

MRT Confidential

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Middle Range Theory is one of the most discussed phenomena in the archaeological theoretical debate. It is a phenomenon that often has to stand in the shadow of its symbol status in the theoretical debate. It is one of the dearest tools of processual archaeology and at the same time perhaps the dearest target of post-processual criticism. This article discusses MRT, how it is defined and how it is used in the debate between processualists and post-processualists.

Archaeology was radically changed in the 1960's. A group of young archaeologists found the current archaeological climate far too limited and sought change. With Lewis Binford in command, a new archaeology was created. The old school (called cultural archaeology throughout) was criticized for being too inexact. The new archaeology wanted answers about the prehistoric times and their theories were preferably statistically proven by tables and diagrams. Backed by the ¹⁴C dating method, this new archaeological school ruled from the late sixties to the early eighties.

The “New Archaeology” was also to be called processual archaeology, because it sought to divide theory into two parts in which one had on the one hand, a general part which contained processes in the social system and, on the other hand one spoke of formation processes (Sjögren 1999:128) in which MRT was an important part. The applicability of MRT within a processual archaeology and its ability to create “answers” through systematic analogies made it embraceable to processual archaeologists. MRT was meant to be the Rosetta Stone of a processual archaeology.

The rejected definition

The form of Middle Range Theory that became the most prominent and was mostly discussed in archaeology was the one that was introduced by Lewis Binford. The term Middle Range Theory seems to have arisen from two places independently of each other. On the one hand we have the already mentioned Binford version and, on the other, we find Mark Raab and Albert Goodyear. They found the term MRT in another discipline: sociology. The sociologist Robert K. Merton had since the 1940’s spoken of a Middle Range Theory (Merton 1957:328) that was meant to provide a logical link between relatively low-level, empiristic generalisations and comparatively high-level theories (Raab & Goodyear 1984:257). The purpose was to neutralize the high level of abstraction in the big theories which can be seen as a scientific strategy instead of an actual theory-building (Sjögren 1999:128). In spite of the critique of Merton, Raab and Goodyear claim that his MRT has had a fundamental influence on social theory (Raab & Goodyear 1984:258).

Raab and Goodyear formulated their own thoughts on MRT in archaeology in 1973 in an article entitled *On the value of Middle Range Theory in archaeological research strategies*. This article was sent to *American Antiquity* but was rejected and was never published, although Michael Schiffer refers to it as widespread (Schiffer 1988:462). Their idea of MRT is illustrated nicely by their scheme (Figure 1).

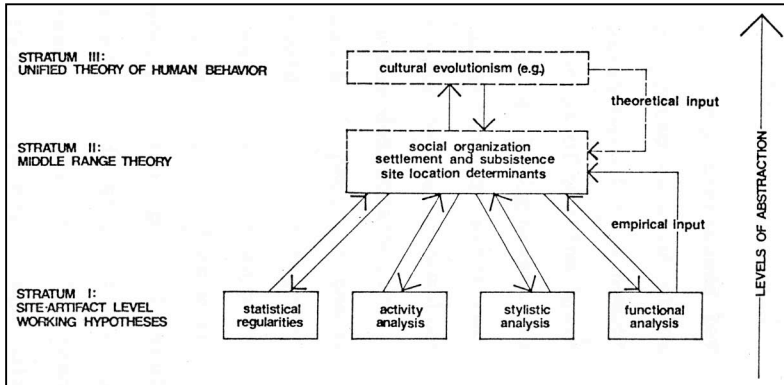


Figure 1. Raab and Goodyear's idea of MRT (Raab and Goodyear 1973:6).

Raab and Goodyear wanted to introduce MRT into archaeology the in true Merton spirit, in order to neutralize the high level of abstraction in the high-level theories and make them relevant to the empiristic, low-level studies. The main point of their paper was that they thought ...

...that we are more likely to make our most productive gains by constructing and testing theories that operate in the middle range of social science inquiry. What this means is the selection or construction of theories that are readily operationalized into their empirical consequences /.../ This suggests that the task before us is to develop hypotheses or special theories that can be empirically investigated, using limited but significant conceptual ranges (Raab & Goodyear 1973:7-8).

Their ideas on MRT were eventually published in *American Antiquity* in 1984. The article provides the reader with a good résumé of Merton's ideas but also discusses problems in Binford's version of MRT, to which I intend to return.

Creation of Binford's monster

Lewis Binford admits to confusion about the introduction of MRT as a term in the archaeological literature. According to Binford himself, he gave a seminar in 1972-3 at the University in New Mexico that was locally known among the students as the "Middle Range Theory seminar". It was during this period he that developed his well-known Nunamiut material, which was also very important for his MRT. Binford's purpose was to get the students to accept the challenge to theory-building. It was obvious to Binford that the strategies that were appropriate for the development and testing of archaeological theory were not applicable to the development and testing of general theory. This elucidatory contrast made the concept of "archaeological theory" seem far too ambiguous, which led to his adoption of the term "Middle Range Theory" or "Middle Range Research".

In the spring of 1974, three of Binford's students who had participated in the MRT seminar handed in an article to *The Society of American Archaeology*. The title of this article was *Middle Range Theory in Archaeology*, in which they discussed the need to draw more attention to linking arguments that could give meaning to the archaeological record (Binford 1983b:18-19).

It is said that Binford took Raab and Goodyear's term MRT and gave it a meaning of his own (Sjögren 1999:128). Binford firmly resents this and states that MRT must have originated from two different places within archaeology. He has no conscious ties with the sociologist Merton and states that his version of MRT has nothing to do with different levels of abstraction but more to do with tactics on theory-generating and examinations in which one uses different kinds of data. In *For Theory Building in Archaeology*, he formulates his purpose with MRT for the first time in print. In this publication from 1977, Binford finds that the challenge of the time was to theory-building and at that time there was neither prosperity nor success to speak of in this field, although many had acknowledged the challenge and accepted it. He thought that there was an urgent need for theory-building on at least two levels, one of which was MRT. This requires that one accepts that

observations made on the archaeological record are contemporary facts and that these facts are static. He thinks that

...clearly basic problems for the archaeologist include (a) how we get from contemporary facts to statements about the past, and (b) how we convert the observationally static facts of the archaeological record to statements about dynamics (Binford 1977:6).

He asks himself rhetorically

What meaning may we justifiably give to contemporary static facts regarding past dynamics? What conditions of dynamics, not available for observation, produce the forms and structures observable as static patterning in the archaeological record? (ibid.).

These questions of Binford are hard but suit his line of argument. To approach these problems, he saw the need to develop ideas and theories, in this case MRT, regarding the formation processes in the archaeological record. Binford draws parallels with theories from geology, which he thinks was centred on assumed uniform factors, i.e. that the same dynamic processes in the past are still operative. Without the development of a theory-building that took into account the relationship between the static and the dynamic, Binford could not see any real progress forthcoming. So, this was the challenge that Binford assumed when he decided to develop a MRT. He considered

... it middle range because I believe that we seek to make statements about the past in order to evaluate ideas we may hold about the conditions that brought about change and modification in the organization of dynamics occurring in past living systems.

When looking at the processes responsible for change and organization, Binford thinks that we should seek the development

of general theory. Binford puts great emphasis on the statement that the development of both general and MR theory should proceed hand in hand. The scientific method can only be applicable in such a relationship and, without a mutual development, the development of MRT may prove to be just needless. The general theory is needed as a criterion of relevance. Advances in MRT separated from the general theory could, in Binford's view, be a complete waste of time (Binford 1977:7).

In this publication of his, a series of articles follows that discusses various themes and examples used for theory-building. Binford develops his idea of MRT together with J.B. Bertram in a chapter on bone frequencies. He thinks that the anatomy of animals is such a well-known subject. It should therefore be possible for us to study the frequency of different animal bones and how they were used, transported or abandoned by prehistoric people as a direct measure of their economic and logistical sophistication and appropriately variable behaviour in different settings and milieus (Binford 1977:7). There is a very interesting chapter in which he also discusses the "N-transforms" that were introduced by Michael Schiffer. I shall examine these in more detail later on.

