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30



**A HISTORY OF CENTRAL  
EUROPEAN ARCHAEOLOGY  
THEORY, METHODS, AND POLITICS**

**Edited by  
ALEXANDER GRAMSCH and ULRIKE SOMMER**



**BUDAPEST 2011**

*Front Cover Illustration*

European archaeologists: Participants of the *Congrès international d'anthropologie et d'archéologie préhistoriques* (CIAAP) in Bologna in 1871  
(image courtesy of Prof. Daniele Vitali, Université de Bourgogne, Dijon)

*Back Cover Illustration*

Polish-German consultations: J. Kostrzewski and visitors from Berlin  
at the Biskupin excavation in 1936 (from left):  
Z. A. Rajewski, unknown, W. Unverzagt, J. Kostrzewski, H. Kothe  
(image courtesy of Poznań Archaeological Museum)

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# **German Archaeology in Context: An Introduction to History and Present of Central European Archaeology**

ULRIKE SOMMER – ALEXANDER GRAMSCH

## **Abstract**

This introduction sketches the history of archaeology in Europe, and in Central Europe in particular, discussing the epistemological and theoretical basis of different approaches. Additionally, the changing definitions of the term “Central Europe” are revealed. Finally, three different research attitudes that characterise German and much of Central European archaeology are presented.

The 19<sup>th</sup> and early 20<sup>th</sup> centuries are characterised by common approaches in all European archaeologies, based upon evolutionist thought derived from anthropology. Three major breaks can be detected that changed this situation. The first is the essentialist quest for links between ancient “peoples” and modern nations characterising much of the first decades of the 20<sup>th</sup> century. This detached archaeology from anthropology and linked it to history and linguistics. Second, before and during the Second World War archaeology became politicised to an unprecedented degree. The third break is the result of the further divergence of existing national schools after the 1950s, leading to three different research attitudes in North-Western, Central and Eastern Europe.

Thus, rather than the simple dichotomy between traditional and processual/post-processual approaches, many of the mutual misunderstandings between European archaeologies are rooted in general (mis-)perceptions of the “Others” in Europe. Moreover, they also have their origins in different academic social identities of archaeology, in a different *habitus*.

## **Keywords**

Central Europe, epistemology, history of archaeology, *habitus*, culture-historical archaeology

## **Introduction**

‘By intellectual self-sufficiency, a lack of discussion and missing methodological perspectives German archaeology is in danger of getting onto a dead track methodologically and in international comparison.’

This was one of the famous ‘theses on the situation of German archaeology’, better known as the Unkel-manifesto (HÄRKE – GECHTER 1982). The Unkel meetings can be seen as the trigger for numerous critical reviews of the state of German archaeology (see below). While other critics were less radical, they nevertheless saw ‘German archaeology at risk’ (BLOEMERS 2000). Trying to understand why, or if, German archaeology developed differently from the rest of European archaeology, the role of Kossinnism and the “Kossinna syndrome” (KLEJN 1974, SMOLLA 1979/80) have been discussed as well as the Nazi-Period and the post-war “*Wirtschaftswunder*”, the postwar rapid economic development (WOLFRAM 2000). Thus the history of German-language archaeology has been mainly considered in relation to the political and social history of Germany, and to Nazism and its aftermath in particular.

While this concentration on a situation perceived as problematical and the search for its roots is understandable (cf. HÄRKE 2000; STEUER 2001; LEUBE 2002), it is not the whole story, of course. With this specific focus, the awareness of the wider spatial and temporal context and of the much older and much more ramified traditions German-language archaeology rests upon got lost. When the board (*Sprecherrat*) of the German Theoretical Archaeology Group, *Arbeitsgemeinschaft Theorie (T-AG)*, decided to organise a session on the state of German archaeology at the 7<sup>th</sup> Annual Meeting of the EAA in Esslingen in 2001, we realised that this topic could not be discussed in isolation, but has roots in a much wider European tradition. The volume presented here results from this session on Central European Archaeology and is a first attempt to sketch the framework of this European tradition, relating German, Polish, Swiss and other archaeologies<sup>1</sup>. We want to move away from the focus on both the Third Reich and on Germany alone to widen the spatial and temporal horizon (see also BIEHL – GRAMSCH – MARCINIAK 2002a), to try and perceive the history of Central European Archaeology (CEA) as a whole.

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<sup>1</sup> Of course, this can only be “a” history of Central European Archaeology (CEA). More studies from different vantage-points are needed to produce a balanced picture. The papers in this volume are based mainly on discussions from the late 1990s and early 2000s, and not all of them could be updated fully. One of the papers presented at the Esslingen meeting will be published elsewhere (NOVAKOVIĆ in press).

## Common trends in the development of Central European Archaeology

Our question was: is German archaeology really such a special case? In the 1980s and 1990s the state of the theoretical debate had mainly been compared with Great Britain and the United States (cf. EGGERT – VEIT 1998; MANTE in this volume), with much less attention on the development in the Netherlands (BLOEMERS 2000) and the Scandinavian countries, or with other Central European archaeologies.

In May 2000 the conference “Archaeologies East – Archaeologies West” in Poznań, co-organised by the *T-AG*, demonstrated the existence of strong West European, Central European and East European (Russian) archaeological traditions that transcend both national borders and academic networks (BARFORD 2001; VEIT 2001). It became increasingly clear that the archaeology of German-language countries is firmly based in a wider Central European tradition. Although today Central European archaeology is characterised by distinct national schools, the overall way in which research is conducted and the explanatory devices employed seem amazingly similar (see the conference proceedings in BIEHL – GRAMSCH – MARCINIAK 2002b).

But questions do remain. What differentiates “a” Central European archaeology from a Western and an Eastern European archaeology? What does “Central Europe” actually mean? Where have, e.g., French, Italian or Spanish traditions to be located? Can archaeological approaches, research questions and paradigms be grouped together in such blocks at all, or do national schools prevail? How profoundly did political borders influence research design? What was the role of 19<sup>th</sup> century research traditions in the emergence of different schools and paradigms (see KAESER and MÜLLER-SCHEESSEL in this volume)? The papers collected here all aim at answering these questions from a particular Central European point of view (see below). Not only do we try to place German archaeology in its wider Central European context, we also present alternative views on the history of antiquarian vs. scientific, nationalist vs. universalist, Central vs. Western archaeologies. Of course, the sample is unbalanced and incomplete. However, we hope it triggers further research on cross-boundary traditions and developments in the archaeologies of Europe.

To set the scene for Central European Archaeology (CEA) – the subject of this volume – we will now briefly trace the history of Central European traditions. We argue for a new understanding of CEA characterized by a particular *habitus* rather than by -isms different from those in the West and East.

## A history of archaeology in Central Europe

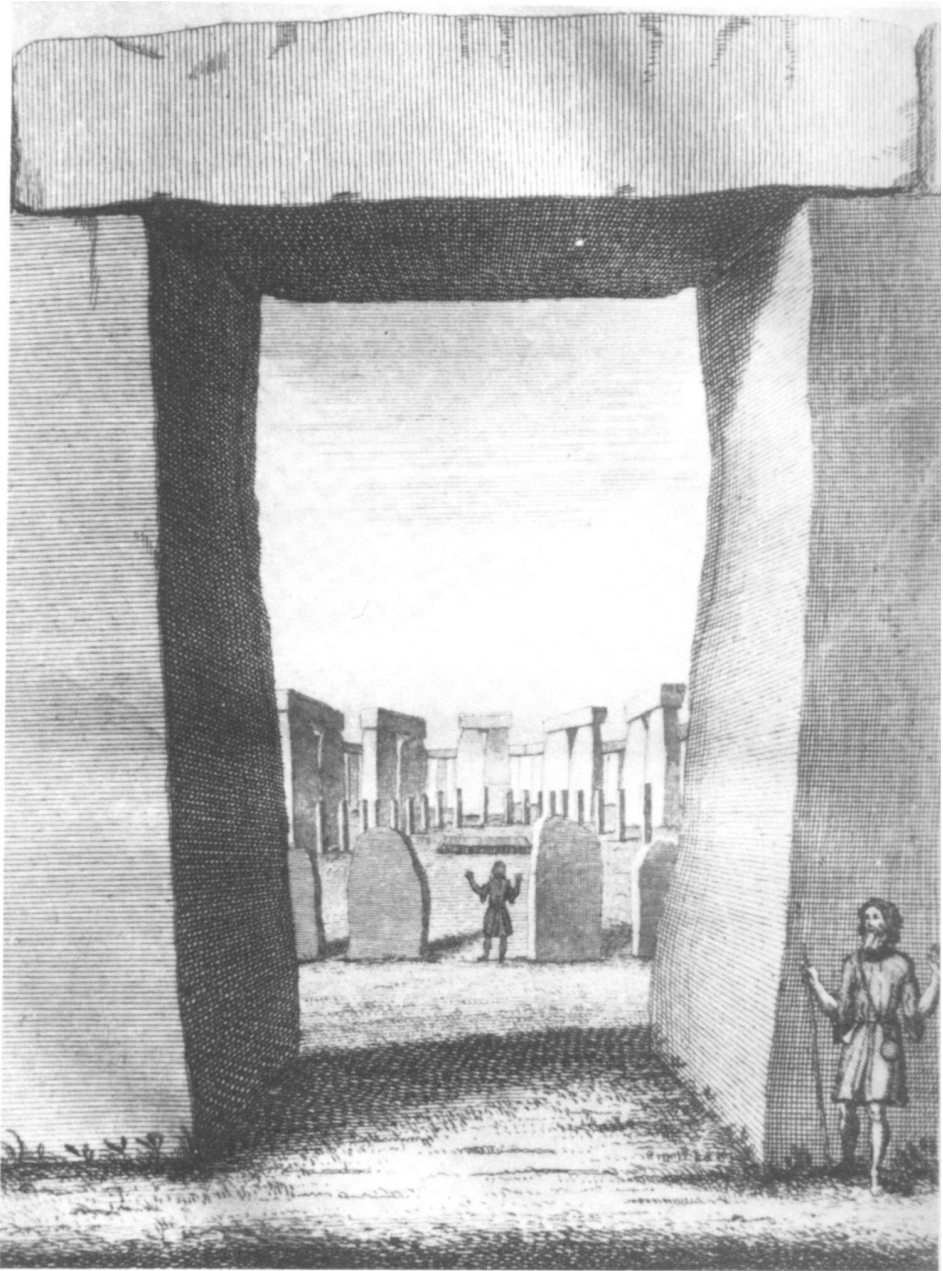
### Antiquarianism

Archaeology as a scientific discipline is a European invention, closely connected to the rise of the nation-state in the 19<sup>th</sup> century (ANDERSON 1983), indeed part of the invention of tradition described by E. Hobsbawm and T. Ranger (HOBSBAWM – RANGER 1983).

The origin of prehistoric archaeology as a discipline lies in 17<sup>th</sup> and 18<sup>th</sup> century antiquarianism (SCHNAPP 1996). At least since the beginning of the 19<sup>th</sup> century it had developed a methodology that separated it from history, dealing with material culture rather than with written sources.

Prehistoric archaeology has always had a national focus, in contrast to classical archaeology. As it were the finds of classical antiquity that corresponded to the aesthetic and civilisatory ideals of the time, while local prehistoric remains were seen as barbarian and savage, it was hard pressed to justify its existence (GRAMSCH 2007). Thus, in an earlier, classicist phase, we see attempts to cast prehistoric remains after the classical mould, as can be demonstrated by, e.g., the depictions of Stonehenge (*Fig. 1*). With the rise of romanticism, the coarse and unrefined prehistoric finds were presented as *national* antiquities, remains of ancestral history ('*vaterländische Alterthümer*', e.g. PREUSKER 1829), thus deserving attention and veneration.

Since the Middle Ages, origin-myths had been based on sources from classical antiquity and the Bible. For example, a Trojan origin was claimed for a number of European ruling houses and was used, for example, in Geoffrey of Monmouth's *Historia Regum Britanniae* or the *Annolied* (cf. THORPE 1966; ROEDIGER 1895; GRAUS 1989). Now, links were created between prehistoric finds and local stories and folkways. New origin-myths, based on local peoples, were created (Germans in Germany, Celts or Franks in France, Slavs in Bohemia, Sarmatians in Poland, etc.). Finds were interpreted as products of the different peoples known from classical and medieval sources. Jacob Grimm was the leading, but far from the only scholar in this movement (SOMMER 2009). This early form of ethnic ascription can be seen as one single European movement, even if opinions diverged on exactly which peoples the finds should be attributed to.



*Fig. 1. Stonehenge as depicted by William Stukeley in 1740  
(after DANIEL 1990: 28, fig. 25).*

## Evolutionism

Not only did the increasing amounts of finds require the development of new taxonomic entities and reliable chronologies. In 1788 the geologist James Hutton presented the theory of strata building the earth's surface and claimed that the earth was much older than previously inferred from the Bible (DEAN 1992). Scholars like DARWIN (1859), WALLACE (1858), and SCHAAFFHAUSEN (1853) claimed that species were not immutable. Boucher de Perthes demonstrated that man had lived together with now extinct animals. The time depth of prehistory was indeed much vaster than previously assumed.

The notion of index fossils was developed in geology and was adopted by anthropology and archaeology. The new paradigm of Evolutionism allowed placing both local finds and prehistory in a general explanatory framework and linking archaeological with geological periods. Now, local prehistory could be placed in a broader framework. The age of international (or at least Pan-European) organisations and meetings began (e.g. the *Congrès International d'Anthropologie et d'Archéologie Préhistoriques* in 1866, see MÜLLER-SCHEESSEL in this volume). Archaeology and anthropology, still *one* coherent discipline, were cast in the universalist, scientific mould. Thus both disciplines became important and growing new fields of research. They were in the centre of public interest and were expected to contribute to fundamental social and philosophical questions of the day (cf. ZIMMERMAN 2001).

In Germany, the evolutionist approach can mainly be connected with the research of the pathologist Rudolf Virchow. The liberal Virchow, who can be seen as the founder of scientific archaeology, practised prehistory as part of a wider discipline of anthropology, which included physical anthropology and ethnology as well and had a potentially universal scope. This science was to supersede the parochial search for ancestors that had characterised the preceding phase (e.g. VIRCHOW 1875).

While in Great Britain Pitt-Rivers, for example, embraced evolutionism as a panacea against revolutionary tendencies (BOWDEN 1991: 141), and evolutionism may have played its part as the ideological foundation of colonialism, in Germany evolutionism came to be identified with Marxism from early on; both adopting the model of progress in stages. Still, the evolution of artefacts continued to be the basis of prehistoric chronology, and has continued to do so to this day. But this was seen as a strictly chronological tool and was not systematically connected to

a theory of the evolution of society (cf. BERTEMES and STOCKHAMMER in this volume).

*“Siedlungsarchäologie”*

While Virchow developed the concept of “types” in the 1870s to differentiate distinct pottery and building traditions (for example, the Lusatian type, VIRCHOW 1872; cf. GRAMSCH 2010a), he was extremely reluctant to link these to prehistoric ethnic units. But at the turn of the century, in a climate of increasing nationalism the distribution of certain types of artefacts became more and more to be equated with “archaeological cultures” and prehistoric peoples. Gustaf Kossinna deliberately cut the connection to ethnology and anthropology and embraced German linguistics instead. The search for ancestors was the main aim of all prehistoric research, the migrationist model its main explanatory tool, the artefact its focus. This tribal-historical approach (JAHN 1952: 8) – *Siedlungsarchäologie* (settlement archaeology) in Kossinna’s terms – was practised not only by Kossinna, but Montelius and others as well. It, too, can be described as a pan-European trend (see KADROW in this volume).

The idea was to trace the “peoples” of the 19<sup>th</sup> century national states back into prehistory. The “essence” of a people was supposed to remain the same through all its history and prehistory. Change was only superficial – a notion going back to the early 19<sup>th</sup> century and to Savigny’s concept of *Volksgeist*, further developed in the *Völkerpsychologie* of Heymann Steinthal and Moritz Lazarus (KALMAR 1987). Thus, no models of social development were necessary, migrations and invasions were the sole means to explain the distribution of cultures and indeed of any change in material culture (cf. GRAMSCH 2009). This also meant a break with 19<sup>th</sup> century’s approaches, which were based on systems rather than on artefacts. Thus, in the first decades of the 20<sup>th</sup> century archaeology not only became national but nationalist.

The potentially essentialist approach of classifying archaeological finds into cultures and tracing their development through time is often described as “culture-historical” by English-speaking authors. This can be misleading, as the German word *Kulturgeschichte*, usually translated as “cultural history” (Burke 2004) describes a school of history that does not focus on big events and big men, but rather on the development of society, intellectual history and lifeways and the way they influence the historical development. It is connected with historians like Jacob Burckhardt, Johan Huizinga and especially Karl Lamprecht (see SCHLEIER

2002 for an overview). There is no German expression for “culture-historical”, perhaps because it is still very much seen as “normal research”.

The demarcation line between an evolutionist, universalist etc. on the one hand and this culture-historical, often nationalist tradition of archaeology on the other hand (e.g. PARZINGER 2002; SOMMER 2002) weaves through much of the current debate on the epistemological identity of archaeology in Europe.

While the Kossinnist *Siedlungsarchäologie* displaced the earlier universalist tradition of Virchow and his contemporaries after World War I (GRÜNERT 2002), this development did not occur contemporaneously over the whole of Central Europe. MÜLLER-SCHEESSEL (in this volume) reveals the political implications of research trends in the growing enmity between Germany/Austria and France after 1871 and after the First World War, and KAESER shows how this universalist tradition could be used for integrative nationalist ends in multilingual Switzerland.

French archaeology for a long time maintained the links to anthropology and the universalist approach (see also MÜLLER-SCHEESSEL), while in Great Britain Myres and Childe in particular elaborated a culture-historical understanding of archaeology. Childe’s notion of culture ‘became the working tool of all European archaeologists’ (TRIGGER 1988, 169) – but to a very different degree in different countries. It is surely fair to say that the influence of Childe’s culture concept decreased even in Britain after the Second World War. Here, the need for tracing ancestors was never as urgent as on the continent, and the attraction of the migrationist model rapidly decreased after the Second World War. Colin Renfrew’s *Before Civilisation* (1973) was very influential here, connecting the Radiocarbon revolution with a spirited refutation of the *ex oriente lux* paradigm. Consecutively, even the impact of historically attested migrations like the Anglo-Saxon conquest has been increasingly minimised (cf. HAMEROW 1997; HÄRKE 1998).

### Historicist tradition

The historicist influences on archaeology, especially the role of Gero von Merhart’s strictly artefact based Marburg school on the concepts of Central European archaeology, still needs to be scrutinised and will be omitted here (see VEIT 2002: 413; THEUNE 2007; SOMMER – STRUWE 2007). However, we can already say that this school considerably influenced the Central European tradition. NOVAKOVIĆ (in press) touches upon the Marburg influence in Slovenia briefly.

### Nazi Archaeology

With the beginning of the Nazi regime, German archaeology was politicised in a quite unprecedented degree. While many archaeologists actively supported the racist interpretations of prehistory, others confined themselves to seemingly objective data collection and classification, relying on the prevalent antiquarian concept, maybe without noticing, however, that they also used an ethnicist paradigm. This approach continued after the war both in East and West German archaeology (see below). However, the racist politicisation of archaeology meant a second severe break in German, but also Central European archaeology with older traditions and with its neighbours (BIEHL – GRAMSCH – MARCINIAK 2002a).

A lot has been written on archaeology during and after National Socialism (e.g. LEUBE 2002; MAISCHBERGER 2002; HALLE 2002; HALLE 2005; ADAM *et al.* 2001). We do not want to repeat this detailed discussion but will now concentrate on the communication between “East” and “West” after World War II.

### Theory in Eastern and Western archaeologies

The existence of the “Iron curtain” and the more or less real adherence to a restricted pattern of “Marxist” interpretations of history does not seem to have affected the basic unity of traditional Central European approaches as much as might have been expected. The communication network among Central European archaeologists was never really broken by the post-war borders. Nevertheless, the fact that only a few established chairholders from the ComEcon countries could travel, while students and younger scholars neither had the chance to get abroad nor to get hold of innovative publications has certainly influenced the development of the discipline. The exchange of ideas and information was incomplete and twisted, due also to epistemological differences and personal biases in the perception of the “Other”. This has led to mutual misunderstandings in European archaeology which we have been able to evaluate only in recent years. After 1990, communication networks opened in an astonishing way, revealing how limited the knowledge of the other *and* one’s own archaeology really was.

Central European and British archaeology parted ways sometime in the early 1970s, with Scandinavia and the Netherlands to follow. Central European archaeology is perceived as traditional, with little theoretical debate, North-Western European archaeology as highly theorised and dominated by post-

processualism. But this dichotomisation ignores the fact that much, if not most “dirt archaeology” on the British Isles is quite traditional, as well as much of US-archaeology is of an extremely conservative nature<sup>2</sup>. The traditional nature of continental archaeology thus seems to be a bit of a straw man. This selective presentation, often uninformed, may even be a part of the construction of the origin-myth of processual and post-processual archaeology. Binary structures like these, the unrelieved black and white beloved by structuralists and post-processuals, have never been true, and prove to be more and more sterile. Discussion is made impossible, not encouraged.

### *habitus*

While many of the mutual misunderstandings are rooted in general, historically and politically founded perceptions of the “Others” in Europe, i.e. in Cold War *Zeitgeist*, they also have their origins in different academic social identities of archaeology, in a different *habitus* – rather than the simple traditional/post-processual dichotomy. The academic culture – teaching and debating, formulating research questions, practising archaeology etc. – differed considerably in the distinct research traditions in North-Western, Central and Eastern Europe.

In Russia, there has been a strong and independent tradition of Russian archaeology since the early 20<sup>th</sup> century. Archaeology has always been part of the history curriculum (VASIL’EV 2002: 258; cf. KLEJN 1993), and students were trained in history with little exposure to the sciences. However, due to language and political barriers, terminology and concepts developed with considerable difference to Western and Central Europe. During the Stalinist dictatorship in particular, communication was both politically and conceptually difficult. Vulgar Marxist evolutionism and a strong ethnographical tradition in the vast Soviet territory led to a clearly differing development.

One reason for a different development in Great Britain may be the long colonialist tradition, leading to a Eurocentric World Archaeology rather than to a quest for ancestors<sup>3</sup>. Later on, this may have led to a stronger involvement in the postcolonial discourse and the early perception of the social role of archaeology,

<sup>2</sup> This becomes all the more obvious since the “TAG-USA” has been established as some sort of anti-conservative movement, with the first US-TAG meeting – ‘inclusive, informal, and low cost’ – in New York in 2008 (see [www.tag-usa.org](http://www.tag-usa.org)).

<sup>3</sup> The question whether colonial archaeologies can only be evolutionist cannot be discussed here; but see KINNAHAN (2000) and LYONS – PAPADOPOULOS (2002).

especially in a multicultural environment. A second reason surely is the influence of North American archaeology, where the situation was completely different, because prehistory could not be used to construct ancestors for the white majority. Prehistory continued to be associated with anthropology. This, to put it briefly, facilitated the adoption of new methods in the 1960s<sup>4</sup>. As Timothy Champion put it:

‘from the early 1970s onwards, explicitly theoretical concerns have become progressively further removed, in language, audience and vehicle of publication, as well as in subject matter, from a traditional, empirical non-theoreticised archaeology’ (CHAMPION 1991: 131).

The drive towards intense theoretical discussions led to a third break in European archaeology. Today, the divide seems still to be growing, partly, but not wholly, owing to linguistic competence (KRISTIANSEN 2001). While Anglo-American theoretical discussions find a wide audience in Europe, European theoretical contributions are more or less ignored across the channel, even if they are published in English. This cannot even be blamed on accessibility, as, in contrast to German periodicals, most of the major French, Polish and Russian archaeological journals are available online.

Thus, while the 19<sup>th</sup> and early 20<sup>th</sup> centuries are characterised by approaches common to all European archaeologies, during and after the Second World War the existing national schools diverged increasingly and led to three different research attitudes: Western, Central European, and Soviet/Russian archaeology. Especially since the 1960s the influence of national borders on research and communication grew increasingly stronger.

What CEA is, what it is supposed to be, what it could be – this is the issue of the next paragraphs. Here we argue that different types of research attitudes exist side by side.

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<sup>4</sup> It has, however, to be noted that the new approaches developed in US American anthropology ‘were slow to catch on in Britain. The “establishment” (most notably among them the editor of the influential journal *Antiquity*) put up a relatively stiff opposition’ (BARFORD 2002: 85). Barford discusses in detail the epistemological history of Western, Eastern and Central European archaeologies.

## Central European Archaeology

When we organised the Esslingen session in 2001, the parameters and models for understanding the order of our world were dissolving. The “East and West” model had lost its importance, and what “Europe” means remains unclear until today. A concept introduced at that time looks odd today: the division between “Old Europe” and “New Europe” proposed by former US Minister of Defence Donald Rumsfeld. Speaking of “Old Europe” to characterise the states resisting and rejecting the bellicist politics of the United States and their allies, Rumsfeld gave rise to high emotions in the spring of 2003.<sup>5</sup> While it was meant as an insult, many commentators did react pretty positive, accepting “Old Europe” as credit and designation of a certain set of cultural values. This new stamp met with attempts to rediscover Central Europe as a *Kulturraum*.

When using the term “Central Europe” it has to be kept in mind that this term and related expressions have been highly politicised notions, *Kampfbegriffe*, since at least the First World War (ASH 1989; PLASCHKA *et al.* 1995; HADLER 1996), up to the *Generalplan Ost* of the NS-regime, and to the less geo-political but rather cultural definitions after World War II and particularly since 1990<sup>6</sup>. It was first used in late 18<sup>th</sup> century handbooks of geography and political science in an attempt to order the dominions between the Urals and the Atlantic Ocean<sup>7</sup> not simply by alphabet, but according to mnemotechnic principles (SCHULTZ 1997a). But the changeover from a genealogical to a territorial definition of the state at the end of the 18<sup>th</sup> century, the assumption of a “natural” and inevitable connection between nation and territory (“natural borders”) led to a normative use of the term<sup>8</sup>.

In the debates about the new political order following the revolution of 1848, this led to the first equations of “Central Europe” and a “Greater Germany” and its dependencies. It was argued that the geographical position in the “centre” of Europe predisposed or even forced Germany to rule central Europe, the borders of which were constantly shifting according to the actual political development.

<sup>5</sup> See, e.g., BBC News World Edition: <http://news.bbc.co.uk/2/hi/europe/2687403.stm>; U.S. Department of Defense News Transcript: <http://www.defense.gov/transcripts/transcript.aspx?transcriptid=1330>.

<sup>6</sup> See SINNHUBER (1954) for a collection of definitions of “Central Europe” that include nearly every European country except Spain and Portugal.

<sup>7</sup> For definitions of Europe see SCHULTZ (1997c).

<sup>8</sup> “Geo-power”, cf. O’TUATHAIL (1996: 7).

In the course of the late 19<sup>th</sup> century, Central Europe increasingly developed into a rallying cry of German expansionism (SCHULTZ 1997b: 8) that reached its apogee during the First World War. Now the German dominance of Central Europe was to encompass most of the territories conquered by the Austro-German armies until 1915, from Belgium to Bulgaria or at least the middle Danube (SCHENK 1995: 25)<sup>9</sup>.

In smaller countries, of course, the vision of Central Europe was very different. When Tomáš Masaryk, the first president of Czechoslovakia, defined *Střední Evropa* in the 1920s, he saw it as a collection of small independent countries, free from the hegemony of the big powers.

In the “Third Reich”, the extended “Central Europe” became just a transition on the way to the German habitat (*Lebensraum*) in “the East” (SCHENK 1995: 29): ‘The slogan of central Europe has been replaced by the “*Großraumidee*”. Central Europe as a political fact is already established. And the world’s largest military power is fighting to preserve it.’ (Rumpf 1942, quoted after SCHENK 1995: 29)<sup>10</sup>.

After the war, the term was no longer politically opportune in the West. ‘Central Europe no longer exists, “*Mitteleuropa*”, that first principal of German thought, has gone [...]’ (MEYER 1955). The political division of East/West left no space for something in between.

For this very reason, the term had a renaissance East of the Iron Curtain. In the CSSR, Hungary, parts of Yugoslavia, the Baltic States and Poland, it had positive connotations for those who opposed Soviet hegemony and strove for more democracy and a closer connection to the countries of the European Union, both culturally and economically. This “return to Europe” could even include countries that did not fit into the traditional concept of Central Europe, like Romania and the Ukraine (SCHENK 1995: 33).

Following the historical changes of 1989, borders shifted again. In the new East-West conflict, the term “Central Europe” became connected to a certain set of values (see, for example, the papers collected in HECKER – POETTGENS 1993). At the turn of the century, the renewed link to the West became of great importance for the countries of the former Soviet dominion. To consolidate this link, the cultural kinship was to be strengthened. For example, in 2001 a new journal was

<sup>9</sup> This is connected with the names of PARTSCH (1904), HASSINGER (1917), PENCK (1915) and NAUMANN (1915).

<sup>10</sup> ‘[...] Die politische Tatsache ME ist heute geschaffen. Um ihre Erhaltung kämpft heute die größte Militärmacht der Welt’ (Rumpf 1942, quoted in SCHENK 1995: 29).

launched: “*Kafka – Zeitschrift für Mitteleuropa*”. It was edited by the *Goethe Institut Inter Nationes e.V.* and published in Poland, Hungary, Czech Republik, Slovakia, and Germany in their respective languages, aiming at promoting the ‘discourse with and between the central European culture areas’ (BRODERSEN – DAMMAN – SÖTJE 2001: 5).<sup>11</sup> Central Europe here is perceived as “*Kulturraum*” and is part of the political attempts to create a European cultural identity – attempts comparable to nationalist identity constructions using archaeology and history (GRAMSCH 2000a).

To define Central Europe always meant to define a centre and a periphery, to exclude Others as not European, not Christian – or not Catholic (Orthodox Christians) – not civilised, not wanted. The definitions of Central Europe have radically changed, from a front against “the West” in the pre-World War I era to the East-West division of the cold war period and today’s stress on the cultural and economic differences to the east and south of Europe.

When we decided to use the term “Central Europe” nevertheless, we did not think of an immutable “*Kulturkreis*” in the tradition of Pater Schmidt (ANDRIOLO 1979) or, later, Samuel HUNTINGDON (1996), or of monolithic entities that ‘spin off each other like so many hard round billiard balls’ (WOLF 1982: 6), or to take part in the construction of a “Myth of Europe” seen as a necessary or at least desirable ingredient of the political entity EU by some (TIELKER 2003: 11).

But even if we try to be aware of the pitfalls inherent in terms and concepts like “Central Europe”, we cannot avoid or omit them<sup>12</sup>. Rather we need to deconstruct and contextualize them. What we want to do here, as good archaeologists, is to try to follow up the historical development of these (culture) areas and to map the shifting borderlines and connected histories, the linkages between different societies on different scales in the timeframe between ca. 1800 and the present.

As we have admitted from the beginning, we approach this history from a German point of view. While German and Central European archaeology are not identical, one cannot be described without the other (see the contributions by BERTEMES and KADROW). German archaeology as a “majority archaeology” (NEUSTUPNÝ 2002) has had a considerable influence on its neighbours, due to its sheer size and economic, political and cultural potency. Of course, the intensity of

<sup>11</sup> It ceased to appear in 2005 (see <http://www.kafka-zeitschrift.de/>).

<sup>12</sup> The appeal of MELNIK (1988) to throw out the term on the rubbish-heap of history has a certain appeal, only this would necessitate a new word to describe the area we are talking about, and there is no word without history.

influence or domination differed according to period, political situation, and the leading paradigm of the time.

German-style archaeology not only has influenced archaeologies practised in Poland and Yugoslavia or Slovenia respectively (NOVAKOVIĆ in press). This overview could be extended to Britain (e.g. Alfred Rust's effect on Grahame Clark, see FAGAN 2001: 93f; KERIG – ZIMMERMANN 2010; Gerhard Bersu's influence, see EVANS 1998; PARZINGER 2002), Ireland (Walter Bremer and G. Bersu in Dublin), Spain (DÍAZ-ANDREÚ 1995), Greece or even Argentina (KOHL – PÉREZ GOLLÁN 2002).

This is, of course, not only a question of theory, methodology or even a hegemonic discourse. Matters of funding are certainly of equal or even superior importance but must per necessity remain outside of our considerations here. While some light has been shed on the political role of the German Archaeological Institute (DAI), after all, affiliated to the German ministry of the exterior (MARCHAND 1996; JUNKER 1998), the history of post-war *Forschungspolitik* (research politics) remains still unwritten. Commemorating the 100<sup>th</sup> anniversary of the *Römisch-Germanische Kommission*, the history of its projects was presented together with a brief overview of its foreign excavations (MÜLLER-SCHEESSEL *et al.* 2001). However, little is said about where the money went.

### Whither Central European Archaeology?

Why, we must ask, did questions posed in Anglo-American archaeology hardly gain a foothold in Germany and Central Europe before 1990? Rather than *only* the Kossinna- Syndrome (SMOLLA 1979/80), *one* further reason may have been the prevailing gerontocratic system in research and teaching with its authoritarian structure (HÄRKE 1995; BARFORD 2002; SOMMER 2002) as opposed to the comparative intellectual freedom of the younger generation of British scholars. Moreover, since the end of World War I archaeology in Germany was to a large degree dependent on the *Deutsche Forschungsgemeinschaft* (DFG) and her predecessors which tended to finance large-scale projects centred on detailed excavations and the cataloguing of the resulting objects (WOLFRAM 2000; KUNOW 2002). And, as a majority archaeology, Germany (West-Germany in particular) formed a stable and self-contained discourse universe. No need was felt to pay much attention to developments in remote areas like the United States.

Let's dwell on German archaeology for a moment. Archaeology in post-war Germany had at least partly lost its responsibility for contributing to a national

meta-narrative. However, in the country that had lost the war but won peace through the economic miracle, which insisted on the homogenous nature of its population despite of massive labour immigration since the 1880s, it found its niche nevertheless. Large-scale excavations and research projects served to illustrate the myth of feasibility, the virtues of thoroughness and diligence (cf. WOLFRAM 2000). The unruly past was tamed through detailed classification, which seemed to exorcize the bad spirits of the past. No one was interested in a theoretical debate that might open up Pandora's Box again. A discussion of concepts like race, tribe etc. was not attempted. No time was wasted on the history of the subject – a history necessary for the understanding of its prevailing paradigms. Comprehending and possibly changing the dominant culture-historical paradigm thus only just begins. Today, German mainstream archaeology emphasises its ability to generate and process large amounts of data interdisciplinarily, rather than on the necessity to reflect on research premises (GRAMSCH 2010b).

After World War II, there have in fact been some attempts to discuss archaeology from a theoretical standpoint. For example, the journal *Archaeologia Geographica* edited by Hans-Jürgen Eggers since 1951 brought up questions of method and theory for discussion (EGGERS 1951; cf. also EGGERS 1950). Symptomatically, this journal ceased publication with volume 10/11 already. The intended methodological discussion didn't get off the ground. In the late 1970s it was Manfred EGGERT (1978) who presented the concepts of New Archaeology to German readers, but met with a cold welcome (see MANTE in this volume; MANTE 2007). Discussions in university lectures or seminars were few – Rolf Hachmann, a student of Eggers, is a rare example (see STOCKHAMMER's paper). The post-war generation of German professors did not discuss theories as such, rather, impressionist approaches were common. That does not mean that they were necessarily theoretically uninformed. But they did not explicitly talk about their methodology, which had to be adopted by imitation rather than understood (see also the contribution by BERTEMES, and by RACZKOWSKI for a corresponding situation in Poland). The epistemological and methodological foundations of the discipline were not open to debate, rather each chairholder strove to be the head of his (rarely her) school and did not react well to dissenters among his students.

### Changes since 1991

In the early 1980s, a group of younger archaeologists, mainly Germans, met to discuss the current situation and the future of German archaeology (HÄRKE –

GECHTER 1983; HÄRKE 1990a; ECKERT 2002). They became known as *Unkeler Kreis*. Their question: whither German archaeology? They started to discuss what had been neglected for decades: the historical and theoretical identity of prehistoric archaeology in Germany and the necessity to revive explicit theorising. However, mainstream “traditional” archaeology either ignored their endeavours or reacted with downright rejection and even hostility (for more details on the post-war reception of new theoretical approaches see MANTE’s contribution).

Less than ten years later, after the unification of both Germanies, Heinrich Härke and Sabine Wolfram organised a session at the 1990 TAG in Lampeter which resulted in the founding of the German *Arbeitsgemeinschaft Theorie (T-AG)* (HÄRKE 1990b; WOLFRAM 2001). Again, the focus was on the epistemology of German archaeology and its prospects. This time the agenda raised gained much more attention eventually, and the debate has not stopped since. G. MANTE (in this volume) gives a short overview on how German archaeologists reacted to processual and post-processual ideas under different political circumstances in East and West.

But, as we have pointed out, during the last decade a new kind of theoretical debate has developed in German archaeology. Indeed, there is not only the German *T-AG* which has contributed to this discussion in meetings and publications (WOLFRAM – SOMMER 1993; GRAMSCH 2000b). There is also a broader discourse on the political history of German archaeology, on new ways of reasoning in archaeology, on interpreting material culture, on reintegrating different schools and the growing awareness for the philosophical and theoretical implications of archaeological practice.<sup>13</sup>

The contributions of BERTEMES and ZIMMERMANN show different ways of refurbishing or rebuilding approaches common to CEA. These new approaches are not restricted to German archaeology, a very spirited discussion on theories, methodology and new research is occurring in former socialist countries (e.g. KUNA – VENCLOVÁ 1995; HENSEL *et al.* 1998; GHEORGIU 2003). However, we do not intend to present an overview of the latest research questions and theories in Poland, Czech Republic etc. Rather, we briefly want to summarize the shifts and turns during the last decade in CEA.

The radical political and social changes of the 1990s have caused innovations and fundamental changes in many Central European archaeologies. Czech,

<sup>13</sup> For example: BERNBECK (1997); KÜMMEL – MÜLLER-SCHEESSEL – SCHÜLKE (1999); HÄRKE (2000); STEUER (2001); LEUBE (2002), VEIT *et al.* (2003); HEINZ – EGGERT – VEIT (2003); RAMBUSCHEK (2009); BURMEISTER – MÜLLER-SCHEESSEL (2010).

