belongs to those few scientists raised in the old, traditional school of archaeology who had the chance, and the ability, to construct a totally new school. Gordon Childe's scientific approach and personality had a fundamental influence on the young Braidwood, as he always remembered (1981). He was, however, also inspired by other contemporary scholars, among whom the most important was probably Julian H. Steward. In spite of their differences, or probably because of them, these scholars all complemented each other in laying down the preliminary theoretical frameworks and methodological quests which went on to be developed in the following generations. In particular, Childe's and Braidwood's approaches - though not always in agreement, and at times quite contradictory, as in Braidwood's demonstration of the insufficient role of environmental deterioration in the process which led to food production - constituted different levels of an unquestionable general framework within which both theory and new field results from 1960 onwards were arranged and assumed meaning.

Braidwood's long scientific course is characterised by an increasing integration of theory, methodology and archaeological documents, which led to a progressive detachment from theoretical inspirers towards a personal elaboration based on direct field experience. Several phases can be recognised in this process. The first, which lasted until 1938, consisted of the Amuq

expeditions, which were carried out as a totally new, almost blind research. Before the second World War, archaeology in the Near East considered exclusively historical sites, with field research aimed mainly at the material documentation of events and cities which were quoted in written texts. Prehistory was almost unknown, with the exception of the excavations at Mersin-Yumuktepe, which started in 1936, just a few years after those in the Amuq (Garstang 1953). Cultural sequences, environmental characterisation and artefact definitions had to be established from scratch. C14 dating did not exist. There was nothing to refer to, if we exclude a slightly more advanced terminology and conceptual construction elaborated in those years for the earliest prehistoric cultures of western Europe. The processing of the enormous bulk of data and artefacts from the Amuq, which spanned almost two decades following the interruption of field activities in 1938, was probably the single, most important factor which led Braidwood to confront the problem of establishing theory-based reference charts and type lists. This constituted the second phase in the evolution of his scientific course. The university lectures he gave in the early forties (Braidwood 1945) reflected his aim to create an appropriate standard terminology in prehistory. The first attempt regarded the specific meaning to give not only to artefact types, but also to more comprehensive terms, such as "assemblage", "tradition", "culture", "aggregation", "industry" and "facies".

By the end of the forties, with few new data acquired but after much reflection, Braidwood had traced a comprehensive framework for Near Eastern prehistory and was able to correct the prospect of cultural development proposed by Steward in 1949. In the following decade, the time became ripe to start the first theory-oriented, substantial field expedition in the Near East: the Jarmo project, in the Iraqi Zagros, was in fact the first interdisciplinary prehistoric project on the hilly flanks, designed to test the concepts of both "incipience" and "nuclear zone" for the ori-

gin of agriculture. In 1959-60, the project was extended to the Kermanshah plain, in the Iranian Zagros. It was only at the end of this intensive, well-oriented field activity that the formulation of a model of cultural development, consisting of stages and eras/sub-eras, was accomplished. In the following decades, right up to the end of the eighties, Braidwood's research was aimed at recognising these stages in the field, with the excavation at Çayönü being its culmination.

The constant research for unambiguity in archaeological definitions, whether they be cultural models, chronological schemes or typological terminology, was one of Braidwood's most prominent characteristics, and one which has had the greatest influence on us all. This tendency was already apparent in his earliest work and was never to be abandoned. Braidwood believed that a high level of artefact, culture or site definition could be obtained using a standard tri-nomial system of terminology, with 3 parameters always being considered: time, description and interpretation. Unfortunately, this proposal was advanced at the beginning of his career but was never adopted, nor even widely discussed. Not only are these parameters those which synthetically convey the basic information on the structure of each culture/site, thereby shedding immediate light upon it, but their use in classifications would have created an unequivocal, uncoded standard system of definitions in archaeology which would, in turn, have facilitated our work today. A tri-nomial definition, in addition, not only defines a unique cultural aspect but also indicates how to relate this aspect to general classificatory systems, whether they be chronological, typological, economic, or social. For example, Jericho PPNB could be classified as a "ninth millennium, a-ceramic, effective village-farming" site. The absolute category of time, which might be considered as a traditional classificatory element used to immobilise cultures in a time-related evolution, was, instead, used in the tri-nomial system to "relativise" cultural characteristics and

depict a more varied, though coherent, cultural world: a ninth millennium village farming site, for instance, should be distinguished, even by definition, from similar seventh millennium sites, as belonging to significantly different cultural processes. Time, which is *per se* meaningless, becomes a meaningful parameter once the culture/site is considered as part of a comprehensive phenomenon.