In his book *Bones: Ancient Man and Modern Myths* (1981) Binford discusses his idea of MRT further. He also uses the term "middle-range research", which should be seen as MRT in its practical execution. Binford thinks that the theories that explain the archaeological material have to be intellectually independent of our prior conceptions of prehistory or our prior theories concerning the processes responsible for past actions, changing patterns or stability. He also thinks that MRT has to be intellectually independent of general theory, even though he has already claimed that their development should proceed hand in hand. He continues:

Middle Range Theory must be tested primarily with documented living systems. Middle Range Theory treats the relationship between statics and dynamics, between behavior

and material derivatives. General Theory may be tested using archaeological phenomena meaningfully operationalized through middle-range research (Binford 1981:29).

He closes the chapter by claiming:

The conclusion should be clear: Middle-range research, with particular emphasis on theory building, is crucial to the further development of archaeology. We cannot ‘know’ the past without it, and we cannot evaluate our ideas about the past and why it was the way it appears to have been without means of monitoring the conditions or variables believed to be important. Both of these tasks are dependent upon the development of middle-range research (Binford 1981:30).

In the 1983 publication *Pursuit of the Past*, Binford writes that his meaning was to study the relation between the static and the dynamic in a modern environment. If one understands this relation in detail, it will provide a kind of Rosetta Stone: a way to “translate” the static, material, stone tools found at an archaeological site to the expressive life of the people who once left the tools behind (Binford 1983a, 24).

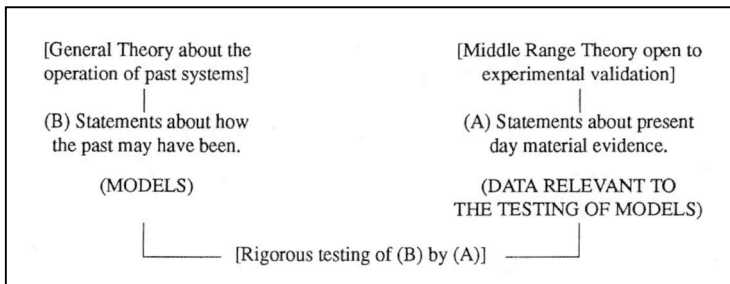


Figure 2. Binford's idea of MR research (Barrett 1990:33).

MRT alive and kicking

The question that the archaeologist is asking himself or herself is: "How do we get the present objects, the artefacts, to tell us about the past?" The processual archaeologists criticized the cultural archaeologists for too clearly lacking theoretical reflection against the archaeological record. There was surely an awareness of theoretical and methodological approaches, but these were just as surely implicit and unspoken. When Binford propagated a development in archaeological theory and launched his own ideas, this was a way of making theory visible and making it clear and explicit.

When the modern archaeologist makes assumptions about the past, this is called an analogy. An analogy is how we use the information derived from one context, the present, to explain data found in another context, the past (Johnson 1999:48). This means that one assumes that things in the past were like the present. How easy and simple this may sound! An analogy is not something ugly, but doubtless the method that has brought the most archaeological knowledge into this world. Analogies are used by all archaeologists, independently of their theoretical camp. The more links that are found, the more one could argue that the two situations are analogous and thereby scientifically valid. An obvious assumption that definitely could do with critical examination. MRT, as it was to be used in archaeology, sought to put various analogies into systems. This principally took place in ethno-archaeology, where Binford himself was one of the major operators but also in other so-called sub-disciplines, such as zoo-archaeology, archaeobotany and experimental archaeology.

MRT as propagated, by Raab and Goodyear, had little influence on practical archaeology and the MRT discussed is that formulated by Binford.

Binford in action

Lewis Binford's desire to test his theories and produce MR information took him to Alaska to study the Nunamiut Eskimos.

The Nunamiut were a people who pursued deer-hunting in an environment similar to that in southern France during the Mousterian period. Binford, long intrigued by the Mousterian collections, thought that by studying the Nunamiuts he could see what kind of hunter-gatherer activities had left an archaeological record. After a couple of articles and seminars, this resulted in the book entitled *Nunamiut Archaeology* (1978). He writes in the introduction:

“My interest is in the past but my observations are on the present. To pursue my interest I must accomplish two separate kinds of acts: (a) I must project my contemporary observations accurately into the past and (b) I must assign meaning to my observations” (Binford 1978:1).

His work in Alaska dealt with the faunal remains that were the result of Nunamiut activities. Binford thought that many of the animal species found in the archaeological record are permanent and that the processes of exploitation and use that were operative in the past are still operative today. The study of faunal factors had, according to Binford, another advantage when studying “utility”. The Meat Utility Index (MUI) or Food Utility Index (FUI) refer to the anatomical parts of an animal that are thought of as useful and are used for food. Binford wanted to study the modern representatives of these animal species and their butchery in a similar environment to gain a greater understanding of faunal bone findings in archaeological contexts (Binford 1978:12).

The book “*Nunamiut archaeology*” provides the reader with an exhaustive picture of this Eskimo economy of hunting, food and butchery. The book is full of charts and tables, the text is often heavy and hard to understand and Binford expresses his views in a far too complicated fashion. The discussion about the “residential sites” contra “kill sites” is one of the profitable parts of his studies. Here he is pointing out the nature of the bone findings and whether the place of excavation was a place of habitation or functioned solely as a butchering station before transport (Binford

1987:482-485). This discussion of his was very influential and is recognized in the presumed hunting stations at Star Carr and Ringkloster (Rowley-Conwy 1998:87-98).

Critique of MRT

The critique of Binford's MRT came from different directions. Voices were raised among processualists that questioned its validity and whether it served its purpose. When post-processualism was introduced in the 1980's, MRT was attacked as a part of the general critique of the "New Archaeology".

Raab and Goodyear get their chance

Although Mark Raab and Al Goodyear may have been and perhaps were the first to use the term MRT in archaeology, their idea has not really been appreciated in the theoretical debate and is rather mentioned as a matter of curiosity. One cannot help wondering what would have happened if *American Antiquity* had not refused their 1973 article. Would Binford have chosen another term for his research?

Despite the archaeologists' professional status in speculation, we shall never know this. As mentioned above, Raab and Goodyear did not get published in *American Antiquity* on the matter of MRT until 1984 when MRT, thanks to Binford was already a well-established institution. From the article "Middle Range Theory in Archaeology: A critical review of origins and applications" I shall examine the critique that Raab and Goodyear directed against the archaeological use of MRT. Faithful to Merton's original idea of MRT, they see no advantages but dangers in calling, for instance, for "principles of site formation processes" (Raab & Goodyear 1984:258:262). They find that this is restricting the term MRT to methodological issues. In their opinion, archaeologists need both an expanded and a more organized view of theory-building. They write:

A narrow focus on methodology will do little to encourage such development if archaeologists become convinced that ‘middle-range theory,’ as presently construed, constitutes an adequate approach to theory-building in its own right. This problem is particularly acute if principles of site formation processes are held to be synonymous with ‘middle-range theory’ (Raab & Goodyear 1984:262).

They find that the appearance of MRT in archaeology has been made obscure by the problems of theory in general, in which they notice confusion over the concepts of comprehensiveness and generality in the development of theories (ibid.).

Despite their critique, they claim that one should not raise unnecessary pessimism about the possibilities of MRT. They actually think of Binford’s Nunamiut research as a good example of an alternative model of the behaviour of hunter-gatherer societies (Raab & Goodyear 1984:263), even though they feel that his perception of MRT is too narrow. Raab and Goodyear want to emphasize that MRT ought to develop in studies of social phenomena, although it is characterized by complexity and a high level of abstraction.

Schiffer’s opinion on MRT

Despite the fact that many notice similarities between his research and Binford’s MRT, Michael Schiffer has never been an adherent of either Binford’s or Raab and Goodyear’s versions of MRT. MRT is simply a term that he does not use and why should he when he has his own view and his own terms to develop and defend? He has, however, discussed MRT and used the term in *American Antiquity* in 1988 an article entitled “The Structure of Archaeological Theory”.

Here he accounts for the problems and possibilities of both Binford’s and Raab and Goodyear’s ideas of MRT (see Figure 3). He questions Binford’s misuse of sociological terms but asserts in the same breath that a literad transference of Merton’s MRT to archaeology is impossible (Schiffer 1988:462). Even if a single

hierarchy of logically related principles can be contemplated in sociology, Schiffer thinks that the principles of archaeology are too diverse for them to be forced into a single hierarchy. He uses wood-rotting fungi as an example, as it is relevant for interpreting certain radiocarbon dates. This is not subsumed by high-level, social theories of change but by theories from biology.