German, Polish etc. archaeologies faced a changed climate which allowed to ask neglected questions – as is true for other humanities as well (e.g. HOHLS – JARAUSCH 2000). Numerous publications on archaeology and philosophy, politics, ideology etc. have started to appear. A new explicit discourse has begun, new approaches, theories and methodological problems are discussed openly and without polemics. In the former communist countries, a widespread application of different explanatory concepts became possible for the first time, and especially younger archaeologists were exposed to the theoretical discussions in the English-speaking world.

Much of the discourse in German archaeology is still characterised by caution in relation to or even rejection of theoretical developments elsewhere. It is implied that we, the heirs of the strong continental tradition, do not have to deal with the ever new -isms introduced into theoretical debate. Rather, prominent representatives of the discipline argue it would be better to keep the discipline united. And this often means: facilitating material-based type approaches at the cost of an explicitly theory-based discourse (GRAMSCH 2010b). Some archaeologists suggested to delegate the discussion about the (political) history of archaeology to the historians, thus trying to keep this issue away from present-day discourse, although it definitely influences present-day practice. However, mainstream archaeology too begins to realise that an opening to a multitude of new questions and approaches does not mean the loss of the discipline's profile. It is slowly understood that this change offers opportunities rather than dangers:

‘For the future of our discipline it will be decisive to come together unconditionally and to see the plurality of thinking in Europe not as result of proliferating misdevelopments, but as a chance’<sup>14</sup>  
(PARZINGER 2002: 35).

### German archaeology – state of the art

Having discussed the future and the past the question remains: what *is* German archaeology? It is still difficult to sketch a general trend in theoretical discussions in German archaeology. In fact, we have to consider the relation between different attitudes, which we want to characterize according to the following three types:

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<sup>14</sup> ‘Für die weitere Zukunft unseres Faches wird es entscheidend sein, wieder vorbehaltlos aufeinander zuzugehen und die Pluralität der Denkansätze in Europa nicht als Konsequenz wild wuchernder Fehlentwicklungen, sondern als Chance zu sehen’ (PARZINGER 2002: 35).

- A) **Empiricist approaches**, following a research aim which focuses on the classification of material culture, without attempting a theoretical discussion and solely relying on traditional methodology, “common sense” and implicit paradigms. The following statement made during an interview of representatives of the older generation may illustrate this attitude: ‘We should be proud not to have become victim of the temptation to say more than the material really tells us’ (NORTHE – SCHWARZBERG – WEGENER 2002: 204)<sup>15</sup>.
- B) **Hidden theory**, i.e. approaches with a similar focus on the classification of finds, but resting on a firm theoretical base, which is, however, not discussed but only shows through in interpretations.
- C) **Explicit discussions** of terms and concepts (e.g. *Kulturbegriff*) without primarily aiming at classifying or interpreting a certain set of material culture.

Some scholars, like K. J. Narr and G. Smolla, discussed the history of the discipline and the continuation or application of paradigms and theories, others, like R. Hachmann and G. Kossack discussed single concepts and applicable theories.

Little needs to be added to characterise the type A) attitude. Some quotes may help to understand the other two types. John Bintliff has reviewed a collection of English translations of essays of Georg Kossack (BINTLIFF 2001). He states that Kossack, while representing ‘a research community whose sharpest thinkers have been relatively or entirely unaffected by the two chief theoretical mini-paradigms of the last 25 years in Anglo-American scholarship’, ‘nonetheless offers a series of impressive contributions to key concepts in those movements’. This view, of course, is worth to be debated.

Can we really talk of impressive *contributions* when no communication takes place? Is the snobbery with which some established German archaeologists meet both younger colleagues and proponents of (theoretical) discussions justified by the fact that they themselves are able to produce respectable knowledge? This attitude of ignoring or rejecting any “new” approaches and refraining from explicit discussions has led to an inability to communicate with proponents of other archaeologies. While a certain scepticism may have been understandable

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<sup>15</sup> ‘Es sollte uns in Halle eigentlich mit Stolz erfüllen, dass hier bisher niemand den Versuchen erlegen ist, mehr sagen zu wollen, als das Material hergibt’ (NORTHE – SCHWARZBERG – WEGENER 2002: 204).

and maybe even justified during the decades of jargon-ridden polemics, today it is the greatest threat for academic archaeologies.

In a reviews-editorial for the *European Journal of Archaeology*, Anthony HARDING (2001) found a nice formula to characterise a new tendency in German archaeology: hailing Christoph Huth's work on Bronze Age hoards he said:

'Huth of course represents the empiricist tradition, which is not to say that he is unable to indulge in speculation; but he refuses to speculate unless he has evidence with which to do so' (HARDING 2001: 406).

So, we should follow him talking about the strengths of Central European Archaeology and the possible paths it could or should take without neglecting its shortcomings and deficiencies it pampers just because of its taste for traditions.

## Conclusion

Today we face an array of new discussions in Germany on the theory and methodology of archaeology, which already has spread into *Festschriften*<sup>16</sup>. A new development is represented by textbooks and handbooks written to explicate methodological and theoretical foundations of archaeological practice (e.g. EGGERT 2001; TRACHSEL 2008; EGGERT – SAMIDA 2009). The investigation of the history of the discipline also gains acceptance. To understand why and how German archaeology and its relatives differ from their neighbours we have to recall their particular histories. We also need to estimate again the relation between ethnicist culture-historical thinking and evolutionist scientific reasoning (cf. KAESER in this volume).

Today we observe a new reflexiveness: there is a need to consider the basics of the discipline, to think about paradigms, implicit assumptions and influences from schools of thought deriving from cultural anthropology, history etc. We need to overcome simple dichotomies such as processual vs. post-processual or theoretical vs. traditional. We need to find a new common language, to develop a terminology which can be understood and applied in the different European traditions – beyond false compromises. We also have to think about ways of communicating: we can agree to disagree, but we should understand what the

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<sup>16</sup> Both POHL *et al.* (2001), and ASLAN *et al.* (2002) contain sections labelled "*Archäologie und Theorie*".



*Fig. 2. European regions as proposed by the  
 “Ständiger Ausschuss für geographische Namen (StAGN)”  
 ([http://en.wikipedia.org/wiki/File:Grossgliederung\\_Europas.png](http://en.wikipedia.org/wiki/File:Grossgliederung_Europas.png)).*

other talks about. And we have to link this discussion to those in other disciplines as well as to the general discourse in European societies: what can archaeology contribute to the “pressing questions of our time”? Maybe it is possible today to find a way to combine a material-oriented type B) approach with a theoretically informed type C) attitude to a new, truly Central European type D) archaeology which overcomes the deficiencies of both.

In the end let us briefly summarise what we intend to achieve with this volume. By publishing the papers we want to continue the lively debate that took place during the Esslingen session and to put it into a wider context. We do not aim at definite conclusions, but try to outline the areas of future discussions. It should be inspiring to compare the development of archaeology in different parts of Europe in more detail; we hope to have shown up some general trends (see also CALLMER *et al.* 2007, offering rich material for further comparative studies). We do not intend to invent or re-invent a homogeneous Central European tradition in archaeology, much less a new “archaeocentrism” (GHEORGIU 2003: 170). Rather we want to point at long standing traditions, similarities and differences which were neglected due to the prevailing East-West-division in order to better understand the history of European archaeology and the archaeology in German-speaking Europe in particular. And we hope to contribute to the development of rich and diverse archaeologies in Europe – rather than a single, homogeneous European archaeology.

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# Prehistoric Archaeology in Central Europe

FRANÇOIS BERTEMES

## Abstract

This paper discusses the existence of “German archaeology” or “Central European archaeology”. Even for the Cold War period we cannot speak of an intellectual divide between archaeologies in Western and in Central Europe. Central European archaeology not only did communicate with other European archaeologies and participate in new developments, but it also advanced its own methods and theoretical assumptions. Thus, this paper will draw a positive picture of the post-war development of archaeology in Central Europe, countering views that see it as undertheorised and underdeveloped. Particular emphasis is laid on its methodology and the consecutive work stages that are crucial for a scientific analysis of the archaeological record: systematic data acquisition, data-inherent analysis, interpretive analysis, and model building. The reconsideration of these steps reveals that it is unjustifiable to reduce Central European archaeology to the mere collecting of data.

## Keywords

Central European archaeology, theory and method, data, interpretation

## Introduction

When A. Gramsch and U. Sommer asked whether I wanted to contribute to this session on German prehistoric archaeology, a series of counter questions spontaneously went through my head: in the ever-increasingly globalised world of today, is there such a thing as a national archaeology or national archaeologies? And if so, are we dealing with methodological or philosophical-theoretical differences? Are these political instead, or grounded more in research history? Is there even such a thing as *German archaeology* anyway? And if so, how is it positioned within the European archaeologies?<sup>1</sup>

Recent discussions in our discipline show that these questions must be posed and that a need exists for their discussion. Although definitive answers based on a comprehensive and comparative epistemological analysis at the European

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<sup>1</sup> This contribution is a reworked version of my German-language article, “Die mitteleuropäische Archäologie: eine Standortbestimmung zwischen Ost und West“ (BERTEMES 2002).

level are possible, the necessary studies have not been done, and this present contribution will also not be able to fill this gap. At any rate, I can only answer the questions – as formulated – in a subjective statement.

Although the Iron Curtain and the resulting political separation into Western and Eastern Europe complicated the possibilities for physical encounters of Central European colleagues (BIEHL – GRAMSCH – MARCINIAK 2002), we nevertheless cannot speak of an *intellectual* division into two isolated blocks. For traditional archaeologists working in the culture-historical tradition it was common sense to look beyond one's own nose, to be eager to recognise and to understand the large-scale supra-regional connections in prehistory, and, thus, to better understand the area under investigation. The necessity for an international perspective is ultimately at the core of our discipline: the origin and spread of archaeological cultures did not take into account modern state boundaries nor the Yalta convention. Correspondingly, the search for differences in the archaeological traditions of Europe cannot be limited to a consideration of Western and Eastern Europe, these can also be detected between Southern, Central and Northern Europe and even within every area.

Seen culture-historically, but also geopolitically, the fall of the Iron Curtain also coincides with the rebirth of Central Europe. Central Europe represents a close cultural and historical, but also religious cohesion between the German-speaking peoples of Europe, the Hungarians, the western Slavs, and, to a certain measure, also the Croats and Slovenians (see also NEUSTUPNÝ 2002). In the following I wish to show that this area also displays common traditions in investigation methods and goals, which encourage me to speak of a Central European archaeology rather than a German archaeology. There is no “German archaeology” *sensu stricto* just as there are neither French nor English archaeologies. While in fact we should not talk about German but about German-language archaeology, I will try to show that the appropriate investigative tradition of Central European archaeology is independent from the language of publication. At any rate, the considerable influence German-speaking archaeologists have had on this Central European archaeology is unquestionable (see KADROW in this volume and NOVAKOVIĆ in press).

Central European archaeology rests upon two elementary pillars, its general methodical basis and its theoretical variety.

## The methodological basis of Central European Archaeology

With the exception of the National Socialist aberration and some German colleagues from the 1930s to the end of the Second World War in particular,<sup>2</sup> one sees that Central European and Scandinavian pre- and protohistoric research share a common historical tradition. It is based upon the methodological foundations laid by O. Montelius, P. Reinecke and others, which many individual colleagues continue to refine up to the present day. Mutual influence or – to put it better – a more active exchange with the French research tradition was also evident up until the First World War (e.g. J. Déchelette's praise of Kossinna's method, see DEMOULE 2002: 135). Within central Europe in particular, this methodical framework and the resulting standardised scientific language and terminology continue to provide an important link within archaeology, independent of the respective national language (NEUSTUPNÝ 2002).

Although a few of our colleagues are unaware of or question this common past,<sup>3</sup> this does not mean that it did not happen. Since academic teachers are primarily responsible for arranging methodological basics, the origins of this ignorance are also to be found here. In contrast to the view particularly promoted by some German colleagues (for example Härke, Bernbeck, Eggert, Sommer), I am of the opinion that the development of the discipline's methodological basis in Central Europe has by no means stagnated in the last 50 years.<sup>4</sup> For reasons of space I would like to limit myself here to two aspects: typology and vertical stratigraphy.<sup>5</sup> The “typological method” – advanced by Montelius and initially

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<sup>2</sup> It is undisputed that nationalistic as well as national-socialist or fascist ideas have had grave consequences for our discipline or – more precisely – for individual researcher personalities. It is especially commendable that this aspect is discussed since the 1990s (e.g. LEUBE 2002).

<sup>3</sup> There is, for example, no explanation why one would redefine the term and definition of the closed find according to Montelius' unmistakably clear definition (and thus thinking not about the unity of settlement pits, but rather evaluating and interpreting them as closed finds) without considering the resultant methodological problems.

<sup>4</sup> Therefore it is fairly common to hear foreign colleagues repeating: ‘Nobody talks as badly about German archaeology as German archaeologists themselves.’

<sup>5</sup> I might be accused of representing a far too positive, or almost idealised picture of Central European archaeology. I do not see it this way. To put the negative in the foreground without offering concrete alternatives and ignoring the positive in this case seems unreasonable. My concern is rather to show the many mostly positive external facets, first of all to illuminate their structural connections in order to make

strongly influenced by Darwinian models – has developed into a comprehensive feature-analytical method since the 1980s through the use of computer-supported seriation process (see among others GOLDMANN 1979; LICHARDUS-ITTEN 1980; GLESER 1995; BERTEMES 1998). The path from a purely qualitative, rather subjective process to a more objective – however ultimately still subjective to a certain extent – quantitative method has long been completed. Reputable central European researchers are busy with its continuous development. The special feature of the new methods is to be found in a combination of the qualitative and the quantified point of view. Numerous colleagues are occupied both with the methodological ways of an appropriate data investigation, processing and evaluation.<sup>6</sup> The special features of archaeological data as well as the formulation of goals and questions of modern culture-historical archaeology (see below) made the development and/or adaptation of conventional statistical-mathematical procedures necessary, particularly during the evaluation of the quantified data. Numerous publications illustrate this.

Moreover, following Eggers' conclusions on stratigraphic methods in (EGGERS 1959), these continued to be refined over the decades and today bear little in common with the former "index fossil method" (see among others HACHMANN 1969; 1982; 1994; KOSSACK 1994; BERTEMES 1998). A system now serves as a basis, by which deposits are distinguished by their origin (as bio-layers, sedimentation layers and culture layers) and organised hierarchically according to stratigraphic association (ECHT 1984). In addition, three consecutive methodological stages of stratigraphic analysis are distinguished: "objective" or descriptive stratigraphy, interpretive stratigraphy (for example causal importance of the shifts within the framework of settlement dynamics and the settlement process), and stratigraphic chronology (BERTEMES 1998). The last two stages as well as the inclusion and the crucial scrutiny of the context of a find (among others the inclusion of the concept "closed find") as well as such further distinctions as

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the errors and shortcomings easier to comprehend. From here, ways can be shown how to eliminate these in the future without questioning the entirety of Central European archaeology.

<sup>6</sup> For example: SOUDSKY (1967); PAVLŮ (1971); STEHLI (1973); IHM (1978); GOLDMAN (1979); KAMPFMEYER (1986); STEHLI – ZIMMERMANN (1980); CASELITZ – MICHL (1988); RULF (1989); SCOLLAR *et al.* (1993); ANDRESEN – MADSEN – SCOLLAR (1993). This list is far from exhaustive. In addition, the individual authors are cited only for an early work on this question.

small finds of primary, second and tertiary deposition are only some keywords that stand for the development and perfection of this method (BERTEMES 1998).

Even if – for the sake of being short – I have not mentioned such other fields as, e.g., relative chronology, the level stratigraphy, comparative stratigraphy, modern chorology and so forth, one can not deny that significant methodological progress has been achieved, particularly over the three last decades.<sup>7</sup>

### Theoretical diversity

To judge the theoretical concepts of prehistoric archaeology of central Europe is difficult for other reasons. As much as I consider it wrong to reduce them primarily or only to Kossinna, so is it wrong to allocate Kossinna only a marginal or historical importance only or to conclude that in Germany and in Central Europe there were no opponents to ethnicity theory.<sup>8</sup> Therefore I must emphatically maintain that while archaeology did indeed exhibit a homogeneous methodological basis in Central Europe until 1945, on a theoretical and interpretative level and in its research goals, however, it already offered a very rich multi-faceted picture. This is easily understandable for a humanist, since there cannot be an all-explanatory comprehensive archaeological theory.

It was the racist-nationalist tumour in pre- and protohistory that held back the level of the interpretation after the Second World War. G. Smolla accurately had labelled this state of affairs as ‘Kossinna-Syndrome’ (SMOLLA 1979/80). Particularly in the German-speaking areas, no one wanted to go out on a limb and therefore most colleagues occupied themselves with innocuous methodological questions on typology and relative chronology. E. Neustupný summarises this neo-positivist situation for Czech archaeology as follows:

‘this means much interest in chronology, the perfection of typology,  
much diffusion (the study of “influences”), a restricted number of

<sup>7</sup> This is not to say that – outside of Europe – there have not been any further methodological development on the two fields reviewed here; for stratigraphy see, e.g., HODDER (1999).

<sup>8</sup> Kossinna’s polarising ideas, which many see as having cleared the way for the Nazi ideology, still keep many German colleagues from approaching his work objectively. I would therefore like to thank J.-P. Demoule for his discussion of the French viewpoint on this problematic topic (DEMOULE 2002). Furthermore, Demoule rightly criticizes the way that – in the 1990s – German archaeology only slowly began to deal intellectually with the political, particularly National socialist past of the discipline and their results.

migrations, a limited interest in ethnicity, an obligatory interest in ecofacts (in the post-war period), some simple settlement history, and almost no other theory, especially no interest in methodology, social theory and symbolism settlement' (NEUSTUPNÝ 2002, 285).

We can take this description to be generally valid for the situation throughout Central European research until the 1960s.

In the following years a number of diverse directions developed, in relation to individual sources or research traditions, which in Germany in particular led away from a national "stamp" to one of increased diversity. We can regard this diversity as different facets of a great homogeneous goal.

### Modern Central European archaeology and its concepts

Essentially, modern Central European archaeology today bears the mark of a strong structural influence, independent of the fact that only few colleagues would admit to being open to structuralism.<sup>9</sup> Accordingly, the exploration of subsystems, such as economy and trade, society, ideological-religious spheres, production and technology etc., is a central goal of research interests, as the topics covered in published works in Central Europe reveal. In addition, one cannot overlook the increasingly important role given to human-nature and/or culture-nature interactions. In order to be able to synthesise the different subsystems, the natural sciences provide indispensable support. Only the synthesis of the single fields and their mutual dependence allows the systematic recognition of the overall structure. Thus, modern Central European archaeology is characterised by an "integrated" consideration of culture in its landscape setting.

This goal can be reached only if there are precise conceptions. I would like to make use of the – strongly schematised and perhaps somewhat simplified – conceptions of Renfrew and Bahn put forward in the second edition of *Archaeology* (RENFREW – BAHN 1996), which I understand as key concepts of "Processual Archaeology".

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<sup>9</sup> With this I do not wish to refer to the rather ahistorical ideas of Lévi-Strauss but to look at the dynamic concepts of, for example, Michel Foucault, whose work, however, has up to now hardly been discussed in Central Europe.

– *Description and explanation*

In traditional archaeology an independent descriptive stage is a necessary precondition for continuing reflection, but by no means is it an end in itself or final goal. Central European archaeology today doesn't aim at simply reconstructing the past but in fact it would rather supply explanations for changes on the most varied levels. The structuralist viewpoint leads away from monocausal explanations to a complex web of causalities.

– *History as an historical process*

The modern historical interpretation, which has been influenced by French and other structuralists and – since the 1980s – has also been established in Germany, caused modern culture history to be interested predominantly in evolutionary and historical processes. The explanation of how conditions change is more important than the description of conditions.

– *Balancing scientific discoveries and limitations in time and/or finances*

For quite some time, archaeological excavations have no longer been carried out in order either to enlarge the database of particular interests of the excavator or to discover the latest sensations. So there is a close connection between the social role of heritage management – on the basis of its legal guidelines – and archaeology. At least since the 1970s research projects and/or the duties of monument protection services have been dependent upon economical considerations. In this case costs and scientific results are usually weighed up. Clearcut scientific research goals and transparency, however, are given prominence. Large-scale linear projects or opencast mining in particular led to completely new approaches.<sup>10</sup>

– *Combination of quantitative and qualitative methods*

As has been discussed above, there meanwhile exists a tradition of combining computer-supported and qualitative processes (see also ZIMMERMANN in this volume).

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<sup>10</sup> The assumption that Continental European archaeology would carry out its research haphazardly with only the goal to generate more information which might not be relevant, as formulated by Renfrew and Bahn, shows ignorance of the actual state of affairs.

– *Interdisciplinary approaches*

Not only the co-operation and crucial debate with the natural sciences but also with other humanities are a precondition for modern archaeological research.

– *Speculation*<sup>11</sup>

Since the end of the 1950s at least (thanks to such traditional archaeologist as G. Kossack and R. Hachmann; see STOCKHAMMER in this volume), Central European archaeologists have devoted themselves not only to problems of social structure, but also to the symbolic and the cognitive spheres.<sup>12</sup> However, it is revealing that neither these works nor those of the numerous other colleagues who have done similar research since then have not been received in the Anglo-American circles (see BINTLIFF in this volume).<sup>13</sup> Since then one has shied away from other fields such as cult and religion or ethnic questions. Like every humanist, one ultimately returns in this case to his subjective viewpoint. The task is, however, always to attain transparency through a rigorously applied methodological concept. The final product of this speculative concept is the development of culture-historical models.

– *Consecutive work stages*

Comparing British and other European archaeologies, T. Madsen rightly underlined the fact that Danish archaeology belongs to Central European and/or continental-European archaeology. He caricatures the image that one has in England of traditional archaeology: ‘A British archaeologist is a person with a theory looking for data, whereas a Danish archaeologist is a person with data looking for theory’ (MADSEN 1995: 13). Subsequently he shows, however, that Danish archaeology is not by any means without theory, nor is it particularly lacking in method. Rather theory has a different status due to traditional methods. Madsen methodologically distinguishes three consecutive work stages (MADSEN 1995: 18):

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<sup>11</sup> This concept is lacking in Processual Archaeology as a result of methodological differences.

<sup>12</sup> The volumes of the *Acta Geographica et Archaeologica* give a good impression.

<sup>13</sup> What is even more worrying is that they are ignored also by younger German colleagues – see for example R. BERNBECK (1997) who misrepresents German archaeology either because important works are not known to the author, or else they are not cited because they do not fit into the picture.

- 1) data are initially categorised from the archaeological record
- 2) creation of data models
- 3) theoretical models.

Thus, theory and the development of models and/or the speculative level first enter at the end of the scientific process. The procedure Madsen outlined for Denmark is that of the culture-historical point of view dominating Central European archaeology. Recently, I myself proposed four steps which might be easily separated from one another:

- 1) Systematic survey
- 2) Data-inherent Analysis
- 3) Interpretive analysis
- 4) Model building

In both systems, the first stage is identical. It is based on the positivist systematisation of data.<sup>14</sup> Many colleagues from Anglo-American archaeologies would wrongly equate this stage with Continental European archaeology. Analytical methods begin with the corresponding second stage. This extends from the modern feature analysis to complex quantitative methods and first leads to purely mathematical-statistically based data models. According to my own opinion, interpretive analysis shifts from this level to the subsequent model formation. It is considerably more subjective, draws results from other sciences, makes comparisons, searches for analogies, raises questions, proposes answers, and attempts to supply explanations. Only on this basis a comprehensive theoretical cultural model can emerge. In particular, this does not supply an infallible, all-explanatory model but remains speculative and, thus, subjective to a certain degree.

It is unjustifiable to want to reduce Central European prehistoric research or archaeology in its entirety only to one of these stages. This does not mean, however, that personal interest will lead some colleagues to limit themselves mainly to one of these stages and – among other things – help to refine these methodically.

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<sup>14</sup> Positivist and systematic collecting and processing of the data in this case does not, however, mean that anyone believes to come to an objective recording by applying qualitative-quantitative data and mathematical and statistical procedures. Rather it is a matter of defining the subjective factor for the reader, and making it understandable.

## Central European archaeology in crisis?

I do not see the crisis of Central European pre- and protohistoric archaeology attested so often. Where there are different opinions regarding individual methodological terminologies, these result from progress and new discoveries that are not (yet) correctly assimilated. It is therefore primarily a question of problems with communication and understanding. Furthermore, there is a lack of syntheses which reasonably and comprehensibly bring together the individual facets of methodological development as well as theoretical diversity.

Starting out from the scientific literature, it might appear justified to accuse German-speaking archaeology of a certain “abstinence from theory.” It seems to me more correct, however, if this reproach were to be read so as to suggest that a lot of German colleagues do not consider it as necessary to pass on their theoretical opinions and foundations in writing.<sup>15</sup> This does not mean, however, that they lack a theoretical basis as a rule, nor that they are generally hostile to theory.

The misperception of the attitude towards theory results from the fact that many German-speaking colleagues define themselves only unwillingly and otherwise insufficiently in terms of methodology and theory. They obviously believe it is not necessary to discuss their means of understanding. Statements such as ‘You don’t talk about methods, you apply them,’ or ‘Why should I talk about stratigraphy since every archaeologist presumably knows what stratigraphy is?’ clearly show what we have to work with.

One result of the old *Ordinariat* system of German university chairs was that many colleagues only felt responsible for their own school, that is, their own institute. The thinking in schools is, however, elitist and outdated. That which is not written down stays only within the circle of one’s own students. Traditionally our teachers were unready to write introductions, manuals or comprehensive syntheses, and to give the student an interregional viewpoint. This may start to change (e.g. EGGERT 2001; TRACHSEL 2008). One did not see one’s own peculiarities or did not want to see the connections. Exactly the latter I consider being a mistake, since one could have spared oneself and the discipline many discussions that are based partly on ignorance.

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<sup>15</sup> That said, by no means do I want this to be considered good practice.

Teaching within the German university system is not nation-wide and homogeneous in either form or content (BERTEMES – RIECKHOFF – SCHIER 2000). For some the special attraction of the German university landscape is, however, exactly this versatility. The danger in the *Ordinariat* system (which has, in the meantime, been swept away) lay, however, in the way that the individual full professor wanted to shape his pupils and thus treated knowledge selectively according to his own interests (cf. SOMMER 2002). It might therefore be true to say that theory or methods were not part of the teaching of some professors.<sup>16</sup>

The methodological potential has grown over generations in very different places and with different emphasis and interest. Handbooks in particular can bring together the various facets of this wealth reasonably, so common characteristics can be brought out, scrutinised and discussed. Particularly in the Anglo-American discourse we find no sympathy for the fact that for several generations no German-language handbook worthy of the name had been published since H.-J. Eggers' *Introduction to Prehistory* (EGGERS 1959).<sup>17</sup>

In addition to manuals, courses of studies in a more standardised nation-wide way are a second requirement to get the discipline out of the training crisis. A main shortcoming was, however, the uneven training guidelines, particularly in Germany. Our partner universities in Poland, the Czech Republic, Hungary and also in Scandinavia have long ago taken steps towards a "standardised" training when they converted their traditional courses of studies into modularised courses of studies with each module including supra-regional examples. This allows a more standardised training, and the courses in Germany that were and still are modularised due to the "Bologna process" in future may meet up with this requirement.

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<sup>16</sup> I know from own experience, however, that there were university institutes in which philosophical-methodological basics were negotiated in undergraduate courses, social theories discussed, and culture terms crucially scrutinised. In the same way students were confronted with the works of Malinowski or Lévi-Strauss as well as with the ideas of modern behavioural research. During my study in Saarbrücken the "culture concept" was the focus of a graduate seminar (see STOCKHAMMER in this volume). A number of the students' seminar papers were published (HACHMANN 1987).

<sup>17</sup> Several handbooks have been published in recent years, e.g. EGGERT (2001; for a critical review see HÄNSEL 2001), EGGERT – SAMIDA (2009), TRACHSEL (2008).

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**‘... dem Romanismus entgegenzutreten’:  
National Animosities among the Participants of the *Congrès  
International d’Anthropologie et d’Archéologie Préhistoriques***

NILS MÜLLER-SCHEESSEL

**Abstract**

Concentrating on the international congresses on prehistoric archaeology, this paper traces the national animosities between the European – especially German and French – scholars of the 19<sup>th</sup> and early 20<sup>th</sup> century, in order to obtain a fuller understanding of the foundations of the discipline of prehistoric archaeology.

In the 19<sup>th</sup> century, the session of the *Congrès international d’anthropologie et d’archéologie préhistoriques* in Paris in 1889 is particularly telling: the atmosphere was so heated, partly because this session paralleled the Exposition universelle of 1889, that even such an open-minded scholar like Rudolf Virchow refrained from participating. Other problems like the obligatory French language contributed to the on-going animosities.

While the language problem was finally solved at the beginning of the 20<sup>th</sup> century, World War I added a further, extreme note of dissonance to the international conferences: After WW I German scholars and former German allies were banned from the CIAAP. This situation was seen as unbearable even by non-Germans, and finally led to the birth of a purely archaeological congress: the *Congrès International des Sciences Préhistoriques et Protohistoriques*.

This development certainly assisted in establishing prehistoric archaeology as an academic discipline in its own right. On the other hand, much of theoretical and methodological value has been given up by loosening the bond to anthropology. It is certainly no coincidence that the main proponents of such a pure prehistoric congress were German archaeologists and that it were – on the other hand – British scholars who were willing to prolong the cooperation with anthropology.

**Keywords**

International congresses, history of archaeology, chauvinism, World’s fairs, German archaeology

## Introduction

The *Congrès international d'anthropologie et d'archéologie préhistoriques* (CIAAP) is a sadly underrated subject in the research on the history of archaeology<sup>1</sup>. “Normal” historical accounts of the latter are usually confined to important finds and individual archaeologists while the networking of amateurs and professionals in and with the help of the institutions of their time is mostly glossed over (cf. CHAPMAN 1989). This is also true for the CIAAP. Already its contemporaries have emphasised in numerous accounts the important role the congress played in their eyes. These meetings were substantial events where the latest archaeological knowledge was exchanged. Personal networks were built up, encouraging communication between scholars working in the same areas of research. Travelling to the congresses itself was used for visiting museums as the only means then available to gain a proper knowledge of archaeological material. From a historical point of view, the congresses are invaluable because they give a very clear picture as to which questions were at the fore at that time and who were the leading figures.

However, instead of concentrating on the latter questions, this paper is concerned with the relationship between the participating scholars of different nationality before and after World War I. Focusing especially on French and German scholars, it becomes apparent that the process of the European institutionalisation of archaeology was fused with nationalistic sentiments and deeply rooted prejudices. This paper seeks to explore these relationships in order to obtain a fuller understanding of the foundations of the discipline of archaeology.

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<sup>1</sup> Up to now, historical treatments have been mostly descriptive and superficial (CAPELLINI 1907; COMAS 1956; DE LAET 1971; RICHARD 1992: 193ff (in a broader context); NENQUIN 1996). The papers of S. WIELL (1996; 1999: Copenhagen 1869), Chr. M. GOVI (1996: Bologna 1871) and M. PALLOTINO (1996: Rome 1962) deal with particular sessions of the CIAAP and the CISPP. Though, with the exception of Wiell's papers, they also lack a critical analysis of the events in question. – KAESER (2002) discusses ‘the international roots of prehistory’ as evident in these early congresses. – For a more in-depth treatment of the international congresses see now BABES – KAESER (2009) and here especially SOMMER (2009).

## The beginning and height of the *Congrès international d’anthropologie et d’archéologie préhistoriques*

The history of the CIAAP began in 1865 when a group of participants of the congress of the Italian Natural Sciences Society in La Spezia who were interested in archaeology and anthropology decided to found an international congress on prehistoric anthropology and archaeology (*Fig. 1*)<sup>2</sup>. The Congress had its first session in Neuchâtel in Switzerland one year later. That means that – with few exceptions – even before national societies or specialised journals had been formed, the CIAAP was founded: in essence, Prehistoric Archaeology was an international science from its beginning<sup>3</sup>.

In most cases subscription to and participation in the congress were quite steady, as *Figure 2* shows: mostly, between 400 and 600 individuals subscribed to the sessions and probably half to two-third of them also participated<sup>4</sup>. If we look at the development of the participation of different nationalities, interesting patterns are distinguishable. The participation of Italian scholars, for example, was very steady (*Fig. 3*). Around 30 Italians subscribed to each session and 5 to 10 took part. The high number of the congress in Bologna, in Italy, is not very surprising. It was also probably just a matter of distance in terms of travelling time that led to a low Italian turn-out at the session in Moscow in 1892. Scholars from Great Britain and Ireland, on the other hand, always subscribed in great numbers, but eventually did not turn up (*Fig. 4*). In most cases, this was again probably only

<sup>2</sup> See note 1 for overviews on the history of the CIAAP and of its successor, the CISPP. – *Figure 1* shows the dates of the CIAPPs along with some important archaeological and international events. The latter lists are not meant to be exhaustive.

<sup>3</sup> HUBERT (1900); cf. A. RASMUSSEN (1989: 28f). – For comparison, the first true international congress of historians was only held in 1900 (ERDMANN 1987: 26ff).

<sup>4</sup> The high number of individuals who subscribed to the session in Stockholm is interesting; this observation will be discussed below. – Sometimes, it was impossible to obtain the exact number of participants. Thus, a missing bar in figures 2–8 does not necessarily mean that no-one participated. For Neuchâtel 1866: no counts available; for Paris 1867: ANON. (1868 [1969]: 25); for Norwich/London 1868: ANON. (1869 [1969]: XXVff); for Copenhagen 1869: SCHMIDT (1874 [1969]: XIVff); for Bologna 1871: CAPELLINI (1873: XXIIff); for Brussels 1872: ANON. (1873: 38ff); for Stockholm 1874: HILDEBRAND (1876: 984ff); SCHAAFFHAUSEN (1874: 274); for Budapest 1876: ROMER (1877: XIff); for Lisbon 1880: DELGADO (1884: XVIIff); for Paris 1889: ANON. (1891 [1969]: XVff); for Moscow 1892: ANON. (1892: 17ff); for Paris 1900: ANON. (1902 [1970]: XIVff); for Monaco 1906: VERNEAU (1907: XLIVff); for Geneva 1912: DEONNA (1912 [1969]: 31ff).

CIAAPs		Archaeological Events	International Political Events
1860		1863 Napoleon III creates Saint-Germain Museum	1860 Unification of Italy (Kingdom 1861)
		1864 G. de Mortillet founds the journal "Matériaux pour l'Histoire positive et philosophique de l'Homme"	1864 German-Danish War
1865	Spezzia 1865 Neuchâtel 1866 Paris 1867 London/Norwich 1868 Copenhagen 1869	1866 Journal "Archiv für Anthropologie"	1866 Prussian-Austrian War
		1869 "Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte"; Journal "Zeitschrift für Ethnologie"	1867 Northern-German Federation; double-monarchy Austria-Hungary; Exposition universelle in Paris
1870	Bologna 1871 Brussels 1872 Stockholm 1874	1870 "Deutsche Gesellschaft für Anthropologie, Ethnologie und Urgeschichte" and "Wiener Anthropologische Gesellschaft"	1870/71 German-French War 1871 Formation of German Empire
1875	Budapest 1876	1871 "Mitteilungen der Anthropologischen Gesellschaft Wien"	1873 "Weltausstellung" in Vienna
		1875 Ecole d'Anthropologie in Paris	1875-77 Uprisings in Herzegovina and Bosnia, Macedonia and Bulgaria against Turkish government
1880	Lisbon 1880	1878 International Anthropological Congress in Paris during Exposition universelle; "Musée du Trocadéro" founded	1877-78 Russian-Turkish War; related international congress in Berlin
		1882 Archaeological department in the "Naturhistorisches Museum" in Vienna	1879 Alliance between Germany and Austria-Hungary
1885		1886 "Museum für Völkerkunde" in Berlin	1881 Alliance between Russia, Austria-Hungary and Germany; Tzar Alexander II. assassinated
	Paris 1889	1890 Journal "L'Anthropologie"	1882 Italy joins alliance between Germany and Austria-Hungary
1890	Moscow 1892	1882 "Reichs-Limes-Kommission" in Germany	1884 Start of German colonialism
		1893 International Anthropological Congress in Chicago during World's Fair	1886 International Bulgarian crisis; French minister of war G. Boulanger (until 1887) propagates revanchist policy against Germany
1895			1888 "Dreikaiserjahr" in Germany; "Boulangism" in France gains wide support (on its height in January 1889)
			1889 Exposition universelle in Paris
			1890 Downfall of German chancellor O. v. Bismarck
			1892 Russian-French military convention
			1896 Declamatory beginning of German "Weltpolitik" by Wilhelm II.
1900	Paris 1900	1898 Death of de Mortillet	1901 Death of Queen Victoria; attempt at communication between Germany and Britain finally failed
		1899 M. Hoernes Professor for Prehistory at the University of Vienna	1902 British-Japanese alliance
1905	Monaco 1906	1902 Death of R. Virchow; "Römisch-Germanische Kommission des Deutschen Archäologischen Institutes"	1904 "Entente cordiale" between Britain and France
			1904-05 Russian-Japanese War
			1907 Tripel-Entente between Britain, France and Russia
1910	Geneva 1912	1910 Prince of Monaco establishes Institut de Paléontologie humaine	1908-1909 International crisis after annexation of Bosnia-Herzegovina by Austria-Hungary
			1912-13 First Balkan war between Montenegro/Bulgaria/Serbia/Greece and Turkey
			1914-1918 World War I.

*Fig. 1. Concordance of CIAPPs and important archaeological and international events.*

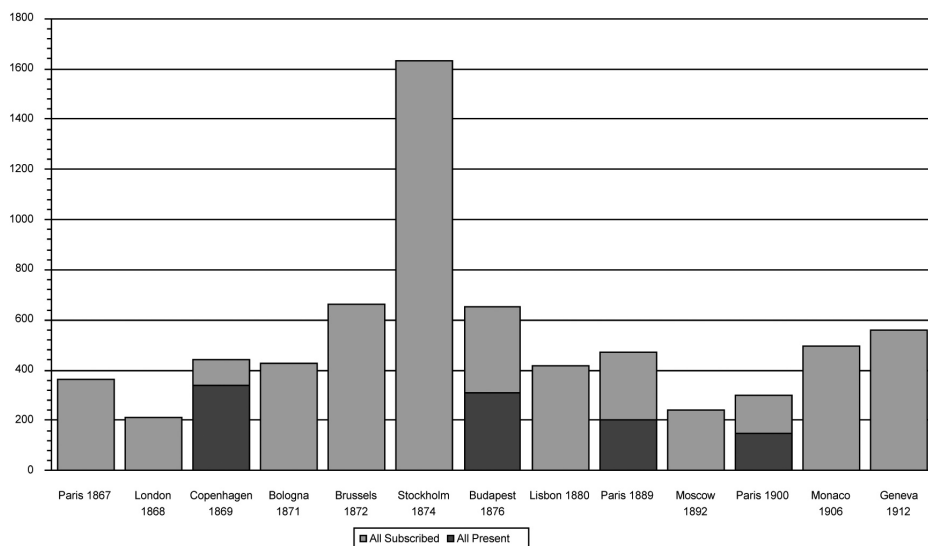


Fig. 2. Subscription to and participation in the CIAPPs.