In the same perspective, besides the research for a standard terminology and a standard system of cultural description, Braidwood's attention focused on the traditional classifications of pre-history in "Palaeolithic", "Mesolithic", "Neolithic", etc. Although these definitions are commonly used, we are all fully aware that their meaning, as well as the meaning of their mechanical subdivisions in early, middle and late, and pre-, post-, epi-, etc., are still highly controversial, at times being used as synonyms for periods, times, industries, ways of life, cultures or stages. Braidwood believed that these definitions should be released from the logic of the tri-partition, which has no specific implication in history, and be replaced by an unambiguous system of primary terminology. Childe's transposition from the traditional tri-partition in technological or ethnological eras into solid economic systems, while introducing structural irreversible changes in the scientific approach to pre-historic cultures, had represented but a minor advance in terminology and the description of processes: neither "Neolithic" nor "Food production" were able to account for the variety of cultural, economic and technological possibilities contained in the process of controlling plant and animal reproduction. By contrast, the reference to subsistence/settlement stages, with the details obtained by adding the description and the absolute time, is more likely to be able to place every possible difference within a general framework.

Although most people agreed on this principle, it was difficult not to fall back on the old, comfortable, all-inclusive categories to which we

had all grown so accustomed. Although I had to make a huge effort to use expressions other than "Neolithic" in the first article I wrote on the site of Till Huzur and the earliest food-producing cultures on the south-eastern Anatolian plateau (Caneva 1992), I forced myself to do so as a matter of principle, to produce a sort of manifesto. The site of Till Huzur, which lies less than 4 km from Çayönü and was excavated in 1991, presented a problematic phase of transition between the earliest ceramic phase documented at Çayönü and the earliest Halaf-related cultures. The word Neolithic was never mentioned in the article. In writing it, I discovered that the rules which obliged me to use such a rigorous form of expression resulted in more carefully thought out definitions and a substantial change in the meaning of what I wrote: the attempt to define the cultural aspects uncovered at Till Huzur without referring to the previous standard definitions pushed me to use periphrases which entailed a deeper analysis of what I had to describe, and not only made me adopt a more appropriate language, but, to a certain extent, also enriched my investigation.

In his reconstruction of several possible stages and eras in the long-term process to farming, Braidwood felt inspired by a framework proposed by Julian H. Steward some years earlier, as he repeatedly stated. In "Cultural Causality and Law", Steward had tried to formulate a system of general laws of cultural development without resorting to the traditional evolutionary perspective: "... all cultures, though unique in many aspects, nonetheless share certain traits and patterns with other cultures.... In fact, the analyses of cultural particulars provide the data for any generalisations" (Steward 1949:1-2). The search for cultural regularities in both sequential (diachronic) and functional (synchronic) cause-effect relationships represented, for Steward, the only scientific approach to history, though he explicitly warned against the risk of equating cultural laws to those of the biological or physical sciences. The requirements in the search for cultural regularities were, according to Steward:

1) to create a typology of cultures, by abstracting similarities from peculiarities; 2) to investigate the possible cause-effect inter-relationships of types; 3) to define independent recurrences of similar cause-effect inter-relationships and demonstrate the multiple origins of certain processes. This entire formulation was speculative. Steward in 1949 had hardly anything to include in the chart of eras of early civilizations. The chart consisted of 5 centres, from Meso-America to China, considered as having a parallel, though not synchronous, prehistoric development from the era of Hunting and Gathering through to those of Incipient Agriculture and Formative. Steward's framework basically paralleled Childe's approach in the sequence of "revolutions", as well as in the search for cause-effect relationships between cultural changes. It was, however, closer to archaeological documentation insofar as it was not limited to theoretical assumptions but aimed at the search for general rules of human behaviour in the data. Steward's formulation was so general that it required an immediate, contextual reply by the Braidwoods to correct and refine the sequence with new information on Near Eastern archaeology. It is probably from these comments and from the many re-elaborations needed to refine Steward's formulation that the basis for the new formulation, which was elaborated by Braidwood in the following decade, emerged. It was actually only the idea of a system of "cultural regularities" and the concept of "incipient agriculture" that were inspired by Steward's work. Braidwood's proposal was a totally new, complex, comprehensive classification of prehistoric cultures into types of subsistence strategies and settlement organisation. Although lithic industries were mentioned in the description of cultures quoted as examples of level types, these types were not directly "translated" into tool typologies. The whole history of mankind was divided into two stages, which corresponded to Childe's distinction between food-gathering and food-producing. These stages included internal subdivisions in two eras, with each of these eras subdivided in three sub-eras. The tri-