He claims that archaeology, unlike sociology, is the most interdisciplinary discipline, as it incorporates varied theories from almost every natural and social science.

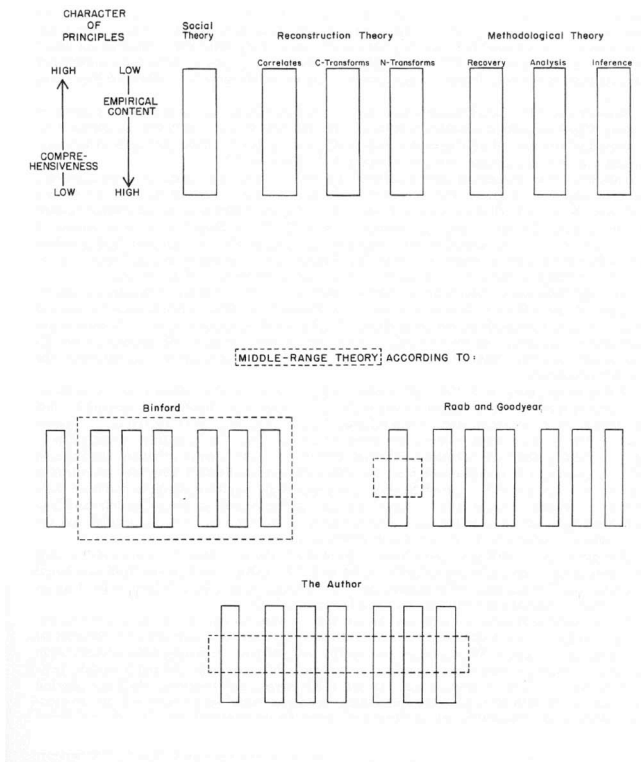


Figure 3. Schiffer's idea of MRT (Schiffer 1988:456).

Schiffer points out that Raab and Goodyear (in their 1984 article discussed above) admits that high-level, social theories cannot be subsumed into all archaeological principles. Their solution to this dilemma is to claim that only principles capable of explaining cultural behaviour are real theory and everything else, i.e. Binford's MRT, is methodology, an assertion that Schiffer finds remarkable. He also reacts against Raab and Goodyear accusing Binford of having a narrow conception of MRT, when they themselves are propagating a restrictive and unacceptable view of theory that they achieve by confusing the function and structure of theory. Schiffer often states, as here, that any theory can function as a method, depending on context.

Schiffer rather wants to advance his own version of Merton's MRT in which he assumes that each hierarchy of archaeological principles contains high-, middle- and low-level theories (see Figure 3). In conformity with formulation, Merton's principles within each hierarchy can be related logically. He writes:

A level of theory (high, middle or low), then, denotes a particular degree of abstraction within one hierarchy of related principles or an analogous degree of abstraction that crosscuts different hierarchies. Thus, we may regard evolutionary theory as high level and the theory of pedestrian tactic survey as middle or low without implying that the former subsumes the latter. Because the study of archaeological theory is still in its infancy, the level of to which a given theory belongs and its relations to other theories may not be easy to determine (Schiffer 1988:463).

Grayson has heard it all before

There were also critics of MRT who found the whole phenomenon by no means new but held that MR research had been long an ongoing strategy. One of these was Donald K. Grayson, who set down his thoughts on this matter in the book entitled *American Archaeology: Past and Future* (1986) in a chapter

called "Eoliths, Archaeological Ambiguity and the Generation of 'Middle-Range' Research". It is principally and almost solely the research of Binford and the notion that this has not been represented before that is the subject of his criticism. Grayson sees that this research emphasizes the study of preserved systems in which both processes and the results of these processes can be observed and where we seek to isolate classified types whose recognition in the archaeological record can inform us of the kind of process that formed this record.

Grayson thinks, however, that MR research and in particular research that is directed towards the diagnostic signatures within formation processes are indeed a part of "traditional archaeology". His view is that this kind of archaeological work is routinely produced by situations in which archaeologists admit to confusing ambiguity in the examples that are presented to them by archaeological data (Grayson 1986:77).

To illustrate his arguments, Grayson uses a situation in which he finds interpretative ambiguity built in from the beginning. This situation deals with the archaeological quest for the earliest inhabitants of Europe after it had been determined in 1859 that human beings had lived together with Pleistocene mammals. His aim in this presentation is primarily to point out how distinguished ambiguity led prehistorians to conduct actualistic research into a MR environment. The goal, he stresses, is not to present a thorough history and analysis of the debate on eoliths (Pleistocene stones) and related phenomena (Grayson 1986:78). That is a pity, because his remarks on the latter are just as interesting if not more so.

Post-processual critique of MRT

The aim of post-processual archaeologists was to lead archaeology as a discipline away from positivism in order to adopt hermeneutic and contextual approaches. Their fundamental disagreement with processualists made it impossible for them to accept MRT. Hodder reads the past without the letters MRT.

Ian Hodder was himself one of the most radical, processual archaeologists in the 1970's. In the mid 1980's, he performed a volte-face and started to propagandize for a different archaeology with a greater emphasis on social context. He became a prominent figure in what was to be called post-processual archaeology. In a chapter of his book *Reading the Past* (1986), he deals with an ethnohistorical example that he uses as the basis of his critique of both MRT and ethno-archaeology. Hodder agrees that MRT is suitable in its relation to physical processes (for instance, ^{14}C) but he finds it hard to see how there can ever be universal laws of cultural processes that are independent of high-level, cultural theories. Although he does not approve of the term, Hodder finds that MRT could be functional, as research is needed on material culture, deposition processes etc. What Hodder resents, however, is the assertion the measuring device termed MRT and advocated by Binford can exist independently of cultural context. He argues against a "materialist" or "archaeological" ethno-archaeology and feels that the emphasis has moved from the "outside" to the "inside" of events. In order to understand material culture in its context of meaning, one must maintain a long and continuous participation in the cultures studied (Hodder 1986:103).

He claims that one's aim can still be the same, asking archaeological questions of ethnographic data. These questions may concern material culture and/or the processes and structures of change, even though he stresses that the methods must be considerably different. Hodder finds that a problem emerges from this situation. He questions the difference between a participatory ethno-archaeology, on the one hand, and ethnography and social anthropology, on the other. Further scepticism is expressed when he wonders whether social anthropologists who are trained in the techniques of interview, data-collecting, sampling and learning languages and also trained to relate to wider literature really are better and more competent in this field. Hodder questions the existence of ethno-archaeology and wonders why it should not be

replaced or integrated with the anthropology of material culture and social change.

Hodder thinks that ethnography can be somewhat helpful to archaeology, although he feels that ethnography has a greater need for archaeology than the other way round. He holds it likely that the archaeological past has a great relevance to the ethnographic present and in this way ethno-archaeology might revive but with a completely different meaning. When the ethnographers realize that they need history in order to explain the present, they have to turn to archaeology to obtain a past in the areas where long-term, written, historical sources are not available. Hodder finds that ethno-archaeology will then evolve into a synonym for ethnohistory, both by definition and in practice. Ethno-archaeology should then, according to Hodder's line of argument, be more closely linked to anthropological and historical theory and method, and thus the debate about ethno-archaeology is just an example of the general debate about archaeology.

Hodder then develops his discussion with an ethnographic example from Baringo in Kenya. The Ilchamus tribe is the only tribe in this area who decorate their calabashes. Hodder raises the question: "Why?"

He claims that, if he were to use general theory or law-like generalizations, he would reach three different conclusions. First, it could mean with a comparison the neighbouring tribes that the decorations relate to a greater social complexity that governs the need to display a better organization. Secondly, one could also assume that the decoration relates to a greater social competition and stress that imply a need to mark access to resources. Thirdly, one could argue that, as the social group increases and a wider interaction between peoples occurs, style and material symbolism increase as well.