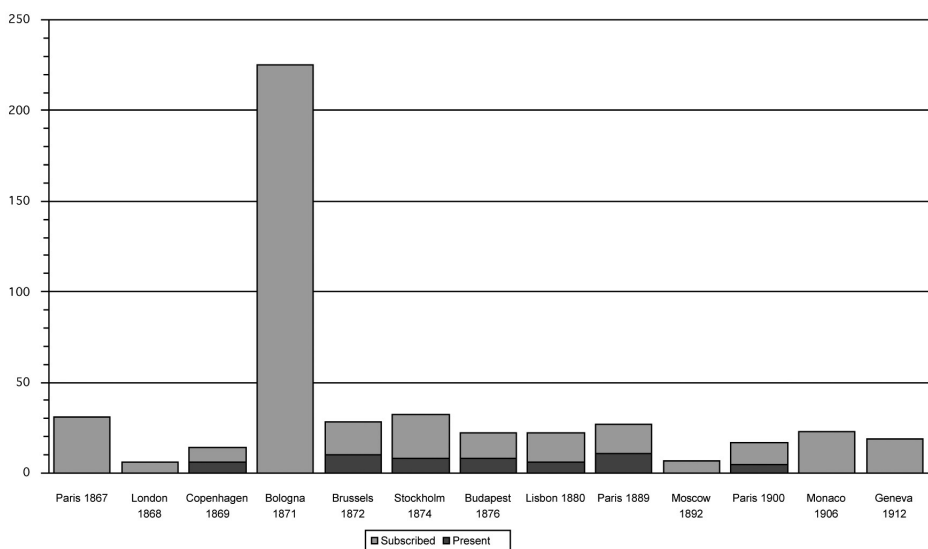


Fig. 3. Subscription and participation of Italian scholars in the CIAAPs.

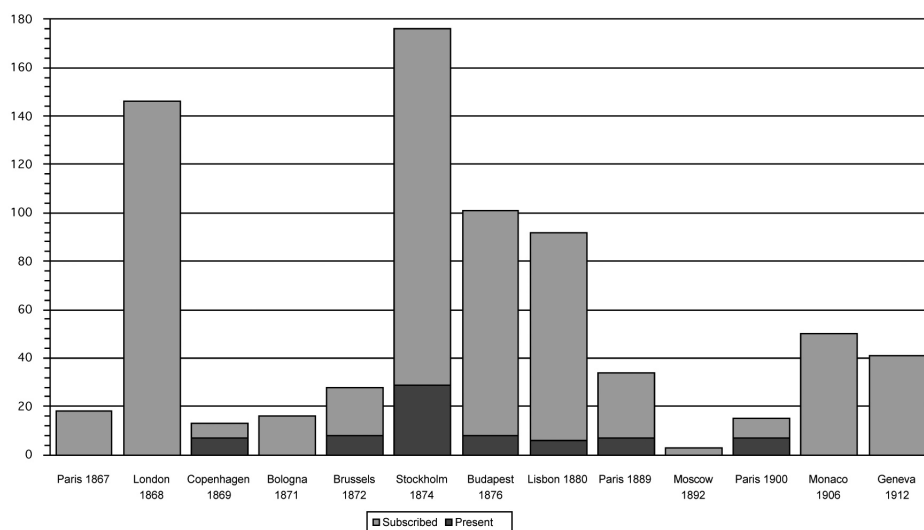


Fig. 4. Subscription and participation of British and Irish scholars in the CIAAPs.

a matter of distance. In some instances, political uncertainties might have also contributed to low numbers as in the case of the session in Budapest in 1876. One observer suspected that the unstable situation in the countries nearby (uprisings in some of the Turkish provinces) frightened some would-be-participants away (KOLLMANN 1877: 10).

What is clear, however, is that nationalistic sentiments always played an important role throughout the history of the congress<sup>5</sup>. For example, the attempts of Austrian and German scholars to stage a session during the *Weltausstellung* of 1873 in Vienna probably failed on political grounds: During the session in Brussels in 1872, German, Swiss and Austrian delegates<sup>6</sup> tried to get a declaration through, which aimed at pinning down Vienna as location of an extraordinary session<sup>7</sup>, but the majority of the delegates declined their request (FRAAS 1872: 483). H. Schaaffhausen had already suspected that the French would obstruct any such attempt (H. Schaaffhausen in: ANDREE 1976b: 442).

<sup>5</sup> See WIELL (1999: 142) for the session in Copenhagen.

<sup>6</sup> The declaration had been prepared by H. Schaaffhausen (German), was signed by him, R. Virchow (German), E. Desor (Swiss) and H. von Dechen (German) and was put forward by G. Graf Wurmbrand (Austrian) (H. Schaaffhausen in: ANDREE 1976b: 442; FRAAS 1872: 483).

<sup>7</sup> At this time, the congress was staged every two years.

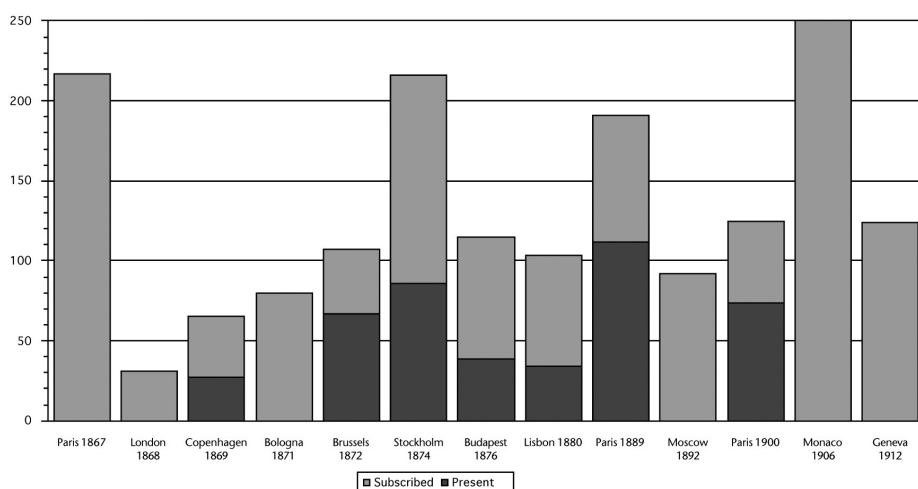


Fig. 5. Subscription and participation of French scholars in the CIAAPs.

The problem started with the genesis of the congress itself. It was dominated by French scholars; this already becomes apparent in the sheer numbers: where the number of French participants is known, it comprises no less than 50 individuals (Fig. 5). Thus, one fourth to one half of all participants of the majority of congresses were French in nationality. In addition, French was the only language used for communication purposes<sup>8</sup>, which was made mandatory in 1871 in Bologna (CAPELLINI 1873: 329f.). The respective paragraph in the rules of the congress was only changed in 1912<sup>9</sup>. The inclination towards French as language was not only impractical, as many non-French scholars spoke French only shakily<sup>10</sup>, but also furthered anti-French sentiments. The German-speaking participants, in particular, lamented about the language problem (e.g. FRAAS 1872: 483; SCHAAFFHAUSEN 1870: 341f; 1876: 278) and futilely tried on at least two occasions to get a declaration passed that would allow more than

<sup>8</sup> Only the British managed to maintain their own language when the congress was held at Norwich and London in 1868 (ANON. 1869 [1969]).

<sup>9</sup> Along with the change towards several parallel sessions this was done with the explicit aim to render the congress more attractive (DEONNA 1912 [1969]: 6). – For the language problem see also COMAS 1956: 31f.

<sup>10</sup> This is at least claimed by the Germans; for example, J. KOLLMANN (1877: 11) emphasised the problems with the French language in his report on the session in Budapest.

one language.<sup>11</sup> Part of the problem certainly lay with the rules of the congress, which specified that declarations proposed during one session were not to be decided until the following congress. This mechanism made changes to the rules a difficult and time-consuming process, especially when the time-intervals between the CIAAPs began to grow. As many scholars agreed that it would on the one hand be highly inconvenient to allow all languages and on the other difficult to decide which languages should be allowed (e.g. MESTORF 1876), things were left as they were. However, more important seems to be the sheer number of French participants that – understandingly enough – did not want to part with their privilege. These attendance numbers, in turn, probably stemmed from the fact that in the second half of the 19<sup>th</sup> century French anthropology served as ‘a focus for a left-wing, antireligious presence in French science’ (HAMMOND 1980: 118)<sup>12</sup>. It thus became home of many individuals with a strong pro-evolutionary, anti-imperial and anti-clerical attitude like its leading figure Gabriel de Mortillet. Finally, many of the participants of the lesser powers in central Europe despised and feared Prussia and later on the unified *Deutsches Reich*. One event, which happened during the session in Copenhagen 1869, is especially telling in this regard: eventually, a banquet was held where selected scholars of the participating nations were expected to propose a toast. Everybody did this toast in French; however, when the turn came to the Germans, their proponent started to speak in German. Immediately, he was shouted down. In the end, a German Swiss had to give the toast in French, but this instance was clearly recognized as politically driven (WIELL 1999: 142f.; MEHWALD 1870: 241).

As a consequence of the persisting language problem, staging a session of the congress in Germany was impossible. When such an idea was brought up again in 1883 within the influential Anthropological Society of Berlin, the chairmen decided that such a suggestion was untimely as long as French was the only language allowed<sup>13</sup>.

However, anti-French sentiments were not confined solely to the Germans. For example, in course of the preparation of the session in Stockholm in 1874 the well-known Swedish scholar Hans Hildebrand wrote a letter to the German

<sup>11</sup> Copenhagen 1869 (WIELL 1999: 141f); Budapest 1876 (ROMER 1877: 20f).

<sup>12</sup> See also WILLIAMS (1985). – After 1900 anthropology suffered a sharp decline in importance (WILLIAMS 1985).

<sup>13</sup> VIRCHOW (1883). – For similar judgements see also: R. Virchow in: HERRMANN *et al.* (1990: 398 letter 394; FRAAS 1872: 483).

Rudolf Virchow, urging him to persuade as many of his fellow countrymen as possible to come to Stockholm with the following argument: he had heard that ‘the French will probably come to Stockholm in great numbers. Because of this, it seems to me more than appropriate if the German science is represented well; only then it would be possible to oppose the Romanism. It seems to me that up to now the French and Italians could view themselves as masters of the congresses with some right [...]’<sup>14</sup>.

In the light of this statement, it almost seems as if the high number of participants in the session in Stockholm (see above) was the result of a kind of national competition between the scholars in order to decide who had the most weight internationally.

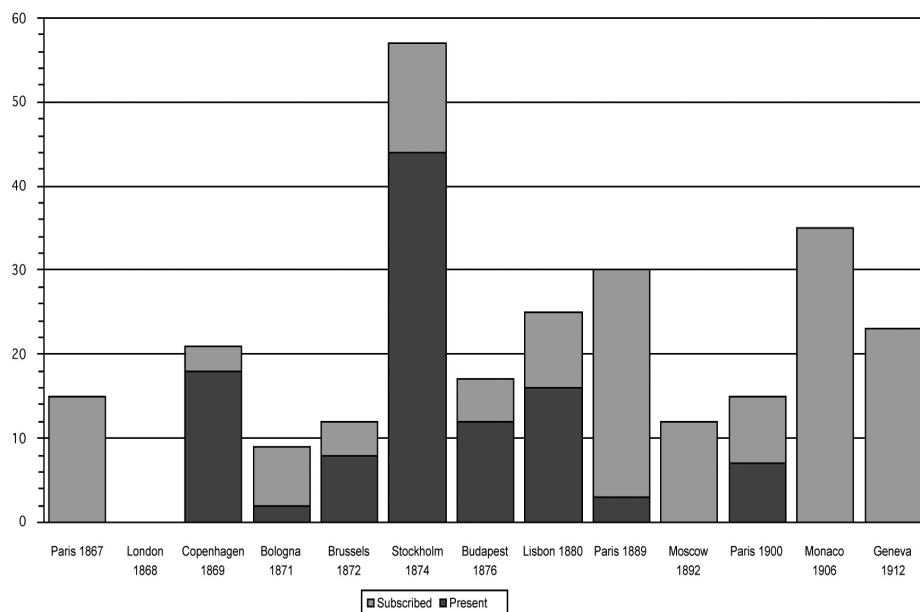
These general anti-French sentiments changed tune, though, when brought in connection with inter-state politics. For instance, relationships between German and French scholars were at zero after the war of 1870/71 between Germany and France, which had already led to postponing the session in Bologna (CAPELLINI 1873: XVI; GOVI 1996: 46). According to Virchow, the French participants in the session of 1871 in Bologna bluntly asserted that personal communication with Germans was impossible<sup>15</sup>.

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<sup>14</sup> Hildebrand habe gehört, ‘dass es wahrscheinlich sei dass die Franzosen sehr zahlreich nach Stockholm kommen werden. Es scheint mir deshalb, dass es mehr als gut wäre, wenn die deutsche Wissenschaft recht reich in Stockholm vertreten wäre; denn zuerst dann wird es möglich dem Romanismus entgegenzutreten. Bis jetzt konnten, scheint es mir, die Franzosen und Italiener sich als Herren der Congresse mit einiger [sic] Recht betrachten [...]’ (H. Hildebrand in: ANDREE 1976b: 228 letter 131 – all translations my own).

<sup>15</sup> ‘Sie werden davon gehört haben, dass die ziemlich zahlreich anwesenden französischen Gelehrten der Meinung waren, dass es auch auf einem internationalen Congresse keine Möglichkeit gäbe, persönliche Beziehungen mit deutschen Gelehrten zu unterhalten, und dass das Aeusserste, wozu man sich verstehen könne, nur ein äusserliches Nebeneinander sei, welches wohl gestatte, in einer Sitzung mit einander zu discutiren, aber es unmöglich mache, irgend welche persönlichen Berührungen zuzulassen. Herr de Quatrefages [...] erklärte mir rundweg: jamais de reconciliation! und einige andere seiner Landesleute zeigten mir ihre Abneigung gegen Deutschland in solchen Formen, dass die Italiener sich verpflichtet fühlten, ihren sympathischen Gefühlen gegen unser Land einen officiellen und nicht misszuverstehenden Ausdruck zu geben’ (VIRCHOW 1871: 139; cf. BASTIAN 1872: 46 note 1). It is very interesting, though, that in her lengthy report of the session in Bologna, J. MESTORF (1871) does

German anthropologists, too, could not free themselves from the political background of their time, as has already been touched upon above. In order to show this in more detail, I will now concentrate on one particular session of the congress – that of 1889 – and on the absence of German scholars during this session.



*Fig. 6. Subscription and participation of German scholars in the CIAAPs.*

The Germans were quite consistent in their participation habits. In most cases, between 20 and 30 individuals subscribed to the sessions, and 10–20 turned up (*Fig. 6*)<sup>16</sup>. However, 1889 saw only three Germans in Paris. Two of them were, at least at that time, totally unknown individuals<sup>17</sup>. The third was Heinrich Schliemann, discoverer of Troy, more cosmopolitan than German. Schliemann gave a quite enthusiastic description of the congress in question:

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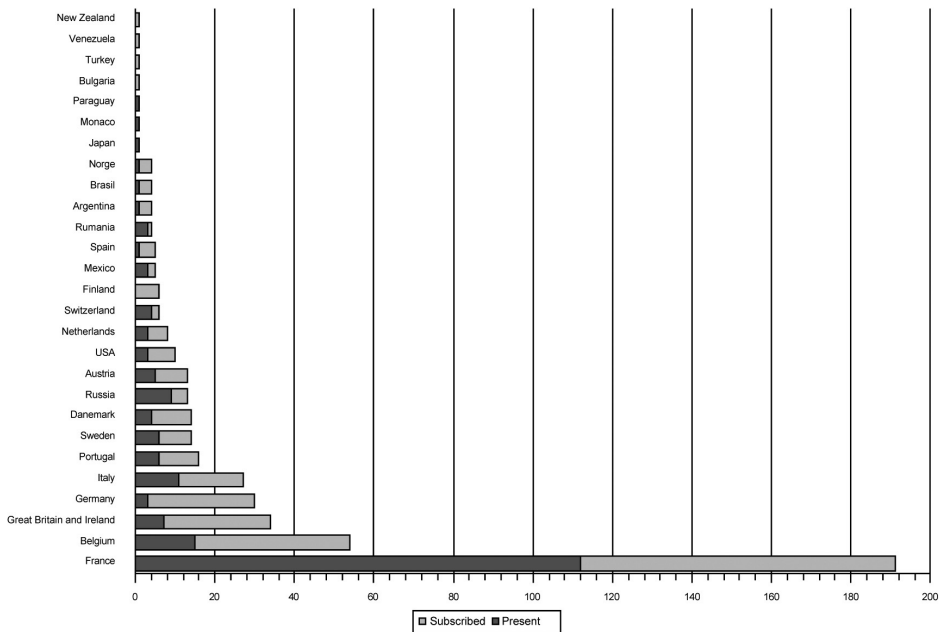
not mention these incidences. This might be due to the semi-official character of her paper which was directed to the Senate of Hamburg.

<sup>16</sup> For the total absence of German scholars at the session of 1868 in Norwich and London I can see no obvious reason.

<sup>17</sup> One of them, L. Stieda was an anatomist based in Königsberg; the other was the young R. Martin who later wrote the German standard handbook of Physical Anthropology (MARTIN 1914).

‘[...] because of the diversity of the questions brought forward, the great number of foreign scholars, the richness of the local collections [...] and much more, this congress is the most excellent and marvellous thing I have ever attended of this kind.’<sup>18</sup>

The absence of German scholars was striking and surprising, because the congress of 1889 was well attended (*Fig. 7*) and the members of the German Anthropological Society had been made aware of the archaeological potential of the *Exposition universelle* that ran parallel to the congress<sup>19</sup>. Ironically, the



*Fig. 7. Subscription to and participation in the CIAAP of 1889 in Paris.*

- <sup>18</sup> ‘Auch ist dieser Kongreß jedenfalls durch die Mannigfaltigkeit der vorgebrachten Fragen, die große Menge der von allen Ländern herbei gekommenen Gelehrten, die Reichhaltigkeit der hiesigen Sammlungen [...] und vieles andere, jedenfalls das Großartigste und Herrlichste, das ich je der Art mitgemacht habe’ (H. Schliemann in: HERRMANN u. a. 1990: 514 letter 538).
- <sup>19</sup> FRIEDEL (1889); *Zeitschr. Ethn.* 21 (1889: 355 [Verhandl.]). – As has already be pointed out by A. RASMUSSEN (1989: 28), of three congresses held between 1880 and 1900 two paralleled universal expositions. – For the significance of archaeology at world’s fairs in general see MÜLLER-SCHEESSEL (2001).

*Exposition universelle* to a large extent influenced the general political situation of that time, which in turn is probably responsible for the non-appearance of German archaeologists at the congress of 1889.

Apart from being ‘laboratories of modernisation’ (RYDELL – GWINN 1994: 1) world’s fairs were showcases for the participants. In this function, they had to withstand an open contradiction: the official doctrine was one of a friendly and peaceful competition between the participating nations. However, this contest often gained quite threatening undertones; these events were, of course, used to make politics, and the exposition of 1889 is a particularly telling case in point<sup>20</sup>.

More or less officially, the exposition of 1889 commemorated the anniversary of the French revolution of 1789. Even in France itself many people were unhappy with this coincidence as it was feared that the other European nations, most of them with a royal family as head of state, would not participate under these circumstances. Indeed, the European great powers declined the official invitation, including, for example, Great Britain, Russia, Austria and Germany. The typical French response was to blame German hegemony for the affront (HALL 1990: 110). The boycott of the exposition was in tune with the policy of the German chancellor Otto v. Bismarck to isolate France internationally, and there is some evidence that the German administration tried to influence the other governments in this direction (SCHROEDER-GUDEHUS 1989). The German boycott of the exposition was quite complete; Germany did not even send an art exhibition to the exposition as it had done at the one of 1878. There were rumours that the German emperor Wilhelm II had forbidden German army officers to visit the exposition (The Times 10/7/1889: 5b). ‘Yet when awards were distributed to exhibitors in September 1889, only the flag of the German empire was missing’ (HALL 1990: 110). Apart from Germany and a few minor countries, most nations had allowed private entrepreneurs to form committees to represent the respective nation semi-officially. Therefore, the German absence at the universal exposition of 1889 was an own goal. It was not France that became isolated, but Germany instead.

It seems that the participants of the CIAAP have drawn a connection between the absence of the Germans and the boycott of the exposition. The Hungarian F. PULSZKY (1889) reported that it was negatively commented on that no important German scholar was present. Rudolf VIRCHOW (1889: 728f.), chairman of the Anthropological Society in Berlin, admitted that the German non-appearance

<sup>20</sup> For the political background see POHL (1989); SCHROEDER-GUDEHUS (1989); MATHIEU (1989: 11ff). – For general accounts of the *Exposition universelle* of 1889 see also: HALL (1990); ANON. (1992).

had been perceived as a kind of insult in Paris. And the American anthropologist Otis Mason judged: ‘This pleasure [he refers to the opportunity of meeting distinguished scholars at the congress] was somewhat marred by the conspicuous absence of our German confreres, who had an excellent opportunity to show their magnanimity, and lost it’ (MASON 1890: 31).

Officially, the German archaeologists denied that the absence had anything to do with politics. VIRCHOW (1889: 728f.) gave the official reason for the German absence: according to him, too many congresses had been held at the same time and the program of the CIAAP had been advertised too late. Though it is true that the joined congress of the Anthropological Society of Vienna and the German Anthropological Society had been held only eight days before the CIAAP was scheduled to start, this argument is shaky on several grounds. Firstly, the French organisation committee explicitly claimed that the date of the congress had been advertised well in advance (HAMY 1891 [1969]: 13). Secondly, the Austrians and Hungarians, who had attended the same congress as the Germans a few days before, were not hindered by the temporal proximity of these two events. While most congresses were only attended by two or three Austrians or Hungarians, in 1889 five of them went to Paris (*Fig. 8*)<sup>21</sup>. And thirdly, Virchow himself travelled to Paris only a few days later to visit Schliemann and the exposition<sup>22</sup>. Therefore, the explanation of Virchow looks very much like an evasion. This is also implied by his friend Heinrich Schliemann who had been surprised by the French friendliness instead of open hostility he had feared. Schliemann suspected that a similar fear had frightened Virchow and the other Germans away<sup>23</sup>.

Considering the fact that Rudolf Virchow was one of the most important figures, probably even *the* most important figure in German archaeology and anthropology of the second half of the 19<sup>th</sup> century, it seems appropriate to stay a

<sup>21</sup> Austrian and Hungarian scholars are pooled together as it has been done in the proceedings of the session of 1889 (ANON. 1891 [1969]: XVII).

<sup>22</sup> See the letters in HERRMANN *et al.* (1990: 509ff letters 531ff).

<sup>23</sup> ‘Ich [...] bin von den französischen Gelehrten mit einer ganz außerordentlichen Zuvorkommenheit empfangen und mit einer warmen, innigen Freundschaft überschüttet worden, die mich umso mehr in Erstaunen setzte als ich eine kalte, abstoßende Behandlung erwartete. Auch Virchow muß eine solche erwartet haben, denn sonst wäre es unverstehlich warum er nicht zum Congreß gekommen ist, da er bestimmt darauf rechnen konnte hier höchst interessante Leute von allen Welttheilen zu treffen’ (H. Schliemann in a letter to v. Humbert, 31/8/1889, in: MEYER 1958: 318; cf. his letter to the editor in chief of an important German newspaper in: MEYER 1958: 394).

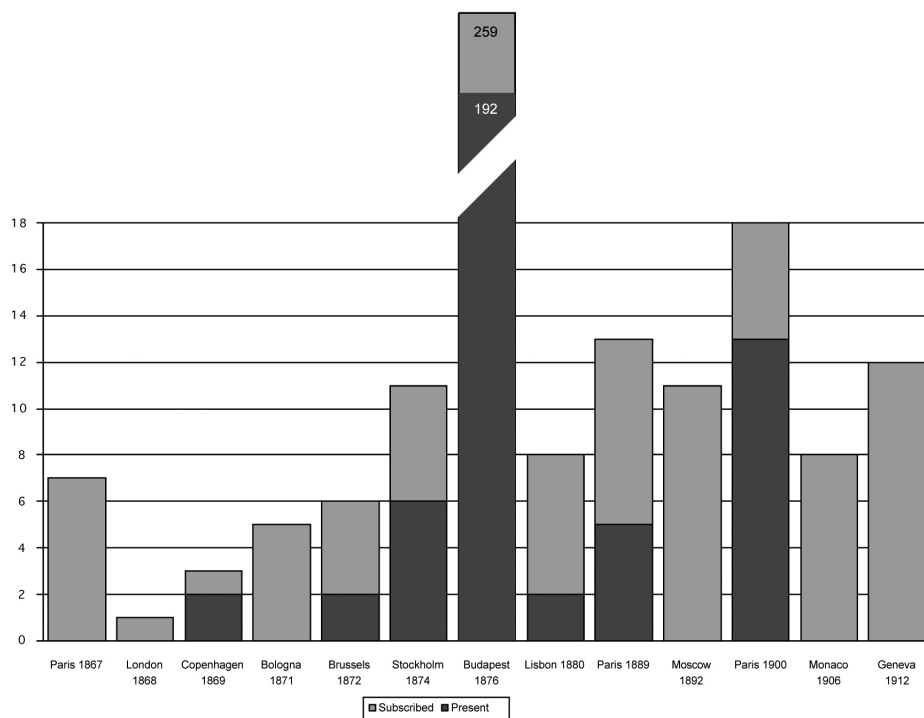


Fig. 8. Subscription and participation of Austrian-Hungarian scholars in the CIAAPs.

little bit longer with him<sup>24</sup>. Apart from his archaeological career, he was also an eminent physician and politician: for example, he co-founded – together with Th. Mommsen, among others – the liberal *Deutsche Fortschrittspartei* and was for many years a member of the *Reichstag*. His political and scientific world view was fused by the belief in the importance of the sciences for the physical and moral betterment of humankind. In this, he was – like de Mortillet – a child of the Enlightenment. In archaeological terms, the importance of Rudolf Virchow does not concern so much his archaeological findings, but relates to his position within the organisational structure of German archaeology. For many years he was chairman of the German Anthropological Society and the Anthropological Society of Berlin, and as such he decided on the location and content of their sessions. Furthermore, he edited several of the most important journals. He was

<sup>24</sup> Plenty of biographies on Rudolf Virchow exist, which are mostly concerned with either his medical or his political achievements (e.g. ACKERKNECHT 1957; BOYD 1991; SCHIPPERGES 1994). For his archaeological carrier see OTTAWAY (1973); ANDREE (1976a; 1976b).

sought out for advice for all sorts of problems by his many friends in Germany and abroad. In short, his dominating influence worked on several levels: on the one hand, he controlled the means of scientific communication; on the other hand, he was highly respected, even adored for his vast knowledge and his achievements in his other regions of interest. After all, it must be said that his influence was not a bad one but, on the contrary, helped to establish archaeology and anthropology as disciplines in their own right.

Apart from the sessions at London in 1868 and in Paris in 1889 Virchow never missed a session of the CIAAP between 1867 and his death in 1902. This makes his absence in 1889 even more suspicious. He certainly could have persuaded many of his colleagues to attend the congress as he obviously did so successfully in 1874 (see above), if he had wanted to. That this was never his intention becomes apparent if we study his private letters. In one of them, written to his friend Johannes Ranke on 8 November 1888, he complained:

‘The Parisians press me to get my consent for a revival of the international congress, of course in Paris next year. Under the current political circumstances I can do nothing, and I presume the other German colleagues will consider themselves as occupied, too. An international congress would be quite timely, but not in Paris.’<sup>25</sup>

Hence, it seems that Virchow not only did nothing to encourage his colleagues to attend the session in Paris in 1889, but that he openly objected their participation. Though Virchow certainly was no nationalist, his objection had obviously to do with the political situation. Presumably, in the light of the boycott of the German Empire of the *Exposition universelle* of 1889, he either saw his projects and ambitions threatened<sup>26</sup> or – as Schliemann suspected – he feared the French hostility as a supposed reaction to the German boycott.

<sup>25</sup> ‘Die Pariser bohren an mir, um meine Zustimmung zu einer Erneuerung des internationalen Congresses, natürlich in Paris im nächsten Jahre, zu haben. Unter den jetzigen politischen Verhältnissen kann ich nichts dazu thun, u ich denke, auch die übrigen deutschen Collegen werden sich als unfrei betrachten. Ein internationaler Congreß wäre ja sehr zeitgemäß, aber nicht in Paris’ (R. Virchow in: ANDREE 1976b: 413). – Virchow probably refers here to General Georges Boulanger and his revisionist anti-German movement which strengthened the German decision to boycott the exposition.

<sup>26</sup> An important event around that time was the founding of the *Museum für deutsche Volkstrachten und Erzeugnisse des Hausgewerbes* that Virchow had initiated (ANDREE 1976a: 41).

In conclusion, it can be fairly said that the absence of German scholars at the *Congrès international d'anthropologie et d'archéologie préhistoriques* in Paris in 1889 must be seen in relation to the boycott of the German Empire of the *Exposition universelle* of the same year and that this connection was also drawn by their contemporaries.

The question remains how this absence influenced the further development of German archaeology and its relation to other countries. The notes of discord that arose after the Germans did not attend the congress have already been referred to. However, it does not seem that these bad feelings were long lasting; at least, documents pointing in this direction are missing. In this vein, the absence had no great effect. On the other hand, the German absence can be seen as foreshadowing developments to come, developments that decided about the future course of international prehistoric archaeology.

### The decline of the *Congrès international d'anthropologie et d'archéologie préhistoriques*

The last session of the CIAAP before World War I had been held in Geneva in 1912, and World War I prevented the staging of its session in 1915 in Madrid. Instead, in 1918 the *Institut international d'Anthropologie* (IIA) was founded in Paris. It differed markedly from the CIAAP: though the IIA was international on paper, the permanent location of the council was Paris, and the domination by French scholars was even more prominent than in the old CIAAP<sup>27</sup>. Furthermore, it was openly declared that German scholars should be excluded from its organisations in reaction to World War I<sup>28</sup>. Only former allies of France and neutral nations were admitted to be represented in the IIA and were invited to its meetings (LAET 1971: 1425). The IIA held sessions in 1921 in Liège, in Prague in 1924, and in Amsterdam in 1927. Meanwhile, the public discomfort with the Institute and its meetings had grown considerably. Firstly, the exclusion of some scholars

<sup>27</sup> 'En un mot, la direction de l'Institut international d'Anthropologie était tout entière entre des mains françaises' (VAUFREY 1931b: 98).

<sup>28</sup> In a first announcement in the journal *Revue Anthropologique* in 1918 the aims of the *Institut* were formulated as follows: the institute should be a 'centre commun d'action [...], pour préparer et amener [...] un renouveau d'activité aux sciences anthropologiques et développer en elles le goût des recherches désintéressées, l'indépendance de pensée, la valeur intellectuelle et morale qu'elles auraient perdu sous l'influence néfaste de l'Allemagne [...]' (cit. in: VAUFREY 1931b: 98).

on political grounds was seen as unbearable and the general non-international character was criticised; secondly, many prehistorians had the feeling that the IIA and its sessions were too “anthropological” in nature, and did not see prehistory as represented well.

After an unsuccessful attempt to revive the old CIAAP for 1926 in Madrid, it was decided during the session of 1927 in Amsterdam to fuse the meetings of the *Institut* and the old CIAAP. This plan was approved by the surviving members of the permanent council of the CIAAP and raised new hopes (MYRES 1930: 37ff; BERSU 1931b: 113). However, the realization of this joined meeting in Coimbra and Porto (Portugal) in 1930 brought a big disappointment: prehistory was still not represented well and, perhaps even worse, the IIA was not willing to respect the wishes expressed during the session of 1927 and afterwards. The President of the Royal Anthropological Institute, J. L. Myres, went as far as stating that the ‘*Institut* had, in fact, swallowed the Congress, leaving only its distinguished name to adorn the title page of the report to be published eventually by the *Institut*’ (MYRES 1931a: 18).

Thus, in 1930 and 1931 a hectic time of private meetings and of exchanging letters started. Mainly, two initiatives were launched to change the situation which was felt as highly unsatisfactory. The first was initiated by J. L. Myres: prominently reflecting on the fate of the CIAAP (MYRES 1930; 1931a; 1931b; 1931c), he belonged to the “little committee” that was founded in order to negotiate the relations between the IIA and the CIAAP. In the meeting of the “little committee” in December 1930 he strongly argued for the separation of both IIA and CIAAP, and the members of the committee voted six votes to two for a separation. Afterwards, Myres also wrote to many scholars in Europe and abroad to find backing for his plans to force the IIA to go back to the situation as it was before 1927. He wanted to revive the former CIAAP in all its details, including the incorporation of more anthropological or ethnological topics in the programme. The support he got for his plans was impressive. However, the IIA delayed any direct response to his initiative until it was too late.

On 28 May 1931 a remarkable meeting had been held in the Historical Museum of Bern that changed the situation entirely. At this informal gathering it was decided to found a new independent international body, called *Congrès International des Sciences Préhistoriques et Protohistoriques* (CISPP). The idea

to hold such a meeting went back as far as 1927<sup>29</sup>. Already during the session of the IIA in Amsterdam the idea was brought forward amongst some of the delegates to organise a conference that would deal solely with prehistory. In April 1930 during the fifth session of the *Congrès International d'Archéologie Classique* a committee headed by P. Bosch-Gimpera was nominated in order to organise a prehistoric section for this congress. In October a small group of scholars met in Berlin to define how it would proceed. The meeting was followed by the aforementioned hectic exchange of letters to pin down a further gathering in February 1931 in Paris. This meeting was attended by G. Bersu, P. Bosch-Gimpera, R. Lantier, J. L. Myres, H. Obermaier, W. Unverzagt and R. Vaufrey. There it was decided to have the conference in Bern that resulted in the founding of the CISPP, a congress of and for prehistoric archaeologists.

Before returning to our main point, we might briefly consider the fates of the CISPP and the CIAAP in relation to each other: before World War II the CISPP held sessions at London in 1932 and Oslo in 1936. These were immediate successes among prehistorians. The CIAAP, still managed by the IIA, had sessions in Brussels in 1935 and in Bucharest in 1937. The session in Istanbul in September 1939 was cancelled because of World War II; the same happened to the session of the CISPP in 1940 in Budapest. However, after 1945 it was only the CISPP that happened to be revived; clearly, the CIAAP had become superfluous.

If we go back to the events that surrounded the formation of the CISPP in 1930 and 1931 there are several remarkable observations which merit special consideration. The first concerns the composition of the members of the meeting. From its geographical location it must have been clear from the outset that the meeting would be heavily biased towards scholars from Central Europe. Altogether, 28 individuals from 14 countries took part. However, from Germany and Switzerland alone came eight<sup>30</sup> and five scholars respectively. Furthermore, while some had taken the invitations as being personal (MYRES 1931d: 132), the German Association of professional prehistorians (*Berufsvereinigung deutscher Prähistoriker*) had sent in two of their members as official delegates (BERSU 1931b: 114 note 4). In the voting, every individual had one vote, notwithstanding the fact that some countries were grossly over- and others under-represented. Despite the claim of the director of the Römisch-Germanische Kommission, G.

<sup>29</sup> The successive events that eventually led to the founding of the CISPP are dealt with by DE LAET (1971: 1427f).

<sup>30</sup> Only six, if we do not count Hugo Obermayer, who spent much of his life in Spain, and Leonhard Franz, who was Sudeten German.

Bersu, that the assembly of Bern was authorised to represent all prehistorians<sup>31</sup>, the meeting had no legitimate power whatsoever. Given this bias and bearing in mind that the single largest group among the participants were the German archaeologists who had the most to gain, it is all the more surprising, as has already been pointed out by J. COMAS (1956: 56), that the atmosphere in Bern was apparently quite friendly<sup>32</sup> and that the agreement accomplished in Bern was widely accepted.

This can be ascribed to the careful manoeuvring of the Germans: during the gathering in Bern and beforehand they were very careful not to seem to be forcing the issue. The invitations for the Bern meeting were sent out by Bosch-Gimpera, though they had been printed in Frankfurt am Main. The location for the conference was Switzerland, not Germany, which probably would have been more convenient for many prospective participants. And lastly, during the Bern meeting the German delegates deliberately withheld their opinion<sup>33</sup>. The events around the session of the IIA in Porto had made it clear that pushiness would give reason for misinterpretation<sup>34</sup>. Despite this careful procedure, there were still some, especially French, scholars, who regarded the Bern meeting as driven by political considerations (e.g. BÉGOUEN 1931).

The other reason for the success of the meeting in Bern is more complex. The crisis of the CIAAP, which had been at first sight induced by the coup of the IIA, symbolised a far deeper conflict: what was at stake was the theoretical stance of international archaeology. By the 1920s, scholars whose theoretical orientation had been fused by anthropology and ethnology had been largely replaced by ones who were first and foremost prehistorians. The reasons for this development, which was not confined to Germany (for France: SACKETT 1981), are complicated and still await closer scrutiny. For Germany, two important

<sup>31</sup> 'Die in Bern Versammelten repräsentieren die legitimierten Fachvertreter der internationalen Vorgeschichtswissenschaft' (BERSU 1931c: 2).

<sup>32</sup> This fact is attested by the French R. VAUFREY (1931a), the British J. L. MYRES (1931d: 133) and the German G. BERSU (1931b: 115).

<sup>33</sup> 'Da [...] die Verhandlungen sich ganz in der Richtung der deutscherseits früher geäußerten Wünsche abspielten, wurde in der Sitzung seitens der deutschen Delegierten weitgehende Zurückhaltung geübt, um den Anschein einer deutscherseits inspirierten Konkurrenzgründung zu vermeiden' (BERSU 1931c: 3).