partite mode re-proposed in this formulation is only apparent and is quite unrelated to the traditional tri-partition of cultural development in chronological phases, such as Palaeo-, Meso- and Neo-, or early, middle and late: Braidwood's eras and sub-eras are intended as ideal types of communities, with no absolute or relative chronological value, and they are by no means considered to be steps which must be present in all sequences. Braidwood's stages constitute the first attempt in the history of archaeological thoughts to define cultural models. The succession of models in any given sequence is neither to be expected nor necessary, the whole formulation being quite independent of the archaeological record. The Near Eastern model itself was later quoted by Braidwood (1962) as showing one possible sequence of levels, Europe and America others, with the complete series of types not being present in any of these sequences (Braidwood 1964). In the Near Eastern model (sometimes mistakenly considered as an alternative formulation when it was actually the application of the theory to a specific geographical zone), a simplified sequence of 8 stages was described and the sequence associated with an analysis of the ecological zones which were presumed to have existed in prehistoric times in the "nuclear zone". This perspective was subsequently elaborated further by Kent Flannery and applied to the peri-Mesopotamian regions, in an accurate analysis of the location of the latest pre-agricultural and the earliest agricultural settlements in relation to the seasonal resource potentiality of the territories (Flannery 1965).

In these formulations, Braidwood referred to phenomena which may have accompanied or favoured the process. One was that men "*settled in*" to their environment (Braidwood and Reed 1957), with an "*increasing tendency for the people to live in particular environmental niches*" (1983). This concept was misinterpreted and criticised as one more evolutionary stereotype of the natural tendency of human groups towards sedentism (Binford 1983). It was, on the con-

trary, intended to account for the cultural regionalisation which, at the end of the Pleistocene, led to very local cultural adaptations which accentuated differences between disparate areas, particularly between the Near East and Europe. There were several ways of settling into a number of different natural environments, and consequently multiple and at least semi-independent responses to post-Pleistocene environmental changes. It was at that moment that the nuclear zone became important as a selective area, not before. In this respect, Braidwood's idea was an anticipation of what Flannery developed later in his "Broad Spectrum Revolution".

The same instance for the research of general laws of human behaviour characterised the New Archaeology movement in the seventies. Resorting to physical and natural sciences, in which the problem of both a terminological standardization and a correct formulation of the epistemological process was considered to have been more precociously and rigorously approached, rendered the risk of a simple transposition to ancient history a common problem for the "New Archaeologists". The greater variability of human behaviour if compared with the natural world was the main problem, a problem which was never solved. For instance, the search launched by Higgs and Jarman for "*which form of economy was the most successful in particular environments*" (Higgs and Jarman 1969), is per se an assumption of natural universality: on the one hand, this statement envisages only one possible solution in human adaptation to the environment, on the other, it attributes to archaic societies the tendency to maximise the work-profit relationship, which is typical of the capitalistic society, with no attempt being made to understand whether other parameters, such as a different concept of time or work, or of social relations, existed in those societies.

One fundamental innovation in Braidwood's stages was the attention given to the increasing

number of culturally- and socially-determined subsistence and settlement strategies. His apparently simplistic explanation for why the whole process had not started before, given that there had been similar environmental conditions in previous geological eras, was the famous, and greatly criticised sentence: "*culture was not ready to achieve it*" (Braidwood and Willey 1962:342). Binford argued that this "vitalistic", anthropocentric explanation could not be accepted (1972:430), unless it was considered within an adaptational perspective, and therefore as a prelude to the search for an external cause which had re-oriented human adaptation towards animal and plant domestication. For Braidwood it is the accumulation of previous experiences which gradually changes the interaction between the ensemble of social and cultural characteristics and the local environment, whereas for Binford this is only determined by broadly defined environmental changes, including demographic pressures. Binford's criticism is, in fact, based on his definition of culture as "... *all those means whose forms are not under direct genetic control which serve to adjust individuals and groups within their ecological communities*" (1972:431). Only one of the two interacting variables, i.e. culture or environment, can, in Binford's opinion, change, the other adapts. There is, however, a contradiction in Binford's position. When he speaks of mobility as a security option, against sedentism, among hunter-gatherers, even in very different and over and over modified environments such as modern Australia, Botswana and Mexico (Binford 1983:204), he considers culture as being largely independent of the environmental variables; Flannery did so to an even greater extent when he denied his previous view of demography as constituting strong pressure for the beginning of agriculture, and made an explicit reference to culture, which is unrelated to the environment, as an imponderable component of the process (1973).