Hodder thinks that, in order to find out which of these theories is the most feasible one does not even need to turn to the Ilchamus tribe, as one can just look at the ethnographical data. This, in Hodder's view, is totally wrong, as one does not consider the

active roles of the individuals, their history, meaning or culture. The social complexity of the Ilchamus has been set aside and Hodder calls this bad science, as it involves what he terms “intellectual colonialism” in which the tribe in question has our Western concepts and values imposed on it in an attempt to explain their culture in our terms.

Instead Hodder tries to immerse the contextual information and asks himself what these calabashes mean. For the next couple of pages, he describes his studies on the cultural and social reasons for decorated calabashes, all very convincing and detailed. Hodder uses this example in order to point out the importance of social and conceptual context in the production of material culture to get a more complex picture, instead of taking the easy road by just using cross-cultural laws.

This means to Hodder that MRT fails because he finds himself proving that there can be no universal, cultural relations between the static and the dynamic, because the historically contextual structuring intervenes. He continues:

Thus the notion that Middle Range Theory is distinctive because it involves independent theory which can be used to test other theories is false. The cultural processes which form the archaeological record are not independent of our general understanding of culture and society. It can also be claimed that Middle Range Theory is distinctive because it falls between general, global theory and data: for example, discussion of the symbolic and cognitive dimensions of site formation processes might be termed middle range (Hodder 1986:116).

He finds that MRT might be MR when applied to data, but as he thinks that all theory has both a general and an applied form, he feels that the term is redundant.

Reconstructors afoot, Shanks and Tilley

Michael Shanks and Christopher Tilley's book *Reconstructing Archaeology: Theory and Practice* is, just like Ian Hodder's *Reading the Past*, a major publication for post-processual archaeologists. The core of this 1987 publication, like the core of the archaeological debate at the time, was concerned with whether archaeology was a part of natural or social science. Shanks and Tilley repudiate the positivistic view of knowledge and claim that positivism within archaeology is dead. Positivism, to them, is an archaeology that says "No" in capital letters (Johnson 1999:46). They consider it a tragedy that most archaeologists feel bound to continue with this kind of completely dishonest tradition of research in one form or another. They continue: "...if positivism was actually taken to its logical extreme we would have to deny the possibility of any knowledge of the past beyond pure subjectivism (Shanks and Tilley 1992:44)."

Their sarcastically styled criticism also has a go at MRT, which they indeed think of as being a part of the reprehensible positivism. They criticise Lewis Binford and think that he is contradicting himself when, in the already mentioned *For Theory Building in Archaeology* (1977), both on the introduction of MRT and the theme of "New Archaeology", he writes that, in the absence of usable theory, there is no "new archaeology", only an anti-traditional archaeology at best. They also put forward their critique of Binford's solution of the problem when he builds his own theory. Shanks and Tilley think that these MRT's are being built from the bottom up. They arrive at empirical facts, which are subsequently employed to invalidate the work of others. It is pointed out that MRT has its followers and to Shanks and Tilley it seems that MRT is developing rapidly towards the status of a new panacea for archaeological diseases. As followers of MRT, they mention Gordon Willey and Jeremy Sabloff and, rather incorrectly, Raab and Goodyear, who indeed are followers of MRT, though not in the Binfordian sense already discussed. Shanks and Tilley, like many others, find it unclear what really is "middle" in MRT.

MRT is to Shanks and Tilley not much more than MR empiricism (Shanks & Tilley 1992:44) and its design is strictly positivistic. In their exposition of their fundamental opposition to positivist knowledge, they claim that MRT is nothing but a form-controlled subjectivity (Shanks and Tilley 1992:40). This leads them to dismiss MRT as practically redundant. According to them, the concept of MRT is just a new fancy icing on the old, empiricist cake (Shanks & Tilley 1992:44), a view that the Norwegian archaeologist Bjørnar Olsen seems to concur with (Olsen 1997:99). When the post-processual critique had sunk in, a number of articles were published as answers to this critique, as well as articles that dealt with the future of MRT. I shall now discuss three such articles.

MRT bridging the gap between processualism and post-processualism

Hartmut Tschauer is in the Department of Anthropology at Harvard University, USA, where he took his doctoral degree in anthropological archaeology in 2001. He has also lectured at the Catholic University in Lima, Peru. In 1996, he had an article published in the *Journal of Archaeological Method and Theory* with the title "Middle Range Theory, Behavioral Archaeology, and Postempiricist Philosophy of Science in Archaeology". In this article, he examines MRT in processual and post-processual archaeology. He thinks that post-processual research is actually based on MR principles that the researches really find utterly reprehensible. Tschauer claims that MRT serves as a bridge over the epistemological gap between processual and post-processual approaches (Tschauer 1996:1).

The article consists of an analysis of three case studies as examples of post-processual practice and its relationship to MRT. The examples used are Braithwaite's study of ritual and prestige in prehistoric Wessex, Hodder's interpretation of the European Neolithic and Hill's contrastive archaeology in southern Britain.

Tschauner's first example is Braithwaite's study of ritual and prestige in prehistoric Wessex, published in 1984. In Wessex, individual burials and henge monuments are fairly contemporary, though their main periods of construction do not overlap. Braithwaite suggests that the henge monuments were communal-ritual sites erected and used by very large groups of people from the surrounding areas. The Beaker complex represents an individualist cult/ritual and an attempt to establish an alternative discourse with a totally different system of prestige, first expressed in terms taken from the traditional discourse that is associated with henge rituals. There was a gradual shift from the system of prestige defined by genealogy to one based on material symbols. Tschauner asks himself how Braithwaite can draw these far-fetched conclusions. He finds that her entire edifice is in fact built on generalizations that have the form of both general and statistical "laws" (Tschauner 1996:10).

Braithwaite's "laws" and operational definitions contain, in Tschauner's view, etic observational categories. He continues: "Accordingly, Braithwaite argues for the acceptability or relevance of a particular generalization to a particular case by linking the theoretical concepts of a 'law' to the archaeological evidence as perceived within the framework of her approach. Many of her 'interpretations' thus take the form of covering-law explanations" (Tschauner 1996:11).

Tschauner finds that, apart from these "laws", Braithwaite's arguments also take the form of simple, logical deductions and others are based on covert, generalizing assumptions. Tschauner gives an example from the early Beaker complex, in which some male burials have certain, exclusive, grave goods in relation to what is found in both female and male graves. Because of this, men, to some extent, had more prestige or status than women (Tschauner 1996:11). Tschauner thinks that the underlying generalization is evidently the association between the number or value of grave goods and the amount of prestige that a person had during his or her lifetime (Tschauner 1996:12). In Tschauner's view, Braithwaite's form of argument is essentially linked to

processualist, MRT-based procedures and is in glaring contrast to post-processual rhetoric.

According to Tschauner, she bases her generalizations on, typical ethnographic analysis and it is analogical reasoning that forms the very backbone of her reconstruction. He concludes: "What distinguishes her work from most of processual archaeology is her interest in ideological, superstructural phenomenon, only covertly involved in some processual explanations" (Tschauner 1996:12). Ian Hodder is the subject of Tschauner's next example. Hodder prefers an empirical, contextual approach to the development of archaeological methods. A contextual analysis primarily seeks to interpret the evidence in its internal relation, instead of using outside knowledge on externally derived concepts of rationality (Hodder 1990:20-21). Tschauner points out that Hodder's critique of MRT forms the essence of this approach. The source of his critique of Hodder is *The Domestication of Europe* (1990), in which Hodder attempts a long-term, contextual analysis of the European Neolithic. As Hodder himself calls *The Domestication of Europe* one of the few substantive, post-processual interpretations of the past, Tschauner uses this book to examine of the contextual method that Hodder represents. In this book, Hodder suggests that the domestication of animals and plants was a part of a much greater, domestication process of the wild and became plausible within existing, yet changing, cultural principles.

Tschauner finds the definition of the contextual method, as the main thesis of the book, quite abstract (Tschauner 1996:12). He thinks that *The Domestication of Europe* displays an abundance of formation-process reasoning and some of its basic, interpretative concepts are immediately dependent on formation theory. Tschauner sees that both non-cultural and cultural formation processes are discussed in this book. For instance, when adult burials have been found in storage pits next to Polish longhouses that do not suit the association of the "domus" with women and children, as postulated on the basis of evidence from other Neolithic sites, Hodder answers with a, for Tschauner, classic N-

transform when he states that “Bone does not survive well in many decalcified loess situations” (Hodder 1990:107).