<sup>34</sup> G. BERSU (1931f) in a letter to R. Vaufrey: '[...] wie ich es überhaupt für gut gehalten habe, dass wir Deutschen uns in der Angelegenheit völlig zurückgehalten haben. Wie Ihre Erfahrung in Porto zeigte, bekommt eine von uns vorgebrachte Opposition bei bösem Willen sehr leicht einen politischen Anstrich'.

factors were the establishment of a nationalistic tradition on the one hand, and a stronger influence of the classics on the other. Both strands rejected the kind of holistic anthropology a scholar like R. Virchow had proclaimed<sup>35</sup>. Thus, one part of the European prehistorians wanted to separate prehistoric archaeology from anthropology. They were not satisfied with the representation of prehistory at the CIAAPs and wanted to establish a purely archaeological congress. The other part, still advocating an anthropological archaeology, wanted to revive the old CIAAP, though they also wanted to separate the CIAAP from the IIA by all means, even if this meant to loosen the connection to anthropology and ethnology.

By studying the table of events (*Fig. 9*) in connection with contemporary letters it becomes clear how the adherents of an anthropological archaeology were outmanoeuvred by the proponents of a purely prehistoric archaeology. This is testified by the series of meetings that bear an almost conspiratorial character and by the correspondence in the archive of the Römisch-Germanische Kommission. It is remarkable how on the one hand G. Bersu (1931d) appeased J. L. Myres, assuring him that he also wanted to reinstall the old CIAAP, and on the other hand left no doubt in his letters to kindred spirits that he wanted to do away with anthropology<sup>36</sup>. It also seems that the organizers of the Bern meeting were careful not to invite too many scholars who could have voted for a continuation of the old CIAAP<sup>37</sup>. However, perhaps the single most important reason that made founding a solely archaeological congress much easier for the adherents of a genuine archaeology was the ignorance of the leading members of the IIA: because of their stubbornness and unwillingness to negotiate, they made reasonable arguing for the proponents of an anthropological archaeology impossible. By clinging to the CIAAP, the IIA finally destroyed what it wanted to preserve<sup>38</sup>.

<sup>35</sup> See F. FETTEN (1998: 91ff; 2000) and W. KRÄMER (1978 [1982]) with interestingly differing points of view on the anthropological tradition of archaeology.

<sup>36</sup> 'Praktisch wäre ja überhaupt die Anthropologie bei der Gelegenheit [des Berner Treffens] ganz auszuschiffen' (BERSU 1931e).

<sup>37</sup> 'Einen gewissen Wert habe ich darauf gelegt, dass Thilenius nicht nach Bern eingeladen wird. Thilenius ist nämlich durch seine lange Verbundenheit mit der Deutschen Anthropologischen Gesellschaft derjenige, der in Deutschland am stärksten für die Vereinigung von Urgeschichte mit Anthropologie und Ethnologie im weitesten Sinne eintritt, und wir hätten dadurch, wenn er nach Bern eingeladen wird, uns einen Vertreter jener Richtung auf den Hals gelockt, die gerade für das eintritt, was wir vermeiden wollen' (BERSU 1931a).

<sup>38</sup> It is interesting to note how far the events surrounding the World Archaeological Congress in 1986 over fifty years later (which finally led to the founding of WAC

1918	Institut international d'Anthropologie (IIA) founded in Paris
1921	IIA in Liège
1924	IIA in Prague
1924	Committee nominated for organizing a session of the CIAAP for 1927 in Madrid
1927	IIA in Amsterdam
April 1930	Committee nominated for organizing a prehistoric section of the „Congrès International d'Archéologie Classique“
September 1930	IIA/CIAAP in Coimbra and Porto
February 1931	Meeting of G. Bersu, P. Bosch-Gimpera, R. Lantier, J. L. Myres, H. Obermaier, W. Unverzagt and R. Vaufrey in Saint-Germain/Paris
May 1931	Meeting in Bern and founding of the „Congrès International des Sciences Préhistoriques et Protohistoriques“ (CISPP)
September 1931	IIA/CIAAP (continued) in Paris
1932	CISPP at London
1935	IIA/CIAAP in Brussels
1936	CISPP in Oslo
1937	IIA/CIAAP in Bucharest
September 1939	Cancelled session of the IIA/CIAAP in Istanbul
1940	Cancelled session of the CISPP in Budapest
1950	CISPP in Zurich

*Fig. 9. Table of events after World War I.*

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[UCKO 1987; NENQUIN 1996]) mirrored the development described above (that eventually led to the founding of the CISPP). While the CISPP was partly directed against exclusion of certain scholars, WAC wanted to set an example by excluding prehistorians from South-Africa. On the other hand, the CISPP/UISPP stood for an archaeology that did not bother about anthropology, while WAC set topics on its agenda which had been neglected by the CISPP/UISPP (parts of them more or less anthropological in nature).

## Conclusion

The hostilities between German and French scholars in particular have a long history. Whether or not there was a direct connection between the animosities in the 19<sup>th</sup> and those in the 20<sup>th</sup> century as has been suggested by M. HEYDRICH (1929–32: 262) remains doubtful. However, it can be safely assumed that the poisoned atmosphere was mainly fuelled by the general political situation of the time. Despite the fact that prehistoric research was international in character from its very beginning (KAESER 2002; this volume), scholars were not able to free themselves from this climate. Thus, the animosities shown had great impact on the day-to-day scholarly communication: for example, it took several years after the German-French war of 1870/71 until G. de Mortillet and R. Virchow, who were very close in terms of biography and world view, again exchanged letters (ANDREE 1976a: 122).

If we compare this to the development following World Wars I and II as exemplified by the history of the Römisch-Germanische Kommission in Frankfurt am Main (Berichte RGK 82, 2001 *passim*), one cannot help but wonder how relatively quickly personal links were re-established after these two bitter events. This is true despite the fact that the political background was totally different for each: while after the treaty of Versailles official German policy was quite reserved towards France, the post-World-War-II Adenauer-Government deliberately strived for a better relationship with its neighbour. In the end, the question of scholarly contact in the face of detrimental political circumstances seems to boil down to the quality of personal relationships.

Still, while it is difficult to say whether or not the animosities of the 19<sup>th</sup> century had a long-lasting effect, those of the 20<sup>th</sup> had a very concrete outcome as to the institutional structure of international archaeology: it made founding a purely archaeological congress, the CISPP, much easier. By hindsight, this outcome can be seen with mixed feelings: it certainly assisted in establishing prehistoric archaeology as an academic discipline in its own right. On the other hand, a lot of theoretical and methodological value has been given up by loosening the bond to anthropology. It is certainly no coincidence that the main proponents of such a pure prehistoric congress were German archaeologists and that it were – on the other hand – British scholars who were willing to prolong the cooperation with anthropology. Although the foundation of the CISPP first of all reflected the crisis of the CIAAP and, indirectly, the decline of an anthropological archaeology, it probably also helped to further it – at least in Central Europe.

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# **Theories in German Archaeology. A Critical Discussion of Theoretical Aspects in the Work of Rolf Hachmann**

PHILIPP STOCKHAMMER

## **Abstract**

The theoretical work of the recognised German archaeologist Rolf Hachmann has largely been underestimated. From the 1940s onwards, Hachmann dealt in depth with the concept of culture in archaeology, inspired by the British functionalism of Malinowski and Radcliffe-Brown. This paper takes a close look at Hachmann's definitions of culture and history and analyses the operationalisation of his theoretical approaches in his evaluation of the Phoenician cemetery of Kamid el-Loz in Lebanon. Moreover, I evaluate the impact of New Archaeology on his work. Hachmann's later work is dominated by cultural philosophy. Based on the dichotomy between norms and values, he differentiates five historical stages of human development, which might be considered to be an outdated evolutionist approach. Nevertheless, his ideas can be seen as an attempt to give archaeology social relevance by stating that archaeology as a part of history is meaningful for the understanding of human existence.

## **Keywords**

Rolf Hachmann, culture concept, functionalism, cultural history, idealist philosophy

## **Introduction**

When confronted with the frequent and rather undifferentiated accusation of working without any theoretical foundation, German archaeologists reacted by enumerating the names of a same small number of scholars, like Günter Smolla, Karl J. Narr and Rolf Hachmann, over and over again. Even though German archaeologists thus acknowledge the existence of theoretical works, these publications apparently have not been discussed to the extent they deserve. Therefore I would like to take a critical look at the work of Rolf Hachmann and its explicit and implicit theoretical aspects, as he is one of the few German-language archaeologists who make their theoretical reflections explicit. This paper focuses on the genesis of some of Hachmann's most important theories and their practical implications for his analysis of archaeological material and its context.

Rolf Hachmann was born in Hamburg in 1917 and studied in Hamburg and Munich from 1945–1949. Having completed his dissertational thesis in 1949 and his *Habilitation* in 1956, he held a chair for prehistory at the University of Saarbrücken. Even after having retired in 1984 he is still active in archaeology and publishes the results of his on-going scientific work (e.g. HACHMANN – PENNER 1999; cf. PÖRTNER 1975: 489–490; KOSSACK 1994: 2–3; 17; KOSSACK 1997; PENNER 1997; FINKBEINER, personal communication).

### Hachmann's concept of culture, its genesis and definition

Already in his first publications, Hachmann intensively dealt with the *Kulturbegriff*, the concept of culture, following an explicit cultural-historical paradigm: 'If we consider prehistory as cultural history, how could anyone at all be able to write cultural history without having a concept of culture and of the functioning of cultural processes?' (HACHMANN 1987a: 10 – all translations are my own).<sup>1</sup>

In the understanding of his concept of culture, Hachmann explicitly follows to the British functionalists B. MALINOWSKI (1949) and A. R. RADCLIFFE-BROWN (1956; 1958). EGGERT (2001: 278) considers Hachmann to be one of the first German archaeologists who explicitly integrated functionalist theories into their work. This concept is basically more or less prevailing in most of Hachmann's work, from the 1950s until today<sup>2</sup>. In 1973 he defined it as follows:

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<sup>1</sup> 'Faßt man Vor- und Frühgeschichte als eine Kulturgeschichte auf, wie kann man dann eigentlich Kulturgeschichte schreiben, ohne zu wissen, was Kultur ist und in welcher Weise kulturgeschichtliche Prozesse funktionieren?' (HACHMANN 1987a, 10). Hachmann also refers to authors such as R. Thurnwald (HACHMANN 1950: 43, 45; 1956: 7; 1973a: 106), W. E. Mühlmann (HACHMANN 1950: 43; 1957d: 66; 1973a: 106) and C. Lévi-Strauss (HACHMANN 1969a: 577; 1973a: 106; 1973b: 536; 1975: 116; 1976: 119; 1982c: 160). However, he also refers to J. Piaget (HACHMANN 1975: 116, 142; 1976: 119; 1982c: 160; 1982a: 40; HACHMANN – PENNER 1999: 28–30), H. Rickert (HACHMANN 1950: 33, 36; 1977: 260; 1982b: 182–183; 1982c: 151; 1987b: 220; 1990b: 221), E. Cassirer (HACHMANN 1969a: 577; HACHMANN – PENNER 1999: 26) and last but not least to H. J. Eggers (HACHMANN 1950: 36) and V. G. Childe (HACHMANN 1976: 119; 1987b: 206–208). HACHMANN (1976: 119) admits: 'I am not able to explain these influences, but I feel them.'

<sup>2</sup> HACHMANN 1950: 45; 1957: 100, 118; 1969a: 577; 1973a: 105–106; 1975: 143; 1976: 119; 1982c: 160; 1982a: 40; 1987b: 208–209, 222; HACHMANN – PENNER 1999: 25–26.

‘Culture as a functional system can be subdivided into several parts such as, for instance, social organisation, economy, art, and religion, to list but the most important parts. These parts mutually influence each other and are partly of material, partly of immaterial character. [... Culture is an] inter-correlated operation of functions’ (HACHMANN 1973a: 82).<sup>3</sup>

In 1982, Hachmann presented a more elaborate concept of culture. Now, he distinguished between a static perspective, which considers and defines the structure of a certain culture as being made up by a number of different parts, such as its social organisation, its economy, religion etc., and a dynamic view, which analyses the functional correlations existing between these structural elements and the processes they cause. Hachmann sees culture as a dialectic relation between state and process, in other words: between structure and function, and thus as a complex entity, which cannot be understood in an additive way (HACHMANN 1982c: 159).

This holistic concept of culture exceeds the cognitive faculties of archaeology and thus calls for the study of ethnology. In Hachmann’s opinion, ethnology is able to identify the laws which are governing human behaviour by analysing working functional correlations of – in his words – ‘primitive’ societies. He postulates (HACHMANN 1957: 118):

‘While the functionalism of ethnology usually neglects historical aspects, a functionalist historical approach would not be impossible in prehistoric research!’<sup>4</sup>

Of course, this is only a rough summary of Hachmann’s concept of culture. However, a critical comparison with the concepts of those two ethnologists to whom he repeatedly and explicitly refers – i.e. Malinowski and Radcliffe-Brown – results in surprising findings. It is in fact only in Hachmann’s reference

<sup>3</sup> ‘Die Kultur als funktionelles Gebilde gliedert sich in verschiedene Bereiche, die Gesellschaftsordnung, die Wirtschaft, die Kunst, die Religion, um nur die wichtigsten zu nennen. Diese Bereiche [...] wirken wechselseitig aufeinander ein und beeinflussen sich gegenseitig [... und] haben teils materiellen, teils immateriellen Charakter. [... Kultur ist ein] Funktionszusammenhang’ (HACHMANN 1973a: 82).

<sup>4</sup> ‘Während der Funktionalismus der Völkerkunde historische Aspekte in der Regel nicht kennt, wäre in der Urgeschichtsforschung deswegen eine funktionalistisch-historische Methode nicht unmöglich!’ (HACHMANN 1957: 118).

to structural elements where he shows some relation to Malinowski's ideas. Malinowski's element of the central function of the satisfaction of basic needs, though, is not mentioned anywhere. More parallels can be found with Radcliffe-Brown's concept of culture, although the latter makes his concept the very core of his socio-anthropological method, to which he feels that archaeological research cannot make any contribution (RADCLIFFE-BROWN 1958: 98–99; cf. VEIT 2000: 81).

This means that Hachmann refers to a concept of culture which has been explicitly kept apart from archaeology by its author. But how could Hachmann apply a concept of culture, which was created by the two British functionalists on the basis of living cultures only and for their analysis, to the skeletonised remains of ancient human culture? These remains, i.e. the material culture consisting of objects and materialised social practices, are the essential sources for all archaeological research. Although attempts to apply this functionalist concept of culture to archaeology can easily fail, Hachmann succeeds in transferring one of the core concepts of functionalism to archaeological analysis: the focus on human social practices. Behaviour, seen from a functionalist viewpoint, forms the connection between the various structural elements and is at the same time the vehicle for functions that are essential for the continuity of social structure. Material objects are no longer central to Hachmann's discussion. He concentrates on materialisations of social practices, on fossilised customs, which he believes to be most easily identified in the interpretation of burial grounds (HACHMANN 1950: 36–37; 1957: 101–102). At the same time the reconstruction of past social practices is, in turn, the prerequisite for their analysis to thus allow for determination of functions and the interaction of those structural elements which they reflect.

### The application of Hachmann's concept of culture to archaeological sources

In order to analyse the customs fossilised in burials, Hachmann first of all makes an effort to clearly define the various customs that he subsequently tries to reconstruct. It would go too far here to discuss his categorisations in detail. However, his elaborate definitions are fairly singular in German archaeology.

Having worked at the tell Kamid el-Loz in the Lebanon for almost 20 years, Hachmann is particularly interested in the reconstruction of the Phoenician idea of afterlife. This he attempts by analysing the structure and function of the north-

western graveyard and reconstructing the various burial customs of Kamid el-Loz (HACHMANN – PENNER 1999: 169), based on R. POPPA's (1978) analysis of the material. However, Hachmann's functional classification of the various objects is not completely convincing. It appears that it implies quite some arbitrariness, as his categories are based on the mental attributes of the objects. While these attributes are certainly presumable, they cannot be proved and are therefore problematic when used as defining criteria (cf. HACHMANN – PENNER 1999: 174–177).

In a next step Hachmann combines his interpretation of the archaeological evidence with written sources about the eastern Mediterranean Iron Age, creating a lively picture, which remains hypothetical in many respects – as he explicitly admits –, but which at least focuses on man and his social practices (HACHMANN – PENNER 1999: 203–205, 237–239).

### Hachmann and the theoretical discussion in Great Britain and the USA

Hachmann's work gives the impression that he only casually dealt with the ideas presented by the representatives of American "anthropology". Yet, he does not completely reject their concepts and theories:

'A certain spatial distance from Europe, from European culture and old-world cultural history has led American "anthropology" [Hachmann here refers to J. Steward and G. Willey] to theorising over and over again and at the same time to do empirical research among primitive societies. The results of these efforts are relatively unknown in Europe and do certainly deserve more attention, but no schematic adoption' (HACHMANN 1973d: 250–251).<sup>5</sup>

Hachmann excludes New Archaeology's more radical representatives from this positive judgement, accusing them of excessively simplifying complex structures, of arbitrary interpretations and, above all, of totally ahistorical methods (HACHMANN 1982b: 184; 1987b: 216; 1990a: 845). However, he regards it as

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<sup>5</sup> 'Eine gewisse räumliche Entfernung von Europa, von europäischer Kultur und altweltlicher Kulturgeschichte hat die amerikanische "Anthropology" immer wieder zum Theoretisieren verleitet und zugleich zu empirischen Forschungen unter Primitiven geführt. Die Ergebnisse dieser Bemühungen sind in Europa vergleichsweise unbekannt und verdienen gewiß stärkere Beachtung, wenn auch keine schematische Übernahme' (HACHMANN 1973d: 250–251).

an advantage that New Archaeology stimulated the reflection upon the basics and conditions of scientific work which has resulted in scientific theories coming closer to reality (HACHMANN 1987b: 219). It is noteworthy in this context that in his *Studien zum Kulturbegriff* (HACHMANN 1987), Hachmann only includes L. R. Binford, alongside V. G. Childe, as non-German-speaking archaeologist. This shows that Hachmann regards Binford's concept of culture to be worthy of more intense consideration.

Apparently, American archaeology had little impact on Hachmann's functionalist theories. On the contrary, the roots of his approaches, as has been said above, can be found in the works of Malinowski, Radcliffe-Brown, but also his teacher H. J. Eggers. Like many contemporary Anglophone archaeologists, Hachmann took his inspiration directly from ethnology and adapted it to his own demands.

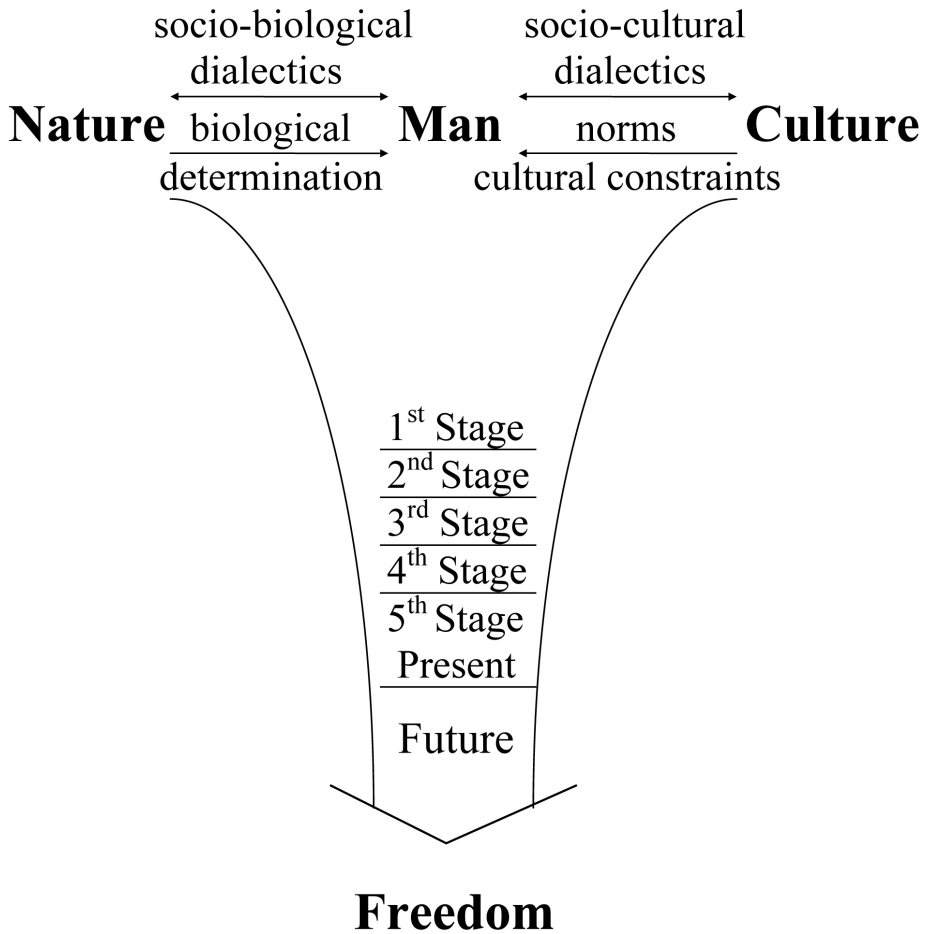
Until today, Hachmann's functionalist approaches found little attention in German-language archaeology, and even less abroad. One of the few who discussed Hachmann's concept of culture was Jens LÜNING (1972). This lack of interest may be due to several factors. Hachmann tended to publish his theoretical considerations in works the titles of which suggest a mainly empirical content (cf. HACHMANN 1950; 1973a; 1973b). Moreover, Hachmann has never published anything like a theoretical manifesto; many of his theoretically oriented essays were hardly accessible to English-speaking scholars (cf. HACHMANN 1957; 1983a). A more intensive discussion of the functionalist theories, which play such an important role in his early works, both by German- and non-German-speaking archaeologists would, however, be desirable.

### Cultural history and cultural philosophy: man and freedom

Hachmann's later work, especially since the early 1980s, is dominated by cultural philosophy: the question about the future of the past (*Fig. 1*).

In Hachmann's theories man is the central element.<sup>6</sup> In his specific character as creator and creation man belongs both to the cultural and the natural sphere. Consequently man holds a dialectic position between nature and culture. Hachmann adopts the concept of nature of H. RICKERT (1926: 18, 21) – a German philosopher of the late 19<sup>th</sup> and early 20<sup>th</sup> century – as being free of any value-

<sup>6</sup> Already in 1950 HACHMANN wrote (1950, 37): 'Das Interesse liegt beim Leben des Menschen. Das Objekt ist nur Indikator.'



*Fig. 1. Graphic illustration of my perception of Hachmann's cultural-philosophical theories.*

judgment, while culture is the creation of the judging man (cf. HACHMANN 1987b: 220; 1990b: 221; HACHMANN *et al.* 1992: 181–182). The category of value and the associated category of norm are thus added to his functionalist concept of culture (HACHMANN 1982c: 151, 161).

Man as both a product and a part of nature, which Hachmann subdivides into an animate and inanimate component, stands in a dialectic relationship to nature. In my opinion, this relationship could be regarded as “socio-biological dialectics”. In this context, man’s natural determination plays an important role; his genetic make-up and biologically determined behaviour constitute constraints

from which man cannot liberate himself on his own, in contrast to many environmental constraints of which he can free himself. Only the development of the human brain made it possible for man to partially overcome his animal instincts. Hachmann believes that it is necessary for a holistic understanding of the human being that physics and biology analyse both the inanimate and animate components of nature in order to determine their impact on human behaviour and thus on the cultural sphere as well. His approach is that especially natural sciences like brain research and language and developmental psychology may allow us to understand cultural behaviour (HACHMANN 1950: 33f; 1956: 14; 1982a: 39; 1982c: 160; 1983a; 1983b: 181; 1987b: 217–218; 1990b: 222–239; 1990c: 677–678; 1994: 73).

Of course, Hachmann focuses his studies on the other dialectical relationship, i.e. that between man and culture, which might be referred to as “socio-cultural dialectics”. These dialectics are based on the constant creation of culture by man. As part of this culture they establish values and norms, which, in turn, narrow down their natural freedom especially in the fields of religion and society (HACHMANN 1982c: 151; 1987b: 218; 1990b: 221).

For Hachmann, it is the task of ethnology and sociology to analyse the socio-cultural dialectics within the framework of cultural anthropology (HACHMANN 1990b: 221–222). Just like the natural sciences, cultural anthropology can only produce a static, ahistorical perspective. But the two dialectic relationships of man also involve a dynamic component, on the understanding of which cultural history should focus. HACHMANN (1982c: 151, 161) states:

‘Cultural history is the eternal antagonism between man’s natural tendency to normalise all values and his equally natural tendency to re-evaluate, i.e. to destroy all those norms. Man is free to do this or not. [...] Human cultural history is an endless and initially extremely slow and then gradually accelerating process which releases man’s will to act’.<sup>7</sup>

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<sup>7</sup> ‘Kulturgeschichte, das ist die ewige Antagonie zwischen der naturgegebenen Tendenz zur Normung aller Werte und der ebenso natürlichen Neigung zur Umwertung – d.h. zur Vernichtung aller Normen. Der Mensch hat die Freiheit, das zu tun oder es zu unterlassen. [...] Menschliche Kulturgeschichte, das ist ein unendlich langer und anfangs unsäglich langsam fortschreitender Prozeß allmählicher, sich dann nach und nach immer stärker beschleunigender Freisetzung des Willens zum Handeln’ (HACHMANN 1982c: 151, 161). In Hachmann’s work, the concept of norm is already present in 1969 (HACHMANN 1969b: 111).

Hachmann sees freedom as the driving force behind history, and only the awareness of freedom really makes free (HACHMANN 1982c: 160; 1990c: 679). In addition, freedom leads to human evolution in several stages, from the emergence of the first humans to the present. Hachmann therefore postulates the existence of five historical stages – the Palaeolithic and Mesolithic, the Neolithic and Chalcolithic, the period of the Eastern Mediterranean civilisations, the emergence of the Phoenician culture and the Yahweh religion, and the fifth period, which began with Greek culture and which is still lasting (HACHMANN 1982c: 151–155, 161–165; 1983b: 181–185; 1983a: 1993, 253; 1996: 288).

From my point of view this theory of stages embodies an old-fashioned and out-dated Eurocentrism, which considers the western industrial nations as the ultimate objective of all human development and is highly evolutionist. In the light of this it is all the more surprising that Hachmann repeatedly rejects evolutionist theories (HACHMANN 1982b: 208; 1983b: 179, 181; 1983a: 1; 1990a: 847; 1991: 708). Whereas Hachmann never explicitly refers to any existing models, it was in fact as early as 1837 that G. W. F. Hegel as a representative of an idealist historical philosophy saw historical development as being the progress made in becoming increasingly aware of freedom (cf. RÜSEN 1993: 99). HEGEL (1970) also already emphasised the significance of the Phoenicians, of the Yahweh religion and Greek culture with regard to the self-liberation of man from cultural norms. The historicism of J. G. Droysen adopted these ideas and considered them as a basis for further reflection (DROYSEN 1960: 238–239, 354; cf. RÜSEN 1993: 109, 223–224, 238).

Consequently Hachmann's theory of stages can be placed in the tradition of German idealist philosophy; he only expands this theory to the Stone Age and illustrates the other periods with new archaeological findings.

But what is Hachmann's objective in proposing this theory? Obviously, he tries to demonstrate archaeology's social relevance within the framework of history. The fact that Hachmann published his theory of stages four times in places which were considered to receive a large and also non-professional public points in this direction. This is certainly a significant and positive aspect of his theory. HACHMANN (1973c: 284) is still one of the few scholars who draw attention to the question of the importance of archaeology:

‘Why look back to the past? Don't we know enough about it simply by the fact that it has already gone by? Is it not primarily a way to explore the future? It is possible to make plans for the future – and time will show whether these plans will turn into reality. One cannot

investigate the future directly, but only indirectly. An investigation of the past reveals those conditions in which future developments will also take place. [...] Future research is the projection of man's experience with the past onto the unknown, which awaits him. The investigation into a considerable part of the human past by means of archaeology will not only help us to elucidate ancient eras, but it will also allow us to bring some light into the darkness that is ahead of us, and which we are facing as an uncertain fate.<sup>8</sup>

This concept for a better self-understanding of archaeology did not receive any critical discussion so far. Maybe this is due to the fact that Hachmann's image of archaeology has been published on some incidental occasions only, or maybe also for the reason that the kind of questions involved are easily asked, but not so easily answered.

## Conclusions

'We built up images. That is unloving, is betrayal'<sup>9</sup>, these are the words of the Swiss writer Max Frisch (1950: 32). Should we follow Max Frisch? Every image is admittedly subjective and therefore dangerous to some extent. But if we refrain from building up images the humanities will be deprived of their right to exist. I have attempted to outline Rolf Hachmann's concepts and theories in an image such as it can be derived from his scientific work, although this image can certainly not do full justice to Hachmann's persona.

Hachmann has rendered archaeology a highly valuable service by adopting various theories from other cultural sciences. His holistic concept of culture –

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<sup>8</sup> 'Was soll der Blick in die Vergangenheit? Wissen wir nicht mit der Tatsache, daß sie vergangen ist, schon von ihr genug? Handelt es sich nicht doch in erster Linie darum, die Zukunft zu erforschen? Man kann die Zukunft planen – und es muß sich dann zeigen, ob die Planung zur Realität wird. Man kann die Zukunft aber nicht direkt, sondern nur indirekt erforschen: Die Erforschung der Vergangenheit klärt die Bedingungen, aus denen sich die zukünftigen Entwicklungen ergeben und unter denen sie sich vollziehen werden. [...] Zukunftsforschung, das ist die Projektion der Erfahrungen des Menschen mit seiner Vergangenheit in das Ungewisse, das ihm bevorsteht. Ein beträchtliches Stück menschlicher Vergangenheit wird von der Archäologie erforscht, und es ergibt sich, daß dadurch nicht nur Vergangenes erhellt, sondern auch Licht bis in das Dunkel geworfen wird, das uns als ungewisses Schicksal bevorsteht' (HACHMANN 1973c: 284).

<sup>9</sup> 'Man macht sich ein Bildnis. Das ist das Lieblose, der Verrat' (FRISCH 1950: 32).

although rarely put into practice – focuses on man and analyses the materialised remains of man's social practices.

In contrast to his functionalist approach, the historical philosophy represented in his late work is based on an idealist tradition. One has to be careful about placing Hachmann's concepts and ideas within the different lines of thought in German archaeology, as an analysis thereof remains to be written. It should, however, be kept in mind that his evolutionist model is the way in which he tries to give archaeology a social relevance by stating that archaeology as a part of history is meaningful for the understanding of human existence. In this regard his theoretical considerations contribute to the appreciation of the value archaeological work has in modern society.

### Acknowledgements

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### Postscriptum

After presenting a version of this article at the EAA conference in 2001, I sent this article to Rolf Hachmann and asked for his opinion on my reading of his work. Having exchanged several letters, he invited me to his house in Saarbrücken, where I had the opportunity to discuss his life and his work with him for half a day on the 1<sup>st</sup> of December 2004. More than four hours of our dialogue were taped and later transcribed as a manuscript of nearly 80 pages – much too long to be included in this article. Instead, I would like to summarize some of the key points:

Of central importance to Hachmann's interest in ethnological theory was his scientific socialisation within the *Museum für Völkerkunde* in Hamburg, where the Prehistoric Department was housed together with cultural anthropologists. In the second year of his studies, Hachmann approached Franz Termer, director of the museum, and asked him for a topic to deal with during the semester break.

He was given the topic “Lässt sich der Funktionalismus auf die Vorgeschichte übertragen?” (“Can we transfer functionalism to Prehistory?”), which marked the starting point of his intense analysis of functionalist approaches in cultural anthropology. Unfortunately, he neither published nor kept this early paper. While dealing with functionalist theory, he recognized that his teacher H. J. Eggers also used such an approach and told him about his results:

‘One day I approached Eggers and told him that I had written some kind of text for Franz Termer; I was supposed to analyse whether functionalism existed in Prehistory, and I discovered that you are a functionalist. Eggers could not believe this at all – what was it supposed to be anyhow, functionalism. He was not interested in it, but the way he thought was actually functionalist.’<sup>10</sup>

Obviously, Eggers had developed his functionalist approach without knowing any related anthropological literature. In our discussion, Hachmann again differentiated between a static perspective, which he called structure, and a dynamic perspective, which he called function. The analysis of structures should be imagined like that of a thin section taken for scientific analysis, showing a sudden and static moment of a process whose dynamics only gather momentum by combining it with a functionalist approach.

When speaking about the origin of his cultural-philosophical theories, he first rejected the notion of Hegelian roots<sup>11</sup> but later admitted that Hegel’s ideas might have re-entered his thoughts via Droysen, who takes this idea from Hegel. He stressed the importance of DROYSEN’s (1960) *Historik* on his thoughts but rejected BERNHEIM’s (1908) work as being too positivistic. For Hachmann, the prehistorian always has to see the beginning and the future of phenomena; for him Prehistory is some kind of Futurology<sup>12</sup>.

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<sup>10</sup> ‘Dann bin ich eines Tages zu Eggers gegangen und habe zu Eggers gesagt, ich hab da so eine Arbeit für Franz Termer gemacht, ich sollte untersuchen, ob es Funktionalismus in der Vorgeschichte gibt, und ich habe entdeckt, dass Sie ein Funktionalist sind. Das konnte er gar nicht glauben, was das denn sei, Funktionalismus. Das interessierte ihn nicht, aber er hat funktionalistisch gedacht.’

<sup>11</sup> ‘Hegel mag ich nicht leiden’.

<sup>12</sup> ‘Vorgeschichte ist eine Art der Zukunftsforschung. Erst dann, wenn ich sehe, wie die Dinge so auf uns zugekommen sind, dann kann ich auch die Grenzen sehen, die sich für uns in Zukunft auftun werden’.

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# **Some Notes on the German Love-Hate-Relationship with Anglo-American Theoretical Archaeology**

GABRIELE MANTE

## **Abstract**

The 1980s and 1990s witnessed the rise of unprecedented debates in German-language archaeology. Several groups and individuals within prehistoric archaeology in Western Germany started to grapple with approaches that were developed and discussed in English-language publications. Despite the obvious differences in premises and/or terms used in diverse approaches in North American and British archaeology, these quite often were subsumed as “Anglo-American theoretical archaeology”. Therefore, the discussion did not centre on processualism vs. post-processualism, but on “theoretical” or “English-language theoretical archaeology” vs. “traditional” or “German-language archaeology”. On the one hand, groups such as the “Unkeler Kreis” and individuals that had studied in the UK or the US embraced not only topics, but also attitudes towards theory and theoretical debate that had been developed in “Anglo-American theory”. On the other hand, traditional archaeology in Germany was quite unfamiliar with both the topics and the according jargon as well as the manner in which the “Young Turks” started to criticise how archaeology was practiced, which research questions were pursued, and where the money went. This paper traces the attitudes of German archaeologists towards “Anglo-American theoretical archaeology”, highlighting four typical responses to it, including also its perception in the last decade of the GDR.

## **Keywords**

Theoretical archaeology, processual archaeology, post-processual archaeology, Western and Eastern Germany, paradigms

## **The individual involvement of selected German archaeologists – Some biographical notes**

German archaeology has seen a number of new developments since the 1980s. One of the results has been, as a Dutch colleague put it, that ‘there is more discussion of basic theory and concepts than many outsiders are aware of’ (BLOEMERS 2000: 378). Yet the reasons for this development (and the variety of forms it has taken) remain poorly understood. It is remarkable that students played the

decisive roles in the introduction of Anglo-American theoretical archaeology to Germany. Already in 1972, Herbert Jankuhn, who was teaching in Göttingen and well-known for his approach of settlement archaeology, encouraged his students to discuss David Clarke's book *Models in archaeology* (1972). His undergraduate student Heinrich Härke went to the University of Edinburgh in 1973 in order to study the British version of "New Archaeology". In the same year, Manfred K.H. Eggert went to the United States as a postdoctoral fellow to study the American "New Archaeology" at Yale. He received considerable support from his German teachers, Rafael von Uslar and Ernst Müller.

Eggert was the first German archaeologist to write a detailed review article on Anglo-American theoretical debates for a German-language readership, published in 1978 in *Prähistorische Zeitschrift* (EGGERT 1978). Moreover, starting in the late 1970s Hans-Jürgen Müller-Beck (University of Tübingen) and other German specialists in the "stone ages" began to address theoretical issues in studies dealing with Palaeolithic economy and ethnoarchaeology that were strongly influenced by contemporary works of Lewis Binford (HÄRKE 1991). This fact, however, is not widely known, perhaps due to the lack of communication between German (Palaeo)lithic archaeologists and their colleagues abroad.

The early 1980s then saw another two German undergraduate students go abroad. Sabine Wolfram went to Sheffield in 1981 where she wrote a work on archaeological theory, which was published in 1986 as a BAR volume (WOLFRAM 1986). Ulrike Sommer went to study in London in 1983. She later published a critical discussion on the taphonomy of archaeological units discussing Anglo-American approaches (SOMMER 1990). Both Wolfram and Sommer can be seen as a second generation of German archaeologists explicitly approaching and participating in the changes taking place in archaeological theory in Western Europe.

Before 1989, East-German archaeologists do not seem to have made any serious attempt to study Anglo-American archaeological theory either in their own countries or abroad. There seem to have been several factors involved, including the restriction to travel to non-socialist countries for all but a small number of archaeologists who were staunch supporters of the official Marxist doctrine. In addition, Anglo-American theoretical publications were rarely available. Indeed, most East-German archaeologists were unlikely to have been aware of the theoretical debates taking place in Anglo-American archaeology. Even though scholars such as Childe and Clarke were mentioned in various East-German archaeological publications, they did not have a significant impact. Another factor

seems to have been the orientation of East-German archaeology towards research carried out in other communist-ruled “Eastern European” countries<sup>1</sup>.

Since the late 1980, a growing number of archaeologists from Western Germany began to visit the United States or England. These include Peter Biehl, Reinhard Bernbeck, Martin Porr, Alexander Gramsch, Nils Müller-Scheeßel, which may be seen as forming a third generation, since interest in studying Anglo-American theoretical archaeology had become much more widespread. In addition, there was a group of German archaeologists who went to the U.S. or the UK to stay and began to return now and then primarily as visitors, such as Bettina Arnold, Martin Wobst, Cornelius Holtorf or Heinrich Härke.