The criticisms made by Binford, though apparently fair, are but poor interpretations of

Braidwood's statements: graduality is not intended by Braidwood as a gradual increase in intellectual capacity in the evolutionary sense, but as a slow increase in social complexity, which is much more similar to the view expressed by Flannery in 1973. To insert culture among the variables leading to the beginning of food production means relativising this phenomenon, which is a very modern perspective: changes are assumed within contexts, mechanistic equations are eliminated and research in other sectors is favoured, though without eliminating that on the economy and environment: "...*human beings adapt not to nature, but to nature as perceived.*" (Hodder 1987:52). In the study on the origin of agriculture, this approach is being increasingly supported by new findings documenting several development centres, at least in the Levant (Syria) and in south-eastern Anatolia, which display very different formal characteristics but share a common economic substance, as well as a similar ideological realm: the central role of death, artistic expression used as self-representation of the community, the standardisation of models in the use of space.

In the history of an archaeologist, the data that grow old most quickly are field data, as they are either replaced by new ones, or become outdated as a result of new analyses. Braidwood's field data, however, were collected and published in such a way that, unlike most of the data from early excavations, they can still be used and re-analysed. This is apparent in the exploration on the Zagros flanks, in the survey in the Diyarbakir plain, and in the excavation at Çayönü and in the Amuq Plain. The attention paid to every sector of the archaeological work, whether it be theoretical elaboration, lithic classification, budget planning or artefact drawing – or even the care he took of the field tools – seems to have been another of Braidwood's characteristics. The lithic collection from the Zagros in the basement of the Oriental Institute in Chicago, which I had the honour of seeing, is an example of how artefacts should be stored to

permit further studies.

The research for field evidence of the theoretically formulated stages and eras did not yield all the results which had been expected and hoped for. It did, however, provide an enormous, systematic data base, which is much more significant than a simple chronological grid, into which sporadic findings could be placed and be given a meaning. The research, conducted above all by Braidwood, though carried on also by other expeditions in both Iraq and Iran, worked within this system, before any specific cultural sequences were developed. An endless variability of cultural contexts appeared to enrich the process, with accelerations, gaps and slowdowns in the various areas. In a summary of the archaeological data available in 1986, Robert and Linda Braidwood assembled all the pieces of the mosaic without ever contradicting their previous, blind projections, except for gaps in the sequences for the various areas. While, for instance, the cultural evidence of both the Levant and the Zagros hilly flanks fit into the scheme, no level of incipience was discovered in the eastern Taurus piedmont. The Joint Prehistoric Project of the Universities of Chicago and Istanbul, with the Braidwoods and Çambel leading a huge interdisciplinary team, undertook an extensive surface survey in the Diyarbakir plain, though no results were achieved in this respect. Research further east, in the region of Mardin, was forbidden, and was thus to remain a dream of promise for Braidwood. That this was the area where the level of incipience on the northern side of the Fertile Crescent was to be found, and that Braidwood's intuition was yet again correct, have, in fact, been confirmed by the recent findings at the site of Hallan Cemi (Rosenberg 1992).

Çayönü was the last field work the Braidwoods engaged in. Although their reputation is largely linked to this prestigious site, Çayönü was probably the only time Braidwood's intuition was incorrect, it being an unpredictable finding. The site revealed the first evidence of what now appears to be the main characteristics of the ear-

liest full farming communities in south-eastern Anatolia: first of all, long-lasting settlements containing buildings which were architecturally highly standardised in size, shape and orientation; secondly, the symbolic role of the buildings, both domestic and "public", as well as of the manipulation of the dead and the monumental representation of the human figure. Çayönü, a site which is famous throughout the world and represents to this day a unique point of reference for the region, with its long lasting occupation, its rich, prestigious artefact inventory and its extensive exposure, was undoubtedly a wonderful, though somewhat unexpected discovery for Braidwood, one that had not been envisaged in his reconstruction. Despite this discovery, his dreams continued to be drawn to the forbidden areas further east, where he expected to find the missing links in the process of the emergence of farming villages.

The belief that independent centres of development for this process existed was based on purely theoretical grounds, well before any archaeological evidence had emerged. In recent decades, the validity of this hypothesis has been demonstrated by the discovery of growing numbers of Anatolian autoctonous cultural developments.