In the way in which Tschauner sees it, the problem is that the reconstructions of prehistoric behaviour that form the foundation of Hodder’s interpretations are taken from already published sources that do not discuss the underlying formation processes. This is the case with *The Domestication of Europe*, which Tschauner calls a synthetical work, in which the lion’s share of the evidence comes from already published sources. Tschauner thinks that “...the information he obtains from his sources includes numerous highly charged and theory-laden concepts and processes, such as agricultural patterns; more selective and organized, procurement patterns; more intensive, organized, and specialized production; archaeological cultures as defined by stylistic complexes; and settlement hierarchies” (Tschauner 1996:13-14).

Tschauner claims that these theory-laden, published reconstructions are, with few exceptions, summarized as quasi-facts, which Hodder’s interpretative models rest on. It is evident that the work and results of research are functioning, according to Tschauner, as unproblematic, confirmed, background knowledge and intellectual, logical tools that make these results play precisely the role of MRT in the processualist research program. If one views the way Hodder in which uses mostly processualist, published material, then it would seem that MRT has done rather well in its aim to make observations of the past, Tschauner says (Tschauner 1996:14).

Tschauner finishes the chapter on contextual archaeology by concluding that Hodder is biting his own foot and claims that some of Hodder’s discussions are merely circumlocutions of Binford’s MR definition (Tschauner 1996:18).

Hill’s contrastive re-analysis of South British prehistory is the third and last post-processual example that Hartmut Tschauner examines. Hill claims that a “pit ritual tradition” is evident in the area. He finds that certain alleged “refuse pits” which contain some bones, broken pottery and other, smaller finds actually display deposited offerings and feasting refuse. Tschauner finds

that Hill's reconstructual inferences do not differ from the mainstream ones and are actually based on "standard" formation theory. The pits that Hill is studying are assuredly different from other excavated pits and were probably ignored by previous archaeologists. The implicit justification in this interpretation is its coherence, as Tschauner puts it: Hill has discovered structures and patterns in the so-called "rubbish" that seem to make sense, both in relation to each other and to the whole context of the time period, assisted by anthropological models of rites of passage, ritual feasting etc. Tschauner finds this procedure not very different at all from processualist research practice. He continues:

In fact, the development of pattern-recognition methods and the explanation of patterns by constructing coherent interpretations consistent with models either derived from actualistic research or borrowed from anthropology is a hallmark of processual archaeology. Most importantly for the present discussion, it is fully MRT-based (Tschauner 1996:19).

Tschauner leaves the case studies in favour of discussing the post-processualist relation to MRT in general. He thinks that, even if the post-processualists attack MRT as the core of the positivistic, processual method, the role of MRT in the processual, research program is ambiguous and has shifted over time. Tschauner feels that, on the one hand, MRT is based on generalizing assumptions about human behaviour and on the paradigm by which these very assumptions are justified. MR research is meant to produce unproblematic, confirmed, background knowledge. It has to be sufficiently established that it has a paradigmatic status and allows the archaeologist to observe the past directly (Tschauner 1996:20). On the other hand, Tschauner finds that MRT quite obviously plays the part of an observation theory and the existence of MRT is therefore a recognition of the theory-ladenness of data, thus making MRT a constitutional element of a post-empiricist model of observation in the processualism-research program (Tschauner 1996:20-21). He continues:

The knowledge, beliefs, and theories we already hold play a fundamental role in what we perceive. This is because in order to derive information from perception, we have to identify what we perceive, and identification requires a relevant body of information. /.../ Theories play the role of previous knowledge and beliefs in scientific observation (Tschauner 1996:21).

Hartmut Tschauner had a processualist standpoint when he commenced this article of his but, as he compared the processual and post-processual methodologies, he found much agreement. He claims that, if he had written it from a post-processual perspective, it would not have made any significant difference to the analysis as a whole, merely a shift of terminology and emphasis (Tschauner 1996:25).

This is Tschauner's fundamental idea: the similarity between the processual and the post-processual. He questions over and over again how the post-processualists can categorically ignore MRT when they are at the same time dependent on generalizing principles (Tschauner 1996:22). He finds it remarkable how MRT can be doomed by the post-processualists as the hard core of a positivistic, archaeological method when it is in fact bridging the gap between the two fractions (Tschauner 1996:25).

MRT as hermeneutics

Peter Kosso is a professor of philosophy at Northern Arizona University. His main interests are epistemology, the philosophy of science, archaeology and history. In 1991, his article "Method in Archaeology: Middle-Range Theory as Hermeneutics" was published in *American Antiquity*. In it he thinks that the MRT approach of Binford and the contextual archaeology of Ian Hodder are in fact clearly similar. He writes in his introduction:

These positions are usually presented in opposition to each other, but here they are shown to present very much the same methodological picture of archaeology (Kosso 1991:621).

In Kosso's view, MRT's are just ordinary theories. They are tested and justified, like any other theory, by comparing evidence, in this case, observations. A kind of circularity appears. Theories in general are confirmed and understood by an appeal to observations and observations in general are understood and verified by the support of theories. This is, according to Kosso, the exact structure of the hermeneutic circle. As MRT participates on both sides of this dialogue between theory and observation, Kosso's line of argument means that MRT's are hermeneutic tools.

He does not say that the studies that Binford advocates as important for archaeology are the same as those stressed by Hodder and other contextualist archaeologists. With Kosso pointing out the similarity of the methodological structure of middle-range theorizing and the hermeneutics of contextual archaeology, this is not a way to force processualism into a study of the mental component of the archaeological record. Kosso explains that

The point is rather that the different concerns and different objects of study are in a similar epistemic predicament that calls for a shared method (Kosso 1991:625).

Kosso points out the need for some assumptions to be made in order to break into the circular association between theory and observation. As he puts it

There will be neither meaningful evidence nor theoretical understanding without initial hypotheses and preliminary middle-range theories (Kosso 1991:626).

All these assumptions are revisable, which is exactly the kind of epistemic responsibility that we, or at least Kosso, demand of science, as they are revisable against the standards of coherence with other theories. Kosso thinks that in general

...the acceptability of middle-range theories and the evidential and theoretical claims they support is governed by a requirement of consistency and coherence and a constraint of independence in the accounting for evidential claims (*ibid.*).

No claims should, however, be accepted if they would lead to contradiction. What Kosso finds interesting is that independence seems also to be the answer to objections of circularity directed against the contextual, hermeneutic approach. The “assumptions of subjective meanings” (as spoken of by Hodder (Hodder 1986:79)) that influence our observations of the archaeological record are themselves accountable, in Kosso’s view, to other evidence that is influenced by various independent hypotheses of subjective meaning. He continues

This contextual method need not be problematically circular or left to unsubstantiated speculation as long as one insists on a coherence among independently arrived at claims about the past (*ibid.*).

Kosso does not find it coincidental that the key to objectivity is the same for MRT as for the hermeneutic approach. It is simply the result of their common structure and the fact that they are fundamentally the same method. Kosso finds both Binford and Hodder right and he writes:

It is not that archaeology is a rigid and segregated system of theories and observations in which brute facts are used to test theories. It is rather that the methods of natural science, and those advocated by Binford for archaeology, are more like the contextual, hermeneutic, back-and-forth model than Hodder’s original opposition seemed to recognize (Kosso 1991:627).

So the argument for this kind of similarity is displayed in the nature of the claims about the past and of evidence. It is not the content of these claims that is shared but the method of justification and the standard of objectivity, according to Kosso.

Peter Kosso has also authored a book entitled *Knowing the Past: Philosophical issues of History and Archaeology* in which he discusses MRT in a similar way in chapter three although more embroidered, perhaps to suit the book as a whole.

Trigger happy to expand MRT

Bruce Trigger is a Canadian archaeologist, active at McGill University in Montreal. He has had his archaeological research published since the 1960's and his *History of Archaeological Thought* (1989) is a thorough exposition of the history of archaeology and its different directions and aspects. It is on the reading lists of numerous archaeological courses in universities throughout the world. In the early stages of his career, he was somewhat pulled to pieces by Lewis Binford, who during his unorthodox lectures in Los Angeles in the 1960's dismissed Trigger's *Beyond History: The Methods of Prehistory* as a stupid publication. Binford summarized his critique by suggesting that Bruce Trigger should make himself a career as a shoe salesman (Schiffer 1995:3).