### Recurrent patterns of German critiques

German archaeologists’ perceptions of Anglo-American archaeological theory in the 1980s and 1990s reveal conflicting attitudes, ranging from a largely implicit admiration to highly critical stands. At risk of caricature, let me try to summarise the five main types of German reactions and illustrate them with some quotations:

- 1 *German archaeologists can learn a lot from Anglo-American colleagues by considering new paradigms and perspectives as well as getting a certain sense of the theoretical structures underlying archaeological reasoning.*

Several examples for this attitude can be presented. In his 1978 paper mentioned above, Manfred Eggert argued that the New Archaeology was of fundamental relevance to European archaeology (EGGERT 1978: 145). Also Heinrich HÄRKE and Michael GECHTER (1983a: 4ff) at the first meeting of the “Unkeler Kreis” (see below) were concerned to demonstrate the importance of developing the kind of theoretical thinking that was shaping British and American archaeology. This tenor also can be found in Sabine WOLFRAM’s book *The theoretical debate in the prehistoric archaeology of Great Britain* (1986).

Other examples for a positive reception of the new discourse include, e.g., reviews of English-language books, such as those of B. Trigger’s *Time and Tradition* (SIMPERL 1983) and G. Clark’s *The Identity of Man* (PITTIONI 1985),

<sup>1</sup> On the other hand this does not imply that there was an intense theoretical discussion between East German, Soviet and other East European archaeologists (personal information by Heinz Grünert and Joachim Herrmann).

hailing their ability to address questions beyond typology and chronology (PITTIONI 1985: 311).

Since the collapse of the Berlin Wall, many more German archaeologists have become involved in debates over Anglo-American theoretical traditions. This new era of interest has made possible a number of significant monographs, including Reinhard BERNBECK's textbook *Theorien in der Archäologie* (1997), and collections such as *Theorie in der Archäologie: Zur englischsprachigen Diskussion* (1998), edited by Manfred EGGERT and Ulrich VEIT. Articles and reviews on Anglo-American theoretical archaeology written in German were also becoming more frequent in the 1990s<sup>2</sup>. Although these works cannot be discussed here in detail<sup>3</sup>, it is worth mentioning that they represent an increasing variety of theoretical points of view.

This is perhaps a good place to turn to the far less friendly response to Anglo-American archaeological theory. Expressed polemically, this is the view that:

- 2 *Anglo-American archaeologists have not taken into consideration the contributions to theory of German-speaking archaeology. This is why they re-invent German ideas, calling them Processual or Postprocessual Archaeology.*

For example, in 1972, Karl J. Narr in his reviews of both Butzer's *Environment and Archaeology* and Binford's *New Perspectives in Archeology* argued that they did not appropriately acknowledge German-language publications (NARR 1972a; 1972b). According to Narr, several themes in these books 'were discussed decades ago by other archaeologists who did not devote themselves to biological-archaeological positivism' (NARR 1972a: 607). In 1978 Narr went on to express his disappointment with American archaeologists' disregard of European researchers (NARR 1978). Other reviews contain similar complaints (e.g. KORBEL 1982). Korbel noted that some of the most fundamental aspects of the

<sup>2</sup> For example: WOLFRAM (1990), HÄRKE (1991), HOLTORF (1997), KALKBRENNER (1997), EGGERT (1998), MÜLLER-SCHEESSEL (1998), KERIG (1998), MANTE (1998; 2000).

<sup>3</sup> I would like to point to the interesting fact that this growing engagement of German archaeology with Anglo-American thought ran parallel to the intensified discussion of National Socialist influences on German archaeology (HÄRKE 2000b; STEUER 2001; LEUBE 2002). Thus, it can be said that the breakdown of the Berlin wall has produced more than one impact on German archaeology.

“New Archaeology” – such as deductive-nomological reasoning, evolutionism, systems theory and cultural ecology – were not that new and that it was only their combination that created a new perspective. According to Korbel, a really new “New Archaeology” did not exist (KORBEL 1982: 171). Similarly, at a meeting of the “Unkeler Kreis” in 1983 (which will be discussed below), Ludwig Pauli noted that the New Archaeology could not be regarded as a new theory, since its main components had existed earlier already, and processualism was largely the sort of common sense and historical awareness that was required of any good archaeology (in HÄRKE 1983a: 102). Until today the problem of mutual ignorance is a recurrent topic (e.g. HÄRKE 1989a, 2000a; BIEHL – GRAMSCH – MARCINIAK 2002; s.a. BINTLIFF in this volume). According to Härke, the problem was not only due to different languages but to more significant divisions that had grown out of responses to the New Archaeology, and misunderstandings (HÄRKE 1989a: 185ff.).

A third type of view is one which Härke describes as a reflection of the “pragmatic” orientation of German archaeologists in his paper *The hun is a methodical chap* (HÄRKE 1995) – i.e., not a methodological or theoretical one. This view might be summarised by the statement that:

3 3 *It's all too theoretical (i.e., not specific, i.e., not relevant)..*

For instance, Narr criticised that Binford’s contributions were limited to the construction of theorems and models, rather than actually leading to specific results (NARR 1972b: 607), a view echoed by EGGERT (1988). Eggert argued that New Archaeologists gave privileged place to unjustified optimism (EGGERT 1976; 1978), and seriously underestimated the importance of collecting “concrete data” for reconstructing prehistoric phenomena (EGGERT 1978: 148). WOLFRAM (1986) argued that theoretical archaeology would be more attractive if its terminology were less complicated and if its models were shown to be relevant to the study of archaeological materials in an appropriate way. She emphasised that critiques of theoretical archaeology put forward by British “traditionalists” should not be ignored, especially their arguments concerning constraints on the nature of archaeological data (WOLFRAM 1986: 104). Wolfram Schier in his contribution to R. Hachmann’s *Studien zum Kulturbegriff* (see STOCKHAMMER this volume) argued that Binford used archaeological data only to illustrate his theoretical concepts (SCHIER 1987: 172). This attitude criticised that since Binford failed to test his premises, it was hard to see the advantages of processual archaeology.

Thus, Binford's approach was seen as a new kind of speculation (SCHIER 1987: 177ff.).

The discussion of these various German points of view has focused exclusively on responses to Anglo-American New or processual archaeologies of the 1970s and 1980s. But the general patterns hold true also in relation to post-processual archaeologies. The responses range from Holtorf's enthusiasm, to critical praise (WOLFRAM 1990; KALKBRENNER 1997; MÜLLER-SCHEESSEL 1998; EGGERT 1998a, 1998b) to LÜNING's sharp refusal (1991). Notably, this diversity can even be found in papers written by one author – perhaps signalling the fascination emanating from Anglo-American theoretical discourses (and contrasting markedly with attitudes towards the “theoretical colours” of German archaeology). Particularly Manfred EGGERT's papers (1998a; 1998b) offer interesting examples of the (sometimes) contradictory complexity of many German archaeologists' responses. EGGERT (1998a: 321) seeks to understand certain tendencies within the Anglo-American theoretical archaeology in a loyal and almost empathetic manner. Eggert suggests that many of the processual and post-processual ideas which continental archaeologists find difficult to follow are consequences of the extremely tense academic job market situation in the U.S. and the UK – the pressure to publish numerous and always innovative papers. Eggert neither refers to himself as traditional processualist nor as post-processualist, and thus in a way he may represent the most widespread German response to Anglo-American theoretical archaeology. For Eggert, there is no need to take up complex philosophical issues since our most important theoretical problems in the field are modest enough not to need to bring in Wittgenstein, Heidegger and Co. Such a view implies that the conditions of archaeological knowledge are defined by the structure of the archaeological record (EGGERT 1998a: 363).

But what about the archaeologists of the former German Democratic Republic and their relationship to Anglo-American archaeology? One might have expected them to have been at least interested in neo-Marxist approaches. Far from it! Indeed the fourth and final example of German responses is a highly critical one:

#### *4 Anglo-American neo-Marxist archaeologists are not informed.*

For example, Joachim HERRMANN (1986a) published a review of the volume on *Marxist Perspectives in Archaeology*, edited by Spriggs, in which he argued that Spriggs was using the same seven-category-model of Marxist materialism as

Giddens. Herrmann objected especially to Spriggs' failure to mention the most important aspect of Marx and Engels, namely that their work is the theoretical basis of scientific socialism (HERRMANN 1986a: 373). Herrmann wrote: 'Who pretends to be a Marxist but treats fundamentals of historical Marxism – such as the categories of the “mode of production” (*Produktionsweise*), “social formation” (*Gesellschaftsformation*) or “base and superstructure” (*Basis und Überbau*) – as “dead wood” or “undeveloped theory” cannot lay claim to have anything in common with Marx' (HERRMANN 1986a: 374 – my translation). He argued that the contributions to the volume had no detailed knowledge of Marx' and Engels' works, but was limited to some selected quotations<sup>4</sup>.

### The ubiquity of Anglo-American theoretical archaeology

The foregoing remarks are not intended to support a generalised thesis concerning the ways in which German archaeology has been affected by Anglo-American theoretical archaeology. Considering various major responses to developments in Anglo-American archaeological theory is not the same as considering the question of whether the latter has changed German archaeology. The following comments concern that question. Anglo-American theoretical archaeology has effected practical changes in German archaeology on at least three levels:

- 1 on the level of influences on the academic biographies of some German archaeologists, which I summarised above;
- 2 on the institutional level, which includes the foundations of the so-called “Unkeler Kreis” and later on the German *T-AG* (Theoretical Archaeology Group), as well as theoretical activities in some German prehistory departments;
- 3 on the level that comprises all practical German archaeological studies that take Anglo-American theoretical paradigms into consideration.

The following paragraphs concern the last two levels. In 1983, in Unkel, a small town near Bonn, there was a private meeting of some German and two foreign archaeologists that came to be known as the “Unkeler Kreis” (J. H. F. Bloemers, J. Eckert, M. Gebühr, M. Gechter, J. Giesler, U. Giesler, H. Härke,

<sup>4</sup> While Herrmann was part of the academic elite in the GDR, some Western Germans also remarked that some neo-Marxist contributions were obviously more “neo” than Marxist, e.g. Härke in a review of Renfrew's and Shennan's *Ranking, resource and exchange* (HÄRKE 1984a: 98; see also HÄRKE 1984b).

K. Kristiansen, J. Kunow and L. Pauli). At a first glance it seems unlikely that this informal get-together could be seen as the beginning of the shift within German archaeology (HÄRKE 1989b; 1990a). In their introductory paper *German archaeology – where to? (Deutsche Archäologie – wohin?)* the organisers Michael Gechter and Heinrich Härke emphasised that German archaeology, by comparison with Anglo-American archaeology, has not been affected by new theoretical and methodological perspectives and discussions. They go on to argue that German archaeology was losing credibility abroad (HÄRKE – GECHTER 1983a: 4f.). Härke and Gechter advance five pointed theses to characterise the situation of archaeology in Germany, using expressions such as ‘an end in itself’, ‘over-specialisation’, ‘rejection of theory’, ‘intellectual complacency’ and ‘getting isolated’ (HÄRKE – GECHTER 1983b: 7). There seem to be two major reasons for their dissatisfaction. The first might be characterised as a “normal” generational conflict and the will of younger people to change things and to get noticed. The second may be characterised as the after-effect of Heinrich Härke’s stay in Great Britain where he saw what else archaeology can and may be. The session papers of this meeting and a second one in 1984 (PAULI 1984a) were published in two volumes, containing studies on the archaeology in Great Britain and on the New Archaeology (HÄRKE 1983b) and two reviews of Anglo-American theoretical publications (PAULI 1984b).

A successor to the “Unkeler Kreis”, the German Theoretical Archaeology Group (*Arbeitsgemeinschaft Theorie, T-AG*), was founded in 1990 by Sabine Wolfram, Ulrike Sommer and Heinrich Härke (HÄRKE 1990b; SOMMER *et al.* 1991; see also GRAMSCH – SOMMER this volume). It is worthwhile to note that the foundation of this group did not take place in Germany, but at the TAG 1990 meeting – the British Theoretical Archaeology Group Conference in Lampeter. The activities of the German *T-AG* since 1990 cannot be described here in detail (for further information see [www.theorieag.de](http://www.theorieag.de)). However, it is noticeable that in the 1990s there has been a continuous emphasis on Anglo-American paradigms – both in terms of organisational matters and content. Later on there also has been an increased co-operation with Central and East European colleagues (see BIEHL – GRAMSCH – MARCINIAK 2000; and this volume).

Another institutional impact of Anglo-American debates at some German university departments of prehistory is worth noting. I already mentioned the activities of Jankuhn (Universität Göttingen), and of Müller-Beck, Eggert and Veit (Universität Tübingen), and I have quoted Eggert’s and Veit’s 1998 volume on Anglo-American theoretical debates in archaeology (EGGERT – VEIT 1998)

which contains a collection of papers which have been written by students during a seminar held at the University of Tübingen. Later on, other German university departments opened themselves towards theoretical issues relating to individual theory-interested teachers such as Johan Callmer and Ruth Struwe (Humboldt-Universität Berlin), Johannes Müller (Universität Bamberg), François Bertemes and Peter Biehl (Universität Halle), Sabine Rieckhoff and Ulrike Sommer (Universität Leipzig) or Andreas Zimmermann (Universität Köln) and others.

The third level, which comprises applied studies discussing and adopting Anglo-American archaeological theories, can be illuminated by referring to Erwin Cziesla's 1990 case-study *Siedlungsdynamik auf steinzeitlichen Fundplätzen. Methodische Aspekte zur Analyse latenter Strukturen* (CZIESLA 1990). Cziesla's work begins quoting Binford and Härke. Cziesla even states that some time ago this work could have never been written (i.e., in German archaeology) because the thread running through the story is not typology or chronology (CZIESLA 1990: 1). His work is of particular interest due to the striking ways in which he synthesises processual and post-processual approaches. He seems to have been unaware of this because he repeatedly quoted and discussed processual archaeologists such as Binford, Schiffer and the "early" Hodder. He compares their orientations with strongly empiricist German colleagues, and accuses them and even himself of being too positivistic and of paying too much confidence in objectivity. Although he did not quote or refer to postprocessual archaeologists he argues in a very post-processual manner, pointing for instance to intuition and subjectivity as important stimulants for archaeological reasoning. Thus Cziesla can be called an explicit processualist but an implicit or hidden post-processualist and can be taken as a good example for future development – not because of his "implicitness" but because of his attempt to hold both perspectives. Furthermore his work demonstrates how strongly Anglo-American theoretical archaeology affected German practical research in some cases. Cziesla may show that similar ideas – including post-processual ones – may be born at different places and/or times.

Let me conclude with some remarks on the practical archaeology of the former GDR. Although Childe has been translated and published (CHILDE 1959) and sometimes quoted by East German archaeologists he was never seen as an "official" example for East German Marxist archaeologists. In a personal comment, Joachim Herrmann emphasised that he felt inspired by Childe, especially while writing his article *Archäologie als Geschichtswissenschaft*, which has been presented at the International Congress of Historians in Gent (HERRMANN 1986b).

Although Childe repeatedly visited the GDR, and was appointed a corresponding member of the East-Berlin Academy of Sciences (*Akademie der Wissenschaften*) his Marxist or Marxist-like ideas were not adopted or discussed by East German archaeologists. According to Herrmann, Childe's Marxist potential was just too primitive. This may be indicative of the lack of impact Anglo-American theoretical paradigms have had on the archaeology of the former GDR.

### Final remarks

This contribution has focused on the period before 1989 and the early 1990s. Since then the German *T-AG* has been established and theoretical awareness in German archaeology is growing. The title of this contribution points to one question that I have not yet answered explicitly: How can it be that German archaeologists both hate(d) and love(d) Anglo-American theoretical archaeology (apart from the matter that most German archaeologists largely ignore it)? They seem to love it for the new questions it poses, for the new possibilities and perspectives it offers and its colourfulness. But why hate it? There are several reasons. Allow me to speak frankly. The first factor seems to be the very different academic cultures Anglo-American and German disciplines relate to. In German academic disciplines there is no place for the kinds of rhetorical discourses that make it possible – for heuristic reasons – to take up different perspectives in a single academic presentation. Secondly, German archaeological departments rarely communicate with other departments of humanities. A third factor may relate to the second. German archaeologists were not comfortable, willing, or prepared to reflect explicitly upon theoretical issues. Manfred K. H. Eggert, for example, notes (without giving the name of the author) that after the publication of his 1978 pioneering study he received a letter from a very famous German archaeologist. It offered the “well-meaning” advice: ‘Good heavens! Don’t you start engaging in theoretical discussion!’ Eggert suggests that this kind of reaction may represent certain fears of the consequences of such a discussion (EGGERT 1998b: 360). So the advice of the writer raises the question: Who then would be up to it?

And why does the “normal” German archaeologist dislike theoretical reasoning?

## Outlook

One of the commentators of a draft of this paper concluded that being a German archaeologist must be "no fun". Well, yes and no: On the one hand it is disapproved to follow deductive research strategies. The mainstream expects archaeological reasoning to fit the inductive-empirical way. This actually is the only restriction which is responsible for a weighty consequence: Explicating theoretical points of view before analysing the material or speculating about things beyond the material often is seen as quite unscientific and non-archaeological. To give an example: East-German Marxist archaeology (which preferred the deductive process) has been criticized after 1990 because it was too theoretical and speculative and because the "facts" did not support the models (HÄNSEL 1993).

Rather, all basic German traditions – antiquarianism, regionalism and historical universalism – follow inductive-empiricist research strategies. This complies with the self-image of German archaeology. That is why Karl J. Narr and others so much insisted on it.

On the other hand – and that is where the "common values" come in – at the same time many practical studies were published that display a high theoretical potential, which, however, is more or less implicit. Every practical study reflects scientific values, and often German and Anglo-American (and other) works are based on the same basic ideas, with or without referring to each other. The work of Czesla discussed above is an example for both explicit and implicit theorising and discussing theoretical perspectives (processual) or not (post-processual).

Common values are also visible when we have a look at the innovations (that is where the fun comes in) of German prehistoric archaeology: There have been major developments of sociohistorical (e.g. STEUER 1982) and environmental archaeology (e.g. GRINGMUTH-DALLMER 1996; KÜSTER *et al.* 1998) that – implicitly – often correspond with Anglo-American, especially processual values. The 1990s in particular have seen a number of interesting publications written from an explicit theoretical basis (e.g. SOMMER 1990; 1996; BURMEISTER 2000), which, however, cannot be discussed here in detail.

Anglo-American and German archaeologies actually do follow some common ideas and values, but for German archaeologists it is not common to make them explicit. The main difference is not to be found within their practical, interpretative approaches but in the German unwillingness of theorising and explicating those values and of transferring system-like theories into archaeology (except East-German Marxism).

The reason for this unwillingness might still be found in an after-effect of the so called Kossinna syndrome (see SMOLLA 1979/80; WOLFRAM 2000): Before 1945 German archaeologists did speculate too much (especially concerning ethnic interpretations) and got involved in politically motivated interpretations, after 1945 they therefore got a bit afraid of it. But – as I have tried to show in this paper – Anglo-American archaeology influenced German archaeology in the sense that theoretical perspectives are more and more taken up and underlying premises become more and more explicit.

In the end, I think to understand the reception or decline of both theories deriving from an Anglo-American context and the attitude of explicit theorising we have to look beyond German-language publications, into other Central European schools such as Poland (KADROW this volume). To what extent have they as well as Swiss, Dutch, Scandinavian etc. archaeologies been affected by Anglo-American thinking? Has there been explicit reflection on processual and/or post-processual? And finally: How did these European archaeologies influence each other? We are still only beginning to answer these questions.

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# The German Influence on Polish Archaeology

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## Abstract

German influences on Polish archaeology from the mid 19<sup>th</sup> century to the present days are outlined in accordance with historical stages of German archaeology. Three parallel traditions in Polish archaeology are taken into consideration; these traditions can be linked with the institutes in Krakow, Warsaw, and Poznań centres. The most effective was the influence of Gustaf Kossinna and his *Siedlungsarchäologie* in the period between the First and the Second World War in Poznań, leading to the so-called “autochthonous school”. Nowadays Kraków is the only significant centre of Polish archaeology in which the inspirations of current German archaeology play a certain part. A new stream of influences from Germany (but not from German *archaeology* this time) in Polish archaeology is visible in the processual (Jürgen Habermas) and post-processual (Hans-Georg Gadamer) branches of our discipline.

## Keywords

German influences, Polish archaeology, Kossinna, *Siedlungsarchäologie*, neo-autochthonous school

## Introduction

To outline the German influence on the Polish archaeology is no easy task. Over the last few decades a network of mutual contacts and interactions has been built between different archaeological communities, if not yet on a global scale, certainly in the North Atlantic area. As a consequence, it is very difficult to define the archaeologies of various parts of Europe, including German archaeology. In the present contribution we have been asked to characterise German archaeology, both in the past and present, and to consider the question if such a phenomenon as a specific Polish archaeology exists at all. Indeed, it might be more appropriate to subsume the activities of Polish archaeologists under a larger category, for instance “Central European archaeology” or “archaeology of the Eastern Bloc”.

## The German tradition

According to the terminology adopted by Evžen NEUSTUPNÝ (1998: 14–15) the German archaeologists belong to a “mainstream society”, characterised by a large number of scholars. Archaeological problems come to the attention of a large number of professionals, which results in a permanent competition, usually generating a high standard of studies. The archaeological community has numerous centres, and its structure is pyramidal, with a broad basis of many specialists. The centres have excellent libraries, and a close co-operation of specialists from different domains brings optimal results.

The scientific activity of German archaeologists takes place under conditions of political stability, with a high degree of financial support. The German language, which is commonly used at conferences, facilitates communication for German archaeologist. A large number of conferences, seminars and other meetings creates optimal conditions for a quick diffusion of new ideas and scientific news. However, despite the internal variety of their society, the relative isolation and self-sufficiency of these “mainstream” archaeologists lets them appear as a more or less uniform group or school to people from the outside, characterised by a number of distinctive features.

For the period between the First World War and the end of the 1970s, the above characteristics seem to fit German archaeology perfectly. The person who contributed most to “typical” German archaeology at the beginning of the 20<sup>th</sup> century was Gustaf Kossinna. His bold and fresh conception of *Siedlungsarchäologie* (that is, settlement archaeology, but maybe better translated as prehistoric ethnography) certainly had a great impact on many European archaeological centres, which, at that time were deeply rooted in positivism. Later, when totalitarian systems were starting to come into power, Kossinna’s ethnic “master-key” found a fertile breeding ground and spread over the whole of Europe.

After the Second World War, in reaction to its previous strong involvement in politics during the Nazi-rule (for which *Siedlungsarchäologie* was well suited), the German archaeological community turned towards strict empiricism, leading to a concentration mainly on studies of typology and chronology. Induction became the predominant heuristic tool.

Even if these assumptions give a rather simplified picture of German archaeology in the period following the Second World War, they are usually taken as the basis for its general assessment. To identify a specific and conspicuous

“German archaeology” among the other “national archaeologies” in the 2<sup>nd</sup> half of the 19<sup>th</sup> century and the beginning of the 20<sup>th</sup> century is far more difficult. In a few German centres, as well as in Prague, Vienna, Budapest or Kraków and Lviv, archaeological sources (mainly artefacts) were accumulated for elaborating the newly established chronological and spatial systematics. For this period, it seems more appropriate to talk of a “Central European archaeology” rather than trying to divide it up according to contemporary political borders (see also BERTEMES this volume; BARFORD 2002; PARZINGER 2002).

Over the last two decades German archaeology has been enriched by many elements debated in world archaeology, while at the same time keeping its specific character. Today, it embraces all major trends: traditional, processual and postprocessual. Interdisciplinary studies are still strongly established, mainly in co-operation with the natural sciences. A great respect for archaeological sources and an avoidance of what Pierre Bourdieu calls ‘methodological fetishism’, the excessive theorizing leading to a hiatus between theory and practice (BOURDIEU – WACQUANT 2001: 28) can still be considered as typical features of German archaeology. In the case of our discipline this methodological fetishism would consist of developing archaeological theory without the support of facts (e.g. HARDING 2001: 406). Research efforts are concentrated more on source criticism than on the criticism of ideas or theories (see SOMMER 2000: 234–235). Even an aversion to theory and its basis is observed (WOLFRAM 2000: 184). In the Anglo-Saxon countries, German archaeology is perceived as ‘disciplined approaches to material evidence and to research history and the technical excellence of excavations and the meticulous compilation of artefact catalogues’ (HÄRKE 1989: 407; WOLFRAM 2000: 180).

## The development of Polish archaeology

### The origins

The next step in approaching the question of German influences on Polish archaeology is to discuss what “Polish archaeology” actually means. Polish archaeology originated in the period when the country was divided between three invading powers, *viz.* Russia, the Austro-Hungarian Empire, and Germany (ABRAMOWICZ 1991: 79–104; LECH 1998: 25–35; see *Fig. 1*). The main cultural and scientific centres of the territory occupied by Russia were Warsaw and



*Fig. 1. Archaeological centres in the mid and the 2<sup>nd</sup> part of the 19<sup>th</sup> century on the Polish territories occupied by Austria, Germany and Russia on the background of borders of modern Poland (dashed line).*

Vilnius. In Galicia in the Austrian sector, which enjoyed a certain autonomy the main role was played by the two university towns of Kraków and Lviv; in the German sector by Poznań.

In the middle of the 19<sup>th</sup> century the first archaeological museums were founded: in 1850 in Kraków, 1855 in Vilnius, and 1857 in Poznań. In 1856 a big exhibition of Polish antiques took place in Warsaw, and two years later in Kraków. They were part of the reaction of the educated classes to a rising interest in the origin of the Polish nation and culture. In the universities of Kraków (1866) and Lviv (1905), Departments of Archaeology were established. In 1872 the Polish Academy of Sciences and Arts, which played an important role in development of archaeology, was founded in Kraków (LECH 1998: 25–27).

In order to characterise Polish archaeology in its initial stage, the time-period until the beginning of the First World War, we will investigate the career of the most important Polish archaeologists of that time. Each of them was exposed to influences of different European centres during their university studies as well as during their professional life. Thanks to their activities, Polish archaeology at that time was not much different from the other countries of our continent.

Włodzimierz Demetrykiewicz from Kraków, a lawyer and historian of art by profession, dedicated himself to archaeology only after he had qualified as assistant professor (ABRAMOWICZ 1991: 80). He had acquired a professional knowledge of archaeology through his academic studies and practical experience by working in the Kraków museum, where he had contacts with archaeologists from Vienna, Prague, Budapest and Berlin (LECH 1998: 28). Karol Hadaczek, head of the Department of Archaeology at the University of Lviv, studied in Lviv and Vienna, in the class of Moritz Hoernes, and took part in Austrian and German archaeological expeditions to Greece, Italy and Egypt. In his theoretical orientation he proved to be a follower of Kossinna's ethnic interpretations (LECH 1998: 34).

In Warsaw, Ludwik Krzywicki, an outstanding sociologists interested in the theoretical basis of archaeological studies, who promoted Marxist ideas (LECH 1998: 29) had a strong influence on three generations of Polish practitioners of the humanities and the social sciences. Another eminent scientist, Erazm Majewski, initiator of the Archaeological Society in Warsaw specialised in studies of flint processing of the Palaeolithic and Mesolithic. He remained under the influence of the French school of typology created by de Mortillet. He was one of the first to criticise the theoretical basis of Kossinna's archaeology, faithful in this respect to his first teacher Moritz Hoernes (ABRAMOWICZ 1991: 93–95; LECH 1996: 50). Majewski founded the "Warsaw prehistoric school". He educated numerous students who, as much as their teacher, were very open to European influences due to extensive contacts (KOZŁOWSKI – LECH 1996: 7–12).

#### After the first World war

For German influences in Polish archaeology, Józef Kostrzewski was the key-figure between the wars. From 1911 to 1914 he wrote his doctoral thesis entitled *East-Germanic Culture in the Late La Tène Period* at the Prehistoric Seminar of the University of Berlin, supervised by Gustaf Kossinna (KOSTRZEWSKI 1970: 72–75). In 1919, he was appointed Chair of the Department of Archaeology at the

Poznań University. Józef Kostrzewski perfectly mastered Kossinna's method of *Siedlungsarchäologie*. Defending the Polish *raison d'état* in the interwar period, Kostrzewski used all the "tools" of Kossinna's school to fight Kossinna's ideas. His main criticism was directed against the attribution of the settlements in the Vistula and Oder basin to Germanic tribes. 'Kostrzewski took upon himself the main burden of fighting Nazi propaganda, exploiting both his own talents for polemics and his in-depth knowledge both of *Siedlungsarchäologie* and of its opponents' (LECH 1998: 43).

Together with Leon Kozłowski, he introduced the concept of "archaeological culture" into Polish archaeology (BARFORD 1996: 36; LECH 1998: 34–35).

The rich scientific output of Józef Kostrzewski and the fact that he taught numerous outstanding students (for instance Konrad Jażdżewski) reserve him an incontestable position in the history of Polish archaeology. His works also transmitted the influence of German archaeology, particularly approaches very close to Kossinna's ideology.

Thus, in the interwar period the Poznań archaeological centre played an important part in the transmission of German influences on Polish archaeology. In Kraków, where Włodzimierz Demetrykiewicz was still active, a more "Central European" style of archaeology predominated that was typical for the countries of the Austro-Hungarian Empire (including Galicia), which had developed under the impact of the Vienna-Berlin culture-historical school. In Warsaw the cosmopolitan trend of the "Warsaw school," developed by Erazm Majewski, was still effective at that time. His pupils, Stefan Krukowski and Ludwik Sawicki, achieved outstanding results in their studies of Mesolithic and Palaeolithic lithics. Krukowski was the first to investigate flint mines and flint workshops. Sawicki – following the example of French archaeology – utilised on geological studies in his research on the Upper and Late Palaeolithic.

One of the most original research workers of the interwar period was certainly Leon Kozłowski. A pupil of Erazm Majewski, he took his PhD at the University of Tübingen under Richard Schmidt. From 1921 on, Kozłowski was in charge of the Department of Archaeology at the University of Lviv. He co-operated with Henri Breuil in the Somme Valley. Kozłowski was well-known and as one of the very few Polish archaeologists of the time was highly esteemed in Germany, Britain and France (see for instance CLARK 1939: 27). He pioneered studies on the influence of climatic changes on prehistoric settlements (ABRAMOWICZ 1991: 112). Kozłowski highly appreciated the anti-positivistic elements of the

*Siedlungsarchäologie*, but at the same time he criticised Kossinna's narrow, politically influenced interpretations (LECH 1998: 42).

An interesting example of a wide and deep education is provided by Włodzimierz Antoniewicz, head of the Department of Archaeology at the University of Warsaw. He studied under Karol Hadaczek in Lwów, Włodzimierz Demetrykiewicz in Kraków, Moritz Hoernes and Oswald Menghin in Vienna, and Lubor Niederle in Prague (LECH 1998: 36). While he shared the ethnical interpretation of Gustaf Kossinna, he was also inspired by the achievements of French archaeology, and he sympathised with Marxism and established contacts with Soviet archaeologists (ABRAMOWICZ 1991: 125, 150).

In the interwar period, apart from the unquestionable domination of *Siedlungsarchäologie* (known as the “neo-autochthonic school” in Poland) with its main centre in Poznań, the cultural-historical school (in the original meaning of the word), concentrated around the Jagiellonian University of Kraków, was also of great importance. The Warsaw school also managed to elaborate its ideas, but remained more open to wider European inspirations (mostly French and British).

#### After the second World War

The Second World War and the resulting political changes in Europe, leading to the emergence of the “Eastern Bloc” had great impact on Polish archaeology. As a consequence, a ‘forced methodological revolution’ took place (ABRAMOWICZ 1991: 146–155), i.e., Marxism was installed, the estimation of which is still hotly disputed (Barford 1995; BARFORD 1997; LECH 1997; LECH 1998: 57–61; TABACZYŃSKI 2000). In the words of Abramowicz:

‘The liberation from the German occupation came from the East, followed by changes of the political system [...]. An all-embracing pressure was put on all spheres of life, including [...] archaeology’ (ABRAMOWICZ 1991: 146).

Those archaeologists who were already familiar with the ideas of communism managed to adapt to the new situation relatively easily (for instance Ludwik Sawicki, Zofia Podkowińska, Kazimierz Majewski). The interference of the authorities in the development of archaeology was most pronounced between 1949 and 1955 (LECH 1998: 57–61), this led to the formation of the so-called “declarative” Marxism (see below), which was very superficial in its substance

(BARFORD 1995: 8–37). In another strand, Historical Materialism had already influenced archaeological thought from the beginning of the post-war period onwards (LECH 1997: 177). However, the main current of Polish archaeology was not linked to Marxism. The interest of several research workers who were avoiding theoretical reflections was rather concentrated on cultural-historical archaeology. Many individual Polish archaeologists (for example Józef Kostrzewski and Konrad Jazdżewski) still identified themselves with the “neo-autochthonous” trend as well.

### The neo-autochthonous school

At the beginning of the 1950s the situation in Poznań exerted a particular influence on Polish archaeology. Poznań was the only Polish university where it was possible to major in archaeology. Therefore many students of the history of material culture from other universities (who are today respected professors already near retirement) had to continue their studies in Poznań in order to graduate in archaeology. Numerous Poznań graduates later became heads of important archaeological institutions in Poland, mostly in Warsaw (e.g. Witold Hensel, Zdzisław Rajewski). The German occupation and attempts to exterminate Poland as a nation during the Second World War was still a recent experience and led to intensive anti-Germanic feelings. This was the most important reason (apart from the sometimes outstanding research results) for the further but incidental occurrences of the “neo-autochthonous” trend in Polish archaeology (c.f. HENSEL 1988; JAŻDŻEWSKI 1981). Paradoxically this definitely “anti-German” tendency had its roots in the theoretical assumptions of Kossinna’s *Siedlungsarchäologie*. The stereotyped expressions used in some works of its continuators and a negation of the achievements of the other European schools led to a adoption of the pejorative term “*Kostrzewski-Schule*” by some German archaeologists, which was supposed to describe the achievement of Polish archaeology in general. For the present-day Polish archaeology this definition is as improper as the classification of German archaeology as entirely empirical and averse to all theory.

### Marxist archaeology

The geo-political situation in Poland after the Second World War caused different varieties of Marxism to find a firm position in Polish archaeology (TABACZYŃSKI 2000). In the beginning – as has been mentioned before – a rather shallow

“declarative” Marxism was predominant, caused by the enormous pressure of the Stalinist regime, which however was soon replaced by a “dogmatic” Marxism (LECH 1997: 183). In the second half of the 1950s and in the 1960s, as a result of political changes (accession to power by Gomółka, the First Secretary of the Polish communist party) and a relative opening to the world, a “revisionist” Marxism came into being under the influence of the works of Western philosophers published at that time (CARNAP 1970; MARCEL 1960; SARTRE 1957; WITTGENSTEIN 1960, *etc.*). Until the end of the 1960s this “open” version of Marxism, which matched the European philosophical achievements, was developing mostly in Warsaw (e.g. Stefan Amsterdamski, Leszek Kolakowski, Witold Kula). After the so-called “March occurrence” in 1968 (a power-struggle inside Polish communist party between “technocrats” and “partisans” cf. DAVIES 1991: 725–727). Warsaw’s environment of “revisionists” was disrupted and modern Marxism took up residence at the university of Poznań (e.g. Jerzy Kmita, Leszek Nowak, Jerzy Topolski, see LECH 1997: 190).

This adaptation of different trends of Marxism by the archaeologist of the Warsaw centre led to a relatively quick elimination of German influences (which anyway never were particularly significant there), both of *Siedlungsarchäologie* and of the strictly empirical chronological-typological school. From the beginning of the 1970s the “revisionist” section of Marxism became a departure point for the adoption of the “New Archaeology” in Poland, mainly in its British version (e.g. TABACZYNSKI 1976; 1983; 2000). In the subsequent period, i.e. in the 1990s, after the break down of the “real socialism” in the Eastern Bloc, the representatives of the “open” version of Marxism started to become prone to postprocessual archaeology (e.g. OSTOJA-ZAGÓRSKI 1997).

### The Kraków school

The so-called “Kraków school” is certainly the most amazing phenomenon. No Marxist trends have developed in any of Kraków’s archaeological institutions – certainly not due to a lack of pressure on Kraków’s academic community by the communist regime. Rather, it seems that conservative feelings played an important part (ABRAMOWICZ 1991: 189), combined with a critically liberal attitude towards science, fashionable at the Jagiellonian University under the Austro-Hungarian Empire and still prevalent today. While a liberal type of Marxism was adopted in Warsaw and later in Poznań, empiricism was practised in Kraków (e.g. GODŁOWSKI 1962). In contrast to Poznań and many other archaeological centres

where the neo-autochthonous school had been continued, Kraków had always remained critical towards it. Instead, in Kraków the “allochthonous” tradition of ethnical studies, initiated already in the 19<sup>th</sup> century by Lubor Niederle was (and still is) cultivated (e.g. GODŁOWSKI 2000; PARCZEWSKI 1993). This attitude has its theoretical roots in critical evolutionism as represented by the anthropologist Kazimierz MOSZYŃSKI (1957).

The traditional cultural-historical school, based on chronological and typological studies (e.g. MACHNIK 1966; GEDL 1988; BLAJER 2001) is Kraków’s most important trend. Until today, no postprocessual work has been published in Kraków. In contrast, processual trends found fertile soil, the most outstanding representatives of which are Janusz Kruk with his “settlement school” (KRUK 1973; KRUK – MILISAUSKAS 1999) and Janusz Krzysztof Kozłowski, co-author of the dynamic interpretation of the flint industries (KOZŁOWSKI 1980).

Nowadays Kraków is the only significant centre of Polish archaeology in which the inspirations of current German archaeology play a certain part. However, this does not mean the adaptation of ready-made solutions. The achievements of German archaeology are often used as a performance-comparison for the research undertaken in Kraków. Archaeologists from Kraków take part in scientific programmes conducted in co-operation with German scientists, for instance the publication series *Prähistorische Bronzefunde* (Corpus of Prehistoric Bronze artefacts), with contributors centered around professor Marek Gedl (e.g. BLAJER 1984; GEDL 1995; SZPUNAR 1987). In Kraków publications in German are more frequent than in the other centres, a good example for this is the series *Monumenta Archaeologica Barbarica* edited by professor Zenon Woźniak (see e.g. WOŁĄGIEWICZ 1995; MACHAJEWSKI 2002). Another common factor characteristic of both German archaeology and the archaeology practised in Kraków is that both are avoiding Bourdieu’s ‘methodological fetish’ already mentioned.