Once Çayönü was discovered, it was impossible to interrupt the excavation of such an important site. Çayönü represented the only case in which Braidwood's investigations were concentrated on a single site for so many years. His natural tendency was to expand his research to an entire territory, so as to gain a thorough understanding of the organisation of space in ancient times. When I joined the Çayönü project, in 1990, I tried to apply the same perspective to the region and decided, in agreement with the new director, M. Özdoğan, to repeat the survey the Braidwoods and Çambel had conducted in the area in 1963 (Çambel and Braidwood 1980; Caneva et al. 1992). It is astonishing that, 30 years later, I obtained the same results: no evidence of what Braidwood was so intensively

looking for, i.e. the level of incipience in the development of food production, presumably associated with microlithic industries. Such industries were, instead, subsequently found, as Braidwood expected, further east, namely in the site of Hallan Cemi, on the Batman river.

As for the Amuq, though few campaigns were carried out in the area, all very early on, and despite the non-prehistoric aims and research strategies used in these campaigns, the reconstructed cultural sequence is still referred to as the most complete and accurate to date. Following a recent re-examination of the earliest pottery from phases A and B, during which the whole stratigraphy and pottery typology were checked against new data, only minor modifications were made (in press Balossi).

Conclusions

There is sometimes an apparent naïveté in Braidwood's formulations, probably due to both his mediatory ideas and the pioneering nature of his work. It was not only Braidwood's organisation of prehistoric expeditions that was absolutely new, but also his investigation into the development of wide regions, with their variety of ecological features, within the framework of a comprehensive project on human technology, culture, economy and environmental adaptation. He never appeared to tire of the role of pioneer: he left Iran "in good hands" when archaeological research had been set up there, before moving on to south-eastern Turkey, which was then still unknown from a prehistoric point of view.

Braidwood's times were those in which little distinction was made between theory and field archaeology. The main emphasis was on methods, rather than on true archaeological theory. Braidwood's developmental stages represent a kind of a middle range theory, designed to conciliate high level generalities with the material archaeological documentation. With great coherency, his field research was always used as

an experimental scientific tool to either verify or reject hypotheses, never as a simple means for data collecting.

Most of his ideas were intuitively expressed. Some were subsequently re-elaborated, often in a more thorough and refined way, by others. Many of the ideas which now constitute our general knowledge, however, were already expressed in the conclusion of "Courses toward Urban Life" (Braidwood and Willey 1962): the formulation of stages based on economic/cultural/technological elements and associated with ecological zones, the beginning of incipience as early as the end of the last Glacial era, the principle of multiple emergences as a result of independent adaptations (several ways of "settling into" a number of different natural environments), and, finally, the introduction of the cultural component among the factors of change and the idea that changes are basically dependent on internal evolutions. As he was the first to radically change the approach to archaeology, he was criticised more than others in the subsequent decades. Every movement of archaeological thought is, however, directly descendant from the preceding one. Most of the new ideas that came later were generated by Braidwood's ideas, particularly by his formulation of rigorous methods to test hypotheses. Some of his concepts are even now being reconsidered. For instance, Braidwood's idea of variations in the rhythms of adaptation due to cultural differences and the concept of "cultural maturity" as a prerequisite for any cultural changes are mandatory steps in the emphasis which is now increasingly given to internal contradiction and conflict leading to change, with the dominant role of social as opposed to economic relations (Bender 1978).

Part of the standardisation effort made by Braidwood has entered our way of thinking, part is no longer necessary, following the development of specific local sequences, with their terminology and chronology. Most people have continued to use terms such as Epipalaeolithic

and Neolithic, though more carefully; others have stopped. The idea of stages has, at any rate, penetrated every work, and the most significant expressions used by Braidwood are now widely used, or, if not used, at least commonly understood.

There was never aggressiveness in Braidwood's criticisms towards his colleagues; on the contrary, they were constantly mentioned with gratitude and esteem, with explicit references to the possible contribution of other people, even unconsciously, to his ideas, frequently being found in his articles. This honesty was not always reciprocated.

Robert Braidwood was never alone. His most

worthy collaborator was Linda. Whenever we read Braidwood, we should actually read Braidwoods, as he himself almost always wrote as a demonstration of their indistinguishably shared ideas and practice. He loved working and collaborating with other people: his article "Archaeological retrospect" (Braidwood 1981) ends with an enthusiastic exhortation to work in joint efforts. He was, in that case, explicitly referring to his best friends and collaborators Halet Çambel and Bruce Howe, with whom he shared so many and such successful archaeological ventures, but he was also thinking of all possible collaborators, as his open and generous confidence in young people, which many of us have directly experienced, constantly proved.

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