When the gunsmoke had subsided between the processualist and the post-processualists in the early 1990's, both sides pitched their camps and the harsh rhetoric that had characterized the debate diminished. In 1995, *Antiquity* published an article with the title "Expanding middle-range theory" written by Trigger. He writes in the introduction:

The obscure and ugly language of theoretical archaeology conceals as well as reveals fundamentals that no real practice of archaeology can actually escape (Trigger 1995:449).

He bases his article on the conviction that a judicious combination of processual and post-processual approaches can significantly enhance the analytical power of archaeology. He thinks that postprocessual archaeology expanded archaeology as a whole and acknowledged cultural differences and saw a wider range of theoretical approaches. Despite vicious attacks on post-

processualism, it has, in Trigger's view, changed archaeology in important and irreversible ways (Trigger 1995:449).

Trigger thinks that MRT, as defined by Binford, has been very successful when it comes to inferring technological processes, subsistence patterns and many aspects of social and economic behaviour. MRT studies, in Trigger's words, assume that these sorts of human behaviour are guided by universally persuasive calculations of self-interest, such as minimizing risk, energy conservation and ensuring more secure control of resources, rather than by concepts that are specific only to individual cultures or to historically related ones (Trigger 1995:450). Trigger takes the view, however, that archaeologists must be careful in their use of generalizations, as all human behaviour is cultural and cognitive.

It is generally assumed, according to Trigger, that practical reason and consequently MRT can only be applied to aspects of human behaviour that are governed by scarcity factors and therefore by the more practical aspects of human behaviour. Trigger claims that this is clearly not the case. He calls attention to his own, then ongoing, comparative study of seven early civilizations that evolved in different parts of the world. This study of his has shown unexpected regularities in terms of general structures of religious beliefs in societies at comparable levels of development. Trigger claims that in each of these societies the cosmos was a function for energy flows. In these systems of thought, the élite was conceptualized as an element between the commoners and the cosmic order that governed all human life (Trigger 1995:451). Trigger also mentions that kings and chiefs throughout the world in all ages have been related to the sun as a symbol of supreme power. The leader is often in many cultures described as a stranger to the society that he or she (Trigger actually puts down only "he", but the present writer prefers to avoid unnecessary critique from the gender archaeologists) is ruling. Many of these regularities occur with equally high statistical frequency as the ones that relate to subsistence behaviour. They do, however, according to Trigger, offer the

archaeologist a valuable understanding of beliefs in the past. He writes:

What is less obvious is how middle-range correlations can be established between such beliefs and material culture that allow these beliefs to be inferred from the archaeological record /.../ The study of these correlations requires a different kind of middle-range theory and different bridging arguments. This middle-range theory takes the form of demonstrations that certain kinds of beliefs and symbolism correlate significantly with specific types of societies (Trigger 1995:452).

Trigger finds that the strongest of the bridging arguments are often written sources, ethnographic data and oral traditions. This is an approach that relates archaeology to other disciplines that are able to provide data regarding the practices and beliefs of individual societies or historically known societies. Trigger thinks that this allows the archaeologist to control the variations between the different cultures. He claims that it is quite possible to use a contextual approach at the same time, to examine whether cultural forms are direct or distorted expressions of social, political and economic reality. This is a reasoning acquired from Ian Hodder's *The Archaeology of contextual meanings* (1987) (Trigger 1995:452). Trigger means that: "Much of the middle-range theory that is relevant to prehistoric archaeology is closely related to older culture-historical concerns with traditions, diffusion, and migrations. Interpretations are not based on cross-cultural uniformities but on demonstrating continuities through time in a single cultural tradition or a series of historically related cultures" (Trigger 1995:453).

They often take the form of a direct, historical approach that seeks to establish parallels between culturally specific beliefs and their material expressions during the early historical period. Then one uses material culture to trace these religious beliefs back to prehistoric times. The execution is, according to Trigger himself, by no means easy or uncomplicated. He points to Alexander von Gernet's and Peter Timmin's research which demonstrates that

the intensity of certain beliefs manifested in the material culture can vary considerably in the long view. He writes: "... hence continuity in beliefs is not necessarily matched by continuity in their expression in the archaeological record" (Trigger 1995:454). Bruce Trigger also recognizes certain limitations in MRT, as he realizes that it is not always obvious which type of MRT is applicable. For a long time most archaeologists assumed, according to Trigger, that in hunter-gatherer societies the females stayed near what one might call the home base, tending children, collecting vegetable foods and maybe capturing small animals, while men were hunting big game quite far from their home base. It was believed that this generalization was supported by all the available ethnographic knowledge. The view of gender roles in hunter-gatherer societies was much governed by generalizations of this kind as they were thought of as universally applicable. This view was obviously heavily criticized by feminist archaeologists and Trigger points out their attempt to stress the cultural rather than the natural status of human behaviour, as they called the older and more biologically grounded interpretations the uncorroborated products of patriarchal bias (Trigger 1995:455).

This discussion suggests, according to Trigger, that "... where the status of universal generalizations remains doubtful, the most prudent tactic may be to restrict reconstructions to what can be recovered using culturally specific middle-range theories. These theories are much more limited in their range of applicability and, in respect of gender, much less powerful than those based on universal generalizations" (Trigger 1995:456).

In his conclusion Trigger writes that, despite the fact that archaeological interpretations are always influenced by their social environment, the constraint of evidence and the refinement of methodology help to limit the effects of this bias. He claims that the archaeological evidence is in fact constrained, which reflects the cultural and natural factors that influenced the human behaviour that produced it, and that this offers grounds for believing that over time archaeology can achieve a more complex understanding of the past.

This, he thinks, does not mean, however, that the archaeologist will ever be free of a certain bias or be able to separate objective interpretations from biased. Trigger believes that new evidence or new techniques could however prove established theories and beliefs false (Trigger 1995:456).

Trigger finds that the expanded range of MRT and bridging argument, as suggested by himself, provides a foundation for a fuller and more diversified understanding of prehistoric times, which welcomes both cultural specifics and cross-cultural regularities. Whether this potential will be fully realized depends, as Trigger puts it, upon the amount of archaeological and non-archaeological evidence that is available and the willingness of archaeologists to make full use of it. In a train of thought similar to that of Philip Kohl (1993), Trigger finds that the chief weakness of numerous, post-processualist interpretations is the much-assumed illusion that it is possible to reconstruct the past by a process of sympathetic interpretation without the kinds of control that MRT in its broadest application can provide (Trigger 1995:455).

Trigger concludes his article by saying: “If archaeologists are to progress in understanding the past, they must be willing to make use of all possible data sources and to expand and develop middle-range theory to provide methodological rigour to a broader range of techniques for attributing human behaviour and ideas to archaeological data” (Trigger 1995:456).

Discussing MRT

Dissension is prevalent on the question of Middle Range Theory. The dissension is almost completely founded on one disagreement.

One big disagreement.

Is archaeology a natural or a social science?

Is one a processual or a post-processual archaeologist?

Who cares?

Yes, who really did care? It is no secret that the impact of processualism was actually limited to the Anglo-American sphere of archaeology (Jensen and Karlsson 1999:13), a fact that also restricted the arrival of post-processualism to that very sphere. Rowley-Conwy thought that neither the new nor the post-processual archaeology had the same impact in central and eastern Europe as in Great Britain (Rowley-Conwy 2001:18) and Bjørnar Olsen holds it likely that it was only smaller and special groups of the world's archaeologists that felt real changes by the arrival of these directions. In both Europe and the rest of the world, cultural-historical archaeology is still prevalent, even though in somewhat modified forms (Olsen 1997:30-31).

Scandinavia proved to be susceptible to the theoretical currents of the West and is pointed out as the area where processual archaeology has had the most influence. It is particularly the milieu around Carl-Axel Moberg and Bjørn Myhre in Gothenburg and Bergen respectively that are pointed out (Tosi 1981:16). Olsen also claims that it is in Denmark that we find some of the most accomplished processual analysis, represented by Kristian Kristiansen, Jørgen Jensen and Klaus Randsborg, among others. Olsen finds that it was mainly the ecological and system-theoretical aspects of the new archaeology that got a hold in Scandinavia, where the research of Stig Welinder made a good example (Olsen 1997:53-55).