## Conclusion

In my opinion, the most active archaeological groups nowadays absorb mainly American or British inspirations. These are, for example, Zbigniew KOBYLIŃSKI (2001) and Przemysław URBĄNCZYK (1992) in Warsaw, Arkadiusz Marciniak (1996) and Włodzimierz RĄCZKOWSKI (2002) in Poznań or Janusz KRUK (1980) in Kraków. The wider basis of Polish archaeology, however, remains inside the frame of the cultural-historical school, stimulated until now by old Central

European or German traditions. These traditions are most vivid in Kraków and among specialist of the Bronze Age and the Roman Period from the whole country. Returning to the classification elaborated by Evžen Neustupný we can describe Polish archaeology as balancing between “mainstream” and “minority” archaeologies (NEUSTUPNÝ 1998: 14). At least three trends characterise it, related to the divisions observed at the turn of the 19<sup>th</sup> century, conditioned by the different possibilities of development in the three annexed territories. The “Kraków school” still exists (ABRAMOWICZ 1991: 189). The importance of the Poznań centre has been mentioned before (Lech 1997: 190–191). It is impossible not to realise the specific character of the biggest centre of the Polish archaeology, Warsaw. These big centres have influence on the smaller ones: Kraków on Lublin and Rzeszów; Poznań on Toruń and Bydgoszcz; and Warsaw on Łódź.

A new stream of influences from Germany – if not from German *archaeology* – in Polish archaeology is visible in the postprocessual branch of our discipline. It is mainly connected with the Frankfurt School of Philosophy and with names such as Jürgen HABERMAS (see Polish translations of his works e.g. 1983; 1999; 2002), Theodor ADORNO (1986), Max HORKHEIMER (1987) but also Hans-Georg GADAMER (1993; 2000). Their philosophical thinking can be detected in some recent works of Polish humanistic archaeologists (e.g. MAMZER 1997; PAŁUBICKA 1997; RĄCZKOWSKI 2002). In my opinion the most promising philosophical perspective for archaeological practice, in Poland as well, is provided by Jürgen Habermas’ *Theorie des kommunikativen Handelns* (HABERMAS 1981a; 1981b) and his theory of social evolution (HABERMAS 1976). I believe that it will allow to overcome the schizophrenic, bi-polar division of archaeology into a scientific and a humanistic branch (e.g. DZBYŃSKI 2008; KADROW 2010), of course not only in Poland.

Today it seems obsolete to talk of national schools, one influencing the other, in Central Europe. Rather, we find different ways of thinking and practising archaeology, both in Central Europe and beyond, cross-cutting what formerly were national schools or traditional paradigms.

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# **Archaeology and the Identity Discourse: Universalism versus Nationalism. Lake-dwelling Studies in 19<sup>th</sup> Century Switzerland**

MARC-ANTOINE KAESER

## **Abstract**

This paper analyses the different ways the pile-dwelling finds of Switzerland have been interpreted in the identity discourse, at the origins of prehistoric research. Curious as it may seem, evolutionary and culture-historical archaeologies could agree on a common interpretation, where ethnicism served a universalist ideal. This conciliation of two radically opposing approaches of archaeological reality shows that in our analysis of the ideological, socio-political functions of archaeological science, we ought to make a distinction between the interpretative and the heuristic logics. The supposed qualities of the Swiss “Lake-dwelling people” showed a way to counter the potential centrifugal forces of German, French and Italian nationalisms by emphasising Swiss liberalism. At the same time, the lake-dwellings were understood as Switzerland’s tribute to the general progress of Western civilisation.

## **Keywords**

History of archaeology, lake-dwellings, ethnicism, nationalism, universalism, evolutionism

## **Introduction**

Since the end of the 1990s, archaeology’s involvement with nationalism has been the subject of harsh and strong criticism among the scientific community. The critics, however, usually concentrate on the most blatant expressions of such involvements: the ethnicist interpretations of the past. Now, within archaeology, nationalism or other ideological prejudices can express themselves obliquely, and alter interpretations in more subtle ways. As a matter of fact, as I will try to show here, the identity discourse does not necessarily ground on basic ethnicism: it can paradoxically appeal to universalist references – references which, at first sight, would not be suspicious of conveying chauvinist values.

From a theoretical point of view, these distinctions question our understanding of the epistemological foundations of the discipline. They reflect the respective

roles of what have been called the evolutionary and culture-historical paradigms (TRIGGER 1989). Admittedly, these two approaches of archaeological reality fit to radically different theoretical positions; at the heuristic level, they lead to apparently irreconcilable research programmes. At the interpretative level, however, both approaches have actually been combined. That amazing combination reveals the necessary differentiation between heuristic logics and socio-political functions of archaeological science.

The archaeology of pile-dwelling sites (MENOTTI 2004) and the history of their interpretation (KAESER 2004a) provide a good example of such a combination. This case may be exceptional, but is nevertheless significant: during the time span under consideration (the 1850s to 1880s), which is recognized as a turning point in the development and scientific establishment of prehistoric archaeology (KAESER 2006; cf. MÜLLER-SCHEESSEL in this volume), these Neolithic and Bronze Age lakeshore settlements represented a major topic of prehistoric research in Europe.

#### 1854: the first lakeshore finds and the invention of the “lake-dwelling civilisation”

Schematically, the archaeological story of the lake-dwelling (or “palafittic”) research begins in 1854, in Obermeilen, on the temporarily dry shores of the lake of Zurich (ANTIQUARISCHE GESELLSCHAFT IN ZÜRICH 2004). Ferdinand Keller, the President of the local Society of Antiquaries, discovers a pile field. He recognises an archaeological layer covering the whole area, and concealing a gigantic amount of man-made implements, which he identifies as prehistoric. Judging from the nature of the remains, Keller interprets the site as a settlement – an acknowledgement of considerable significance, settlements being then extremely rare: archaeology usually focused on graves, places of worship, or allegedly military sites. And Keller’s acknowledgement had all the more significance, since his reconstitution of this prehistoric settlement was far from trivial: wrongly (but with many valid arguments: KAESER 2000), he actually set the ancient village of Obermeilen *above* the water level of the lake, on a vast platform (*Fig. 1*).

That amazing reconstitution was to have far-reaching consequences. With the publicity given to this first discovery, similar villages were soon reported throughout Switzerland. In his first thorough study, published several months later only, Keller could already list as many as forty lake-dwellings, on the shores of eight different Swiss lakes (KELLER 1854). And, arguing from the apparent

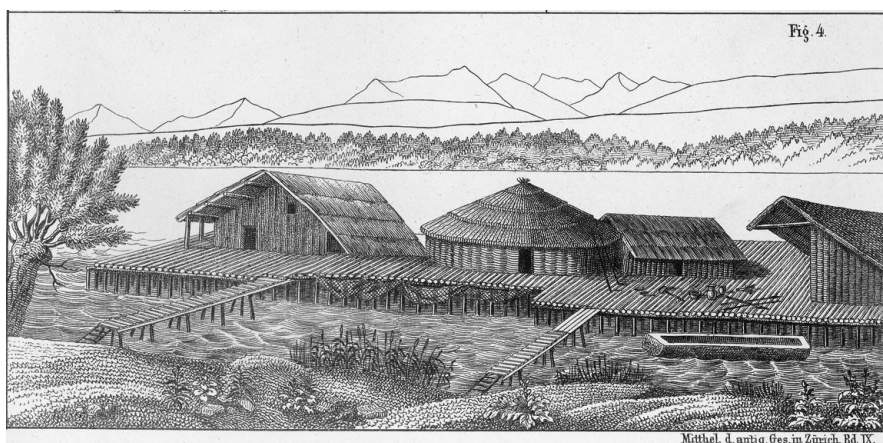


Fig. 1. F. Keller's reconstitution of the Obermeilen pile-dwelling (KELLER 1854: pl. I) sat the pattern to the interpretation of lake-dwellings for more than a century.

commonness of that type of settlement, the President of the Society of Antiquaries felt entitled to relate all those villages to a specific people: the “civilisation of the Lake-dwellers”.

Keller's daring interpretation instantly received an enthusiastic approval among the scientific community, and opened the way to a new, prosperous field of archaeological research (Figs 2–3). In the following years, his “lake-dwelling theory” also met an outstanding popular success (Figs 4–5). Undoubtedly, the evident romanticism of his reconstruction of Switzerland's prehistory may account for the popularity of the “Lake-dwellers”, as testified by the tremendous wealth of iconographic material produced on the topic since the 1850s (KAESER 2008). But the seducing exotism of these images is not sufficient to explain the success of the lake-dwelling archaeology. As a matter of fact, nationalist issues were also at stake. And in the middle of 19<sup>th</sup> century, the building of a Swiss identity was a crucial and urgent problem.

### Lake-dwellers as forefathers

The first discoveries of lake-dwellings closely followed the creation of the Swiss Confederation, in 1848. The birth of the new state had not gone off peacefully: after a series of local revolutions and a civil war (the *Sonderbund* War), the adoption of a new, democratic, federal constitution sanctioned the victory of the liberal, progressive party over the catholic conservative front. In order to heal

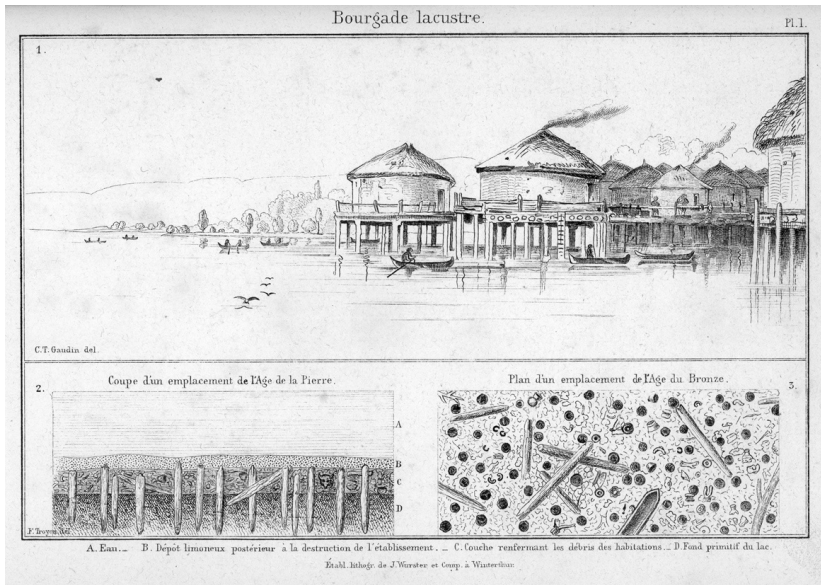


Fig. 2. 'Lake-dwelling town', by Frédéric Troyon (*Habitations lacustres des temps anciens et modernes. Lausanne, 1860: pl. 1*), with a plan drawing and a stratigraphic sketch – almost a copy of Keller's 1854 pattern.



Fig. 3. Lake-dwelling scale models became a widespread attribute for prehistoric collections gathered on the lakeshores: advertising postcard for J. Götzinger's model factory, mentioning the prices won at international fairs and the scientific support of Dr. F. Keller, Prof. L. Rütimeyer and Prof. E. Desor: Laténium – Archaeology Park and Museum of Neuchâtel.



Fig. 4. Albert Anker, 'Lake-dwelling woman' (1873) – a representation of the eternal feminine, by the major figure of Swiss national painting in the late 19<sup>th</sup> century. Musée des Beaux-Arts, La Chaux-de-Fonds.

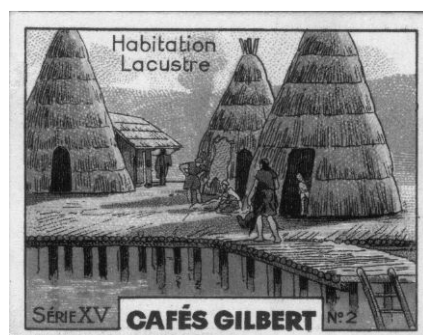


Fig. 5. 'Lake-dwelling': commercial illustration for the French Gilbert Coffee Factory. Private collection, Marseille.

the still open wounds, it was essential to resort to common, national references – all the more so, considering the politic isolation of the new Confederation, which was then the only democratic state in continental Europe, and which was surrounded by neighbours asserting their aims to national unification (Germany and Italy), or proclaiming their imperialist ambitions (France and Austria).

Now, Switzerland being a very diverse country, such common references were not easily at hand. One could not put forward any linguistic, cultural, or

confessional unity: beyond its ideological dimension, the *Sonderbund* War also had religious and economic components. As a result, these references had to be taken from history (a shared past) or from geography (a shared landscape).

Obviously, historical references within the *Ancien Régime* were to be excluded: the overthrow of the cantonal, aristocratic regimes was precisely the pride of the new federal, democratic authorities. While the Swiss federal identity had always been based on mediaeval references, these references were now dismissed: the heroes of the Middle Ages remained those of the defeated, catholic conservative states of Central Switzerland. Moreover, in the previous decades, historians had revealed the imaginary, mythical character of such heroes, like Wilhelm Tell (CAPITANI 1987). Consequently, the discovery of another, more remote past, came as a godsend to the identity discourse.

In fact, judging from the lake-dwelling vestiges, it seemed that through their highly original, amphibious way of life, the inhabitants of the country had distinguished themselves from their neighbours from the most ancient times on. This identification was all the more tempting, since the archaeological evidence endowed the Lake-dwellers with features which were regarded as typically Swiss (KAESER 2004a: 61ff). Unlike the Gauls, Germans, etc., who were particularly known by weapons and jewellery from grave goods, the mundane artefacts of the Lake-dwellers revealed them as pacifist and egalitarian communities, leading a modest, practical and hard-working life, who chose settlements which guaranteed hygiene and cleanliness... Under these circumstances, it seemed quite justified to present these Lake-dwellers as ancestors to the Swiss, and to claim that they confirmed, throughout time, the unity and specificity of the Swiss nation. Besides, these prehistoric villages favourably replaced Switzerland's previous geographical, alpine identity: while the Alps were now the stronghold of the most conservative opponents to the new democratic system, the lake-dwelling sites were suitably located in the modern, industrious, and liberal plains.

Up to this point, this account perfectly matches the connections which are often drawn between nationalism and the beginnings of prehistoric archaeology. Yet it covers only one side of the issue: the antiquarian, purely ethnicist perception of the "palafittic phenomenon". Now, it is a fact that the same "lake-dwelling civilisation" has also been interpreted in a completely different way.

## Lake-dwellings as material evidence of human evolution

Right from the beginnings of lakeshore surveys, many natural scientists were drawn to these archaeological investigations by the exceptional conservation of organic remains on wetland sites. Now, the interest of these geologists, botanists, physicists, and zoologists was far from trivial for the development of our discipline. While the undertakings of the antiquarian community were often looked upon as a hobby for the idle, the intervention of these naturalists led to a new social and scientific legitimization of the research into prehistory. Of course, these scientists also brought their special expertise, which authorised the thorough study of the geological features, as well as of the human, animal, and plant remains. But above all, they opened new epistemological perspectives; actually, they shifted the general theme of antiquarian research.

To put it roughly, the antiquarians endeavoured to tell about the life, the customs and the beliefs of the ancient “peoples”. They were therefore easily subject to compromise with nationalist discourses. That was the way Keller initially tackled the lake-dwelling question: chrono-cultural distinctions did not matter much to him. On the contrary, he strove to relativise the differences between Stone, Bronze and Iron Age<sup>1</sup> settlements, stressing the permanence of the lake-dwelling way of life, which he saw as the mark of a particular people: the “Lake-dwellers”.

The naturalists’ perspective was quite different (KAESER 2006). Instead of focusing on peoples, they focused on mankind and its relations to the social and natural environment, as well as on the evolution of these relations. This particular archaeology concentrated on prehistoric technology, industrial dynamics, and trade. Starting from an evolutionary viewpoint, it characterised cultures as the outcome of an interaction between society and nature, following a progressive line, according to the “law of natural progress”. Consequently, this archaeology did not care much about ethnic distinctions; in principle, it was to remain impervious to the nationalist obsessions of late 19<sup>th</sup> century.

To tell the truth, one must point out that such a clear-cut division between antiquarians and naturalists may be misleading (SHERRATT 1996). As a matter of fact, Keller himself, who had studied natural history in his youth, was in constant contact with the community of natural scientists; and it was he who had

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<sup>1</sup> Wrongly considering the site of La Tène as a lake-dwelling, the archaeologists of the 19<sup>th</sup> century assumed that the palafittic phenomenon had lasted from the beginnings of the Neolithic up to the times of the Roman conquest.

requested their assistance. It would therefore be simplistic to label the antiquarian and naturalist perspectives as “paradigms”: in reality, we are faced with various measures between two dominant interests, which we should rather call “research programmes”.

All the same, the intervention of the naturalists aroused general attention on the evolutionary side of the lake-dwelling prehistory. The chrono-cultural issue was even the main subject of interest for two Swiss geologists, Edouard Desor (KAESER 2004b) and Adolphe Morlot. Their publications on palafittes (MORLOT 1860; 1861a; DESOR 1865a) were translated into English (MORLOT 1861b; DESOR 1865b), German (MORLOT 1865; DESOR 1866), and Italian (by PIGORINI 1863; DESITTERE 1991), and considerably contributed to the spread of Thomsen’s Three-Age System on the Continent<sup>2</sup>. Among Scandinavian authors, this system had besides remained fairly abstract: drawn from the funerary world, the find-combination method could hardly convince the sceptics. The lake-dwellings, on the other hand, were better suited for a demonstration, because they concretely underlined the cultural evolution of their inhabitants. Judging from the trivial remains of ordinary daily life, one could not disprove the progress of techniques and technology, from the Stone to the Bronze, and to the Iron Age settlements. As Gordon Childe stressed, lake-dwellings really allowed the international diffusion of the Three-Age System (CHILDE 1955).

### The Lake-dwellers, sons of Prometheus

The light thrown on lake-dwellings by the representatives of the natural sciences had some effect on the general interpretation of the palafittic phenomenon. Actually, a tacit conciliation of both approaches could be agreed on (KAESER 2004a: 70ff). The Lake-dwellers remained a specific people. However, their specificity did not rest on an exclusive, essentialist quality, but on the particular degree of an industrious, missionary energy: they were seen as a heroic people who brought civilisation to the rest of humanity.

In fact, at that time, the most ancient villages known in the Western world were the Swiss lake-dwellings of the Neolithic. One could thus consider that after the savage stage of Palaeolithic times, civilisation was born in Switzerland, among the Lake-dwellers. It was on the shores of the Swiss lakes that men had

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<sup>2</sup> Cf. WILSON (2002). On the weakness of the previous diffusion of the Three-Age System outside Scandinavia, cf. KEHOE (1998: 30ff); MORSE (1999: 1).

joined together, to form the first organised societies, moving from predation to the productive stage of humankind. And whereas the other, subsequent, protohistoric peoples of Europe were principally known by their weapons and by their religious customs, the Lake-dwellers were known by the countless variety of their tools. Judging their nature by the incredible effort of will which they had proved to build their platforms, Lake-dwellers appeared to be driven by an especially diligent spirit, which could explain the dawn, among them, of civilisation.

Now, as lake-dwellings illustrated at best the prehistoric cultural evolution, the Swiss were happy to persuade themselves that the Lake-dwellers had actually been the active force of that progress (KAESER 2004a: 75ff). Contrary to their neighbours, whose energy was taken up by wars, by the delusive worship of gods or idols, and by the maintenance of an expensive aristocracy and clergy, the Lake-dwellers could dedicate all their strength to the advancement of industry, to the improvement of their living conditions. Moreover, as they had settled on the lakeshores, they had direct access to strategic waterways; thus, they had seized the opportunity, becoming actively involved in trade. And it was through trade that they had brought technological, economic and social progress to the rest of humanity. Admittedly, in accordance with the "law of progress", human evolution was necessary: it was a natural law of history. But naturalists and antiquarians could agree that that law had been able to rely on the Lake-dwellers as a special instrument of propagation.

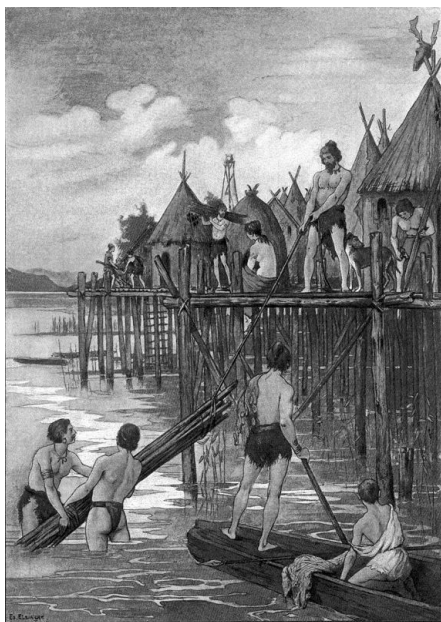
### Ethnicism within evolutionism – universalism as a national value

The particular case of the Swiss 19<sup>th</sup> century lake-dwelling studies shows that ethnicism is not necessarily the result of an antiquarian approach: here, an ethnicist perspective has been applied within the framework of evolutionist interpretation.

In other words, the identification of the Swiss with the Lake-dwellers was not really based on the sharing of a "Swiss blood", or on the assertion of an essential, exclusive character. This identification rested on the idea that the Lake-dwellers had in the past fulfilled an ideal which was still the ideal of modern Switzerland: an ideal of peace and tolerance<sup>3</sup>, built on freedom and progress,

<sup>3</sup> The strength of the pacifist ideology in 19<sup>th</sup> century Switzerland has been somewhat neglected in historical research; cf. at least ZWAHLEN (1991), or STAWARZ (2002), who specify the three (bourgeois liberal, labour, and Christian) streams of the movement, up to the First World War.

through education and labour (*Fig. 6*). Admittedly, these industrious values were those of the financial and economic elites leading the Radical Democrat party in power. But all in all, this ideal was the one of liberal democracy; and far from being peculiar to Switzerland, it claimed to a universal significance.



*Fig. 6. The Lake-dwellers as a people of labourers and builders: engraving by Edouard Elzingre published in the schoolbook Cours d'histoire – Premier livre d'histoire de la Suisse (Berne 1906).*

Unquestionably, the nationalism of that reading of the past is of a particular kind. Even if this interpretation of the lake-dwellings was integrated into, and was actually proceeding from the national identity discourse, it clearly had a universalist function. In the end, we are dealing with a discourse where national identity was used to assert universal values – even if instrumental to the interests of the ruling Swiss bourgeoisie.

Actually, the political situation and the diplomatic actions of the Swiss Confederation between 1848 and the 1870s can partially account for the cosmopolitan nature of this identity discourse. At that time, Switzerland indeed strove to build its identity on such an ideal with universal signification (HERREN 1998). As a matter of fact, Switzerland was an isolated democracy, an island<sup>4</sup> of liberal progressivism in the centre of Europe, surrounded by aristocratic or autocratic

<sup>4</sup> On the Swiss feeling of insularity, cf. RESZLER (1986); on the Rousseauistic inspiration of its application to the lake-dwelling *imaginaire*, cf. KAESER (2000: 90–94).

regimes that ruled the whole continent. Isolated like the Lake-dwellers on their platform, the Swiss were facing the increasing nationalisms of their neighbours. Now, the only way to counter the potential centrifugal forces of German, French and Italian nationalisms, to prevent the splitting of the country, was to underline Switzerland's moral and ideological specificity: the defence of liberal democracy and of humanitarian values (*Fig. 7*). Being perceived as universal, that ideal was not specific to the Swiss: their only specificity laid in their being on the *avant-garde*. As the leading Swiss politician Alfred Escher put it:

‘Switzerland is destined to promote the holy cause of the liberty of the peoples through the strength of its example. Yes, Gentlemen! Our Alpine country is to be the high altar of Freedom in Europe’ (1850 parliamentary discourse quoted by SCHIEDT 1998, my translation).



*Fig. 7. ‘The peace island Switzerland and its humanitarian politics’: a postcard printed in 1917, praising the international benefits of Switzerland’s neutrality in the midst of World War I: reception of refugees, care for war orphans, etc.*  
 Swiss national Museum, Zurich (LM-73693.42).

Preaching by example, the Swiss patriots were actually proud to follow the path of freedom and progress opened by the ancient Lake-dwellers, leading the way for the other nations.

## The internationalism of prehistoric, lake-dwelling studies

The popular and scientific reception of the Swiss lake-dwelling studies abroad indicates that certain kinds of nationalisms are actually compatible with universalist ideals. As a matter of fact, the lake-dwellings have often (and for a long time) been perceived throughout Europe as Switzerland's tribute to the general progress of Western civilisation. Of course, such assessments were more likely to come from progressive circles, as testified by this quotation from a French popularizing book on prehistory (DU CLEUZIOU 1887: 306–307, my translation, and *Fig. 8*):

‘That is how the numerous lake-dwelling towns were born, in that solitary area which seems to have been created for the Union, the great union of humanity, and is still called today the Confederation of free states. From that primitive settling, modern Switzerland still keeps the special character which makes it different from all the other European nations. It is still, in the bloom of the 19<sup>th</sup> century, the old homeland of the Lake-dwellers. [...] From the origins, Switzerland founded the Republic, that government of human law by essence, which is its strength and its glory, which has stood up to all the clashes, and whose stability demonstrates that Republic is the ideal government – a government to which all free peoples will come back, when they get rid of all the royal superfluities introduced through force into their customs.’

Such vigorous declarations speak for themselves. Testifying at best to the progressive development of humankind, the lake-dwellings have justified evolutionist and cosmopolitan creeds everywhere – even in countries facing serious national identity problems, like Austro-Hungarian Slovenia of late 19<sup>th</sup> century (SLAPSAK – NOVAKOVIC 1996).

Actually, while so much criticism has focused on the past nationalist involvements of (prehistoric) archaeology, the history of early lake-dwelling studies helps us to assess also the importance of universalism, at the roots of the discipline. As a matter of fact, many of the first prehistorians were driven by a powerful cosmopolitan ideal. Contemplating prehistory through an evolutionist point of view, they considered that its lessons on human destiny as a whole legitimated the truth of this cosmopolitan ideal. And in our viewpoint, it is of no consequence if this ideal was actually functional to their political interests or to their theoretical doctrines, for these beliefs were unquestionably genuine.

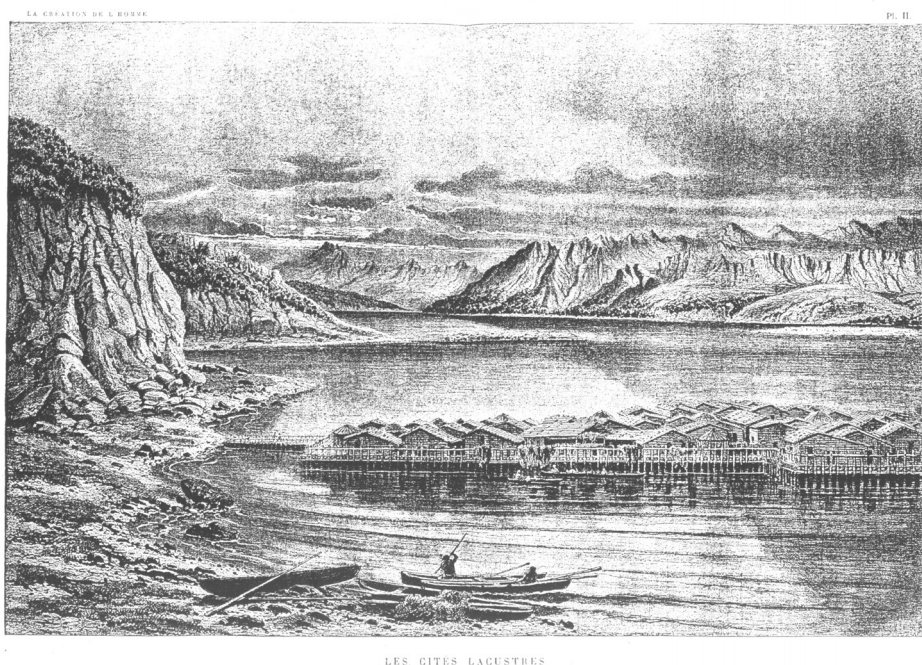


Fig. 8. Engraving of a lake-dwelling village, from a famous French popularizing book on prehistory (DU CLEUZIOU 1887: pl. II). The dramatic, mountainous landscape underlines its location in Switzerland, ‘that solitary area which seems to have been created for the great union of humanity, and is still called today the Confederation of free states’ (DU CLEUZIOU 1887: 306, my translation).

In this respect, the foundation of the International Congress of prehistory in 1865 (see MÜLLER-SCHEESSEL in this volume) offers a good example, anticipating by several decades the institutionalisation of prehistory at the national level. For the Congress of Prehistory was not just international: it was impelled by a strong internationalist faith, which its promoters skilfully implemented in order to establish their evolutionist approach of human antiquity. Under those circumstances, internationalism significantly influenced the epistemological foundations of prehistoric science to come (KAESER 2002a; 2010). And it is no coincidence, that the founders of this international institution (Edouard Desor and Gabriel de Mortillet, both geologists and keen advocates of cultural evolutionism) had been attracted to the field of prehistoric research by the general trend of lake-dwelling surveys.

## From history to contemporary Europeanism: concluding remarks

The specific case-study analysed here points to the complexity of the relations between identity discourse, nationalism, and archaeology (with its respective, evolutionary and antiquarian, research programmes). But in order to grasp that complexity, it is necessary to acknowledge that the archaeological discipline did not follow a linear path, going from one “paradigm” to the next. Actually, a close examination of the history of archaeology shows that the epistemological variety of our discipline involves more than the basic theoretical choices only. Depending on the topics, archaeologists have been induced to substantial shifts on the heuristic and interpretative levels, regardless of their theoretical positions<sup>5</sup>. All in all, the articulation between science and society therefore has to be investigated on the level of the construction of archaeological knowledge, and not on the level of the construction of archaeological discourse only (KAESER 2002b).

Besides, the involvement of archaeology in an identity discourse capable of transcending internal differences through an appeal to universalist values should be considered carefully, while much attention is paid today to the possible definition of a common European identity (GRAMSCH 2000). This past example may prevent us from an unwitting repetition of such logics, all the more so since our present touchiness obviously focuses on the ethnicist biases in archaeological interpretation.

Admittedly, it is a fact that the progressive, internationalist ideal which the lake-dwelling identity discourse was based on eventually ended up as a political failure. From the 1880s on, the rivalry of the great European powers and the economic crisis lead to a defensive withdrawal on a more selfish nationalism, which had consequences also on the interpretation of lake-dwellings in Switzerland – especially during the 1930s and the two world wars, when Lake-dwellers were to incarnate the Swiss love of independence and spirit of resistance (KAESER 2004a: 86ff). Similarly, on the epistemological level, the evolutionary perspective implied by this internationalist ideal also led to a dead-end: everywhere on the globe – with or without archaeology – it has offered a legitimisation to colonialism and colonial undertakings.

However that may be, we should not forget, today, the original importance of this internationalist, progressive ideal, at the roots of our discipline. For through

<sup>5</sup> Cf. COYE (1997: 181–237), who offers a conclusive analysis of the French Neolithic research of late 19<sup>th</sup> century, between the respective lake-dwelling and megalithic topics.

subsequent expressions of evolutionism, this ideal surely still has an influence, concealed or not, on some of our present thought processes as archaeologists.

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# Archaeology between Hermeneutics and Quantitative Methods

ANDREAS ZIMMERMANN

## Abstract

An example from settlement archaeology is presented to show that scientific reasoning and applications of quantitative methods in archaeology correspond quite well to the six step hermeneutics of the German historian J. G. Droysen developed in third quarter of the 19<sup>th</sup> century.

## Keywords

Hermeneutics, quantitative methods, settlement archaeology, early Neolithic, *Linear-bandkeramik*, Central Europe

Archaeologies (as is probably the case with most humanities) are not based on specific theories that were developed solely for their own particular purposes. Cultural history, for example, is related as much to ethnology as it is to archaeology, and evolutionism is not only a constituent of biology but also of archaeology and ethnology. Most of the theories developed in the 19<sup>th</sup> century are focused on the macro-scale, e.g. a biologist's species, an ethnologist's ethnic groups, a sociologist's societies, an historian's states, and an archaeologist's cultures. During nationalistic periods the words "peoples" or even "races" were used as a substitute for the term "culture". Moreover, it was not until the 20<sup>th</sup> century that the meso- and micro-scale moved into the focus of interest.

For archaeology, two groups of theories are of particular interest. Both were developed in the 19<sup>th</sup> century. Let us call the first group cultural history. It deals with the identities of populations whose material culture is being investigated. Other important questions for this school of thought are both the emergence and particularities of culture<sup>1</sup> and mutual influences (diffusionism). The second group of theories goes back to evolutionism and represents the tradition of natural sciences in the archaeologies. Quantitative methodology belongs to this latter group. For half a century, the archaeologies have remained almost completely oblivious to the exploding growth of the spectrum of theories since 1900. This

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<sup>1</sup> The term "culture" here is not used in the sense of the *Kulturkreislehre*.

can be easily observed if we look into the history of German research and the dominance of ethnic interpretations in research questions prevailing in the 20<sup>th</sup> century.

Another point, which is less often taken into account, concerns the nature of sources during this time. The first large-scale excavations were not conducted until the 1920's and 1940's. To name a few examples (from west to east): Star Carr (UK), Köln-Lindenthal (Germany), and Biskupin (Poland). Prior to this period, the micro-perspective remained out of sight for mainstream archaeology. It was only with the emergence of processual archaeology in the 1960's that theoretical approaches, developed in other scientific disciplines, became noticed. Postprocessual archaeology fills in the remaining gaps.

The discussions revolving around the usage of a "correct" theory in the humanities creates the impression that a single, optimal, universally valid theory is required. In the various sub-divisions of quantitative methods, however, there is a growing, reasonable differentiation of the spectre of models and theories preferably used for analysis. Thus, in mathematically modelling, ecological gradients as much as linear or unimodal methods are applied in developing evolutionary or diffusionist trends.

Looking for the most general evolutionary changes with a pattern, for example, of "the younger the more complex" or similar, linear models and in multidimensional approaches factor analysis based on correlation coefficients and their variants are appropriate.

When looking at a more detailed evolutionary development with many traces or variables, unimodal methods such as correspondence-analysis (CA) and their variants are the methods best suited. Expecting a unimodal development of a variable is related to the idea that at a time when a certain trait is innovative (candle light, presenting papers with slides) only a few of such traits exist. At times when everybody is used to this trait, a lot of them can be observed, while when the trait is being replaced by a new one (gas light or transparencies) again only a few outdated specimens will exist (*Fig. 1*). For a multidimensional analysis of these observations, a CA based on the  $\chi^2$ -metric is best suited. What actually happens when one of these cycles is observed in more detail may be a little confusing. In the period between invention and maximal use of a trait and between maximal use and replacement by another trait near linear properties are expected.

Nevertheless, such mathematical models allow an informed choice of a specific method of analysis, and in many cases there are good reasons to choose one method and avoid another. It appears to me that choosing an optimal

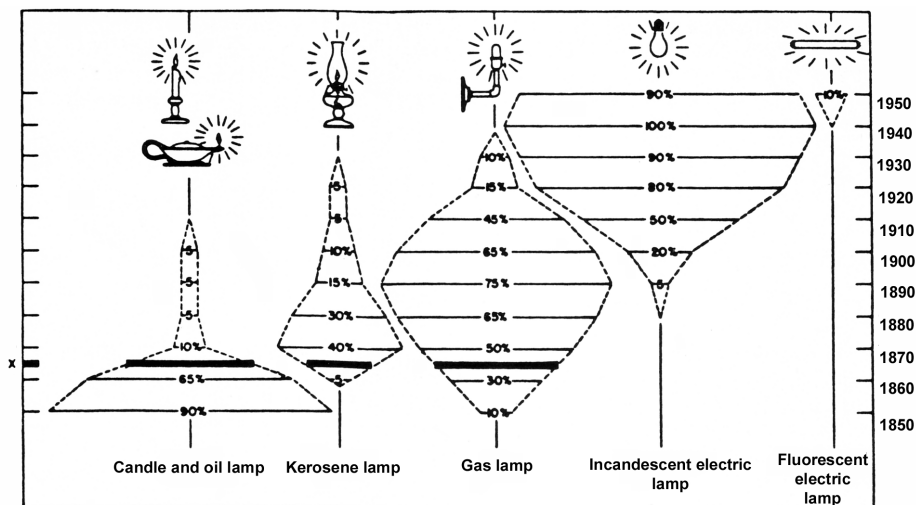


Fig. 1. The proportion of different lighting-devices in Pennsylvania (USA) between 1850–1950 (IHM 1983: fig. 2).

mathematical method related only to the archaeological question and to the kind of empirical observations at hand could also represent a model of how to choose a theory for a defined research problem.

In fact, general differences between a scientific and a hermeneutic approach do not appear to exist in every single case. Using the term hermeneutics I am referring to J. G. Droysen's textbook *Historik* (LEYH 1977; summarised by GOERTZ 1995: 110ff). He presented a six-step system of comprehension. We have chosen this method to study the settlement structure of Linearbandkeramik sites. These early farming settlements in Central Europe are represented mainly by pits and the postholes of houses. However, as the Neolithic surface is not preserved stratigraphic observations rarely help to understand the sequence of development (BOELICKE *et al.* 1988: 891–900).

In the following, the interpretation of LBK-settlements is discussed in the framework of Droysen's scheme.

1. Heuristics: Which features and houses are contemporaneous? How is the development of the settlements related to their general duration? (*Heuristik*)

2. Source criticism: (Quellenkritik) As long as only small parts of settlements had been excavated it was debated whether or not the settlers may have moved periodically from site to site using the slash and burn technique. After the excavation of complete sites and their analysis – at least in Germany – a continuous development of settlements seems more probable.

According to this model, the following sequence of quantitative methods has been used for the analysis:

- A) Single features (pits) were dated according to the ceramic decorations, using correspondence analysis. Features with similar contents were assumed to be approximately contemporaneous, whereas inventories with obviously different decorations were assumed to represent different times of origin.
4. Space and Time Relations (Interpretation der Bedingungen) The beginning and the end of any ceramic sequence can be identified by cultural historic comparisons. It is also possible to trace the development of the houses of the following Middle-Neolithic period out of the LBK architecture.
- B) The dated features were analysed according to their geographical location on site. Groups of approximately contemporaneous pits were attributed to the next possible house with respect to their chronologically relevant attributes. In special cases, with very dense features, these groups were elaborated by means of cluster analysis. In some cases, refittings of artefacts were used (BOELICKE *et al.* 1988: fig. 513).
3. Pragmatic Interpretation (Pragmatische Interpretation) Not all houses can be dated by means of the ceramic-chronology. It is certain, however, that these houses also existed in the LBK period. As it seems reasonable to assume a continuous development at one and the same site, the houses that could not be dated are used to fill the gaps in the dated sequence.