MRT, however, does not seem to have got such a good foothold in Scandinavia and northern Europe. MRT's fields of application are nevertheless represented in Scandinavian archaeology, but there is no explicit support that says that such studies are governed by MRT. The Swedish articles and texts that discuss MRT do so mainly in general or descriptive terms (Sjögren 1999).

One of MRT's fields of application, ethno-archaeology, has, however, a prominent Swedish representative and he is also the only one that this writer has found who has written anything on MRT in practice. Göran Burenhult, one of Sweden's more influential archaeologists, has long conducted ethno-

archaeological research and has also authored a short article entitled "Ethnoarchaeology as a method of establishing Mid-Range Theory: A new era in European Archaeology or exotic nonsense?" (1987). Burenhult thinks that ethno-archaeological studies in Europe have been thought of as slightly doubtful. It has been said that it is not possible to transfer experiences from, for instance, the aborigines of Australia to the European hunter-gatherers of the Mesolithic. Prejudiced, direct comparisons or parallels that necessarily risk being completely wrong have, according to Burenhult, scared off archaeologists from making further attempts. He continues:

Yet, the ethnographic analogy is probably the only way European archaeologists can bridge the gap between the silent archaeological find material and the living society of the past. Ethnoarchaeology is simply a way of understanding how the things we excavate may have worked in the past (Burenhult 1987:322).

Göran Burenhult's interest in ethno-archaeology culminated in the book *Speglingar av det förflutna*, which is in many ways a fascinating publication. Beautifully illustrated and written with an apparent enthusiasm, it is, however, somewhat uncompromising in its argumentation. At the beginning of the book, one is met by a montage of two images in the shape of a face, in which the one half is a picture of a prehistoric cranium from Gotland, Sweden, and the other half is a picture of a young woman from a present day tribe in New Guinea. The caption tells us that, despite the five-thousand-year difference between these individuals, their lives are "exactly" the same. He even calls the girl from New Guinea "the Stone Age girl from the South Sea" and thinks that she is a "mirror image of the Stone Age". Through her, we are meant to understand our own people of the Stone Age better, according to Burenhult (Burenhult 1986:6). The present writer does not wish to concern himself with this rhetorical double nelson of colonialistic character. Burenhult has been rightfully criticized for the lack of theoretical reflection in his approach (Jensen, viva voce, 021028).

So, what is MRT then? It seems to be impossible to picture an unambiguous image of this phenomenon. Robert Bettinger finds that, though such a widely accepted and discussed approach, the idea of MRT is surprisingly problematic (Bettinger 1991:64).

One thing is quite certain: MRT is not synonymous with Binfordian research. This we notice when we search for the origin of MRT in archaeology. Raab and Goodyear's adaptation of Robert Merton's term is relatively easy to understand but did not have the same impact as Merton's original idea in sociology. Even though Merton thought that Raab and Goodyear had understood his theory correctly (Goodyear, e-mail 030116), the lack of success of their thesis could simply have been due to the fact that it had been rejected by *American Antiquity*.

The works of Lewis Binford are sound but difficult to understand and unnecessarily complicated in their formulation (David and Kramer 2001:122). His idea of MRT is no exception (Cornell and Fahlander 2002:5). This may be the reason why it is easy to look upon his research and his results as applied MRT. This nevertheless gives a far too narrow image of MRT, even though it is Binford that one associates with MRT.

When MRT arose, as a vital part of processual archaeology, what was assumed to be needed was obtained. A key-code or a Rosetta Stone as Binford himself wants to call it. This was a reaction to the cultural-archaeological approach that was accused of being too untheoretical and inexact. This revolution, which was launched in the 1960's, could be seen as a phenomenon typical of its time when rebellious tendencies and insubordination were trends. Whether this was in accordance with the truth or not is of marginal significance. Its importance was the aim to change the view of archaeology.

To join the New Archaeology was to concur with the opinion that archaeology was a natural science and that knowledge could be obtained by statistics, tables and charts, complemented by all sorts of technical devices and well-meaning theories. It was thought that the underlying theories had to be explicit and could not remain unspoken and implicit, as they had been during the

period of cultural archaeology. The new theories that it was desired to create might have been slightly self-evident, obvious and under-developed, just as MRT has been accused of being. What was definitely of importance, if not directly crucial for processual archaeology, was the impact of radiocarbon dating in the 1960's. This method threw over earlier interpretations of the time ranges of specific prehistoric cultures, which led to the critics of cultural archaeology gaining support for their arguments. Peter Rowley-Conwy argues that the theoretical program of processual archaeology was developed, as new theories were needed that could be adapted to the new methods that sprang from radiocarbon dating (Rowley-Conwy 2001:19-22).

When it was introduced, Binford's version of MRT produced different kinds of response from other processual archaeologists. We have seen how Grayson (1986) thought that MRT was nothing new and that this approach had been used in archaeological studies since the 1800's. In this matter, he was surely right, but one can, needless to say, question the theoretical awareness in these older studies that undoubtedly was quite implicit.

Another prominent, processual archaeologist who had once been a student of Binford's, Michael Schiffer, has also found Binford's concept of MRT questionable. We have already looked at parts of his critique and his own ideas on MRT, but he has also written a review of Binford's *For Theory Building in Archaeology* published in *American Antiquity* (1980). Schiffer, who obviously had a copy of Raab and Goodyear's rejected article as well, recognizes the term MRT in archaeology from this very article. He points out that, despite Binford's definition of MRT at the beginning of the book, there is only one chapter that discusses the subject. He feels that the term is too vague, unorganized and separated from the rest of the book and thinks that Binford actually fails with his introduction (Schiffer 1980:377).

Despite the fact that Schiffer and Binford are close to each other, both theoretically and ideologically, they do not agree at all and the debate between the two is quite instructive and

entertaining. Binford goes from using Schiffer's terms (Binford 1977:8) in certain matters to disagreeing with his former student completely (Binford 1983b:234). Schiffer thinks that his own terms are more exact and that Binford's carelessness in his terms is misleading (Schiffer 1985:192).

Binford views the confusion with MRT with repugnance. On several occasions, he has pointed out that one of the goals of ethno-archaeological studies is to contribute to the development of methods for the justification of inferences and that this research has been called MR research. He reacts, for instance, against Raab & Goodyear and Schiffer placing MRT on an equality with formation processes, which is also the meaning the term has got within archaeology (Sjögren 1999:128) and finds the entire matter misleading. Binford claims that he has consistently suggested that MR research should be directed towards the isolation of variables of organisation that are characteristic of historical systems (Binford 1987:449). The classic Binfordese is palpable and is, as always, demanding on the reader.

It is not just critique that characterizes the debate on MRT, even if one can assume that MRT is often silently defended. To some extent, the theoretical discussion on MRT is separated from its practical use and those who are using MRT are concerned more with its practice than with discussing its philosophical aspects (Rowley-Conwy, e-mail 030115).

A great many, especially American, archaeologists embraced processual archaeology and two of these, Gordon Willey and Jeremy Sabloff, express their support of Binford's research and his version of MRT in their book *A History of American Archaeology* (1980). They share the same position on numerous, different, archaeological phenomena and agree with Binford when he recommends that interest in MR and general theory should not be separated (Willey and Sabloff 1980:254). One should maybe consider that Sabloff and Binford are close colleagues, as well as close friends (Sabloff, P., 1998:xi) even though Sabloff in retrospect feels that MRT was a confusing term (Sabloff 1998:88). With the post-processualist impact in the mid 1980's with Ian

Hodder's conversion and Shanks and Tilley's *Re-constructing Archaeology* (initially published in 1987), the critique of MRT came from a totally different direction. When Binford and his MRT were criticized by other processualists, they objected, as we have seen, to specific details. This new, growing school advocated a hermeneutic approach to science and raised massive objections to the entire, positivistic nature of processualism. Archaeology as a discipline was rather seen as a part of social than natural science. This fundamental difference of opinion made MRT an obvious target for the post-processual criticism and the objectivity that MRT was thought of as providing as in its capacity as an independent, measuring device also got its share of criticism. To incapacitate MRT was logical and strategically correct as it was perhaps the dearest tool of active processualism to produce archaeological knowledge. It was their Rosetta Stone and flagship. An attack on MRT was to thrust the lance deep into the processual midriff. One could and should see the critique of MRT as an indirect and masked attack on the main figure of processualist archaeology, Lewis Binford.