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|---|--|
| 5. Psychological Interpretation<br>( <i>Psychologische Interpretation</i> ) | When interpreting the resulting patterns it has to be asked why, for example, settlements of medium size only existed during the second half of the LBK, and why the ceramic decoration would seem to be quite monotonous at the beginning. These observations would appear to indicate a considerable social control in the LBK society due to a small population density at the beginning of the period, which only diminished when more people and settlements came into existence. |
| 6. Generalisations<br>( <i>Historische Schlussfolgerungen</i> )             | One interesting conclusion of the analysis of the LBK settlement structure can be summarised as follows: Crises seem to be not only a particularity of late capitalistic societies but already exist in tribal and to a certain extent also in segmentary societies.   |

In my opinion, the example presented shows that a substantial difference between a hermeneutic and scientific approach is not present in every single case. Even if there are areas of research where both traditions used different approaches, examples could also be presented of how quantitative methods can be used to support arguments about the reconstruction of ancient value systems or the rights of use of certain resources (ZIMMERMANN 1995).

I am convinced that it is important to invest more effort in theories and the combination of quantitative methods while simultaneously considering the interests of actors (micro-scale), large scale influences, and culture historic traditions, as it is done in some approaches of the so-called landscape-archaeology. Perhaps methods are needed which are as yet not widely applied in the archaeological community: social network analysis (applications: SCHWEIZER 1996; theory: WASSERMAN – FAUST 1994), wombling (ODEN *et al.* 1993) etc. The observation that certain theories and mathematical methods have specific ideas in common, as demonstrated for evolutionary thinking, might be a help in this direction.

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## **Comments**



# **Does German Archaeology have a Future?**

## **Some Reflections on the Esslingen EAA Session**

JOHN BINTLIFF

### **Abstract**

Merely fifteen years ago German archaeology was considered a suitable target for jokes in the English-speaking world, conservative and hostile to theory. This paper aims to show that on the contrary German archaeology has sustained rich veins of theory in diverse forms throughout the twentieth century, even if these were seen as minor specialisations compared to culture history and the study of finds. Moreover, dramatic changes in the present century have brought method and theory to the front of officially-supported research programmes.

### **Keywords**

Theory, culture history, *Landeskunde*, social archaeology, iconography

The session organised by the German *T-AG* for the 2001 EAA meeting was more prosaically and less controversially billed as ‘German Archaeological Theory and Practice in its European Context’, but one could, I think, reasonably retitle its message in the terms of my title above. Ignoring the high standards of German or German-speaking field archaeology and archaeological science, if we concentrate on theory and general interpretation, German achievements since the 1960s New Archaeology revolution in these areas have become a byword for backwardness and conservatism – to the point of aggressive rejection. Paul Bahn, in his humorous *Archaeology: A Very Short Introduction* (BAHN 1996), sums up the view from within the lively theoretically-aware community of the English-speaking world:

‘some archaeologists in Germany, where little attention has been devoted to theory, tend to consider the theoreticians as eunuchs at an orgy (especially as they are most unlikely to have any successors)’.

Those younger German scholars who have been tempted by the “orgy” have indeed in the past found themselves “emasculated” by finding employment in German academia unobtainable for their kind of research and teaching interests. One thinks of Heinrich Härke, welcomed as a “refugee thinker” at Reading

University in the U.K., and the even more extreme case of Cornelius Holtorf, whose love of theory led him to a pilgrimage to the very heart of cutting-edge Post-Processualism at Cambridge University, literally to fill the shoes of Ian Hodder. Perhaps significantly, whereas Härke's work still represents an intriguing bridge between the thorough knowledge of "the material" and modern "mid-Atlantic" theory, Holtorf's work is typically characterised by increasingly-limited interest in the archaeological data itself – focussing rather on the contemporary sociology of monuments (e.g. HOLTORF 2005). I shall argue that Härke's approach, far from being a transitional state of liberation between the artefact-bound meticulousness of anti-theory German scholarship and the fully-mature, state-of-the-art practice of Holtorf, is in fact evidence for a much brighter future for German method and theory than traditional conservatism *and* Cambridge can offer.

I have here opened to view, those strengths (or are they weaknesses?) of traditional German archaeology: the intensive study and presentation of the data, in endless fat catalogues with their global distribution maps and exhaustive footnotes, all too often rounded off with an interpretative section of minimal proportions and – to mid-Atlantic eyes – of theoretical naivety. On the other hand, although *Stilgeschichte* as an end in itself can hardly be defended, German archaeologists have to "know their material" in a way which very, very few U.K. and American university archaeologists are now able to display, especially those specialising in the production and consumption of higher theory.

But – how accurate is this picture as a complete overview of German theoretical achievements? At least in one major research area familiar to me I think not at all, and here I am thinking of *Siedlungsarchäologie*. Throughout the twentieth century ties between German fieldwork and historical geography, in what in the latter can be called the *Landeskunde* tradition, have produced innovative and important applications to archaeological landscapes of theories concerning human settlement systems in relation with the changing natural environment (cf. GRAMSCH 1996). Concepts such as the *Siedlungskammer* have been influential wherever scholars have come into touch with German research publications: here I could mention the Czech "community area" theory recently developed by E. Neustupny, M. Kuna, D. Dreslerova, N. Venclova and others (cf. DRESLEROVA 1995, papers by Venclova – Neustupny, and by Kuna in BINTLIFF – KUNA – VENCLOVA 2000); Tony Heidinga's studies of Early Medieval *Siedlungskammern* in the Low Countries (HEIDINGA 1987); and my own interest in its applications to the Greek landscape (e.g. BINTLIFF 1994; BINTLIFF *et al.* 2000).

Neglect of German innovativeness in landscape studies stems, I think, almost entirely from the fact that so few, and increasingly fewer, foreign scholars can read academic German publications. At the Esslingen session indeed Kristian Kristiansen presented powerful evidence that this process of linguistic closure is a worrying development in general within major national periodicals of Europe (British, French and German), despite a parallel process of Europeanisation in many other aspects of academic life (cf. KRISTIANSEN 2001).

One could counter that *Landeskunde* research has been a minor area of scholarship even in German-speaking lands, and almost exclusively confined to later prehistory and the Early Middle Ages; this may be true in recent decades, but for example in the Mediterranean the remarkable work of PHILIPPSON (e.g. 1950–59), LEHMANN (e.g. 1937; 1939; see BINTLIFF 2009) and KIRSTEN (e.g. 1956) in the early twentieth century on Classical landscapes remains a rich mine hardly probed at all since their time.

Moreover there are other areas of scholarship where outstanding innovative applied theory can be found within purely German traditional work. Apart from the well-known concern with Social Archaeology, often associated with an interest in Marxism, found in archaeological writings on both sides of the former East-West divide amongst German scholars (a point also stressed by MANTE in this volume), I was from my first encounter with it frankly amazed to discover the fruitful merger of such interests and techniques with the *Siedlungsgeographie* tradition in the work of Georg Kossack. The classic work of Kossack and others on the Sylt peninsula (KOSSACK 1974) is still head and shoulders above most landscape archaeology being produced today, but it remains virtually unknown in English-speaking scholarship.

Indeed, when asked to review a collection of Kossack's articles, translated into English (HÄNSEL – HARDING 1998; BINTLIFF 2001) I was even more struck by the breadth of theoretical interests shown in Kossack's work, which also includes readings of art and symbolism which reminded me very much of recent Post-Processual essays. The following quotation would have been very much at home in Kossack's oeuvre:

‘the argument is made that the articulation of an ideological field lay at the core of the changes of the early city states such as Corinth’.

Actually this is Mike Shanks (1995).

I think one can make a case for a growing ignorance of German publications since the last world war in the English-speaking world, leading to a neglect which has become blanket condemnation of German archaeology as the Anglophone tradition assumed hegemony in theory and method. It does seem that there have been significant internal German traditions in several major fields of applied theory. On the other hand, as was pointed out in discussion at the Esslingen session, scholars such as Kossack and others who could be mentioned in this connection (Hachmann was presented as another example, see STOCKHAMMER in this volume) were isolated and had no “research schools” built up around them. Their work was marginal to mainstream “normal science” in a Kuhnian sense, where most German university archaeology academics did indeed pursue traditional goals, in which explicit theorising played a minimal role if any. But a positive conclusion can be drawn: that German archaeology has always had a minor strand of innovative theory and method, distinct in its recent pathways from developments in other language traditions. This is very important in my view, for if German scholarship is to stand up as an independent and lively contributor to global debates on archaeological method and theory, it has to find a distinctive voice and not run after the disappearing image of core theorists in other countries.

As it happens, I find this prospect a very likely one, and for these reasons. Observers from outside the U.K. theory community, and some even within it, have benefitted from being somewhat behind the cutting-edge of new approaches or at least retaining a critical reaction to its manifestations, by achieving a more balanced view of the changing theoretical scene. One can reasonably characterise that scene, since the 1960s, as “serial murder”. New perspectives succeed one another via the assassination and rapid *damnatio memoriae* of their predecessors. Being accepted as a “player” in this world of seminars and symposia involves uncritical or at most muted criticism of the latest theory direction, lengthy deconstruction of previous studies, and casual application to case-studies where success is mainly measured in ideological correctness and increasingly little in terms of improved, demonstrable explanatory power for the complexities of the data under consideration. The history of research into a particular site, region, culture or phenomenon is utilized less and less as a rich foundation for deepening our understanding through new work, and more and more as a library of straw-persons whose supposed naivety or ideological incorrectness rule out claiming any benefit from their efforts – thus clearing the field for the display of the unassailably cleverer performance of a contemporary theorist, i.e. the author.

I note with amusement that much play is currently made of David Clarke's concept of "critical self-consciousness". This he meant as an attempt to release archaeology into an explicit science, but a revisionary interpretation sees him as anticipating the relativistic stance of Post-Processualism with its thesis that all research is unavoidably ideologically-biased. In embracing "self-consciously" any particular ideology, current archaeological theory forfeits its ability to act self-consciously as a critical force on distortions and manipulations of the past for presentist agendas.

It is my own feeling that ever since I and my generation were swept up into the New Archaeology as a messianic, self-promoting vehicle for young scholars of our age, short-term status in the discipline has taken precedence over an appreciation of its long-term development. We must jump after the latest intellectual trend in adjacent disciplines (*note none ever begin in Archaeology!*) and dare not stand back and review such novelties in any critical way for fear of being left behind amid the leaders of the theoretical chattering classes. Consulting studies made even ten years ago, is not recommended for current students of the discipline, let alone those of greater antiquity. And this despite the proven case that most ideas brought into our discipline are part of cyclical waves of popularity for well-known intellectual positions.

The increasing position taken outside of the U.K., and I observe this most particularly in my own new home in the Netherlands, is a far more balanced respect for tradition *and* innovation. Research work finds much still of importance in the attempts of earlier generations of scholars to make sense of particular facets of the past, and sees itself as carrying this work on with improved methods and theories. These new approaches, however, are looked on with a healthy scepticism, and (I see this as critical) – more as *models* than philosophies, ideologies or truths. 'Sounds interesting – let's see if it helps' is the common attitude. Secondly, most Dutch archaeological research rests either on a German-speaking tradition of close-study of the material (in the case of artefact or structure research), or on the Dutch tradition of wonderfully intensive dissection of landscapes and their occupation traces. Knowledge of the material – how the data are derived, their properties and limitations – and involvement in practical research are central to truly convincing applications of theory.

Here I see better guidelines for a future German Archaeology. This has retained its respect for earlier scholarship, for close reading of the material, and also has its own traditions which demand greater attention from the global archaeological community: in social archaeology, settlement geography, iconography, etc. If

explicit theorising has been under-developed since the 1950s, there are many healthy signs of a gradual blossoming of such studies in the younger generation of German-speaking scholars (symbolized by the establishment of a regular theory conference, for example). Just to illustrate this from my own professional field of Classical Archaeology: till quite recently German textbooks for this subdiscipline still marginalised theory discussion, and only isolated individuals (e.g. ZANKER 1988; 1996) kept us mindful of the buried potential of combining traditional German archaeological skills with greater theoretical awareness; I received just a few years ago a wonderful new student handbook in German from a friend and colleague which looks a more balanced introduction to the subject than currently available in English (LANG 2002)!

### Postscript

This paper was written in its main form for the Esslingen annual conference of European Association of Archaeologists in 2001. Now 10 years have passed and it is worth adding some further observations. Indeed what I have seen has been a veritable flourishing of German theoretical archaeology, and as a mainstream subject. Particularly striking, and a phenomenon I have been privileged to share in, has been a series of inter-university thematic research programmes sponsored by the DFG (German Research Foundation), where that powerful combination of deep knowledge of the material and an open embrace to trans-Atlantic theory has been very much to the forefront, as I hoped would come to pass. A fine publication from such a network is KIENLIN (2005). It is a combination of the stimulus that certain senior scholars have given, who were keen to change the character of German archaeology, and the coming of age of a new generation of young German scholars with an open mind and immense talent. Things can only get better!

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# Some Remarks on History and Methods of Central European Archaeology

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## Abstract

Herrmann brands the common characterisation of German archaeology in general as atheoretical as misfounded and argues for the existence of implicit theory instead. He then sets out a history of East German Archaeology. Herrmann rejects the imposition of any deductive goals on archaeological research, but sees the search for regularities governing the course of human history as the ultimate goal of archaeological and historical research, and the development of the means of production as the basis of human development. He insists on keeping Historical Materialism as a method of research apart from Marxism in general.

In the second part of his contribution, Herrmann discusses the development of archaeology in the GDR, which he sees misrepresented in the discussion following the unification. Directly after the war, prehistoric archaeology had a bad standing in the future GDR, as her practitioners had been heavily implicated in the ideological support for Fascism. The first bigger projects, coordinated by the Academy of Sciences, were rescue excavations in the bombed-out medieval city-centres. Academically, interdisciplinary research was encouraged. After a period of consolidation, the foundations of an archaeological theory were established, based on historical-materialist tenets.

Herrmann also emphasises the role of Wilhelm Unverzagt for the development of prehistoric archaeology in the GDR.

## Keywords

Historical Materialism, Marxism, means of production, archaeological cultures, economy, hidden theory

During the Esslingen conference and in the present volume, the history of archaeological research and of archaeological methods in Central Europe and adjacent areas have been assessed from various points of view. It is generally claimed that Central European Archaeologies abstain from theory in the methodological discussion. This is taken to be exemplified by the widespread

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<sup>1</sup> Translated by Ulrike Sommer.

German reticence about the theoretical assertions and the excessive theoretical debate championed by certain scholars especially in the second half of the 20<sup>th</sup> century in the Anglo-American language area. By this, I mean “New Archaeology”, “Processual Archaeology” and “Post-Processual Archaeology.” The reticence of German archaeologists regarding these directions of theoretical archaeology is often emphasised, sometimes in a rather stigmatising manner (MANTE this volume). In my opinion, it would be far more appropriate to classify the situation in Western Germany (Eastern Germany is excluded here) and in adjacent countries, and what can be deduced from the publications of scholars working there into the following three types, or rather approaches:

- A. empiricist approaches;
- B. hidden theory (MANTE 2007).

In my view, this does not only, or not even primarily include the concepts of the Anglo-American schools of thought mentioned above. Important studies are based on other approaches, for example, social critique or culture-anthropological approaches. For the term “military democracy”, for example, coined by L. H. MORGAN (1877) and adopted by Fr. ENGELS (1884), it could be shown that the same content, with a slight change of perspective, can be traced under the terms “Military Democracy”, “Early State” or “Chieftainship” (HERMANN 2002b: 28).

C. ‘Explicit discussions of terms and concepts (e. g. “culture”) without primarily aiming at classifying or interpreting a certain set of material culture [...]’ (SOMMER – GRAMSCH this volume: 15).

In regard to Slovenia/Yugoslavia it is claimed, for example that ‘[i]t was the use of models and explicitly pre-determined theoretical frameworks that critics were not reluctant to accept [...]’ (NOVAKOVIĆ in press). The quantum jump of Yugoslav archaeology is supposed to have happened under the influence of Childe’s works and to be based on ‘[...] the traditional culture history approach’ in the five volume *Praistorija jugoslavenskih zemalja* (NOVAKOVIĆ in press).

As already has been emphasised in the collected volume *Archaeologies of Europe* (BIEHL – GRAMSCH – MARCINIAK 2002), it is clear that there were distortions of archaeological research that could not be tolerated in a scholarly perspective. These arose from the heuristic requirements for the nationalist interpretation of the “Germanic Master-Race”. Deduction from models by the New

Archaeology and other Anglo-American trends or from the dogmas of Marxism, which in the main are not based on empirical results of archaeological research or were imposed on empirical archaeological research by outsiders, belong to a different category. As has been repeatedly emphasised, facts have been and still are adapted to deductive goals, which has produced speculations. Tendencies in this direction have been and still are present in the branches of Anglo-American Archaeology already referred to ‘as consequences of the extremely tense academic job market situation [...] following the pressure to publish’ (MANTE this volume: 112). Surprisingly, the contributors to the volume *Archaeologies of Europe* state that in archaeological “pure research” (*Grundlagenforschung*) no fundamental fault-lines existed between East and West – that is, across the Iron curtain (BIEHL – GRAMSCH – MARCINIAK 2002). Presumably, these fault-lines were only created by the emotionalised stigmatisation of historical-dialectical Marxism (see below). Goals introduced into archaeology for religious reasons probably only had a narrow scope, for example in the work and concepts of K. J. Narr. In his *Handbuch der Urgeschichte* (NARR 1966: 56) for example, Narr writes on human evolution:

‘The situation is designated by continuity in the physical sphere and discontinuity in the psyche, caused by the spark of the personal spirit in the objectifying referentiality to the environment and in its transference back to the Ego proper.’

For the confessing catholic, the precondition for this mode of thought was the papal Encyclical *Humani generis* from the 12<sup>th</sup> of August 1950 by Pope Pius XII. Under the influence of the archaeological-anthropological facts about the human evolution, the Encyclical separated the evolution of the body from that of the spirit and stated: ‘[...] the Catholic faith obliges us to hold that souls are immediately created by God.’<sup>2</sup> This also became the foil of a “hidden theory”.

The critical attitude to deduction does not imply that results derived from the course of human history in general should be taken account of in the orientation of archaeological research in general. But deductive goals (*Setzungen*) cannot be a-priori goals as they were for Narr, Hachmann or the exponents of a dogmatic

<sup>2</sup> *Litterae Enzyclicae de nonnullis falsis opinionibus, quae catholicae doctrinae fundamenta subruere minantur* (Pius XII, *Humani generis* 12<sup>th</sup> August 1950. Köln 1950: 37). English Version at [http://www.vatican.va/holy\\_father/pius\\_xii/encyclicals/documents/hf\\_p-xii\\_enc\\_12081950\\_humani-generis\\_en.html](http://www.vatican.va/holy_father/pius_xii/encyclicals/documents/hf_p-xii_enc_12081950_humani-generis_en.html) (last checked 27<sup>th</sup> July 2011).

“Marxism-Leninism” which could not prevail outside of the Soviet Union and that even in Russia had only limited impact compared to what the extensive archaeological “holistic research” in connection with large-scale excavations could attain. Meaningful archaeological research must aim to recognise social or cultural development, to understand it and to explain it in its context. This goal includes different levels, for which adequate methods have to be used or developed. A general goal for research can be enunciated; but it should not be determined by models or deductive goals such as ideas about the Creation, the “Quest for freedom” or continuous law-like progress. Nevertheless, the present state of knowledge implies that connectivities in prehistory and historic times did not occur by chance. The acquisition of the means of subsistence by productive activities, that is, by labour, is and has been the foundation of any progress of humanity. This is beyond dispute (TJADEN 1990; LAMBRECHT – TJADEN – TJADEN-STEINHAEUER 1998). *Man makes himself*, as CHILDE (1951) concluded. Even before that, based on the rather limited knowledge about human development at his time, the American ethnologist and social reformer MORGAN (1877) had constructed a model of social evolution in which the development of human production and its increasing perfection formed the key for periodisation. Marx and Engels studied Morgan and modified and complemented his results by their own research (cf. HERRMANN – KÖHN 1988; KRADER 1972; HARSTICK 1977). This was based on the philosophical foundation of dialectics and of Historical Materialism.

The philosophy of Historical Materialism requires to search for the impetus of human history in history itself, and for the course of history to be interpreted as an open-ended process (in opposition, for example, to Augustine, Kant, Herder and Hegel, who endorsed determinism – cf. HERRMANN 1999a; 1999b). Historical Materialism is the foundation for the analysis of capitalism and was the basis for creating a model according to which capitalism could finally be overcome by subjective forces, by the proletariat, through a struggle of interests or the class struggle and a new socialist society would come into being. This is the essence of what has been called Marxism. For pre- and protohistory in *sensu stricto*, Historical and Dialectical Materialism is the fundament of any research that has been labelled Marxist (cf. WAYARD 1991). This may also explain why for example V. Gordon Childe, although he had noticed the “real-socialist” distortions in the Soviet Union, did not abandon the approaches that Historical Materialism offered for the study of prehistoric social formations (SHERRAT 1997/98; TRIGGER 1997/98). It was only McCarthy and the ideologues of the

Stalin-era who equated the fundamental method of “Historical Materialism” with Marxism or Communism. One result of this irritating equation was that scholars and philosophers who accepted Historical Materialism as a research-method also thought they had to prune “dogmatic Marxism”. At the international Congresses of Historical Sciences, the debate about early societies was implemented as a co-operative scrutiny and by the discussion of research results. In practice, this meant that terms like forces of production, mode of production, social struggle and the emerging results and ideologies and their interdependence had a special relevance. The term “social formation” was and is used for the entirety of interactions that determine a society (ERDMANN 1987: 437 ff). After the social changes of 1989 and the new distribution of social power, people who had come into conflict with real-socialist conditions and the conditions of immobilisation during the Cold War vehemently repudiated Historical Materialism as a method of research as well as the results derived from it. This took place at the Meeting of the North-West German society for Antiquarian research (*Nordwestdeutscher Verband für Altertumskunde*) in September 1991 in Berlin, in an evening lecture where no discussion was permitted, and was repeated at the Archaeology Congress (*Archäologenkongress*) 1993 in Siegen in the study group “German archaeology after the unification” (*Die deutsche Archäologie nach der Vereinigung*). Results of archaeological research based on historical materialism that had received international recognition were branded as heretic and abnegated.

The contributions at the Esslingen-Conference and the contributions to the present volume ignore the fact that research-methods in Pre- and Proto-history had been widely and continuously discussed, not only in Poland, but also in the GDR. In addition to numerous monographs about different prehistoric periods, over decades several textbooks had been developed in interdisciplinary cooperation that define the state-of-the-art knowledge of historical and culture-historical conditions not only in Germany, but also beyond the German language-area. Numerous international conferences had assembled competent scholars from several academic disciplines, whether they worked in the so-called Eastern-block, in the countries of the West or in countries with other social systems. All these textbooks are based on the study of archaeological sources and their treatment in monographs or corpora. More detailed information on the publications of archaeologists in the former GDR is listed in the annual accounts (*Mitteilungen zur Alten Geschichte und Archäologie in der Deutschen Demokratischen Republik* 1, 1973–17/2, 1989).

According to the theoretical discussion of recent years, the collation of source-material represents “positivistic” work, which could be characterised as an “empiricist approach”. In fact, they make the source-material accessible, from large-scale excavations to extensive, often interdisciplinary analysis of archaeological sources and their comprehensive incorporation into the description of historical processes and periods. They follow the principle of trial and error, that is, the verification of deductive postulates by comparison with the sources. In relation to the above-mentioned three approaches or rather methods of research, these studies are based both on A, empiricist approaches as well as B, hidden theories and C, explicit discussion of terms and concepts. A consensus about theoretical principles was reached in study groups at international conferences and in various sub-projects. Between 1953 and 1988, over 250 diverse contributions to the discussion of the fundamental questions of periodisation, the essence of history, the history of social formations, the methodology of research and the presentation of the history of pre- and protohistoric periods were published in diverse annuals, periodicals and edited books. The theoretical content of the contributions differed, but the active interest in these problems led to numerous new approaches. Terms that have mushroomed in an inflationary way in the New Archaeology, like “revolution”, for example as “Palaeolithic revolution” (FEUSTEL 1969), “Early Iron Age revolution” in addition to the Neolithic and Urban revolution already discovered by Childe, have also played a role.

Anglo-American publications were available to students and scholars at least in the library of the former Central Institute for Ancient History and Archaeology of the Academy of Sciences (*Zentralinstitut für Alte Geschichte und Archäologie der Akademie*) and, for the most part, in the State Museum (*Landesmuseum*) for Prehistory in Halle.

The claim that ‘in the GDR [...] prehistoric research was developed [...] into a legitimisation science of the ruling state ideology’ (HÄNSEL 1991: 14) does not meet the facts. The theoretical discussions were based on Historical Materialism, but also on the results of behavioural sciences, cultural anthropology and on diverse other hypotheses (cf. COBLENZ 1998; GRINGMUTH-DALLMER 1993). Not a few chairholders in Western Germany categorically refused to discuss Historical Materialism and any results obtained by this method. There were no such prejudices in the discussion of theoretical questions in Eastern Germany. The numerous publications in periodicals, edited volumes and monographs of mainly international nature under the participation of scholars from several countries bear witness to this.

K.-H. Otto gave the first lecture about Pre- and Protohistory at the East-Berlin Humbolt-University after 1945. He introduced it with the words of an emigrant poet:

‘We do not want to be above or below other peoples  
from the Ocean to the Alps, from the Oder to the Rhine.’  
Berthold Brecht, *Kinderhymne*, 1950

This was the poetic expression of an unequivocal demarcation from the doctrine of the Germanic Master-Race that had been advanced in Berlin up to 1945 and that had formed the archaeological support for the justification of territorial expansion and conquest and the millionfold murder of “racially alien” people and people resisting the Nazi regime.

The aspiration to establish theoretical foundations for prehistory in a spirit of humanism, in clear opposition to nationalism and racism, was not easy to realise. As Prehistory had actively supported National Socialism ideologically, the reservations of the occupation forces and of government officials were deeply rooted (STEUER 2001). After all, before 1945 Berlin had been the institutional centre of Nazi prehistory, and thus the University of Berlin saw no urgent need to continue such a “rotten subject”. In contrast, in Western Germany, for several formerly active National Socialists the way to a Chair was open after “de-Nazification”. They mainly turned to positivism. Others were not satisfied with that but wanted to build a new framework for prehistoric and early medieval research. They turned to new tracks. H. Jankuhn, for example, in spite of being severely implicated during the Third Reich, built up a new research tradition, first in Kiel, then in Göttingen. Questions of economic and social research became central (STEUER 2001). In Eastern Germany, only great personal commitment made a makeshift heritage management by the Federal Antiquities Services possible (COBLENZ 1998). New approaches began only in 1948/49 with the foundation of a Committee for Pre- und Protohistory at the Berlin Academy of Sciences under the direction of F. Rörig, due to an initiative of W. Unverzag. The first objectives were excavations in the urban centre at Magdeburg and attempts to secure archaeological finds left in external storage or buried under rubble. Before 1949/50 there were no public deliberations about the theory of archaeological research. In contrast to the discipline of history, no archaeologists who had resisted the Nazi-regime were left in Eastern Germany. Constrained by this, new approaches had to be considered. The first lecture on Prehistory at the Humboldt-University for an audience of historians in 1951 thus introduced

a new epoch, encumbered by excessive theoretical demands but with only very general aims. In this situation, the theoretical-philosophical foundations of Marxism, and especially of historical and dialectical Marxism were tested and found to be applicable. The first paper that took the methods of historical materialism as its foundation was the habilitation of K. H. OTTO (1957). Before that, Otto had grappled with the fundamental tenets of Kossinna's teachings (OTTO 1953). He was sharply attacked from Munich (WERNER 1954). Surely, these early publications did have a number of weak points. These publications started a prolonged discussion of the nature of archaeological cultures, on ethnic interpretation and the necessity of tedious, small-scale work in preparing the sources. When P. Grimm started to teach at Humboldt-University in 1951/52, where he conducted lectures, seminars and field-trips, the archaeological research into the early Medieval Period was emphatically introduced to the students' horizon. Older periods were covered by the lectures of G. Mildenerger. The conditions of teaching, the broadening of an interdisciplinary horizon by the introduction of Mediaeval studies, physical anthropology, quaternary geology, ethnology and comparative linguistics developed the responsibility for thinking beyond disciplinary boundaries. This grew into an open discussion of theory and an assessment of the deductive foundations of scholarly work in general. Stalin's model of formations was rejected by Ch. Welskopf. She described her arguments in depth in her habilitation (WELSKOPF 1957). In the course of the discussions, new principles for acquisition of knowledge in history and culture-history<sup>3</sup> in close dependence on the sources were explored, in parallel to archaeological field research. As "hidden theory", these principles started to influence and to determine research and the layout of monographs. In 1965, the *Ethnographisch-Archäologische Zeitschrift* published a résumé. The fundamental agenda was to build up theoretical foundations that could lead from excavation/research in the field up to the incorporation of the results into the general conception of history (HERRMANN 1965). In the same year Jankuhn used the so-called "settlement-archaeological method" to research into settlement history and the structure of single settlements. On this foundations, he claimed that

'insights into the economy of certain periods and the social structure of the inhabitants are achieved. A necessary and, because

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<sup>3</sup> In contrast to common English archaeological usage, the German term "*Kulturgeschichte*", culture history denotes the historiography of cultural and social development, as opposed to the political history of the great deeds of great men [US].

of the nature and information value of the sources, exceptionally useful supplement of the potential for acquiring knowledge is thus created by the so-called archaeology of cultural groups' (*Kulturgruppenarchäologie*, JANKUHN 1965: 2).

After more than ten years, and after a repeated scrutiny of the "hidden theory" in monographs and catalogues, a synopsis of theory could be published in 1976/77 (HERRMANN 1977; reprinted as Herrmann 1986). The way from archaeological sources, that is, archaeological features and finds leads in five steps to the definition of an archaeological culture. An archaeological culture thus defined does not primarily form the foundation for any discussion of ethnic affiliation, but is a precondition for any further research into the inherent historical associations, of economic as well as social and cultural links. At the same time, it forms the fundament for any social and economic analysis (HERRMANN 1986).

Any inferences about the condition of a society – this seems to be uncontested – can be made primarily or even solely were the economic foundations of social groups, of larger and smaller settlement units have been sufficiently explored (HERRMANN 1986: 349). Scientific analyses are indispensable for any modern research into the economy or social structure. In this regard, it should be tested whether DNA-analyses can be useful to deduce family structures in cemeteries. Attempts to elucidate family-relationships in cemeteries excavated *in toto* by means of analysing the similarity of somatic traits have been proven to be extremely labour-intensive and not very successful (ULLRICH 1969).

Several monographs as well as textbooks on the history and culture of the Slavs (HERRMANN 1970, considerably reworked 1985) were generated, based on the analysis of settlements as described above and on social-economic analysis. The textbook on the *Germani* and other overviews are also based on this approach. The methodology for writing textbooks and overviews in a way was not so different from the principle described by Müller-Karpe:

'Time and time again, scholars have had to confront attempts to desert the methodologically clean approach of obtaining insights by a subtly detailed critical analysis analysing of the available sources in favour of theoretical-systematic direct approaches. Such schools of research that often claim to be innovative tend to compensate a lack of thorough knowledge of the archaeological record by a certain type of models and number-crunching statistics' (MÜLLER-KARPE 1980: VI).

For a long time, economic and social analyses have claimed much attention in the methodological discussion, based either on Historical Materialism or with a background of borrowings from other, different concepts, both in the European and in the Anglo-American theory-discussion (CHAPMAN 2003; VEIT 1998: 15–66; KÜMMEL 1998; VEIT *et al.* 2003; SOMMER 2003; WAYAND 1960; SPRIGGS 1984; HERRMANN 1999 b). This is not the right place for an in-depth discussion of the different aspects and results this discussion is based on<sup>4</sup>. I would like to mention the emphasis on economic aspects in the research on subsistence (LANGE 1971; LAMBRECHT ET AL. 1998; TJADEN 1990). Already Herder saw the environment as an important factor in the creation of theoretical approaches. For Marx and Engels, these aspects were included in Historical Materialism, the foundation for elucidating the nature of work and of human behaviour.

There have been numerous publications concerning the baselines of pre- and protohistoric research (BIEHL *et al.* 2002; NEUSTUPNY 1998; contributions in this volume). In my view, the development of prehistoric research in Germany and neighbouring countries has been following two directions. A “pre-eminently national research” was based on Nationalism and Germanophilia. This school was founded by G. Kossinna, who formulated guiding principles on the culture of the *Germani*, who were seen as racially superior. Archaeological finds were supposed to document the validity of his principles. This direction of research culminated in the Third Reich and foundered with it (LEUBE 2002; STEUER 2001; GRÜNERT 2002).

In contrast, the school founded by R. Virchow together with H. Schliemann (HERRMANN 1990; ANDREE 1976) was to be continued across epoch-boundaries. It was based not on theoretical determinants, but on research in the field, aimed at elucidating human history in the periods before written sources. Together with

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<sup>4</sup> I refrain from reproducing the schema of the development of archaeological sources (on the three steps in detail see HERRMANN 1996: 386, 399). BRATHER (2003: 42) also refers to three steps, which he calls planes. However, the ranking of his planes is different. “Archaeological sources allow structural insights. They elucidate relationships, not processes” (BRATHER 2003: 41). These statements, presented as results, obviously need to be relativised. Even written sources do not contain objective statements about historical processes as such. The presentation of universal history or of regional prehistory is based on the fact that archaeological sources allow statements about historical processes as the result of their methodical decipherment. Therefore, I follow the concern of different directions of Anglo-American archaeology, even if I evaluate the ways leading to corresponding statements and thus the certainty of the statement differently.

other scholars who shared his views, Virchow in 1869 created an organisational base, the *Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte* (Berlin Society for physical Anthropology, Ethnology and Prehistory) and the *Deutsche Gesellschaft für Anthropologie, Ethnologie und Urgeschichte* (German Society for physical Anthropology, Ethnology and Prehistory), founded in 1870 (HERRMANN 1990; ANDREE 1976). Virchow himself, interested in history already as a schoolboy, tried to systematise archaeological research by careful and well documented excavations, purposefully using stratigraphy and typology to interpret the finds. Furthermore, he called for the use of scientific methods in archaeological research by including physical anthropologists, zoologists, chemists, physicists, palaeobotanists etc. Virchow's position at the University of Berlin and the Berlin Academy as well as in the above mentioned interdisciplinary Society for Anthropology, Ethnology and Prehistory was a good starting point. Based on this, between 1869 and 1872 he could, for the first time, differentiate between Stone-Age, Bronze-Age, Slavonic and early German pottery and thus also between the corresponding fortifications and settlements. The stratigraphic excavations of Schliemann in Troy were to become his biggest field for experimentation. Since his studies in Paris, Schliemann perceived stratigraphy and the three-dimensional metric recording of finds as axiomatic for prehistoric research. Thus, Virchow and Schliemann could easily agree on the use of this fundamental method of archaeological research. Together, Virchow and Schliemann worked out a stratigraphic sequence for Troy that is more or less still accepted today (HERRMANN 2000; HERRMANN 1990; HERRMANN – MAASS 1990). C. Schuchhardt followed this tradition and continued it in his research on fortifications and settlements, especially in Lower Saxony and since 1906 as director of the Prehistoric Collections of the National Museums at Berlin. In 1926 Schuchhardt retired for reasons of age and was succeeded by Unverzagt.

Between 1926 and 1932, Schuchhardt and Unverzagt worked out diverse methods for advancing prehistoric research in Eastern Germany. One of the most important projects was the foundation of a "Study-Group for Research into the Northern and Eastern German Pre- and Protohistoric Fortifications" (*Arbeitsgemeinschaft zur Erforschung der nord- und ostdeutschen vor- und frühgeschichtlichen Wall- und Wehranlagen*). Unverzagt became secretary and scientific coordinator, Schuchhardt was elected chairman. Up until 1932, the work progressed speedily. A catalogue of prehistoric fortifications was generated, based on archaeological archives, written sources and fieldwork. They aimed for the establishment of a research institute, based on the experiences accrued

by the *Römisch-Germanische Kommission* (RGK). But in 1933, the funding for the inventory of fortifications was withdrawn, the foundation of the institute failed (UNVERZAGT 1985). Unverzagt preserved the catalogue recording the fortifications till after 1945. It became the primary foundation for the survey of fortifications in Eastern Germany, for the *Textbook of Pre- and Protohistoric Fortifications* (1958 ff) and the *Corpus of early Medieval Archaeological Sources on the Territory of the German Democratic Republic* (HERRMANN – BARTHEL 1968 ff) and further studies (HERRMANN 2002a: 92 ff). The problems the research on [mainly Slavonic] early medieval fortifications in Eastern Germany faced between 1933 and 1945, the difficulties and distortions have been discussed at length (HERRMANN 1982; 2002a; UNVERZAGT 1985; 1988; BRATHER 2001). When the Berlin Academy was officially re-opened in 1946, Unverzagt compiled a programmatic article intended to initiate the foundation of an institution for pre- and protohistoric research (UNVERZAGT 1985: 51). The president of the Academy decided to wait and see. Under the presidency of Rörig, a member of the institution that was now called *German Academy in Berlin* the research institution Unverzagt had requested for was finally installed as the Prehistoric Commission, with Academician Rörig as the chairman. The first major project was the urban archaeology of Magdeburg. Unverzagt became secretary of the newly founded Committee for Pre- and Protohistory, and later, after his re-election 24/03/1949 as a member of the Academy, chairman. In 1952, a Section for Pre- and Protohistory of this committee was founded; all important prehistoric archaeologists of the GDR and some colleagues from the FRG became members. On the 14<sup>th</sup> of October 1953 the Institute for Pre- and Protohistory of the GDR at the German Academy of Sciences was founded, based on the Commission, with Unverzagt as director. In 1954–1958 he was secretary of the Class for philosophy, history, political sciences, law and economics of the Academy for one elective period. At the same time, he organised the first state antiquities service via the Section for Pre- and Protohistory of the Academy. In 1954 a bye-law for the protection and conservation of pre- and protohistoric monuments was passed, based on the work done by the Section, and a scientific board for archaeological heritage management was instituted, directed first by W. Unverzagt, later by W. Coblentz.