Binford has made himself famous not only for his revolutionary, archaeological work but also for his somewhat blunt way of delivering his opinion and mainly the harsh rhetoric that he uses to dismiss the colleagues whom he does not agree with. Binford's harsh manners and status probably made the post-processualist critics more eager to direct their attacks at him. To criticize the functionality of MRT and dismiss it as invalid was an easy way to undermine the research of Binford and to invalidate his studies. In the same fashion that Binford had once criticized the cultural archaeologists, he was now himself the target of criticism from the postprocessual archaeologists who wanted change just as Binford had once wanted it.

Because of MRT's processual status, its close connection with Binford and its somewhat rudimentary character, MRT was a dear and easy target for the post-processualists (Hodder, e-mail 021128; Johnson, e-mail 030113; Rowley-Conwy, e-mail 030115). It was simply a matter of a pure dismissal. We have also looked at

articles written by authors who responded to this criticism. Hartmut Tschauner thinks that the post-processualists are using methods and theories that are in the highest degree like MRT. The philosopher Kosso claims in his discussion that MRT in fact are hermeneutic tools. Their arguments are convincing and well directed and even if they do not affect archaeology in the same way as Hodder or Binford, they are still making a rather good and interesting point that more people should take the time to explore. Tschauner's discussion about the post-processualist way of using MRT principles has impressed Ian Hodder, who also finds his argument convincing (Hodder 1999:27).

The arguments that Tschauner puts forward may be the those that would have expected from Binford, but he has kept surprisingly quiet as regards the refutation of the post-processual critique of MRT. He has, however, reviewed Hodder's *Reading the Past in American Antiquity* (1988) and is not very gracious in his critique, even if he is not explicitly defending MRT. Binford opens the review by writing:

This is a little book with a little message being blown through a large horn with a loud noise (Binford 1988:875).

He is criticizing Hodder for singing two tunes at the same time and thinks that *Reading the Past* is full of contradictions, misinterpretations and distortions. Hodder has, according to Binford, completely misunderstood the challenges that an archaeologist is faced with. Binford claims that the book contains much less than meets the eye. In the way that Binford sees it, Hodder is involved in a power play and is seeking dominance for his value-laden ideas. The whole review is characterized by a sarcastic and pungent rhetoric and one feels that Binford does not really want to lower himself to Hodder's level and acknowledge his work by reviewing it. He concludes the review by writing: "This is a book about politics negotiated by Hodder, not about archaeology" (Binford 1988:876).

Peter Kosso, who writes about MRT as hermeneutic tools, is, as already mentioned, not really an archaeologist but a philosopher who deals with questions about epistemology and the philosophy of science. He is correct that the circular relation between observations and theories is problematic, but there is nothing to do but to accept this. One has to break into this circle to obtain a starting-point and if MRT is really on both sides of this circle, maybe this ought to be taken advantage of? The level of abstraction may be too high in this discussion and may not be all that relevant to the archaeological fieldwork but should nevertheless be kept in the back of one's head.

Naturally, MRT has hermeneutic qualities that Hodder and his contextualists have not acknowledged, as Kosso points out, but they would never use such a Binfordian term. Tschauer also claims that post-processualists are using similar principles, although in a somewhat camouflaged fashion, for purely political reasons.

Today, Lewis Binford is getting on and has since 1991 been "University Distinguished Professor" of anthropology at the Southern Methodist University in Texas, USA. In 2001, he published his latest book *Constructing Frames of Reference*, which is impressive both in extent and in content. This book is based on 340 historically known, hunter-gatherer societies and contains in the usual processual style a lot of graphs and tables.

MRT is given little space, maybe to avoid already delivered criticism. He mentions it as the foundation of his studies, though (Binford 2001:114). In a note, he claims that, despite his efforts to bring clarity to the confused situation in the archaeological literature about the nature and meaning of MR research there are still many misconceptions of the term (Binford 2001:479). Perhaps this is another reason for the sparse usage of MRT.

What can be said about the future of MRT?

A renaissance does not seem to be approaching. This is because of the charge that this term implies. The term Middle Range Theory

is so infected that perhaps it should be left to its own devices. The principles that MRT is built on and their applicability in archaeological studies will certainly live on but with a different terminology. As Tschauner has pointed out, there is not much difference in post-processual studies, compared with a MRT approach, and Kosso questions the positivistic status of MRT.

Bruce Trigger's attempt to expand MRT is well-meaning and interesting, but the core of his discussion, to make archaeology more open to other disciplines, loses focus when he uses the term MRT. Instead of interest in his discussion, his article is more noteworthy for his new view on MRT. Whether this is an honest attempt to expand MRT or whether he is using MRT as an easy way to sell his ideas is hard to tell. Nevertheless, he has got a point when he states that archaeology must avoid isolation from other sciences and one should be careful about making generalizations as an archaeologist.

One could keep the same line of argument regarding Christopher Carr and Jill Neitzel's *Style, Society and Person* (1995). This is a sound work and a heavy publication that seeks to revise, explain and increase the understanding of the term "style". Carr tries to build an unified MRT about style and thinks that the definition of style has made the construction of a MRT about material style in this purpose impossible. Carr mentions six strategies necessary for the construction of a MRT about material style that he develops and finally he actually constructs a MRT for his purpose (Carr and Neitzel 1995:143; Carr 1995:171).

Carr's discussion, argument and creative, theoretical abilities are also well-meaning, but his use of MRT takes the focus off his actual interpretation of the text. David and Kramer point out that this attempt of his to develop an unified MRT about artefact design in this massive publication has been more quoted than read (David and Kramer 2001:224).

The British representative of processual archaeology, Colin Renfrew, who through the years has been defending Binford and has shared his view on archaeology, is nowadays showing a scepticism towards MRT. In the third edition of Bahn and Renfrew's

Archaeology: Theories Methods and Practice Binford's MRT is discussed as a means of bridging the gap between the archaeological remains and the societies that the remains represent. At present, they feel that it is too difficult to justify the division of archaeological theory into high, middle and low levels. Renfrew and Bahn choose not to use the term MRT (Renfrew and Bahn 2000:182) probably to avoid the critique that this infected term risks bringing.

The question is, whether the debate about Middle Range Theory is particularly constructive. This article, which does not really take any sides, hopes to bring at least some clarity into the confusion. Michael Schiffer described the situation well in our correspondence:

...all the debates about middle range theory end up being little more than rhetorical exercises that clutter up the literature!
(Michael Schiffer e-mail 021210)

Conclusion

Middle Range Theory is one of the most discussed phenomena in the archaeological theoretical debate. Despite this, a slight confusion seems to be evident about the meaning of the term. I have in this article tried to search for the term's origin, development, practice and status in the archaeological theoretical debate. The version of MRT that has been the most influential in archaeology is the one formulated by Lewis R. Binford in 1977. This is a definition that is completely unconnected with the original MRT, defined by sociologist Robert Merton in 1947. Binford's ability to formulate himself arduously has led to the term MRT being made equivalent to Binford's own studies and the study of formation processes, despite his efforts to explain his true intentions about MRT.

The post-processual criticism that was directed against MRT was founded on the opposition to the positivistic approach that processual archaeology propagated and on the arguing for a hermeneutic approach. MRT, which was thought of as a tool of

positivism, became a dear target for this new school's critique, partly because of its processual status and rudimentary character and partly because an attack was a camouflaged attack on Lewis Binford. I have also discussed articles that claim that there are in fact fundamental similarities between processualism and post-processualism that the authors have chosen to illustrate, using MRT. These articles maintain a convincing line of argument but have as yet not been very influential in the theoretical debate.

The term Middle Range Theory is considered so infected by prejudice and preconceived understandings that one should actually avoid it, in order not to risk attention being taken from one's real argument and attracting unnecessary critique.

MRT will live on but probably in other shapes, because the infection it suffers from is far too persistent. A renaissance is not imminent and will almost certainly take more than one archaeological shift in generations.

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