Unverzagt did not burden himself with theoretical considerations. After his experiences in the First World War, the Weimar Republic and in the time of National Socialism, he was an open-minded pragmatist and lobbied for the development of pre- and protohistoric research in Eastern Germany. His aim was the creation of a prehistoric archaeology for the whole of Germany. The rejection

of the “Draft for a peace agreement with Germany” by the Western Allies and the Bonn government led to the longterm partition of Germany. Unverzagt kept his connections to the now West German Archaeological Institute (DAI) and to international bodies, especially the *Union Internationale des Sciences Préhistoriques et Protohistoriques*. In 1965, he became the co-founder of the *Union Internationale d’Archéologie Slave* and was elected vice-president. Until his 71<sup>st</sup> year he remained director of the Institute at the Academy. In 1963 K.-H. Otto became director. Unverzagt kept his office and the employees who conducted the excavations at Lebus. On his way to Lebus, a few days before Christmas, he suffered a fall, broke his thigh and died of after-effects on the 17<sup>th</sup> of March 1971.

Under Unverzagt, scientific facilities were established at the Institute, physical anthropologists, biologists, zoologists and physicists were employed and a radiocarbon-lab set up. In 1968, he advised on the structure of the future Central Institute for Ancient History and Archaeology. He was an advisor for the first part of the Textbook *The Slavs in Germany* (1<sup>st</sup> edition 1970). Unverzagt saw historical materialism as a fertile method of research, but without using it in the few works he published after 1945. But he established a close connection to G. Childe, invited him to Berlin and campaigned for his election as an international member of the Berlin Academy. At the evening lecture at the Leibnitz-day of 1965, Unverzagt emphasised the merits of Childe, especially in popularising prehistoric archaeology.

Except for the Soviet Union, it is especially in England that the pre- and protohistoric remains have been used to elucidate the social and economic structures of antiquity. Publications like those of Gordon Childe and Graham Clark’s *Archaeology and Society* can be considered exemplary in this context (UNVERZAGT 1952: 485).

Unverzagt made comparable statements in the periodical *Ausgrabungen und Funde* (UNVERZAGT 1959), but at greater length. The orientation of prehistoric research that had been inaugurated by Virchow was not only administered by Unverzagt in the Schliemann-Department of the Museum for Pre- and Protohistory in Berlin, he also tried to continue Virchow’s broad approach. He respected and supported new theoretical approaches and modern interdisciplinary research. Archaeological research in Eastern Germany, respectively in the former GDR is connected with his name. His open-mindedness encouraged the development of archaeological research in the GDR, following empirical principles and a

theoretical multiplicity, from cultural anthropology to Historical Materialism, whenever these were based on a solid knowledge of the archaeological sources.

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# **The “German School of Archaeology” in its Central European Context: Sinful Thoughts**

WŁODZIMIERZ RĄCZKOWSKI

## **Abstract**

There is no doubt that the pejorative meaning of the term “German (continental, Central European) school of archaeology” is deeply rooted in archaeological thought. But what exactly does “German school of archaeology” mean? I would like to discuss certain aspects of the “German school of archaeology” and its Central European context, assuming they are scholarly myths. If scholarly myths appear in the realm of generalised statements based on induction, then perhaps one needs to move towards detailed studies of individual events in their historical context in order to dispel them. In this paper I will discuss: the conflict between Kossinna and Kostrzewski, the domination of German archaeology in Central Europe, theoretical (or atheoretical) issues of German archaeology, issues related to power/knowledge within Central European archaeology and critical reflections on archaeological practice. I come to conclusion that in the studies of the history of archaeological thought, which include the phenomenon of the “German school of archaeology” as well, we need to focus on comprehensive analyses of individual achievements. I worry that in many modern analysis of the history of archaeological thought, we apply common-sense thinking and factographical approach to historical events, the approach which we otherwise criticise.

## **Keywords**

History of archaeology, theoretical archaeology, German school of archaeology

## **Introduction**

I have to start with a confession: I often use the term “German school of archaeology” in an official or informal capacity. And it will not come as a surprise to anyone that for me the term is associated with a negative appraisal of a certain approach to archaeology, as compared to the Anglo-American approach. The hidden implication is an ossified archaeology oriented towards typological and chronological studies that does not see the human behind the artefact (the sin of contempt). Thus, on a general level of reflection I have a set of contrasting terms showcasing clearly what is good and what is bad in archaeology, a very

useful form of categorisation indeed that can be used in any form of simplified and persuasive narrative. Having such a useful dichotomy at hand, we eagerly make the criteria fit the theory. Certainly, I have not invented this dichotomy – it has been widely used in archaeology, specifically by the advocates of theoretical archaeology. The pejorative meaning of the term “German (continental, Central European) school of archaeology” is deeply rooted in archaeological thought. I, too, felt calm and at peace with it because it made the world look simple and obvious.

And yet, my peace was shattered when I was commissioned to write this article. My first thought was: ‘Is there anything to add to that barrage of criticism of the “German school of archaeology?”’ The second thought followed immediately: ‘What kind of school is it, what is typical of it?’ A storm of conflicting and chaotic thoughts led me to the conclusion that the world of Central European archaeology is neither simple nor obvious. Why not try to cope with this problem?

### The question of definitions

The essence of the “German school of archaeology” boils down to some characteristic features such as cognitive optimism, empiricism, the imperative function of description, inductive reasoning, the emphasis on typology, chronology and simple distribution-maps (for example, OSTOJA-ZAGÓRSKI 1988; HÄRKE 1991; MAMZER 1997; MINTA-TWORZOWSKA 2002; BINTLIFF in this volume). These traits are recognised on different epistemological levels. At the general level there are traits inspired by August Comte’s philosophy such as the conviction of the duality of the subject and object of cognition (the object/the past has a definite form that existed prior to cognition and independent of it), the conviction that real knowledge is governed by experience (empiricism) and that it is cumulative (including the role of induction, for example in ZYBERTOWICZ 1995). The more specific epistemological level refers to the acceptance of evolutionism and diffusionism and points towards the methods and tools of cognition (the role of analogy, the importance of chronological and typological findings in the forming of developmental sequences, the role of diffusion and migration as the explanation of culture change etc.).

On closer examination these are the traits that describe the fundamentals of culture-historical archaeology. Can culture-historical archaeology be put on a par with the “German school”? The achievements of European (and not only European) archaeology in the first half of the 20<sup>th</sup> century are the legacy of the

culture-historical approach. Why is the “German school” blamed for its every misconduct (such as the proclivity towards nationalist interpretations)? Besides, it still has many followers in Great Britain, Scandinavia, France or the USA.

On the other hand, an analysis of the works by such German archaeologists as Ernst Wahle, Karl Hermann Jakob-Friesen, Hans-Jürgen Eggers, Rolf Hachmann, Herbert Jankuhn, Georg Kossack and Manfred Eggert, inspired by their contemporary and widely accepted theoretical concepts, reveals a lot of modern thoughts that cross the traditional borders of the “German school of archaeology”. Can anyone imagine German archaeology without these names? If they do not satisfy the selection criteria, who does? Who and what constitute the ‘German school of archaeology’?

It seems that already at the level of definitions we are relegated to the domain of stereotypes about the ‘German school’. The criteria are blurred and ambiguous, the school is not easily definable (cf. BARFORD 2002a; BERTEMES 2002). Is the category “German school” consigned to oblivion? I doubt it since it has become deeply rooted in archaeological consciousness. It has the function of an academic myth.

### In the fetters of myths

When studying the literature on German and Central European archaeologies several common threads emerge, such as the Kossinna syndrome, ethnic interpretations of archaeological cultures, the conflict between German and Polish archaeology and the domination of the “German school”. One has the impression that attractive metaphors (for instance, the Kossinna syndrome) and spectacular oppositions (Kossinna – Kostrzewski) have taken root and dominated the entire discourse and, in a way, they have been transformed into academic myths. For TOPOLSKI (1996: 203f) it is obvious that myths are also part of academic thinking. In scholars’ minds (and naturally in the images of the world they create) myths originate from two sources:

1. from the ways the world is comprehended in the society (the so-called fundamental myths – for instance the myth of the objectivity of cognition) and
2. from the methods of scientific development in which beliefs once formed are never verified and become rooted.

Mythologized beliefs and the images of the world exist in scientists' minds and also in the theoretical-ideological (deep, governing) layer of narration. Thus, they influence the logical-grammatical and persuasive layer of narrative, they spread and become rooted. Specifically, myth-forming is the empirical approach which uses induction in formulating general conclusions. Separate individual events are treated as if they represented a class of events. Once formulated, a persuasive opinion based on an individual event or idea remains in scientists' minds and governs their thinking.

It is difficult to free oneself from the influence of myths on scientific procedures and their effect, i.e. narratives. On the one hand, one needs to realise that widely accepted beliefs are not necessarily final and ultimate, on the other hand it requires a certain discipline of thinking and the ability to recognise the so-called black boxes (cf. JONES 2002: 29–35). Obviously, when debunking myths we create new ones.

I would like to discuss certain aspects of the “German school of archaeology” and its Central European context assuming they are scientific myths; in doing so I am most probably committing the sin of pride. If scientific myths appear in the realm of generalised statements based on induction than to dispel them one needs perhaps to move towards detailed studies of individual events in their historical context.

### Gustaf Kossinna vs. Józef Kostrzewski

The conflict between Kossinna and Kostrzewski is part of the classic discourse about the history of Central European archaeology, and specifically about the relations between Polish and German archaeology (ŽAK 1974; HÄRKE 1991; WIWJORRA 1996; LECH 1997/1998; GEDIGA 2000; BARFORD 2002b; etc.). We have a clear plot-line – Kostrzewski, Kossinna's student, applies his method to formulate utterly dissimilar views on ethnic issues. The nationalist motif is present on both sides. The problem can therefore be viewed from two perspectives – the perspective of academic research and the perspective of the nationalist-emotional discourse. In the literature of the 1990s there is a prevailing belief that these two perspectives cannot be separated and that scientific research is closely bound to ideology and politics (e.g. KOHL – FAWCETT 1995; CHAMPION – DÍAZ-ANDREU 1996; LEUBE 2002; etc.). While not disregarding the aforementioned belief, I would like to deal with these issues separately.

Józef Kostrzewski studied archaeology in Berlin with Gustaf Kossinna at a time when the *Siedlungsarchäologie* concept was already been well known. Naturally,

he knew it and he could apply it though he did not entirely accept its foundations and suggested several amendments. Kostrzewski's remark that a change of an archaeological culture does not necessarily involve a change of ethnos was crucial but it did not challenge the validity of Kossinna's ideas. There is no strident criticism of Kossinna to be found in Kostrzewski's works, rather a typical scientific discussion that conformed to the standards of the time. How can we talk about a serious conflict between Kostrzewski and Kossinna or Kostrzewski's anti-German attitude on such vague grounds?

In fact Erazm MAJEWSKI (e.g. 1905) and Leon KOZŁOWSKI (e.g. 1920; 1928) were more anti-German insofar as they vehemently rejected *Siedlungsarchäologie* as a method of scientific research. Not only did they question its scientific basis (for instance, the classification of archaeological material based on an *a priori* accepted concept) but they also pointed to some threats (like the proclivity to ideological interpretations) (ŻAK 1974: 55f; WROŃSKA 1996: 84).

If denying Kossinna's ideas were “anti-German” (and I do not doubt that this is a gross overstatement) then Majewski and Kozłowski should go down in history as “combating German nationalism”. Strangely enough, they are not the main characters in the plot. From the point of view of discursive persuasiveness, the opposition of scientific views of Kostrzewski and Kossinna can be easily replaced by another opposition, namely that of a German and a Pole. Kostrzewski was far more suited to play this part than Majewski and Kozłowski, because he came from an anti-German *Wielkopolska* (Great-Polish) family and the negative attitude to Germany “ran in his blood”.

From a scientific perspective the conflict between Kossinna and Kostrzewski is not that obvious or maybe even nonexistent (!). Let us therefore take a closer look at nationalist interpretations. Kostrzewski made a fierce contribution on this issue (cf. KOSTRZEWSKI 1970). Was his adversary Kossinna? Not at all! The emotional exchange about misusing archaeological record in ethnic interpretations to justify expansion on neighbouring territories took place between Józef Kostrzewski and Bolko von Richthofen from the late 1920s to the mid-thirties (cf. ŻAK 1974: 62; RĄCZKOWSKI 1996). However, the political situation and the historical context were slightly different. Kostrzewski already held clear political views (conservative, nationalist-democratic) and a dominant position in Polish archaeology. Was Bolko von Richthofen's position in German archaeology equally strong? Though very vivid and emotional, the dispute was limited and drew a muted response. Almost no other archaeologists from both countries participated, and the bulk

of the texts were published by local publishing houses, and only infrequently in important Polish and German archaeological periodicals.

The problem thus existed, but in my opinion it was inflated by historians of archaeology. By combining both perspectives, the interwar archaeology could stage a spectacular show which guaranteed immediate success in the European literature of the subject!

### Domination of the “German school of archaeology” in Central Europe

As I have already mentioned, the “German school of archaeology” is not easily definable (and I am not the only one to point it out, cf. for example BARFORD 2002a; BINTLIFF this volume). Intuitive criteria seem to be more important here than any detailed analyses of the term. Additionally, the criteria used are usually more relevant to the culture-historical approach than to the specificity of German archaeology. However, some traits of the culture-historical approach seem to be specifically German, such as acute descriptions of archaeological material resulting in powerful catalogues and detailed distribution maps or in Herbert Jankuhn’s *Siedlungsarchäologie* (cf. BINTLIFF this volume).

Taking these two issues into consideration, is it possible to maintain that the entire region has come under the domination of German archaeology? My impression is that outside Germany it is hard to find any descriptions of archaeological finds that can meet German standards. Naturally, there are many descriptive studies and catalogues, because the culture-historical approach sees the description of material and chronology as its major scientific task. That is why we can find descriptions and catalogues in any country that used the culture-historical method, be it in the Balkans, Scandinavia, Russia or Great Britain (not to mention non-European countries). Still, these catalogues differ substantially from the German ones. In my opinion, the “German school of archaeology” did not dominate the archaeology of Central Europe in this respect (for a different view, see KADROW this volume).

Herbert JANKUHN’s *Siedlungsarchäologie* (1977) is one of the most interesting attempts at transcending the limitations of culture-historical archaeology from the “inside”. It consisted of multi-faceted studies of settlements in their environmental context, taking into consideration the role of economy and resource exploitation in the process of settlement. It stimulated the application of analyses borrowed

from natural sciences in the studies of environmental changes in the past and in determining the role of humans in these processes (possibilism).

Polish settlement archaeology kept reproducing the standards developed in the 1950s and 1960s, which led to stagnation in this field of research (cf. ŽAK 1983; RĄCZKOWSKI 1997). Although *Siedlungsarchäologie* did not transgress the framework of culture-historical archaeology and its limitations (cf. ŽAK 1983), the concept offered the potential of improving the standards of Polish settlement pattern studies (RĄCZKOWSKI 2001). Jankuhn’s book was published in Poland in 1983 and neither provoked any discussions nor wielded any influence on archaeological practice. The example of Czech archaeology, where independent concepts of settlement studies had emerged (for example NEUSTUPNÝ 1998; GOJDA 2000), proves that in this respect the “German school of archaeology” did not establish any domination either.

It is definitely justified to speak about the domination of German archaeology in terms of finances, human, material and university resources. In terms of research, to emphasise again, the obvious similarities, one could speak of family-likenesses in the approach to research problems developed from the acceptance of culture-historical archaeology and ... the language.

Central European archaeologies seem to be dominated linguistically by German archaeology. The bulk of scientific papers in these countries are published in German – a quick glance at library catalogues proves that texts in German occupy second place behind the national languages. Does it attest to the attractiveness of German archaeology?

I am probably committing the sin of disbelief, but in my opinion it does not. I can still recall my first days at the faculty as a student. From the very beginning we were made to study three languages: Russian, Latin and German. There was no option, which even back in the early 1970s was interpreted as a breach of student rights. Russian was taught for ideological and political reasons, Latin has always been on the humanities curriculum. But why German? Our inquiries into that matter (most inappropriate, of course) were usually answered in this way: ‘Most analogies to archaeological finds and features in Poland are published in German! How can you become an archaeologist if you cannot read the German literature on the subject?’ The meaning did not dawn on me at that time. Now it seems very obvious, because it reflects a certain theoretical attitude in archaeology.

Since its emergence in mid-nineteenth century, Central European archaeology was published in German. A look at the political map, and everything becomes evident – the German language dominated in the area from the Balkans to the

Baltic Sea, from the western Ukraine to France. Added to this the archaeological practice, which focus on the record that needs to be compared with records from neighbouring countries in order to determine similarities and mutual influence (and all the sources in the neighbouring countries are already written in German), no wonder that the German language has become the language of scientific debate (with the exception of Palaeolithic studies, usually carried out in French). Why would Polish archaeologists need other languages? They would never seek analogies to pottery or stone tools in Great Britain or Portugal! Even 50 years of Russian domination did not change this tradition.

Once again, I would like to stress that the language kinship emerged from the acceptance of culture-historical archaeology and not from the domination of German archaeology. I have yet another sin on my conscience – that of uncompromising stubbornness.

### Atheoretical German archaeology

One of the dominating threads in the discussion of German (continental, Central European) archaeology is its relation to theory. The opposite poles are Anglo-American archaeology (good because theoretical) and German archaeology (bad because ... atheoretical). Once again I allow myself to voice my doubts (cf. also KLEJN 1993), and thus commit a sin of an exceptionally malicious provocation.

I would like to refer to TOPOLSKI (1983) who, analysing the problems of the methodology of historical sciences in a modernist manner<sup>1</sup>, pointed to mechanisms governing scientific procedures. These are the vision of the world and people, the ideal of science (its overriding objectives from which specific scientific tasks emerge) and methodological principles. The mechanisms also include the scientists' methodological awareness in which, according to Topolski, theoretical knowledge plays an important part. Thus, theoretical knowledge has influence on the formation of research objectives, the selection of material, hierarchy and systemisation, and, in consequence, on the formation of narratives.

From the hermeneutic point of view, scientific practice is also influenced by pre-understanding (for example HODDER 1999), which determines its field of interest and the selection of tools/methods. Theoretical assumptions, be they conscious or unconscious, are part of this pre-understanding.

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<sup>1</sup> In my understanding of modernism I follow D. MINTA-TWORZOWSKA (2002), where modernism equals antipositivism.

Seen from this perspective German archaeology (and other archaeologies as well) is theoretical, but its theory differs slightly from the ones that dominate science today. Within this theory we can distinguish two basic levels that govern archaeological thought. The first level emerges from a philosophical concept formed in the first half of the 19<sup>th</sup> century called positivism. It is positivism which assumes that an objective perception of the world through experience is possible. It is positivism which states that the truth about the world is acquired through the collection of empirical facts and their description. It is positivism which states that cognitive activity is neutral in relation to disputes about values, and it is positivism which rejects theoretical statements as non-binding.

The other level that governs archaeological thought comprises two basic theories borrowed by archaeology from the outside, namely evolutionism and diffusionism. Despite the fact that diffusionism and evolutionism as paradigms were in opposition, they soon merged. In archaeology, evolutionism manifested itself in the speculation about the origin of phenomena, culture changes and the need of dating. That is why the main tools in archaeology used in the dating of artefacts are typology and analogy (at present also other methods). Diffusionism introduced maps as the main research tool in archaeology, and also such categories as diffusion and migration and the concept of archaeological cultures. The fusion of both theories on the basis of positivism resulted in the theoretical concept of culture-historical archaeology (for instance, TRIGGER 1989). The fact that German archaeologists construct incredibly erudite catalogues and nothing but catalogues is the effect of this adopted theoretical reflection, namely, positivism. In this sense theory is present in German archaeology in the form of the conceptual apparatus (archaeological culture, diffusion, analogy etc.) and research tools (PALUBICKA – TABACZYŃSKI 1986: 78), but also as the set of general statements about the past and/or about the methods of studying it (positivism, evolutionism, diffusionism). And no-one can ever convince me that German archaeology is atheoretical.

I can see a problem somewhere else – in the reflection about what and how is done in archaeology. This issue, however, is partly about the relation between power and knowledge.

### Power – knowledge and ... “thoughtless” archaeology

At least since FOUCAULT (for instance GORDON 1980), the relation between power and knowledge is recognised as one of the fundamental factors of the development of science. Many examples can be given to illustrate this relation (for example,

the influence of politicians and ideologists on nationalist interpretations of the past – cf., among others, LEUBE 2002 and the comment by BURDA 2003). The relation is also present at other levels of social life. The organisational system of science is a good example. Undoubtedly, the highly hierarchised system of science (including archaeology) in Germany (cf. SOMMER 2002; BERTEMES this volume), Poland and other countries affects the knowledge produced in scientific circles.

### Groups controlling power/knowledge

University hierarchy has a long tradition. When archaeology emerged as an academic discipline, it was incorporated into the existing system, with the entire burden of archaeological theories and procedures. Let us take a closer look at this system. High in the hierarchy are scholars who have the knowledge about the past and the experience of dealing with the past. Their knowledge is positivistic, they are convinced that science (archaeology) uncovers the truth about the past. They already know the past (at least to some extent). Positivistic truth is singular and absolute, objective and irrefutable. Thus, high up in the university hierarchy there is a group of scholars who know the truth about the past, who have developed certain tools and procedures that lead to truth and knowledge. Moreover, this group is also equipped with socially accepted means of evaluation (repression) of those who try to infiltrate it.

Thus, what is the prescription for a successful academic career? The answer seems very easy indeed: write what you are expected to write. Accept the theoretical foundations, the tools and scientific procedures as used by your masters because they lead to real knowledge about the past and will be positively evaluated (cf. HÄRKE 1991; MINTA-TWORZOWSKA 2002: 53). In this way the masters clone themselves (at least as far as archaeological procedures are concerned).

This is, of course, a simplification. Nevertheless, let us examine two examples in which the prevailing relation of power/knowledge was in a way violated: Herbert Jankuhn's *Siedlungsarchäologie* (Settlement archaeology) and Hans-Jürgen Eggers's concept of the selection of archaeological material. Jankuhn introduced some new elements to the traditional set of ideas and tools of culture-historical archaeology but he followed its spirit and fundamental principles. Jankuhn did not question the prevailing practice, rather he enriched it. No wonder that his theory was accepted and it still remains one of the specific features of German

archaeology (irrespective of the fact that it evokes dangerous associations with Kossinna’s theory).

The second example is different. A simple proposition by EGGERS (1959) pointing to the positive and negative selection of materials by people in the past dangerously challenged the prevailing conviction of the total objectivity of archaeological record. Any discussions of this proposition (anticipating later suggestions by SCHIFFER – for instance 1987) could necessitate significant changes in the procedures utilised by culture-historical archaeology and even question the prevailing truth about the past (cf. MAETZKE 1986: 265f). Consequently, Eggers’s ideas never gained serious recognition among German archaeologists.

These two examples (chosen in a biased way, I admit) the consequences the relation between hierarchised power and knowledge has on the development of academic debates. The analysis of these examples also shows that too much thinking can be quite dangerous. In saying so, I am reminded of the words an “important” archaeologist once addressed to his young students. Characterising “a good archaeologist”, he remarked: ‘The archaeologist must be strong, healthy, fit and resistant to changing and difficult conditions out in the field but ... s/he does not need to think’. A thinking archaeology student, or, even worse a thinking archaeologist, poses too large a threat to the prevailing relations between power and knowledge.

The aforementioned remark applies also to the present condition of archaeological thought. Thinking is not what contemporary archaeologists cherish the most. They have learnt from their masters how to uncover the truth. But their masters never told them what the selection of research procedures should be based upon (maybe they altogether forgot the criteria?). Therefore, young scholars thoughtlessly copy the patterns utilised several years ago. I do not doubt that the scholars who invented those patterns had good and acceptable reasons for doing so, but today, as often is the case with culture, the meaning of certain signs, symbols and canons is distorted or completely obliterated. Any work based on such old patterns is bound to be ‘intellectually sterile’, as professor Bogusław Gediga once pithily stated. And if archaeologists lack critical (or any) reflection on what they do and why they do it, they do not approach new tasks but merely copy the existing ones. Consequently, they do not develop new research tools (new archaeological theories).

## “Thoughtless” archaeology and reflexive archaeology

In all discussions about Central European and Eastern European archaeologies the point of reference has always been Anglo-American archaeology and its theoretical foundations. My remarks made above have led me to a provocative idea of an entirely different classification – into “thoughtless” archaeology and reflexive archaeology<sup>2</sup>. I can already imagine torrents of accusations of unreasonable evaluation. But is continental or German archaeology not an evaluative term as well? Is atheoretical archaeology not an evaluative term? Every categorisation is evaluative.

Previous categories were related to trends and approaches (processual archaeology, contextual archaeology, “logicist” archaeology) or geographical and political criteria (Soviet archaeology, German archaeology). This time the evaluation is related to certain relatively coherent concepts or scholarly traditions which aim at discovering the past. Postmodernism has questioned the existing concepts of truth. On what grounds can we say that culture-historical archaeology is bad? It only propagates one of many possible images of the past. On what grounds can we say that this picture is worse than the picture inspired by phenomenological reflection? Postmodernism assumes tolerance for other images of reality. Let us then be tolerant ... towards the followers of culture-historical archaeology (read “German school of archaeology” but not only that) as well. This proposition, however, does not imply unconditional tolerance.

Let us now focus on the categorisation suggested in the subtitle. In scientific life, irrespective of the views people hold, we need evaluations, commentaries and opinions. In a departure from traditional “pidgeon-holing” let us apply yet another Postmodern postulate and look at people, their decisions and activities in specific historical contexts. Let us look at scientists as individual social agents from the perspective of the contexts they have worked/are working in, the objectives they choose, their understanding of social needs and research procedures. Let us look at them individually and ascertain whether they are aware of the theoretical implications of the theories they apply. We will discover that there are many who simply copy old patterns. When analysing their works, we will find no theoretical reflection whatsoever. This situation needs to be diagnosed and analysed by all those who really care about the standards of archaeological debates and by those who are involved in the process of education of prospective archaeologists.

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<sup>2</sup> In this case in a slightly different and more general meaning than that suggested by HODDER (1999).

On the other hand, we will discover that in addition to such “thoughtless” archaeologists there are many self-reflexive archaeologists. A deeper analysis of their works can produce surprising “discoveries”. It may turn out that there are many interesting ideas in the literature of the subject which have never been noticed or acclaimed, cited or developed. We will be amazed at the enormous potential of archaeological thought lost due to an unreasonable focus on the leading proponents of archaeological debate or superfluous categorisation. From this it follows that contemporary theoretical discussion is stretched between the opposite poles of dynamic theoretical archaeology and ossified “atheoretical archaeology”. I do not doubt that this picture is highly distorted, and it would be more appropriate to talk about a *continuum* between these two poles (cf. also BIEHL – GRAMSCH – MARCINIAK 2002a: 28). The example of Polish archaeology should suffice to illustrate my point. Apart from a vast group of archaeologists who give no thought to what they do, there are some who consciously apply Kossinna’s method, culture-historical archaeology in pure form or with Marxist, Functional or Processual inspirations, and even some who are dedicated followers of Postprocessual archaeology (see KADROW in this volume). A detailed analysis of publications composed in Poland would reveal an even wider range of ideas, most probably also a category of archaeologists who “think that they think”. And again we could give a lot of examples of apparent understanding of new theoretical inspirations (cf. RĄCZKOWSKI 2002: 12f).

Summing up, it is worth noting that the world of archaeologies in Central and Eastern Europe is very diversified. It is not only culture-historical archaeology but also a wide spectrum of individual approaches. Many of them are based on culture-historical archaeology but they still draw their inspiration from other trends (cf. BIEHL – GRAMSCH – MARCINIAK 2002b). Not to differentiate between them is an unfounded generalisation.

## Conclusion

I have committed many sins in this text, expressing my doubts about widely accepted concepts. Let me sin once again in the conclusion. The dispute sparked off by Postmodern theoretical archaeology stressing the culture entanglement of archaeology has borne fruit in the form of publications on the political, social, and ideological contexts of archaeology in different periods and countries. These are studies on the history of archaeology (or history of archaeological thought). I must admit I feel uneasy about them. Archaeologists know perfectly well how

to apply tools, concepts and theories delivered by Postprocessual archaeologies (cf. THOMAS 2000) in the analyses of archaeological material and they also know how to construct attractive images of the past. However, I wonder whether they know how to write about the history of archaeology. It is an altogether different field of historical studies. It requires different competence, that of a historian (for example TOPOLSKI 1984; 1996). Without it the result might fall short of expectations – paradoxically the works on the history of archaeology written by Postprocessual archaeologists might be burdened with commonsense thinking and factographical approach to historical events.

In the studies of the history of archaeological thought we need to focus on comprehensive analyses of individual achievements (cf. BIEHL – GRAMSCH – MARCINIAK 2002a: 30f; see also PARZINGER 2002; STOCKHAMMER, this volume). Let us try to liberate ourselves from the overwhelming influence of general concepts, paradigms and archaeological schools. Paraphrasing FLANNERY (1967), we should reach the individual archaeologist behind archaeological theories, schools and paradigms.

I do not remember any more of my sins or perhaps I do not want to remember them. And although I made an honest confession I do not think I can expect absolution.

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## Biographical Notes

### **François Bertemes**

Following studies at the *Cours Universitaires* in Luxembourg and at the *Universität des Saarlandes* in Saarbrücken, François Bertemes finished his PhD in 1985. In the following years he participated in the research projects in Kamid el Loz, Lebanon, funded by the German Research Foundation (DFG), and in Drama, Bulgaria. Since 1999 he holds a chair for prehistoric archaeology at the *Martin-Luther-Universität* in Halle/Saale. He conducted the excavations of Neolithic circular enclosures at Goseck and Pömmelte (Germany) and the subsequent reconstruction of the ditch and palisade system on the site at Goseck. Since 2006 he is head of the excavations at Tavşan Adası, a Minoan Bronze Age site near Didyma in Turkey.

### **John L. Bintliff**

John Bintliff (London, England 1949) is Professor of Classical and Mediterranean Archaeology at Leiden University, The Netherlands. He studied Archaeology and Anthropology at Cambridge University, where he also completed his PhD in 1977 on the (pre)history of human settlement in Greece. He was Senior Lecturer in Archaeology at Bradford University, where he taught from 1977, then moved to Durham University as Reader in Archaeology in 1990, where he taught till moving to Leiden in 1999. In 1988 he was elected a Fellow of the Society of Antiquaries. Since 1978 he has been co-directing (with Cambridge University) the Boeotia Project, an interdisciplinary programme investigating the evolution of settlement in Central Greece, widely-recognised as one of the most significant regional research programmes in the Mediterranean region. Since his first paper in 1975, he has published on average 4.5 academic publications per year, amongst which are 16 books. Almost all his publications since 1975 are available to be downloaded (contact: [j.l.bintliff@arch.leidenuniv.nl](mailto:j.l.bintliff@arch.leidenuniv.nl)).

### **Alexander Gramsch**

Alexander Gramsch received degrees from Cambridge University, UK, and Leipzig University, Germany. Currently he is head of the Museum Herxheim, home of the “LBK cannibals” ([www.museum-herxheim.de](http://www.museum-herxheim.de)). His research interests include the archaeology of ritual, and the history and epistemology of archaeology as a discipline. He has done work on topics such as Bronze Age cremation rituals

as social communication, on the Bell Beaker phenomenon, and on the ritual practice of TRB barrowbuilding. He has edited *Vergleichen als archäologische Methode* and published his PhD thesis as *Ritual und Kommunikation* in 2010. He has worked for private archaeological companies and the State Archaeological Service in the Palatinate, lectured at universities in Berlin, Freiburg and Leipzig, organised conferences for the German Theoretical Archaeology Group (T-AG), and was part of a research project on social interpretations in archaeology at Basel University, Switzerland. Currently he is also editor of *The European Archaeologist* and one of the editors of *Archaeological Dialogues*.

### **Joachim Herrmann**

In 1958 Joachim Herrmann finished his PhD at the *Humboldt Universität* in Berlin (East). In 1969 he was appointed professor at the *Akademie der Wissenschaften der DDR* and at the same time became head of the newly created Central Institute for Ancient History and Archaeology at the academy. As such he was the most prominent archaeology manager in the GDR. His recurring references to the works of Marx and Engels were not only lip-service to the regime, he was also a member of the ruling party *SED*. In 1982 he became a member of the *Deutsches Archäologisches Institut* (West-Germany). His publications include *Die Slawen in Deutschland. Geschichte und Kultur der slawischen Stämme westlich von Oder und Neisse vom 6. bis 12. Jahrhundert*. Veröffentlichungen des Zentralinstituts für Alte Geschichte und Archäologie der Akademie der Wissenschaften der DDR vol. 14. Akademie-Verlag, Berlin 1985. Herrmann died in 2010.

### **Slawomir Kadrow**

Slawomir Kadrow (born 1956) is professor of archaeology in the Institute of Archaeology and Ethnology at the Polish Academy of Sciences in Krakow and at the Rzeszów University. His main interest is the Copper Age and Early Bronze Age in Central Europe. He is also editor of *Sprawozdania Archeologiczne*; member of *Deutsches Archäologisches Institut*; and was holder of the Humboldt scholarship at *Freie Universität* in Berlin (1994–1995) and the University Bamberg (2001–2002).

### **Marc-Antoine Kaeser**

Marc-Antoine Kaeser is Director of the Laténium – Archaeology Park and Museum of Neuchâtel, and Associate Professor at the University of Neuchâtel (Switzerland). A specialist of the history of archaeology, his research interests

include Neolithic and Bronze Age wetland archaeology, Celtic studies, cultural and intellectual history of the 19<sup>th</sup> and 20<sup>th</sup> centuries, as well as epistemology and theory of archaeology.

### **Gabriele Mante**

Gabriele Mante was a student at the *Humboldt Universität* in Berlin, where she completed her master's thesis in 2000 on processual and post-processual methods ("Prozessuelle und Postprozessuelle Methodik am Beispiel kultischer Interpretationen. Eine exemplarische Studie anhand der neuen Befunde Arkonas") and her PhD in 2004 on a history of ideas of German-language prehistoric archaeology, published in 2007 as *Die deutschsprachige prähistorische Archäologie. Eine Ideengeschichte im Zeichen von Wissenschaft, Politik und europäischen Werten*. Since then she is supervising fundraising projects of universities and non-profit organisations.

### **Nils Müller-Scheeßel**

Born in 1970, Nils Müller-Scheeßel studied Archaeology and Social and Physical Anthropology (*Vor-/Ur- und Frühgeschichte, Ethnologie, Anthropologie*) in Mainz and Tübingen from 1992 onwards, and finished his studies in 1997 with an M.A. Subsequently, he spent a year in Newcastle upon Tyne/UK, doing a Master of Arts in Museum Studies. From 1999 to 2003 he was employed at the *Römisch-Germanische Kommission* (Frankfurt am Main) as an assistant editor. From 2005 to 2010 he was research fellow in a project funded by the German Research Foundation on the Late Neolithic in Central Bosnia. In 2010 he started a project on Iron Age settlement burials in Central Europe. In 2006 he finished his PhD thesis on the changes of Early Iron Age burial customs in Middle Europe. His Master's thesis is available as hardcover (*Die Hallstattkultur und ihre räumliche Differenzierung* [2000]), he co-edited *Archäologie als Kunst* (1999), *Soziale Gruppen – kulturelle Grenzen* (2006), *Der gebaute Raum* (2010) and *Fluchtpunkt Geschichte* (2011) and has published widely on the theory and history of archaeology, museology as well as on topics covering diverse archaeological periods.

### **Włodzimierz Rączkowski**

Włodzimierz Rączkowski was graduated in history (1979) and archaeology (1982) at the Adama Mickiewicz University in Poznań. From the beginning of his research and teaching work he was interested in theoretical archaeology and the

application of theoretical concepts in settlement pattern studies. For 15 years he carried out excavations and archaeological survey in Middle Pomerania (North Poland), managing an interdisciplinary project on changes in settlement patterns from the Bronze Age through the Middle Ages. Aerial photographs were one of the methods applied. During his postdoc research he focused mostly on applications of aerial photographs in archaeological research practices as well as in the protection and management of archaeological heritage, both within theoretical contexts. He was involved in the international project *European Landscapes: past, present and future* granted by the EU Culture 2000 programme, and recently in the project *ArcheoLandscapes Europe*.

Current research interests focuses on: theoretical archaeology, aerial archaeology and other remote sensing methods, GIS as a tool in archaeology, settlement pattern studies and landscape archaeology.

### **Ulrike Sommer**

Ulrike Sommer is Senior Lecturer in European Prehistory at the Institute of Archaeology, University College London. She is also teaching courses on Experimental Archaeology and Material Culture Studies. She has worked as curator at the Open Air Museum Groß Raden and the Museum Association of Lüchow-Danneberg, and has been a lecturer at Frankfurt and Freiburg Universities. Her research interests include archaeological theory, taphonomy and the history of archaeology

### **Philipp W. Stockhammer**

Philipp W. Stockhammer studied Prehistory and Early History, Classical Archaeology and Old History at the Universities of Erlangen-Nuremberg, Tübingen and Brussels. In 2003, he graduated from Tübingen with a master's thesis on Urnfield metal-hilted swords (printed as a monograph in 2004). He wrote his PhD thesis "Kontinuität und Wandel – Die Keramik der Nachpalastzeit aus der Unterstadt von Tiryns" (published online in 2008) at Heidelberg University from 2003 to 2008 with the support of scholarships by the Gerda Henkel Foundation and the Foundation of the German Economy. Since March 2008 he has been working as a Postdoctoral Researcher at the Cluster of Excellence "Asia and Europe in a Global Context" at Heidelberg University on the appropriation of Aegean pottery in the Late Bronze Age Southern Levant. Moreover, he has been lecturing at the University of Erlangen-Nuremberg since 2006, and he has been working as a freelance soft skill trainer for several Bavarian universities since

2007. His main research interests are the Early and Late Bronze Age in Central Europe and the Late Bronze Age in the Aegean and the Eastern Mediterranean as well as method and theory in prehistory.

**Andreas Zimmermann**

Andreas Zimmermann is professor for prehistoric archaeology at the Institute for Prehistoric Archaeology at the University of Cologne. He conducted research on several themes of the early Neolithic *Linearbandkeramik* in Central Europe, in particular settlement structures and exchange patterns of stone materials. In the last years he was also involved in developing upscaling algorithms to estimate prehistoric population densities applied for different periods of prehistory. Recent publications include *Studien zum Alt- und Mittelneolithikum im Rheinischen Braunkohlenrevier. Beiträge zur neolithischen Besiedlung der Aldenhovener Platte VII* (Rahden/Westf. 2009) and the paper “Landscape Archaeology in Central Europe” in: *Proceedings of the Prehistoric Society* 75, 2009, 1–53 (together with K. P. Wendt, T. Frank and J. Hilpert).

